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UNITED STATES OF AMERICA
NUCLEAR REGULATORY COMMISSION

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

SUBCOMMITTEE ON WASTE MANAGEMENT

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Room 1046
1717 H Street, N.W.
Washington, D.C.

Friday, April 22, 1983

The Subcommittee on Waste Management met at 8:30 a.m., pursuant to notice, Dade Moeller, chairman, presiding.

PRESENT FOR THE ACRS:

D. Moeller, Chairman
M. Steindler, Consultant
R. Foster, Consultant
S. Philbrick, Consultant
R. Thompson, Consultant
R. Wright, Consultant

R.C. Tang, Designated Federal Employee

Audience Participants:

S. Price
D. Knapp
H. Miller
R. Wright
P. Presholt
B. Verma
M. Logsdon

Audience Participants (continued):

P. Justus
J. Greeves
R. Cook
M. Knapp
K. Kim
D. Stahl

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P R O C E E D I N G S

MR. MOELLER: The meeting will come to order. This is a continuation of the ACRS Subcommittee on Waste Management. As most of you know, yesterday we heard from the DOE and Rockwell/Hanford staff in the presentations of their material that has gone into the site characterization report, and today we are going to hear about the site characterization analysis that is in progress and being prepared by the Nuclear Regulatory Commission staff.

There is much material to be covered today, and many people have reservations on flights this late afternoon and evening, so we have got to maintain our schedule, and at the same time, I want to try to add or weave into the schedule this afternoon, perhaps about 2:30 or so, an opportunity for the DOE and Rockwell/Hanford group to respond to what they have heard today from the NRC, because to some degree the NRC today, of course, is responding to what they heard yesterday in those presentations.

We are going to ask, therefore, that the NRC staff try to cut down on their presentations, time-wise. Where we have 50 minutes listed, I would like to try to keep those to 40 minutes, and if you can keep your formal presentations from 25 to 30 minutes, we might be able to pick up some time. I hope, too, that today we can concentrate primarily on the areas of controversy or disagreement.

I understand Hubert Miller will lead off for the staff.

MR. MILLER: Thank you, Dr. Moeller. We are pleased to be here before the ACRS to review with you our analysis of DOE's Hanford site characterization report. I think that before we get into that presentation, it might be useful to you to say a few words about the process that we are going through. There have been some questions about that, and I think a good way to set the stage -- in setting up this whole licensing procedure, the Commission very consciously weighed two options concerning interaction with the Department of Energy prior to formal licensing.

One option was to, in the interest or the purpose of assuring independence, to maintain a distance during the period of site characterization, and the other option was to provide a broad consultation feature in the regulatory process, so that before and during the period of data-gathering for licensing, the staff could be consulting with the Department to assure that early on the issues were identified, and early on, the question that Dr. Philbrick was asking yesterday, "How much is enough? What kind of data? Of what quality? How much is enough?" can be settled, and effectively the process that we are going through here is one of doing just that.

The high-level waste repository, of course, involves many new issues. It is very site-specific, and inherent in trying to make predictions over thousands of years there is uncertainty, and therefore there is judgment involved in the kinds of analyses that we will be doing.

This called for a flexible kind of prelicensing activity, and the specific mechanism that was identified in the regulation are this consultation; the most visible, I guess you would say, of the interactions between DOE and the NRC, is the submission of a site characterization report and then analysis of that by the NRC staff.

(Slide.)

Prior to the passage of the Nuclear Waste Policy Act, we had laid out a schedule or a sequence of events that looks like this. The effectively corresponds to what was in the procedural rule, the submission of the SCR and draft analysis by the staff, which is what we have completed this past month.

A period of public comment was then to occur on the draft analysis of the NRC, and then a final site characterization analysis and an opinion of the Director of the Office of NMSS on the programs of the Department of Energy would be issued; and then, on an ongoing basis, because this is an unfolding, investigative type of activity, there would be periodic updates by the Department of Energy to the Commission, which would also be analyzed by the staff.

As the DOE folks yesterday described, the Act has called for DOE to submit, prior to the full site characterization program beginning, a completion of a site characterization plan which has virtually the same scope and purpose as the SCR, to the NRC, and in fact, to be made available to states and the public.

The DOE will be to meet the letter of the law submitting a site characterization plan, yet, this here on the Hanford site, and it will cover, as I say, virtually the same points that are covered in the SCR.

Given this change, we have at this point not instituted a formal public comment process. The DOE SCP will be coming in about the time that we were trying to gather comments and finalize an SCA. The SCR, as you can see from yesterday's discussion, is already a dated document, so the approach we have taken is we have published our analysis as a NUREG document, and we have made notice of this in the Federal Register and have invited any comments that the public has on this, as we would any document that we would produce.

But our next step will not be to take comments and finalize this, but it will be to review the site characterization plan submitted by the DOE, and go through a process on that.

We feel that the draft analysis should be useful to the DOE as they prepare their SCP, and while it will not be possible to get resolution of all of the questions and concerns that we raise, we hope that at least a start can be made in addressing comments that we raise.

Quite frankly, the exact process that we will go through upon issuing another draft analysis on the site characterization volume is not firm at this point.

The Act did another thing: It specified pretty clearly and specifically a process whereby the DOE would obtain public comment, go through an extensive public process on their site characterization plan, and also go through an extensive process of consultation with states.

The Act also specified that the Department of Energy issue environmental assessments which treat the question of site selection, how you got to the site that you intend to characterize. This kind of process did not exist at the time that the Commission put in place this regulatory process and this period of formal public comment.

We will, this year, be revising the procedural regulations to come into conformance with the Act. And one of the questions we will have is exactly what process we will go through, and so upon the receipt of the SCP, we are certain to do another rapid review and turnaround on the SCP when it comes in.

But after that point, it is still a little uncertain. In general, I think the regulatory picture, as far as the technical rule is concerned, is a pretty firm one. As we heard yesterday, the Commission is days away from finalizing or approving the technical rules. And I might say a word about that.

For the past year or so, we have been engaged in considering public comment on the draft technical rule that was issued in July of 1981, and with the exception of clarifications and some changes to definitions, some modifications to the performance objectives, the rule has been a pretty steady target and has, I think, been -- and it still is, I think -- a good target for developing the site characterization programs, identifying the issues that we have to come to grips with during site characterization.

So I guess that what we are involved, now, in is the first step in a longer process that leads up to the day we will first begin to consider an application and begin that review, and I think the spirit with which we are here today, and with which we complete our report, was to give advice and to consider this an opportunity, really, to establish an agreement with DOE before they carry out these programs, on what we, at least, would consider necessary to be able to make findings against the performance objectives and technical requirements of the rule.

The only other comment I would make before Dr. Wright, who is the project manager for this effort -- it's just a few remarks about how we approach the job, and Bob, of course, will go into this in greater detail.

But we have been very sensitive to a number of things. First of all, to keep our sights on licensing and not ask for more information than what we feel is going to be needed to make the licensing findings; and to turn it around, not to settle for or to attempt to assure that we have no less than what is needed for making the licensing assessments. And we can go into some of the specific methods.

I think Appendix C came up yesterday as one of the specific approaches that we took to the analysis of that Appendix. It was an attempt on our part to try to piece things together, make sure that we are approaching this matter not in piecemeal fashion, isolate just on waste package or geochemistry or hydrology, but what will be needed to assure that the overall system can be analyzed, and that we have assurance that the EPA standard will be met.

Also, in our organization --

(Slide.)

-- just in the way we organized our review, there are three branches within the Division of Waste Management that were contributors to this effort: two technical branches, the branch that is headed up by Dr. Michael Bell, which has the waste form and waste package technical area, and also has the technical area of performance assessment, and Mike has had responsibility, of course, for development of regulations. That's the high-level waste licensing branch. My branch, the high-level technical development branch, has the technical disciplines of geology, hydrology, chemistry, mining, engineering, design; and also the lead on the projects, the reviews of the site characterization plans, are in my branch.

Now, because the technical problem doesn't sort out quite that way, and because there is the interrelationship between these various issues, we formed a team, and Bob will describe this in more detail later, to pool from within these branches, and in fact from the Office of Research,to assure that even in the way we were organized, approaching this from a systems point of view.

The last remark I would make, I guess, is that we have a sense of urgency, and I think that DOE does, also, to begin the process of face-to-face discussion and detailed technical exchange that is appropriate now to settle as best we can for the first steps that will be taken during the site characterization activity -- settle what is going to constitute adequate data-gathering programs.

MR. MOELLER: Thank you. Are there any questions for Mr. Miller?

Dick?

MR. FOSTER: In view of the revised overall procedures triggered by the Act, do you still plan on issuing a final site characterization analysis? Will a final document be issued?

MR. MILLER: There will be a final of some sort, but this is an unfolding, evolving process, and whereas we may call something "final," we know that that will be just -- that will occur at the beginning of a longer process, and as any investigative work, later steps, are not known, and in fact are determined by what you learn from earlier steps, and so what we see is an ongoing process of consultation and interaction with DOE, all the way up to licensing.

So there will be a final site characterization analysis, but there will be after that time an ongoing exchange.

MR. FOSTER: The reason for asking it was that in your old plan, the so-called "Final SCA," I think was to include a recommendation by the Director of NMSS as to whether or not the site looked good enough to continue characterization --

(Slide.)

-- which I view as a pretty important decision.

I am wondering whether the current plans still have that NMSS recommendation decision in them, and if so, what the timing is.

MR. MILLER: The only answer that I can safefy give you now, Dr. Foster, is that we will still issue a Director's opinion. The rule is still there and still calls for that opinion, but things have changed somewhat.

I think that the effect of what we have done in this review is to say that at the current time, with the information we currently have, we do not have the basis upon which to say that that site will not work. It might fail, but we don't have the basis now. That is an implied -- that is implied in this document.

We are just uncertain as to how we will complete this process.

I would like to say one more thing, and that is in response to your request, Dr. Moeller, which I am sure we can accommodate, and that is to concentrate on points of difference.

We heard a lot of information for the very first time yesterday, and we are very pleased that very much of it seems to be addressing points that we were concerned about, but we did hear it for the first time yesterday, and I think the presentation folks will not dwell on points that are obviously being addressed. It just adds to my sense of urgency to get out to Hanford or have Hanford folks come and visit us, because there are programs ongoing right now about which we have questions, but we should be able to accommodate you.

MR. MOELLER: Okay. Why don't we move on, then, to Dr. Wright, and he will present an overview of the SCA.

(Slide.)

MR. WRIGHT: I appreciate the fact that these letters may not be entirely visible in the back of the room. I have provided to Ms. Tang some additional copies of the Vu-graphs which I believe are up here on the table, in case they would be of assistance to the folks in the back rows.

(Slide.)

My intention is to talk about three items this morning. However, as a result of the excellent presentations of geology, particularly, by Dr. Price yesterday, I won't spend too much time on the initial item. I will chat briefly about certain geologic features that are of particular interest to us, and then go into how we went about the review of the SCR, and then deal with some overall impressions of the SCR.

(Slide.)

I think I will skip the next Vu-graph, which shows the Columbia Basalt Plateau, and turn to this one, in part because of some interest shown yesterday in the matter of fracturing. And you will recall that typical flow has the flow bottom, which is characterized by columnar jointing. The more or less vertical joints in place is transected by horizontal joints. Above that, the entabulature, as the geologists call it, are the dense interior, and above that, the flow top, which is the more permeable section of the flow.

(Slide.)

These features are characteristic to a greater or lesser degree of most basalt flows of this type, since it has to do with the way they cool.

The SCR and other documents have quite a few pictures of flows in the Columbia Plateau with the basal colonnade, the entabulature and colonnades further up, with the flow top, in this case, toward the top of the picture.

(Slide.)

And in look g through some old geological texts, I came across pictures of similar features elsewhere, simply to illustrate the fact that these characteristics are featured in basalt flows throughout the world. Off the coast of Scotland, a flow with the basal colunnade and the Hackley entabulature. If one could imagine erosion having removed the upper part of this material, so that we got down to colonnades with just stumps or stubs left you have something that is called the Giants' Causeway off the coast of Ireland.

(Slide.)

MR. PHILBRICK: Can I ask you a question?

MR. WRIGHT: Yes, sir.

MR. PHILBRICK: You showed a section of a basalt flow. Now, is that a representation of the conditions that might be present in the Umtanum flow from top to bottom?

MR. WRIGHT: Well, I will get to that in just a moment. I think, as your question, particularly, with respect --

MR. PHILBRICK: I am particularly concerned about flow tops. What are they? Is there one flow top in the

Umtanum? Is there one in the Cohasset?

MR. WRIGHT: I could attempt an answer. I think perhaps the Rockwell people might be in a better position than I.

I think we concur that the flow top which is primarily a brecciated zone made up of solidified crusts that formed earlier, and is incorporated in more molten material below, usually vesicular, full of gas holes, is the more permeable portion of a typical flow.

MR. PHILBRICK: At the stratigraphic upper level of the Umtanum, and the same type of thing at the Cohasset?

MR. WRIGHT: Yes, that's correct.

MR. PHILBRICK: Are there any others coming down through the Umtanum or the Cohasset?

MR. WRIGHT: A flow top is a typical characteristic, a typical feature, of all of the roughly 30 flows between the land surface and 3700 feet, where the Umtanum is.

MR. PHILBRICK: No, you're not with me.

Is the Cohasset made up of a single flow? Is the Umtanum a single flow?

MR. WRIGHT: I could express my own opinion.

MR. PHILBRICK: I want the facts.

MR. WRIGHT: Well, I think that the interpretation is that the Umtanum is a single flow, the Cohasset is a single flow that had more than one phase of igneous movement, so that there is a vesicular zone toward the top of the flow, above which there is more dense interior, and then finally the flow top above that.

So the Cohasset has had a more complex history, in terms of origin, than the Umtanum.

MR. PHILBRICK: Then you could have two flow tops in the Cohasset which would be two zones of high permeability?

MR. WRIGHT: I didn't intend to say that there is only one flow top, but some distance below the flow top, 20 or 30 feet, is another somewhat vesicular zone of higher permeability, but it is not as permeable as the flow top per se.

MR. PHILBRICK: How thick is the single flow that makes up the Umtanum? Are we talking 100 feet?

MR. WRIGHT: The entire flow or the dense interior?

MR. PHILBRICK: The whole flow, one unit, which occurred at one time, which is one extrusion.

MS. PRICE: I am just trying to see if I can get a figure that would illustrate this better. If you look in your handout that we had yesterday, I think it's about in the middle of the presentation, there's a graph that shows total flow thickness for the Umtanum and Cohasset.

9:00 a.m.

MR. PHILBRICK: Well, then, what I am trying to find out is if you have a repository situated in either of those two, that should be essentially a homogeneous mass of lava, plus or minus jointing.

MR. WRIGHT: Not minus. Including pervasive jointing.

MR. PHILBRICK: Some of that stuff above the colonnade section didn't look like it was jointed.

MR. WRIGHT: There are joints all through the rock. I think it was you who inquired about the density of jointing yesterday, and if my memory serves me correctly, the impressions that I have from looking at the core and the geomechanical logs and the studies that have been made on fracture density indicate that fractures are a few inches apart.

One figure that sticks in my mind, the average of one study was 10 fractures per meter, which is roughly a fracture each four inches, and these fracture counts are primarily from drilled core in vertical holes. Since the fractures themselves are dominantly oriented in a quasi-vertical direction, this may be a low count.

MR. PHILBRICK: Under these conditions, with a tight fracture, did you lose water, or lose fluid, inside the Cohasset, below the bubbly zone at the top, and did you lose water in the Umtanum the same way, only at the top of the flow?

MR. WRIGHT: Well, speaking from what I have seen of the project -- and this is based on last week's visit -- as you may have heard yesterday, the practice is to cement off the highly permeable zones that are encountered down to the top of the grande ronde. Below that, of course, is the Cohasset, the McCoy, and the Umtanum.

From that point downward, the flow tops are not cemented off. There is fluid loss, mud loss, but where that loss is taking place is not known. It's probably taking place --

MR. PHILBRICK: In what part of the flow would you have the fluid loss, unless it was at the flow top?

MR. WRIGHT: The assumption is that the fluid is lost in the flow tops. That cannot be demonstrated.

MR. PHILBRICK: I am still not getting a clear picture as to whether you believe that these flows, minus the top, are tight, impermeable.

MR. WRIGHT: It depends on the confidence one places in the figures that are released on horizontal hydraulic conductivity. Those figures indicate very low conductivity, 10^{-13}.

MR. PHILBRICK: If you lost fluid, what does that do to those figures of conductivity?

MR. WRIGHT: If you lost fluid in the dense interior, it would call 10^{-13} meters into question. It is believed that the losses in the flow tops where the values are supposed to be 10^{-7} meters per second.

MR. PHILBRICK: But you don't have the facts to say that you have absolutely tight rock below the flow top in the Umtanum, and a tight flow top in the Cohasset.

MR. WRIGHT: Well, let me explain it this way: The values that have been obtained have been obtained in the hydrologic testing, and indicate that that is an extremely permeable rock in the dense interior.

We have some question about these calculations, because they are basically point values that have been taken in a limited number of holes, in a limited number of locations, and it is not clear that the areas that have been tested are representative of the large mass of the rock.

MR. MILLER: At this stage in the site characterization program, that is the uncertainty, one of the major uncertainties. I think DOE said that yesterday, and I think that's what we are saying.

The programs are ones that are going to have to get the facts on the point that you are asking, Dr. Phillerick.

MR. PHILBRICK: Thank you.

MR. MOELLER: George?

MR. THOMPSON: How can we ever find this out until the site itself is explored and drilled?

MR. WRIGHT: Well, I think yesterday we got an inkling of the approach that DOE is considering and it is one that we certainly feel is one way of getting at it, and that is to develop openings underground, particularly openings of sufficient size so that one can test the bearing strength of this rock, and also observe whether any water is leaking through the fractures.

There is no question there are a lot of fractures. On the other hand, we understand that most of the fractures are filled, but they are filled with minerals like clays, which are not known for strength. If you have an unfractured piece of basalt, like that, that's some of the hardest rocks around; there's no question about it.

One of the things that concerned us in the SCR is the fact that the design criteria for the repository, the underground repository, used strengths on unfractured rock, 29,000 pounds per square inch. It did not take into account the large number of fractures that are present, and one of our concerns is flawed, the mass rock strength actually is, and this can only be answered after one gets underground and at the time the SCR was issued, the SCR treated the underground testing in a very superficial fashion, so that it wasn't until yesterday that we actually had knowledge of some of the plans that are going on now.

In my judgment, it isn't until one opens a full sized span of 20 feet and sees how that rocks performs as to -- that one will know what the mass rock strength is.

One of our consultants said there is no question the basalt is strong. The question is the strength of the fractures.

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MR. MOELLER: Okay, let's go ahead with your overview, and of course, some of this hopefully we can cover in the next presentation.

MR. WRIGHT: How did we get organized? What was in our minds as we set out to review the SCR? Of course, we didn't know what the SCR would contain, but we had some clues. We had been out to the site and knew some of the problems that were being examined. 10 CFR 60 itself speaks of certain things that are to be include in the SCR.

We had turned out a Regulatory Guide 4.17, which contained suggestions for the form and content of site characterization reports, so we had a pretty fair feel and we knew that a number of technologies were involved. As Hub Miller mentioned, we decided to divide our review team, our in-house review team into seven review areas: one headed by Paul Prestholt dealing with geology and tectonic stability, seismic risks, earthquakes and that kind of thing; one headed up by Dr. Tik Verma concerned with groundwater flow and hydrogeology; a third headed up by Dr. Phil Justus involved in geochemistry, concerned with what you might call the natural setting or, in terms of the isolation system, the part of the isolation system provided by nature; then two areas, one headed by John Greeves in repository design; a second headed by Bob Cook in waste form in package, having to do with the man-made, the engineered barriers; then one headed by Dr. Mel Knapp, which

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is concerned with performance assessment, attempting to knit the whole thing together; finally, a seventh headed by Dr. Regis Boyle concerned with institutional and environmental matters.

The reason I ticked off these individuals' names and their subjects is due to the fact that that is the order in which you will hear our presentation today. I will be immediately followed by Paul Prestholt. We had planned no presentation on institutional and environmental matters, but Dr. Boyle is here in case something in that area comes up.

These chiefs, or sub-chiefs, we call group coordinators. During the review period we had meetings of the group coordinators every week at a minimum, and frequent individual meetings in addition. As Hub mentioned, we relied not only on the resources in our own branches, we had considerable assistance from the Office of Research, from the Nuclear Reactor Regulatory Group, we had assistance particularly in the area of seismology and earthquake hazard and in the area of quality assurance.

In addition, we felt that there were certain specialties or certain disciplines in which we needed additional outside support, so we engaged some consultants and contractors to assist the individual review groups.

I will ask the group coordinators as they give their presentations to you today to introduce any contractors that may be present.

Okay, so much for organization.

(Slide)

How did we then get our thinking caps on to take a look at the SCR? We reckoned there were 40 questions that had to be addressed. The first question was does the SCR contain materials that are prescribed for Site Characterization Report in 10 CFR 60? Secondly, does the SCR identify potential licensing issues? Third, does it give a good description of the investigations of what has been found, and then, what does it say about the programs and plans?

(Slide)

I will run through each of these questions, describing them in a little more detail and explaining in very abbreviated form our general conclusions.

The first check was to see completeness with respect to the requirements of 10 CFR 60. After going through the SCR a couple of times, particularly in the design area, it became apparent that one item required in 10 CFR 60.11 was not present. It was thought during site characterization one would be poking holes in the ground and perhaps be putting down shafts. The Commission was concerned that these not provide pathways for rapid movement of groundwaters containing radionuclides coming up to the surface, so that is why this requirement was written in.

MR. PHILBRICK: Then your assumption is that you cannot seal them, that you can't seal the holes.

MR. WRIGHT: This is not an assumption of that nature; it is a requirement that the Department of Energy investigate the implications of the penetrations of the repository host rock by vertical passageways and to take into account what provisions or advise us what provisions they are considering to ensure that these do not provide pathways, along with the appropriate quality assurance programs.

So having determined that this was lacking, it was decided to move quickly to advise DOE, which we did by letter early in January. A response on the subject has come back in two parts. The second part has just been received and that area is under review at the present time. That was an example of NRC communications with DOE about a perceived inadequacy of the SCR.

MR. PHILBRICK: Did they indicate in those letters that they could perform and install --

MR. WRIGHT: John, would you like to address that later when you speak?

MR. MILLER: The short answer is yes, they did, and we are reviewing the details of that.

MR. WRIGHT: Yes. If you wish to hear more, John Greeves can address it.

(Slide)

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MR. WRIGHT: We knew that the question of licensing issues was going to be an important one, so beginning almost a year ago, the NRC Staff put together what it considered to be potential licensing issues. Now, "licensing issue" is a term that is used with a specific meaning, i.e., a question about the site that needs to be addressed at licensing time. Note, I did not say closed out or finally resolved, because particularly in terms of the natural environment, it may be impossible to bring to absolute final closure to everybody's satisfaction all the questions about the natural environment.

On addition to that, our efforts about a year ago, we decided as time approached for the SCR to arrive that we needed to provide a more rigorous definition of licensing issues, so an exercise was gone through during August and September of last year in which we pretended that a unit of water, groundwater flowing downstream came into the disturbed zone surrounding the engineered system, passed through the backfill, the packing around the canister, into a canister, into the waste form, attacked the waste form, picked up a radionuclide and exited through these various components.

At each interface the question was asked as to what were the conditions and the processes that would be involved in this movement.

After we got done asking those questions about each of these elements that is involved in a total performance of

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the system, we put them all together, and as you might imagine, it was at least a shoebox full, if not more. There was quite a bit of duplication, redundancy, so we weeded the duplications and redundancies out and came up with a final list, which is presented in Appendix C of the draft site characterization analysis, which contains not only this listing of issues, it also contains in detail the process that I just described from which these were derived.

So when the SCR came in, one of the early questions was, hey, have the issues been properly identified? We noted that there were 15 issues. We also noted that there were many more work elements. Since the work elements seem to be an embellishment of the issues, we elected to consider issues and work elements collectively as issues presented in the SCR.

Upon inspection, we found that avoiding questions of semantics and splitting and lumping and so on, if you looked at the total technical area covered by the SCR issues and looked at the total technical area covered by the NRC issues, the results indicated that substantially the same technical material was being covered.

MR. MOELLER: Martin Steindler.

MR. STEINDLER: Is water transport the only transport mechanism that you looked at in that analysis, and do you feel that is sufficient?

MR. WRIGHT: It is certainly the -- it's the only one

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I can think of at the moment. I don't know of other means of transport that were considered credible or likely.

MR. MILLER: We concentrated on the post-closure. I don't know if you are referring to pre-closure and gas escaping through ventilation and so on.

MR. STEINDLER: Nothing as devious as that. I just wondered if the scope of what you considered was credible.

(Slide)

MR. WRIGHT: The next question was does the SCR adequately describe the present level of knowledge and adequately describe the uncertainties? Here we had a bit more of a rocky road than we did in the case of the issues. There are a number of statements in the SCR, particularly concerning elements of suitability of the site, specifically dealing with radionuclide solubility, tectonic stability, groundwater travel times, that appear to express a greater level of confidence in the site than the NRC Staff could see upon examination of the same material.

This has been pointed out in our analysis. It was alluded to indirectly yesterday when some of the speakers indicated that the SCR had not particularly addressed the level of uncertainty about questions. To us you cannot separate the two, and if I were to describe this in terms of a metaphor that is suitable to springtime, I would be inclined to say that the SCR program is the top of the ninth in the baseball

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game, with the home team well ahead.

Our viewpoint upon looking at the same information was that were in earlier innings than the ninth and the home team might have an advantage but the outcome of the ballgame might still be in doubt.

MR. PHILBRICK: What specifically is in doubt?

MR. WRIGHT: Well, as you will see in the following presentations --

MR. PHILBRICK: If that is going to be covered then, all right.

MR. WRIGHT: It will be covered later, but basically in every one of the technical areas, particularly in those -- I was going to say dealing with the natural setting, but that's not quite accurate. I would say in each one of the technical areas there are uncertainties which we feel are not adequately assessed in the SCR and which need to be addressed in the plans.

MR. STEINDLER: Are you able to provide either your own little group or Rockwell or somebody a reasonably coherent description of how you went about assessing data for accuracy?

MR. MILLER: Yes.

MR. STEINDLER: That's a loaded question. A lot of people cannot do that. And I find that perfectly acceptable, but since there is apparently some sort of a flap where you folks look at essentially the same thing and one says it's

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sufficient and the other one says it isn't --

MR. MILLER: I think that's exactly what folks will be addressing today, is the basis for the differences, and out of that will come how they looked at it.

MR. STEINDLER: I guess what I'm asking is: prior to the reviews that you folks went through of the SCR, did you get together all of your committees and say here is uniform way of looking at the data you are about to get?

MR. MILLER: Yes, there is a review plan that we didn't bring in --

MR. STEINDLER: I have seen that.

MR. MILLER: The general guidance is to put yourself into the -- project yourself forward to 1988. You are going to receive a license application and you are going to have to draw independent conclusions upon data and data analysis as to whether or not the findings that have to be made -- or on the findings that have to be made on the technical role.

There was a question you were asking yesterday, Dr. Steindler and Dr. Philbrick, about how much data is enough, how many measurements do you need. Dr. Knapp in particular will cover the kind of mathematical modeling and performance assessment that we have made attempts to do to keep some perspective and get a handle on when is enough. I think we are still early enough in site characterization and

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there are enough uncertainties about the natural setting that we don't have yet a rigorous model that we can count upon to calibrate, but we are constantly striving through application of these models to come up with an answer.

MR. STEINDLER: I gather the key word you have just said is "independent." That is the review of the data in the licensing area is an independent review; is that right?

MR. MILLER: That's correct.

MR. WRIGHT: Finally, about the plans. I have alluded to some plans that appear to be lacking: for example, plans for sealing the exploratory shaft. I mentioned in situ testing, in which there is just a bit of flavor of what might be done under ground rather than any sort of systematic plan. Some plans appear to be on target. Redirection is recommended in the SCA for certain other plans.

(Slide)

So in summary, one could say about the SCR first of all that it's a well-organized document in the sense of putting a description in Volumes I and II, leading up to development of issues, particularly in Volume III, plans for resolving those issues also in Volume III.

It generally follows the scheme, and that was the scheme suggested in Reg Guide4.17, and we view it as a three-volume work that represents a large amount of sound investigative

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work. We have some differences about where to go from here, but it provides an excellent basis for further dialogue between the NRC and the DOE on the kind of a program to efficiently address licensing needs.

(Slide)

Now I would like to say a few words about the suggestions and recommendations that we put into the SCR. The investigation on the licensing and construction and operation of a high-level waste repository is very much a real world sort of an exercise, and it undertaken, as we all know, with a finite amount of resources, and it is on a tight schedule, part of which was mandated by the Public Law of January 7th. So if our guidance is to have any real value in the world, it must be effective. It must stay off the critical path. It must be effective with respect to results as compared with cost, cost-benefit.

Furthermore, we are not running the program; the Department of Energy is. So we cannot step into the shoes of the manager. We cannot sit down and write out a prescription for all the work that has to be done. We attempt to speak up early, as we did in the case of the sealing of the exploratory shaft. If we see a problem that after thoughtful consideration is something that appears to need immediate attention, we will bring it to DOE's attention promptly.

We tried to be complete. This is not to say that ar2joyl2

we might not have some further thoughts as we go down the road, but we certainly shook the tree in a pretty hard fashion to see what would come out.

Now, what I described to you is a pretty tall order, and we are not bigger than life but we certainly have attempted to more vigorously in following all of those guidelines.

Finally, about our presentation during the rest of the day this morning and the early part of the afternoon, as I mentioned, it will be given to you by the group coordinators in the sequence that I indicated. Yesterday we heard a large amount of interesting and exciting information about developments at the site, about plans for future work.

The group coordinators as they speak may be able to talk to you and give some comments on certain aspects of these developments and plans; however, for the most part, as Hub Miller indicated, this is our first exposure to these things, and rather than shoot off our mouths prematurely without knowledge of the analysis that went into the ideas, the thinking that went into it, the details -- which might do, in fact, injustice to the plans that we heard yesterday -- for the most part we will need to restrict our discussion to the SCR itself.

That concludes my presentation, Mr. Chairman.

MR. MOELLER: Okay. Well, it is now 9:30 and the

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schedule shows that we would cover groundwater. Do you still want Mr. Prestholt to make his presentation?

MR. WRIGHT: Yes.

MR. MOELLER: I intend to maintain the schedule today, so each of these presenters are scheduled, Mr. Prestholt for 50 minutes. Let's keep your presentation to 15 to 20 minutes and for each of the subsequent people so that we will have time to ask questions and to delve into the various subjects.

Let's move ahead.

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(Slide)

MR. PRESTHOLT: My name is Paul Prestholt. I will be giving the presentation on geology and tectonic stability.

First of all, I would like to introduce Mrs. Martha Pendleton, my co-worker on Team 5, Geology and Stability; Mr. Jeff Kimball, who was our consultant from NRR. On NRR, I am very happy to see that Dr. Alderman is in the audience. She also gave great deal of input in the review of the SCR. She was one of the prime authors of NUREG-0892, which is the SER for the power plant, FSAR that came in to the WPPSS power plants in particular.

(Slide)

In contrast to Dr. Price's very excellent presentation yesterday, my remarks are going to be to a great extent historical. It is true that we did have a meeting with the BWIP investigators last week, and we were given a great deal of information at that time. However, we haven't really been able to assimilate it and talk about it among ourselves, nor have we been able to review the documentation that we brought back with us and other documentation that has been, I believed, promised to us.

So in answering specific questions, I probably will make remarks of the new data that we have. Please keep in mind, however, that it is on the thoughtful side.

ar3joy2

(Slide)

This discussion of the geology and the geological stability of the BWIP analyzes DOE's preliminary geologic investigations in stratigraphy, structure, tectonics and seismology, and the plans to characterize the geology in the reference repository location and surrounding area are summarized in Chapters 3 and 13 of the SCR.

I will also attempt to set the stage geologically for the presentations that follow.

A little geography. This slide shows the general area surrounding the reference repository location and the repository site. The Columbia River is the outstanding feature. Three tributary rivers, the Snake River, the Walla-Walla River and the Yakima River.

The Pasco Basin is outlined with this dashed line, and the Hanford site is shown here, the reference repository location here, and this circle represents the 10-kilometer boundary to the accessible environment. The Pasco Basin is bounded to the north by a series of hills and to the south with Rattlesnake Mountains.

(Slide)

As shown by Dr. Price yesterday, the SCR identified two prime issues in the area of geology. The first issue deals with the geologic, mineralogic and petrographic characteristics of the basalts. Our review, Chapter 4 of the

ar3joy3

SDSA, deals with the geologic aspects; the petrologic and mineralogic characteristics of the basalts is dealt with elsewhere.

The second issue deals with the past, present and projected structural and tectonic processes as found within the geologic setting. We found in our review of issues, as related to the issues identified by the NRC Staff, in our particular group, as was pointed out by Dr. Wright, that with these two broad issues and the related work elements, that we felt the DOE covered the area of issues quite adequately.

(Slide)

In our review of the SCR we identified two major areas of concern, tectonics and seismicity and the stratigraphic and structural discontinuities found within the basalt flows themselves.

(Slide)

There are three statements found in the SCR that the Staff found to be particularly significant. They are that no faults have been identified on the Hanford site that would have an adverse impact on a repository construction; that the presently calculated rate of deformation poses no threat to the long-term integrity of the repository, and that a preliminary quantitative assessment indicates that the tectonic processes within the Pasco Basin do not pose a hazard to the repository.

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Concerning the statement about possible faulting within the Pasco Basin, this slide shows a number of geophysical and remote sensing anomalies that were identified by the BWIP staff early in their investigations. Again, the Columbia River, with the Yakima River down here, the Cold Creek syncline, the reference repository location, and these dashed lines represent the anomalies that were identified.

The one anomaly that was of particular interest to us in our review was the Nancy linear, shown here with these three short dashes. That is a geophysical anomaly in this area, a remote sensing anomaly as you project it toward the horn of the Columbia River.

We settled on this particular feature because of the hydrologic head difference between the area to the southeast and the area to the northwest, roughly 500 foot of head change, as was discussed yesterday by Dr. Baker. We found nothing in the SCR that indicated that DOE was planning to identify the nature of these anomalies, are they structural, and if they are structural, what structure are they? Are they faulty? Does this faulting, if it exists, pose any kind of seismogenic threat to the repository? Does it in fact help the repository by creating barriers, as may well be the case with the Nancy linear if it is structural?

These questions we felt needed answer and an adequate effort must be put forth to answer them.

(Slide)

This was the same slide that was presented by Dr. Price yesterday, and it shows an exploded view of those same linears, again the Nancy linear coming down through here, the reference repository location. I don't think I will say anything more about that.

(Slide)

In the area of tectonic models on on the general tectonic setting of the area, the determination of conceptual tectonic models is important for two reasons. One, to explain the structural evolution of the Yakima folds, the site must be compatible with the model of the region, and to predict the location of strain over the area of concern. In other words, is the site stable?

A great deal of work has been put in by the DOE investigators on this problem, and that work is continuing, as we found out last week. The Staff is concerned, however, that the investigations are not taking into full account much data that is available through other investigations, particularly the nuclear power plant investigation, investigations conducted by the Corps of Engineers and others in the area outside of the Pasco Basin, and this data should be incorporated in any tectonic model or models that are finally used to explain the tectonic processes that are taking place within the Pasco Basin.

Such areas that might be incorporated might be the plate boundaries to the northwest if they are found to be in any way related to the tectonics within the basin or possibly to the basin in Range Province to the southeast.

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(Slide)

The second part of our concern in tectonics deals with seismicity. This Vu-graph is a smaller picture of the area surrounding the RRL. Again, the Columbia River is flowing here. The site, or the RRL is located here with a 10-kilometer boundary to the accessible environment.

One feature that was identified in nuclear power plant siting as Rau, as the Rattlesnake alignment, is a fault zone roughly 120 kilometers long to its intersection with the Heit fault system, which is running in this area in the Milton-Freewater, Oregon area.

In their investigations of this particular feature it was decided by NRR that this was a continuous seismogenic feature roughly 120 kilometers long and was capable of a magnitude 6.5 earthquake. In the SCR the BWIP investigators recognized the existence of Rau. They stated they felt it was possibly a segmented feature, not one continuous feature, and they planned investigations from Rattlesnake Mountain to Wa lula Gap, which is located right here.

The Staff felt in reading this that the investigator should take into consideration the full length of the feature to the Heit Fault intersection and that the ability of this feature to generate seismic motion must be assessed and taken into consideration in the tectonic models.

Further, there has been no indication of investigations of the feature as it continues to the northwest to the Cascade Mountains. That normally is referred to as the Cle Elum-Wallula alignment. The Rau is simply one segment of that feature. There has been no proof that I know of that that is considered a continuous feature to the Cascades, but that, again, should be factored into the tectonic model.

MR. PHILBRICK: Was the Cle Elum considered continuous with this?

MR. PRESTHOLT: I don't believe so. I believe they used only the Rau portion of it.

Dr. Alderman, could you possibly answer that question?

MS. ALDERMAN: As we understand it, Cle Elum-Wallula is a larger linear feature somewhat less distinctive in character than the Rattlesnake-Wallula alignment itself. The Rattlesnake-Wallula alignment would be a segment of the Cle Elum-Wallula zone, a linear zone.

Does that answer your question?

MR. PHILBRICK: Thank you.

MR. PRESTHOLD: That is also a segment of a larger feature called the Owl-Wallula, which has been identified by some investigators to extend all the way up in to the Wallula --

MR. PHILBRICK: I thought the Cle Elum would have been to Canada going north.

MR. PRESTHOLT: The Cle Elum section is the town of Cle Elum, which is up in the Cascades and then it continues. That would be off in here somewhere, well up to the northwest. The other feature that is identified on this particular Vu-graph are the swarm earthquakes or the micro-earthquake swarm events that occur quite frequently within this general area within the Pasco Basin.

These large, dark blobs indicate areas of great activity, and that apparently is primarily related to irrigation. This area here is a large irrigation area, as is this. This particular area is a wooded island in the middle of the Columbia River. However, as you can see kind of shadowed in there, events here, here and here and here have been mapped within the general area of the reference repository location, in fact within the 10-kilometer circle that describes the accessible environment.

These features are small in magnitude, roughly 2.5 or less. Actually two negative magnitudes. They occur at depths between slightly less than a kilometer to as much as 5 kilometers. They are very shallow events that occur within the basalt itself and, in fact, do occur at the depths that we would expect the repository to be located. They are characterized by high frequency, short wavelength energy that does not readily attenuate or attenuate as rapidly within the basalts themselves since this is a more competent, higher velocity-type material than do surface earthquakes, which normally attenuate high frequency very, very rapidly.

MR. MOELLER: You mentioned some of them having negative magnitudes. I'm not familiar with that.

MR. PRESTHOLT: Jeff, did I say something wrong? All right, could you explain that concept?

MR. MOELLER: I assume a positive magnitude shakes you and a negative is a calming influence?

(Laughter)

MR. PRESTHOLT: Mr. Kimball was the seismologist for NRR on the nuclear power plant siting studies.

MR. KIMBALL: It is easier to answer this question than a Charleston question. Magnitude is a relative scale, a logarithmic scale, and it is negative compared to the base case that was developed as part of the definition.

MR. MOELLER: Thank you.

MR. PRESTHOLT: The SCR treated the swarm earthquakes very briefly, and the statement was made that a microseismic event was not expected to cause any particular problem with the underground facilities, and I don't think anybody would have any particular quarrel with a single event. However, as Sue noted yesterday in her presentation, we are not dealing with a single event, we are dealing with many, many events or a period of, say, 10,000 years, and the Staff considers these to be important possible factors in the future groundwater flow paths. They could change flow paths, they could open up new flowpaths, they could close old flow paths. They could cause all sorts of mischief over a long period of time.

Additionally, we believe that these events should be factored into underground facility seismic design. They could possibly impact such things as retrievability with horizontal emplacement. We could conceivably have spalling caused by one of these high frequency events that could block the ability to remove waste canisters from the openings that they have been placed in.

MR. STEINDLER: Is there any evidence that earthquakes of the small magnitude you have indicated have caused the effects that you are postulating?

MR. PRESTHOLT: That is one of the problems here. There is no evidence either way.

MR. STEINDLER: Is there any reason to presume that your presumptions are irrational in the sense that they have got a reasonable chance of being true, especially when you talk about long-term effects in the sense that you are talking about many, many of them on one side and you are talking about shaft and retrievability problems, which are clearly short-term, on the other side?

I am having some difficulty trying to assess whether or not the concern you have comes under the heading of Dr. Philbrick's question, whether you have more data than you really need.

MR. PRESTHOLT: I think not, in that we are not asking for any kind of investigation that isn't normally at the present time ongoing. These events are being measured. The epicenters and hypocenters are being measured to the extent possible.

We are asking that the effect of these, if it is shown -- considering that there are a number of investigators that have raised the question concerning these events, that these events have to be looked at and a determination made as to whether they will be a problem or not, particularly considering that in a licensing situation, an earthquake is an earthquake and a tremendous number of people out there are going to be considering these things very important, so we have to put it to bed.

MR. STEINDLER: I am likely to agree with that. The impression I have is that the SCR puts it to bed. The second impression I have is that you are not willing to pull the covers over it. You are apparently challenging the approach that Rockwell has taken.

MR. PRESTHOLD: We are, indeed. And if I might, I

I will ask Jeff Campbell to follow through, then, with a bit more, if you would, Jeff.

MR. KIMBALL: I think I could summarize the concern in two areas in terms of the short-term and long-term concern. In the Columbia Plateau it seems that the swarms have been repeated more than once in the locations where they have occurred. Within the RRL area there has been with 10 kilometers two locations where there have been swarm earthquakes. One is up by the Coyote Rapids, and the other is near the 200-W well area.

The short-term concern was r aised because, one, they have had swarm activity very close to the repository and there is a possibility that swarms may be a result of man's activities in the region in general. Paul mentioned the irrigation as one possible reason for swarms in the 2-W area. The 200-W well itself might be one explanation of why those swarms have occurred, and the short-term concern basically is when man goes in there and mines out the repository or has any kind of influence there, that they may induce seismicity.

When the swarms have occurred, they have occurred with many hundreds of earthquakes. That was the short-term concern. The long-term concern is just the fact that over a long period of time, over 1000 to 10,000 years, many, many repeated swarms may add up essentially to some type of cumulative change in the fracture distribution in the repository.

MR. STEINDLER: One last point. That short-term concern seemed fairly clearly stated. Has it been transmitted to Rockwell in that clearly stated form?

MR. PRESTHOLD: I believe so, sir, yes. I might make one point about the swarms. They are earthquakes, and at least in this particular environment that means that they are caused by movement on a structure such as a fault or a relatively large fracture, and such movements are of concern, particularly when they are occurring that close to a structure such as an underground repository, and they do indicate that the stress levels within the rocks at those depths are very close to their failure point.

MR. MOELLER: Bob Axtmann.

MR. AXTMANN: What is the prognosis for being able to resolve that issue?

MR. PRESTHOLT: I believe the prognosis is relatively good, from what we heard last week. They are investigating these swarms with sophisticated seismic net. They have developed techniques to find epicenters and hypocenters to a very fine number of feet of their actual location. They are expressing an awareness of the problem and are developing those techniques to take care of it.

MR. MOELLER: If, as you say, building the repository could affect the phenomenon, are you that good at looking in the future?

MR. PRESTHOLT: I think that is one thing we have to look at for the future, and I think it is a concern, and we are expressing the concern and we are saying, hey, guys, if you believe us and if there is anything to support such a contention, it is something that you are going to have to look at.

MR. PHILBRICK: Have you got any indication that there has been surface offset?

MR. PRESTHOLT: On Gable Mountain there is Holocene offset a few centimeters.

MR. PHILBRICK: At the present time with the swarms that you have now, has there been any surface offset?

MR. PRESTHOLT: No, sir.

MR. PHILBRICK: Does this have any indication as to what the removal of the weight of the rock in the repository would have on this?

MR. PRESTHOLT: That is part of our concern over actually building the repository within these materials where the stress differential --

MR. PHILBRICK: What is the motion on these faults?

MR. PRESTHOLT: I don't believe that is known at the present time. It is very, very small. I would assume that it is more likely kind of a strike/slip motion rather than a vertical displacement motion. It is probably associated primarily with a brittle characteristic of the material and simply the adjustments of the Columbia Plateau in the basalt in this particular area --

MR. PHILBRICK: If it is a strike/slip situation, removal of the load shouldn't make a whole lot of difference, should it?

MR. PRESTHOLT: That is outside my area. Jeff? And was I right about the strike/slip?

MR. KIMBALL: Many of the swarms that have had either repeated events or big enough events, they have been able to do either single-fault plane solutions or composite-fault plane solutions. The majority of those have been up near Santa Mountains and a few in the middle of the Pasco Basin in the center. Most of the fault plane solutions range from what I will call reverse oblique slip to reverse slip. The fault plane solutions have a wide variety of possible fault planes.

In other words, the orientation of a possible fault that these events are occurring on is not the same in all the swarms. However, generally the direction of maximum compressive stress measured by the fault plane solutions is north-south, so generally they are all responding to north-south compressive stress. However, it appears that slip is taking place on a wide variety, ranging from strike/slip to reverse slip on individual small faults or small fractures.

MR. PHILBRICK: If you reduce the load by removal of rock in the repository, what effect does that have?

MR. KIMBALL: The change in the stress conditions due to either removing material or, in the case of irrigation, to changing essentially the strength, the inherent strength on a fracture, could have an influence on changing the stress there compared to the strength of the material and causing seismicity.

In other areas where there has been mining, there are rock-bursting phenomena observed in some mines.

MR. MOELLER: George?

MR. THOMPSON: What I think I hear you saying is that the effect of stress has been changed in the irrigation areas, either by withdrawing or adding water. Where did the earthquakes occur: where they are withdrawing water or where they are adding water?

MR. KIMBALL: The irrigated areas are adding water, essentially. The water table itself has risen from a few tens of feet to many tens of feet.

MR. THOMPSON: So they are not getting little earthquakes right around the bottom of wells where they are withdrawing water, or are they pumping it on the river?

MR. KIMBALL: To the best of my knowledge, the correlation of irrigation in the swarms is not exactly a one-on-one correlation. I do not know of any area within the plateau where they are withdrawing water where there has been swarm activity near the base of the well.

MR. THOMPSON: Wouldn't the limits that we have on this kind of thing -- the dewatering of the site would probably lock up the fractures, but you might expect some triggering of earthquakes at the time the water comes back into them. Of course, you will have stress concentrations due to excavations, too, but I would think the main effect throughout that area would be the dewatering, which would increase the effect of stress.

MR. KIMBALL: That would be something that would have to be taken into account, yes. The main concern here is the fact that there has been observed swarm seismicity in close proximity to the repository.

MR. THOMPSON: That is a good observation.

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MR. MOELLER: How much more do you have?

MR. PRESTHOLT: Another 10 minutes.

MR. MOELLER: Okay. Go ahead.

(Slide.)

MR. PRESTHOLT: On the subject of stratigraphic and structural discontinuities, again, there were three statements found in the SCR concerning these subjects that the staff considered significant.

They are that the general stratigraphic setting of the Pasco Basin and Cold Creek syncline is well understood, and there are no currently nonstratigraphic or lithologic factors that would preclude the siting of a repository in one of the two candidate horizons.

Secondly, the basalt flow is located more than 610 meters below the ground surface, are not subject to significant erosion, and several of the flows may have thick enough flow interiors and sufficient lateral continuity to accommodate the construction of a nuclear waste repository.

Thirdly, the Umtanum and the Middle Sentinel Bluffs are the leading host candidates within the reference repository location, and both flows are interpreted to have sufficiently thick dense interiors to meet design and isolation requirements.

(Slide.)

This is a map of a majority of the deep borehole

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that penetrate the grande ronde. One borehole that is very important that is missing is RRL-6, which would be located approximately right there. Also, the parent holes, the DC-4 and DC-8, are not shown in this particular chart. The significance of this map is that the data base for the stratigraphy and the intraflow structures is based on point measurements or point information of a borehole that big around, spread out over the whole Hanford site, plus one or two that have been located off the site.

In other words, the data are sparse.

(Slide.)

This is the cartoon that Dr. Price showed yesterday, and I would like to go into a little bit more deeply than she did.

The Umtanum flow has been -- these, by the way, are the three flow tops that have been identified by the Rockwell investigators. The Umtanum flow has been designated a Type III flow, consisting, basically, of a coarsely fractured, basically vertically fractured, basal colonnade, a more densely fractured and with vertical and subvertical fracture entabulature zone, and vesicular and rubbly flow top zone.

The Umtanum was at the time of the SCR the primary repository flow -- candidate flow, so most of our comments and investigation of the situation related to the Umtanum, and we found in our review that the Umtanum flow had a very -- had a

csk3

thick flow top that was very changeable over the whole area. In one area it was relatively thin, and then in other areas, suddenly there was a much thicker flow top. One outcrop area at Sentinel Bluff shows a very nice section of this flow. I did have a Vu-graph of it, but it was so poor that it simply was more confusing than anything else.

But it does show the flow top of the Umtanum to have significant dimples, roughly every 50 meters or so in this particular outcrop, that were connected with another little feature to these inverted fans that I will refer to in a few minutes.

Because of the problems with this flow top, particularly in borehole RRL-2, where the prediction for the thickness of this particular feature was considerably in error, we found there was something better than 50 percent of the flow was flow top, and there was 84 feet of dense interior, which is very, very close to the minimum amount of dense rock that Rockwell had set as their goal.

The Cohasset flow, on the other hand, is a Type II flow. Type II flow is a much more complex flow, and is characterized by multiple basal colonnade zones, multiple fractured entabulature zones, a flow top, and in the case of the Cohasset, a short distance below, roughly 40 or 50 feet, another brecciated or vesicular zone. That, however, does not indicate any weathering.

csk 3

So the question as to whether this is indeed one single flow or a number of flows is kind of a "glop-glop" situation that might occur as these very liquid materials are flowing, I don't think, has been settled. The Rockwell investigators, who looked at a lot more of this rock than we have, are of the opinion that this is indeed one flow, and some of the features such as the vesicular zone found below may be an inclusion of some cooler material that kind of got turned over and got stuck down in there.

The very complexity of this flow causes some problems. We did not look into the Cohasset. We did not have a great deal of information on the Cohasset at the time of the submission of the SCR, so we did not do very much in our review with the Cohasset.

We will be looking at that as we did with the Umtanum, in the future.

Now, one of the significant things about the intraflow structures, the stratigraphic problem with the flow top may not be significant or as significant with the Cohasset. I understand that BWIP is considering this internal vesicular zone to be the flow top, and that they are not taking any credit for the materials above that, that the repository will be sited below that vesicular zone, within those dense rocks. The fracture patterns, the coarser possibly larger fractures associated with the basal colonnade, the finer fracturing, the

csk4

subvertical fracturing, sometimes they even turn over and curl around, so that to characterize them strictly as vertical fracturing is not correct.

However, in fracture investigations, the BWIP investigators have had to rely primarily on vertical hole, and a vertical hole going right down the middle of one of those columns is going to show dense rock. We are just a matter of a few inches, or two-tenths of inches, on another site and we have a significant fracture. The same is possibly true, but less so, with the entabulature, and we see this in the core, that vertical fracturing and subvertical fracturing does show within the core. But is it truly representative of the amount of fracturing that is in place? And the staff does not feel that that has been determined at this time.

(Slide.)

To illustrate this flow top thickness situation, this is a very, very poor Vu-graph. It is a Venn diagram that I believe came out of SD-14. The orientation of it seems to be a little strange. None of the RRL holes are shown on this, however. The Emerson Nipple outcrop is designated here. DC-4 is here. And this DC-4 is directly to the north of the RRL, more or less on the border. RRL-2, which is suggested to be down here. Now, we know that RRL-2 has actually a thicker flow top than is represented here at Emerson Nipple, so coming down this direction, you would continue the very thick flow top.

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You will notice in this area with DC-8 and DDH-3 over toward Richland that again we have the thick flow top, so the feeling that this is an anomalous situation may not be strictly correct. It may be more or less a 50-50 thing, and as Dr. Price told you yesterday, they are aware of this and have taken this into consideration in their plans for repository location.

This particular Venn diagram shows up one other situation, how the Umtanum may be considered Type III flow. You will see that here there is a colonnade zone shown, that there is a colonnade zone -- it's a little hard to see this -- in here.

In other words, there are elements of the Type II flow incorporated in the Umtanum, and this points out the fact that these indeed are not simple horizontal layered, relatively homogeneous pieces of rock; they are extremely complex environments in which to work, basically.

(Slide.)

So, in closing, the staff developed a number of recommendations for tectonics and seismicity. We recommend that all geologic data be reviewed to develop a good regional synthesis and develop one or more tectonic models that are consistent with the geologic data from the Pasco Basin and the surrounding area; that the field program be expanded as necessary, and structural geology, which of course is one of the basic elements in any tectonic model of an area, and in areas

csk 6

adjacent to the Pasco Basin, to supplement the work within the site area; to establish the maximum credible earthquake for each seismogenic structure or region that could affect the site, and to specify the appropriate ground attenuation for any credible earthquake developed.

MR. PHILBRICK: Supposed you established the maximum credible earthquake. What would you do about it?

MR. PRESTHOLT: That would then be factored into both the tectonic model, the seismicity related to that, and the causes for it; and also into the design of base you would normally be incorporated into the surface facility design, which may not be directly related to our work, but also into shaft sealing and just the structure of the shafts. And if it is important, to underground facility.

Now, there is a body of evidence that indicates that surface earthquakes, no matter how large, do not have anywhere near the impact on underground facilities that they do on surface facilities. So you're right, this would not be as important to waste handling situations as this where it would be to a nuclear power plant with the containment and all that sort of thing.

But we feel that it's important enough to the Salem situation and to a general understanding of the tectonic fabric which, of course, is what this repository is going to have to live in, to understand it and to get that amount of effort that

csk 7

is necessary to do that, and it is being done. They have the instrumentation in place now at the University of Washington seismic net, and the nets that they have themselves in place, so that this is not a large effort, and is certainly to be -- they will accomplish it.

Our point is, though, that they should take into consideration such things as the segment of Rau, southeast of Wallula Gap, and there are some other situations that might be important to this.

(Slide.)

As far as adequately characterizing stratigraphic and structural discontinuities, we recommend a well developed and well designed exploration program be put in place to define to the extent necessary and practicable. We don't want something done that isn't necessary or that is found not to be practical. The heterogeneities within the candidate basalt flows. And any uncertainties that remain after this investigation has been completed -- and there will be a number -- must be factored into conceptual models for performance assessment, ground model flow paths, et cetera, and that concludes my presentation.

MR. MOELLER: Any additional questions?

Dick Foster.

MR. FOSTER: You voiced your concern about this Nancy linear thing earlier. I don't see any specific recommendations relative to that particular feature.

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MR. PRESTHOLT: A recommendation to not only the Nancy linear, but to all of those geophysical features and anomalies that have been found, and mapped, is that for licensing they must be investigated to the extent that is practical, and that the nature of those features must be determined, or that the story be complete enough to satisfy questions concerning them.

MR. FOSTER: I guess if I were DOE, I wouldn't know how to interpret what you are really saying there.

MR. PRESTHOLT: Well, we have talked about it, and I think they know how we stand on that. The fact is that once such things are put on a map, when we come into licensing, they are going to be questioned: What is the nature of them? How are they going to affect the site? Are they structured? Are they faulting? Is this faulting going to -- how is it going to affect the movement of water through the area?

And this has to be determined, and they are, in fact, developing what I feel is one of the most innovative geophysical investigation programs, and they are specifically working on the Nancy linear at this time. I looked into that briefly when we were out there last week, and I was very, very pleased. I happen to be an exploration geophysicist.

The kind of data they were developing, and the level of their interpretations on this data -- now, if they can continue that level of effort and level of success with those

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other features, I don't think that there will be a problem. Where a feature is finally discovered to be just absolutely without reason from all of the efforts that you can put to it, then I suppose a decision is going to have to be made as to whether it's worth trying it out.

We are not necessarily recommending a whole bunch of drill holes out there to make Swiss cheese of those features, but we do feel that they have to be investigated to the extent to make the scientific community and the intervenor personnel, yourselves, and ourselves satisfied that they are not going to cause licensing problems and hazards to the public health and safety.

MR. MOELLER: Marty Steindler.

MR. STEINDLER: You don't seem to draw a distinction between the Nancy linear and any other anomalies. Do you consider them pretty much equal or equivalent?

MR. PRESTHOLT: At this time, I have to, because we don't know what the others are. At this time, what we do know is what they have discovered about the Nancy linear and its particular importance because of that hydrologic head difference. We don't see that hydrologic head difference with the others, to my knowledge, and for that reason only, it takes on a greater level of importance, and it is the one to investigate first.

MR. STEINDLER: My other question is your last

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Vu-graph had two recommendations. Have you transmitted these to DOE or Rockwell in a more site-specific fashion?

(Slide.)

That first one is the great motherhood statement.

MR. PRESTHOLT: The answer to that is yes and no. The SCA is a little bit more specific in the body of the SCA, and in our conversations last week we went into some of these things in more detail, in a conversational mode, and as we get into technical meetings where their technical people and our technical people actually sit down nose-to-nose and discuss a particular problem, it will be accomplished at that time.

MR. MILLER: Dr. Steindler, your question is essentially the same as what I interpreted Dr. Foster to ask, specifically, what do you want? What are you telling DOE to do?

And I think that in the SCA, every time we noted a deficiency, beyond stating why we thought it was a deficiency and why it was important, to the extent we thought we could do it without becoming overly prescriptive we identified the kinds of things we thought should be done, or at least considered to be done.

For example, in the Nancy linear, a combination of further geophysics and possibly some drill holes. But our fear is trying to solve the guidance problem by writing a lot of overprescriptions. And I think the only solution to this is cskll

the face-to-face interactive kind of thing, give DOE a chance to propose, and not get into our really prescribing or becoming overly prescriptive.

MR. MOELLER: Okay. Any other comments or questions?

Well, thank you for a most interesting presentation.

The next topic on the agenda is groundwater, which is going to require a little bit of time, so let's take a break now until 10:35.

end4

(Recess.)

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MR. MOELLER: The next presentation on our schedule is on groundwater. As I mentioned, we do want to cover this thoroughly because it is one of our more important aspects, and I gather that we will have a team of four people who will be discussing various aspects of this and we are ready to roll.

Bob, you are going to tell us roughly how they are going to do it, or you are just going to help them?

MR. WRIGHT: I'm just the mechanical operator.

MR. MOELLER: All right.

(Pause)

MR. MOELLER: The people who will be speaking to us are Mr. Verma, Mr. Logsdon, and two others. Who will be leading off?

MR. VERMA: I will.

MR. MOELLER: Fine.

MR. VERMA: Thank you, Dr. Moeller.

My name is Tik Verma and I will be leading this discussion about the groundwater chapter in the draft SCA.

In reviewing the groundwater materials in the BWIP SCR, NRC Staff was assisted by two technical contractors, Gold and Associates and Williams and Associates. This morning we have Mr. Jerry Rowe from Gold Associates, Roy Williams from Williams and Associates, Mel Knapp, Mark Logston, and myself representing the groundwater team.

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This morning I will talk about the importance of groundwater system and waste isolation.

(Slide)

And the major conclusions in the SCR and the hydrologic information in the SCR and the NRC Staff's analysis of the SCR data. And then finally some of the recommendations we have for the DOE.

(Slide)

It is generally agreed that the most probable mode by which radionuclides could be released from a repository is through the groundwater system. DOE recognizes the importance of our system in waste isolation and has placed major emphasis on groundwater characterization programs at the Hanford site.

(Slide)

On the basis of their investigations, DOE has drawn three major conclusions that relate to the groundwater flow system in the basalts at the Hanford site.

(Slide)

The first of these conclusions deals with the groundwater flow path from the two candidate horizons, and DOE states that this flow path is predominantly horizontal and is restricted to the grande ronde basalts. If you look at this schematic here, it is saying that water is moving from the left to the right, passing through the repository and

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getting out of the grande ronde some tens of miles away.

The second and third conclusions in the DOE SCR deal with the pre-emplacement and post-emplacement travel times, and DOE concludes that travel times in both cases are in excess of 10,000 years.

(Slide)

DOE's conclusions are based on a simple conceptual model of the groundwater flow in the basalts under the Hanford site, and secondly on selected data on hydraulic parameters, and finally, the use of hydrochemistry data for groundwater flow interpretation.

(Slide)

MR. MOELLER: By the use of selected data, you are saying they use the data that accomplish the goal they are seeking?

MR. VERMA: What I mean by selected data is that they use the data, like it was mentioned by Dr. Steve Baker and Dr. Baca yesterday, that for the horizontal permeability they use the mean value of 10^{-7}. That is not the value that is reported in the SCR, and that is not the values they have for the candidate formations, like for the Umtanum flow and for the Cohassett flow. The permeability values in there, in the SCR are much higher.

For the Umtanum it is reported as high as 10^{-4} and 10^{-5} meters per second. For the Cohassett flow top, it is

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reported as high as 10^{-5} to 10^{-6} meters per second.

MR. MOELLER: Okay.

MR. PHILBRICK: That table that was on the screen yesterday was centimeters per second.

MR. VERMA: They were meters per second.

MR. PHILBRICK: No, sir, they were in centimeters, because that enabled me to relate them to earth dam construction.

(Slide)

The NRC Staff's analysis of the SCR is divided into two parts. The first part deals with the analysis of the information that has been collected and is used in the modeling for groundwater travel time predictions, and the second part deals with the site characterization plans in the SCR.

(Slide)

First of all, let's talk briefly about the type of data they have in the SCR. Most of these data are from the small-diameter, single boreholes. These boreholes are cored and drilling mud has been used in coring of these boreholes.

The second thing about these data is that most of these data are collected through a drill-and-test sequence. When you collect the data in a drill-and-test sequence, you are always hard-pressed for time because while you are testing, your rig is sitting idle. So there is always pressure to move foward, so there may not be sufficient time for the system to

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equilibrate after you have stressed the system.

The other problem associated with these type of data --

(Slide)

-- is that when you are using a small-diameter single hole, you are only testing a very small volume of the rock. Here in a single-pump test if you have isolated a 20-foot interval for your hydraulic testing, then the volumes on each side of the well -- the diameter of the volume being tested -- should usually not be greater than 2 or 3 feet, or at the maximum, tens of feet.

So if you look at this, the volume you are testing is more or less represented by that dot. And if you recall the presentation by Dr. Baker yesterday, they have done this type of testing in 30-some wells that are scattered around a 30 or 40-kilometer area. So the measurements you are getting are these point values.

The other problem associated with these type of tests is that in looking at the data, because they are coming from a very small volume of the rock, there is a high degree of variability, indicating that there is very poor correlation. Yesterday a Vu-graph was shown and in there they showed an excellent correlation. In there what they were doing is they were comparing the data from different flows. When we are talking about when we are trying to determine a representative

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value, you are dealing with one particular flow, so they are -- depending on the number of measurements in that flow, there is a very high degree of variability in the hydraulic data in that flow.

MR. MOELLER: Mr. Verma, are you saying that although these types of measurements may be adequate for other purposes, they are not adequate here?

MR. VERMA: They are not adequate here. For example --

MR. MOELLER: And are these what you might call standard techniques -- they are standard for other things?

MR. VERMA: They are usually standard techniques, but here for the layered basalt system they cannot be considered standard techniques.

Here, for example, in the Umtanum flow, when we are comparing the data, we should only compare the data from the flow top of Umtanum flow. We cannot compare the permeability or hydraulic test data which has been collected at McCoy Canyon or the Middle Sentinel Bluffs or so on. So you should be looking at the data from that particular flow.

MR. MOELLER: Okay.

MR. VERMA: In reviewing these data we also find that there are no measurements of vertical permeability made so far, and again, in a layered system where one aquifer is stacked on the top of the others and there are several

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aquifers and you are trying to determine what kind of leakage there is, you must have a good understanding about the vertical permeability. Yesterday they were mentioning about what a low vertical permeability is. These are not major vertical permeabilities. These are inferred vertical permeabilities which have been calculated from the major horizontal permeabilities.

Another point about these data is the fact that in most of these core holes, drilling mud has been used as a drilling fluid, and when drilling mud is injected in a system, it has a significant effect on the permeability and hydraulic head of the system. None of the data we saw in the SCR has been evaluated for the effects of mud.

MR. PHILBRICK: What is the standard well-drilling procedure in that part of the country?

MR. VERMA: When you drill wells for hydrologic measurements, you use rotary drilling with reverse air, but because here --

MR. PHILBRICK: How are the irrigation wells drilled?

MR. VERMA: These are rotary wells with air. Mud is not used.

MR. PHILBRICK: And they can drill down through this?

MR. VERMA: Yes, they can drill down to the depth

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they are dealing with at Hanford.

MR. PHILBRICK: Have they gotten down as deep as the Umtanum flow?

MR. VERMA: Yes, they have gone to that depth. I also should have Dr. Roy Williams comment on this question because he has considerable --

MR. PHILBRICK: So then you could drill with water and get a satisfactory hole down at the depth you are concerned with.

MR. VERMA: You could drill with water or air or foam using soap, air and water.

MR. MOELLER: Is the use of the mud cheaper or why are they doing it with mud?

MR. VERMA: They are using mud because when they core, it is more efficient that way.

The other point about the data, because most of these data come from the small-scale measurements, these data do not reflect any effects of structural and stratigraphic discontinuities that have been observed at the Hanford site. The final thing, or the final comment about the data is the long-term measurements of hydraulic heads are also missing, with the exception of one borehole. Most of the data on the hydraulic heads is from a point measurement at the site.

MR. PHILBRICK: A point measurement. Do you mean a single hole?

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MR. VERMA: A single hole. When you have isolated the hole, at that time you measure the head, and --

MR. PHILBRICK: The point to you is a vertical consideration?

MR. VERMA: No, because we don't know whether the pressure in that hole -- what the level of the water in that hole or the pressure they are measuring, we don't know whether that is stabilized or not, whether that has come to equilibrium with the system or not.

MR. PHILBRICK: Are you coming to the conclusion what you want is a pumping test with a full array of observation wells?

MR. VERMA: Either that or piezometers, which have been installed in the boreholes and left in place so that we get a continuous measurement of hydraulic head over a longer period of time.

(Slide)

MR. VERMA: From the evaluation of these data, NRC concludes that alternate conceptual models of the hydrologic system at the Hanford site are plausible.

(Slide)

This Vu-graph I am using -- this is Paul Prestholt's Vu-graph but I am going to use it -- shows that this hydrologic system is not all that simple. So in a conceptual model when you try to oversimplify, you may not present some of these

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discontinuities and it may not represent the homogeneities that they have in the system.

The second conclusion we have with the hydrogeology of the Hanford site at this point is it is too poorly characterized to develop or defend any single conceptual model of the groundwater flow.

The third conclusion we have, that DOE's assertion that the groundwater flow in the Pasco Basin is to the southeast is not supported by the data in the SCR. If one looks at the hydraulic head distribution data that is provided in the SCR, one could easily conclude that flow could be to the northeast or to the north.

The fourth conclusion is that conclusive definition of separate flow systems in the grande ronde are separate systems is not supported by the hydrochemistry data at the site.

The fifth conclusion we draw from the data is that regional groundwater modeling has not been used to get the boundary conditions for basin scale.

The last two conclusions. One is about the sensitivity studies, and the final one is about the large range of possible travel times. Mark Logsdon will cover in detail, and the only thing I will say here is that we find that re-emplacement groundwater travel time could vary several orders of magnitude.

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(Slide)

The second part of our analysis deals with the site characterization plan. In evaluating these plans, we find that a unique conceptual model which is based on the stratified nature of the groundwater has been used or DOE is planning to use that conceptual model.

Also, we find that DOE plans to collect additional data from about 30 small-diameter single boreholes, and from about four dual boreholes and one three-borehole cluster. Again, the trend here we see is that the bulk of the data is going to c ome from these small-scale tests that are to be carried out in these small-diameter holes. The plans do not include reasonable alternative conceptual models that include hydrogeologically important geologic features that have been considered at the site.

Also, the plans do not include any large-scale measurements of hydraulic parameters. The only multiple cluster test, cluster well test is DC-16 cluster. This is located to the south of the RRL area, and looking at the area in the RRL, this test alone will not produce sufficient information about the hydraulic parameters.

Also, we find that long-term measurements of hydraulic heads at locations near or within the RRL are not included in the plans. The plans also do not make any mention about the regional groundwater modeling for the infer boundary

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conditions. From the evaluation of these plans, we conclude that additional site characterization proposed by DOE may not produce hydrologic information that will be need by licensing time.

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MR. MOELLER: Martin.

MR. STEINDLER: Are you going to address the question of whether the things that are not being done by DOE are absolutely necessary?

MR. VERMA: Yes, I will do that.

(Slide.)

Our recommendations, based on the analysis of the data in the plans presented in the SCR, we feel that the following areas should be addressed to get the information that would be needed by licensing time.

The first one is about representative hydraulic perimeter values. When you use modeling for predicting the groundwater travel times, or modeling for transport, you cannot use the mean values because then the hydrogeology mean values don't mean much.

You have to have representative values, and our recommendation to DOE is to get these representative values. They should consider some large scale, multiple well tests, in combination with the continuous monitoring of heads. In addition to the DC-16 cluster, we are recommending that a single cluster to be north of the RRL area and to the east side of the RRL area.

We realize that these cluster tests, or large scale, multiple well tests, are fairly expensive and they take a great deal of time to conduct, but as I mentioned, unless you have

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representative values, there is no way out of these tests. We are not saying that you go out and drill additional clusters; we are only saying that you consider drilling the pumping well for these clusters to use existing wells for observation wells.

MR. PHILBRICK: Are you also saying until this is done no further work should be accomplished?

MR. VERMA: No, we are not saying that unless this is done, no further work should be accomplished. What we are saying is that these tests require a great deal of lead time, and unless DOE gets started on these now, they may not have sufficient time to resolve some of these concerns that are to support the conclusions that they have.

MR. PHILBRICK: What is your opinion of the site?

(Laughter.)

MR. VERMA: My opinion of the site?

MR. PHILBRICK: From where you are standing.

(Laughter.)

MR. VERMA: My opinion of the site is that all these formations of the candidate horizon, all these flows, the flow tops, have considerable amount of water, and unless there is a way to take care of this --

MR. PHILBRICK: All the way down to the Umtanum?

MR. VERMA: Yes, sir, because you had a question, "What do you think of the dense interior in the Umtanum?" The dense interior is really dense. From the information from RRL-2

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we have data that there is a fracture zone towards the bottom of the dense interior which is quite transmissive, which has permeabilities on the order of 10^{-5} meters per second.

MR. PHILBRICK: That's based on what? On what you said will occur?

MR. VERMA: Yes, that's based on the hydrologic testing.

MR. PHILBRICK: I thought you said it wasn't satisfactory.

MR. VERMA: What was not satisfactory?

MR. PHILBRICK: I thought you had been complaining that the testing so far done by DOE --

MR. VERMA: I am complaining that the testing done so far by DOE is not good enough to take representative values. Because we have 10^{-5} meters per second probability value in the dense interior, do we take that value or 10^{-13} meters per second?

MR. PHILBRICK: So would you go ahead if you had a 10^{-5}?

MR. VERMA: Would you repeat that question, sir?

MR. PHILBRICK: Would you go ahead with further work if the actual permeability of the candidate repository horizon was 10^{-5}?

MR. VERMA: Hydrology alone is not the major consideration in deciding whether this site is okay or not.

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There is geochemistry. There is the engineered barriers and so on. If the geochemistry part is good enough that they could take care of that kind of flow, then there is no problem.

MR. MOELLER: Bob Axtmann.

MR. AXTMANN: Did you have any hint of your present judgment before the SCR arrived?

MR. VERMA: We had a workshop with Rockwell, and we reviewed the information they had available at that time, from the testing, the hydrologic testing, done at the site.

MR. AXTMANN: Did you transmit your concern at the time?

MR. VERMA: Some of the concerns were raised at that time, but the information I just mentioned from RRL-2 became available after the SCR and SCA had come out.

MR. WRIGHT: If I might interject, you might mention that the concerns about the hydrologic testing method were expressed as the result of a workshop held in September of 1981, so this concern has been expressed since that time.

MR. VERMA: Yes. We have expressed our concern about the use of mud as a drilling tool. We also have expressed concern about the small scale of the testing being carried out, and we also have expressed our concern for the lack of long-term measurements of hydraulic heads at the site.

MR. MOELLER: Now, some of the other NRC people who have appeared this morning have stated that they have -- they

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have given us the impression that DOE has been responsive to their criticisms, and that lines of communication are being established, and, you know, things look much better for the future.

Have you not experienced that same type of situation?

MR. VERMA: What I gathered from the presentation by Dr. Steve Baker yesterday, some of these concerns are being taken into account -- but from the presentation by Dr. Baca, there is no indication that these concerns are being taken into consideration.

MR. MOELLER: So you need to see more movement.

MR. VERMA: Also, we haven't had an opportunity to review with DOE what information on the hydrogeology had to be collected, since we had a workshop in September of '82, so considerable information has been gathered since then.

MR. MILLER: I think the point that Tik is mentioning right now is crucial. After we received the SCR, we had the feeling that the best interest would be served by not having the NRC staff take a long, long time to read the draft, but to give DOE the courtesy of a prompt response. We effectively stopped the workshop for a period of time, and what one sees is a lot of things happening in the intervening time, and what is also a theme throughout the comments that come up in each of the areas -- and Tik is saying it now -- we need in order to do our job, which is to raise early the concerns that we have,

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not only about the broad shape of the program, but about the specifics of the program, to have timely access to data being generated, because it is upon consideration of that that you shape these programs; and also have a timely consultation with DOE as they conduct these programs.

We area little bit behind right now, and we have got a lot of ground to make up. This is a general concern you will see. It stems from Dr. Steindler's question about what's the basis upon which we review these things. It is upon the presumption or upon the charter we feel we have to independently draw conclusions about this data, and it takes getting down to the specifics, and we hope that within a month, for example, to be out meeting on hydrogeology.

MR. MOELLER: Yes. Dick Foster, and then George Thompson.

MR. FOSTER: Have you done any thinking about this high head behind the Nancy linear hydrologic barrier? And if so, what kind of thoughts have you been thinking about that are relative to the repository?

MR. VERMA: No, we have not done much thinking along that line. We realize there is an enormous head drop, and as far as we are concerned, there is some impermeable boundary being created along the line. So, we are not that much concerned about it.

In fact, it gives us a very defined boundary condition

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on the west side of the site.

MR. FOSTER: Have you thought at all about what would happen if those pressures a few kilometers away should somehow get moved down to the area of the repository? What would this do to all your travel times, and directions?

MR. VERMA: It would change those travel time, directions, but as far as we see it, it is not yet established which way the water is traveling; the direction of the groundwater flow is not established from the limited data we have on hydraulic heads.

As I was mentioning, one could easily conclude that flow could be to the northeast, or to the north. If flow is to the north, that is our biggest concern, because that is where you are closest to the river. Also, you are very close to that Gable Mountain, Gable Butte structure, and Rockwell has pointed out that there is upwelling of water in that area. So our concern is: What if that flow is to the north that is providing a very rapid path for the water to move in that direction, and move up?

MR. FOSTER: What happens to your travel times postulated to the accessible environment if the head changes at this point, from an effective difference of a few feet to an effective difference of 400 feet?

MR. VERMA: The pressure differential is the driving force, so there is a direct relationship between the head and

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velocity, so your travel time would increase accordingly.

Mark Logsdon will be going into details on the effects of pressure differential on the velocity and travel time calculations.

There was one question yesterday about the inflow, and I will take a few seconds to go into that, too, at this point.

There is enormous head in those deep basalts, and each of these flows has a flow time which has a considerable amount of water. So when you are excavating or when you have excavated the underground repository, you have created enormous pressure differential, so it doesn't matter how low the permeabilities are. Unless the material is totally impermeable, the flow in there is going to be very fast, and that could amount to a significant amount of water going into the repository.

Let's talk about the Umtanum, for example. The heads in the flow top is about 400 feet above mean sea level. That translates into about 1500 psi of pressure differential, so there is quite a driving force, and it's not that the water would have to come from way up from Frenchman Springs. The water is in the flow top formation.

MR. MOELLER: George.

MR. THOMPSON: You pointed out the lateral homogeneity. Would it be your recommendation that in order to

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resolve these things that the kind of testing that you are suggesting, that that testing would have to be done within the candidate site --

(Slide.)

MR. VERMA: Yes, sir. We are saying within and around the candidate site. We are talking about the -- this is the slide we are recommending that they use the large-scale, multiple well pump test. The advantage you have in this large-scale, multiple well test is that when you pump from a larger well, you could stress a much greater volume of the rock, and with the help of the observation wells you could use as many observation wells -- as many as you like.

MR. THOMPSON: If that is done, is it going to come from us, the integrity of the site?

MR. VERMA: I don't believe so, sir.

MR. THOMPSON: It's clear in your mind, then, that what needs to be done is to have definitive tests at the candidate site itself?

MR. VERMA: Yes, sir.

MR. THOMPSON: And that this will not -- these holes can be sealed and will not disrupt it?

MR. VERMA: Yes, sir.

MR. THOMPSON: Thank you.

MR. PHILBRICK: I got the impression that if that's what your recommendation would be, that you believe the

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candidate site is worth investigating in detail.

MR. VERMA: I don't think I can comment or respond to this question, because, as I mentioned, hydrology is only one part of this system.

MR. PHILBRICK: Let's leave it alone. You're only working hydrology. You are only in your own ballpark. Would you go ahead and spend the money to do what you're talking about at that site?

MR. VERMA: If I were doing it, I would go ahead and carry out these multiple well tests to get the uncertainty associated with these hydraulic data reduced, so that when I use these data in modeling, I am not being wishy-washy, I could say definitely that we are talking about a travel time of X number of years.

MR. MILLER: As I said at the beginning, we feel very, very strong about our need to ask for all of what's needed, and not more than what is needed, and we are taking the systems approach and evaluating the success of this site. And as Tik said, I believe, Dr. Philbrick, that that requires you to look at the geochemistry and the other compensating factors that might be at work here.

And so, at this stage, it has been revealing that there are uncertainties, but we have not been able to turn it around and take a position that we know that there -- we know the system well enough to say it won't work, and we struggled with

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this whole question very, very hard, about the site, and the best we could say or the best we could do was just lay the facts on the table as we know them.

It is a complex site. It has got uncertainties. But I think we just did not have a basis upon which to say that we know it won't work. We are always asking that question in our own minds, but we cannot --

MR. PHILBRICK: So you don't have a negative?

MR. MILLER: We do not have a negative that we could stand up here and substantiate to you, Dr. Philbrick.

MR. PHILBRICK: This is a positive situation. I think it is good enough on the basis of information which you don't think is very good to go ahead.

MR. MILLER: I would prefer to put it as a double negative, emphasize the uncertainty. We do not have the basis upon which to say it will not work.

The potential is there for failure.

MR. MOELLER: Martin Steindler.

MR. STEINDER: I assumed that you, on the basis of your recommendations, do not believe that if DOE persisted in its current program, that they would be able to get the information that would be satisfactory to you.

MR. VERMA: Yes, sir.

MR. STEINDLER: Is it also true that if, for example, they were to drill an exploratory shaft and run some tests in either direction that they could not get the data in situ that would satisfy you if they were willing to take the risk of making their own decision, saying that it looks like a good enough site, we'll find out the details of what NRC wants to know by the time we get down there?

MR. VERMA: In answer to this question, yes and no. From those holes they are talking about from the shaft, again, you are faced with the same kind of situation that you face with the holes from the surface. These are small diameter holes, and you are somewhat limited in terms of the type of testing that you do from these holes.

Of course, you could get these type of information when you break out this, but again, that information you collect will only be from that particular horizon, that candidate horizon. You will not have the benefit of having information on the formations below, or the formations above.

MR. PHILBRICK: Why wouldn't you? You're drilling a hole. You are drilling it open. You know where you are going to lose the fluid.

MR. VERMA: That shaft is -- what are you talking about, sir?

MR. PHILBRICK: The shaft is open. It isn't cased until you get through with it. It's bare rock.

MR. VERMA: It is bare rock, but there is loss of water, and they are going to have to find some ways of stopping

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that flow.

MR. PHILBRICK: Well, we aren't getting together, so --

MR. MILLER: Dr. Philbrick, maybe DOE could better answer this, but it's a mud d_illed shaft, and they will be lining that shaft before they pump out the mud, so they will never see the face of the excavation with their eyeballs, except when they get down and break in at the given horizon.

MR. PHILBRICK: This is the same kind of a thing as a diamond drill hole; when you are using water for fluid, you lose the water in various places going through fractured rock. You have the same situation here, so you will get some information.

MR. MILLER: You will get some information, but as was discussed yesterday, exactly what formation you are going to be losing mud into, you won't know.

MR. PHILBRICK: Certainly you will, when you make the cut, and you hit that formation, and you have got an opening. Your are going to lose in that area.

Now, what I would like to know, in the Umtanum, what has been the core recovery? Is that information available?

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MR. VERMA: Yes, it is.

MR. PHILBRICK: What's the fracture frequency in the Umtanum, or do you have the channel through which you can lose water at the rate you are talking about, at that $^{-5}$?

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MR. VERMA: That is a tested fact. They have that information. The Umtanum flow at RRL-2, for example, they lost about 100,000 gallons of mud in that one foot-and-a-half --

MR. PHILBRICK: At the top?

MR. VERMA: No, it's in the dense interior. In the Umtanum from RRL-2, when they went back to hydraulically test the interval, they found that the permeabilities in that interval are something on the order of 10^{-4} to 10^{-5} meters per second.

MR. PHILBRICK: Where was this in the Umtanum?

MR. VERMA: Towards the bottom part of the dense interior.

MR. MILLER: We just received this in the past several months. We are trying to sort that out. It's very hard to put caveats on this.

MR. WRIGHT: It's in RRL-2 at a depth of 3882 feet, about 20 feet above the bottom of the dense interior, 20 feet above the bottom.

MR. PHILBRICK: So the candidate horizon has got a high permeability?

MR. WRIGHT: This is for the Umtanum over a section of approximately one-and-a-half feet.

MR. PHILBRICK: That's right. I understand.

MR. WRIGHT: And this is only one of three candidate horizons.

MR. MOELLER: Okay. Let's move on to the other

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presentations. If you are through -- or are you not through?

MR. VERMA: I have just a couple of brief comments to make about the recommendations.

We find that the effective porosity data is quite critical when you are dealing with the predictions of groundwater travel times, and it is a recommendation to DOE that more measurements of effective porosity be made at several stratigraphic locations.

The fourth recommendation is about the use of hydrochemistry data. If they plan to use the hydrochemistry data for instrumentation, this should be integrated with the more defensible head on the hydraulic parameters; and the fifth one is the alternative conceptual models. Appropriate sensitivity studies should be considered for testing alternative conceptual models. I think I skipped the boundary conditions.

Boundary conditions are also critical in any type of modeling, and these should be either inferred boundary conditions that are determined by the regional scale modeling or from the actual measurements of areas at the site.

MR. THOMPSON: Have you and DOE agreed on the model to be used?

MR. VERMA: I think Dr. Knapp should answer that question.

MR. KNAPP: So far, we have not had a great deal of success in discussing all the models with DOE. They, in earlier

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request, in model development, recommended that we wait until their models were further completed before we enter into some of those discussions.

Now, we do have a pretty good idea, based on discussion, of a site-specific basis, what some of the models are, and we are proceeding to benchmark and test some of those ourselves, but this is an item that will, I think, require a lot of attention from both we and DOE in the next few years.

MR. STEINDLER: Did you say "years"?

MR. KNAPP: Yes.

MR. MOELLER: We are moving on, then, to Mark Logsdon, and he will be covering the uncertainty in groundwater travel time calculations.

Roughly how long is your presentation?

MR. LOGSDON: Unless there are a number of interruptions, I can probably do it in seven minutes.

MR. MOELLER: Thank you.

(Slide.)

MR. LOGSDON: I am a hydrogeologist with the Division of Waste Management for the NRC, a member of the groundwater team of the BWIP project.

(Slide.)

Dr. Verma has referred to statements in the site characterization report concluding that the groundwater travel time under both pre- and post-emplacement conditions is expected

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to be greater than 10,000 years, and that the flow paths are restricted to paths which will continue through the grande ronde basalts. The previous NRC speakers, particularly Mr. Prestholt and Dr. Verma, have emphasized the principal thrust of the site characterization analysis is to analyze the sources and possible magnitude of the uncertainties that remain in site characterization.

In terms of groundwater, not only are there major uncertainties about the hydraulic heads and critical hydraulic parameters, such as effective porosity and critical permeability, but in large part because of these uncertainties and parameters there is still substantial uncertainty about even the basic conceptual model of groundwater flow for the Hanford system.

The NRC staff wholeheartedly supports the new trend by Rockwell to recognize more explicitly the nature and magnitude of these uncertainties and to begin to account for them in assessing the status of the BWIP progress and characterizing the Hanford site.

Since variability, complexity of natural materials and systems is the rule in the earth sciences, and since common sense and practical realities dictate that the data base will always be limited, the real question facing the NRC staff and the technical community is what kind of effects do the current uncertainties have in assessing the site as a location for waste emplacement.

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One of the few quantitative performance objectives in the NRC rule addresses the minimum pre-emplacement groundwater travel time to the accessible environment. The rule is specific about how that groundwater travel time is to be defined from the disturbed zone to the accessible environment. I will not address the material that's in your handout pertaining to those definitions. Dr. Knapp is better acquainted with the rule than I. If you wish to pursue that, you should probably pursue it with him.

(Slide.)

Because the SCR does not address the effects of uncertainties in groundwater travel time, the NRC staff conducted a simple analysis designed to show how sensitive the travel time calculations are to uncertainties and conceptual models and uncertainties in hydraulic parameters. The results I present today are primarily those dealing with sensitivity of groundwater travel time to uncertainties in hydraulic perimeters for a single conceptual model equivalent to that used by Rockwell in the SCR.

(Slide.)

Any modeling effort depends on certain assumptions. The model used in Appendix D comprises a series of hydro-stratigraphic units representing the dense interiors and flow tops of the basalts in sedimentary interbeds. Each hydro-stratigraphic unit is horizontal, of uniform thickness,

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laterally extensive, and homogeneous. The model extends vertically from the middle of the dense zone of the Umtanum flow to the face of the Mabdon interbed at the top of the Wanapum basalts and laterally from the point of release of the underground facility for the full 10 kilometers per minute under the rule to the boundary of the accessible environment.

MR. PHILBRICK: Now, that is a lateral distance.

MR. LOGSDON: Yes, sir. This is basically the same modeling setup assumed by Rockwell on the modeling in the SCR. As with the modeling approach of core flow, the numerical code used in the SCR results, the NRC's analysis is limited to the classical approach of groundwater flow through a porous medium, though Appendix D of this draft site characterization analysis does contain some qualitative discussion of how the results might change if the flow is primarily through a small number of discrete fractures.

Each hydrostratigraphic unit in the model has a set of hydraulic parameters which describe its properties, effective porosity, and because it is a two-dimensional system, vertical hydraulic conductivity and horizontal hydraulic conductivity, and the model has a set of boundary conditions that are related to the vertical and horizontal hydraulic radius effective throgh the system.

We are concerned with the pre-emplacement of groundwater travel time only in the rule, so the system is at

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steady state. Thus, the conditions of flow are totally determined at every point in this hypothetical system in all particles passing through the point of release of the underground facility will follow a unique flow path across the system, and thus the travel time could be calculated. However, if any or all of the hydraulic parameters or boundary conditions are changed, then the flow path will change and there will be new travel time associated with that new flow path.

All of the data used in the modeling of Appendix D of the draft site characterization analysis is taken from the Site Characterization Report or from earlier Rockwell documents cited in the SCR as the primary sources of data.

(Slide)

The calculation of groundwater travel time is conceptually simple. As with any travel time, it is given by the length of the flow path divided by the average linear velocity along the flow path. In terms of the hydrogeologic setting, we are interested in the parameters which can actually be measured or somehow estimated or determined for the system that actually exists. The flow path length is a function of the conceptual model that is used, of the boundary conditions, and of all of the hydraulic parameters that are used in the model. It will vary with any changes in any of those inputs.

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Effective porosity, to which groundwater travel time is directly proportional, has been measured for the Hanford site in only one test and in only one interval, the flow top of the McCoy Canyon. In that test the measurements are interpreted in the SCR as representing a range from that single test of effective porosities from 10^{-4} to 10^{-2}. In the modeling of pore flow in the SCR, values from the high end of that range were assumed, 10^{-2} for flow tops, 10^{-3} for the dense interiors, and the value of 10^{-4} was not considered at all although this one single test, I again emphasize, was conducted in a flow top.

The travel time will vary inversely with hydraulic gradient. Hydraulic gradients under steady state conditions are expected to be quite low, as indicated, but as Dr. Foster has pointed out a number of times, Dr. Moeller has indicated an interest in the effects on groundwater travel time with withdrawals of groundwater for irrigation purposes.

A hydraulic gradient is an ephemeral parameter for the system, subject to several possible sources of change, including not only some catastrophic event or the withdrawal from irrigation; it is also very sensitive to the buoyancy effects associated with the emplacement of waste. Consequently, we have hydraulic gradient both vertically and horizontally as part of our analysis.

Hydraulic conductivity, as I mentioned initially, for a multidimensional system must be considered both horizontally and vertically. Dr. Verma has already indicated to you for the Hanford site there have been no field measurements of vertical hydraulic conductivity.

The data base for calculation of groundwater travel time used in the SCR depends almost entirely on the data base or horizontal conductivity. Those are the hydraulic conductivity numbers that were given to you yesterday by the Rockwell people. We emphasize those are horizontal conductivities only.

(Slide)

This is a set of histograms representing the measured values for horizontal hydraulic conductivity. As represented in the SCR, the value is in gray. The actual histogram that was used as the base for this diagram was taken from the Site Characterization Report. The values in gray are the measured values available in the Site Characterization Report.

To that I have taken the liberty, since none of those values represented measurements from wells within the reference repository location, of adding to this historgram the values reported in the recently released Principal Borehole Report for RR-L-2. That is, these are values within the reference repository location. In the grande ronde they are added here in orange in the Wanapum.

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The value highlighted by the black line at 10^{-7} meters per second is the mean value indicated yesterday by Dr. Baca for all of the horizontal conductivity measurements from the site. It was assigned to all of the flow tops uniformly, to all of the flow tops in the pore flow modeling.

It is apparent to me and I hope it is apparent to you that the value of 10^{-7}, while it may be the mean value for all of the measurements that have been taken, certainly does not actually represent the mean or median value of the horizontal hydraulic conductivities over the flow tops in the Wanapum, and it is substantially lower than most of the measured values from RR-L-2, the only data we currently have available for the reference repository location.

The values indicated in cross-hatch in red was the range of values used in Appendix D to test the sensitivity of the model to variations in horizontal hydraulic conductivity.

(Slide)

More specifically, this is a graph taken from Appendix H of the DSCA. It was done for a slightly different purpose in support of our analysis, but it shows the distribution. The black dots are the means of the N number of measurements for the horizontal hydraulic conductivity for each of these layers. From my pen down, we are in the grande ronde. From my pen up, we are in the Wanapum.

In assigning values of hydraulic conductivity to the

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layers we used in our modeling, we went back to the basic data and assigned values based on the actual measurements for each of the hydrostratigraphic units that we defined rather than using the single value representing full range of values measured over many units.

Again, this emphasizes the difference between the values measured in the Wanapum basalts and the values assigned in the pore flow modeling. It is probably worth emphasizing two additional points here. The range of horizontal hydraulic conductivities, including both the Wanapum and the grande ronde, on the basis of currently available to us is nine orders of magnitude. That would be a mean plus or minus 4.5 orders of magnitude for the full range of data.

In the modeling used in Appendix D, we did not take values from the extremes of the distribution, from the tails of the distribution. Our values our mean values from within those distributions.

(Slide)

The material on the left is largely self-explanatory. A brief word before I enter into it for just a moment to explain that this is a cross-section taken from Appendix D of the DSCA. The point of release at the repository is located here at the top of the model. The model extends for 10 kilometers laterally from the point of release at the repository to the accessible environment. The lightly-colored lines are

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the boundaries of the hydrostratigraphic units assumed in the model. The dark lines are the flow paths that result for the various simulations that we conducted.

We report in the DSCA 17 different cases that we considered in the sensitivity study. In terms of the conclusion referred to by Dr. Verma, that the flow paths from the repository expected to remain within the grande ronde basalt, it is clear that some of our flow paths do remain within the grande ronde basalt; other flow paths in our sensitivity study leave the grand ronde basalt, some of them leaving the grande ronde basalt very steeply and going at least into the Saddle Mountains.

MR. PHILBRICK: How did you determine that? How did you determine that they went out there?

MR. LOGSDON: You begin with a point down here. The particle leaves here, it moves through this layer in some two-dimensional fashion defined by the hydraulic properties of the layer and the boundary conditions. It then goes through the next layer defined by the boundary conditions and the hydraulic properties, and you trace the course of the path. Every time you change the properties of the hydraulic gradients, you get a different flow path.

MR. PHILBRICK: And the least resistance to flow was vertical.

MR. LOGSDON: That is for a particular set of

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conditions, sir, but there is a set of hydraulic parameters --

MR. PHILBRICK: The least resistance to flow is horizontal?

MR. LOGSDON: That is somewhat complicated because the travel time in this case is, in fact, slightly longer than the travel time in this case, or, in fact, the travel time in this case. These are just the flow paths. The numbers that are off in obscurity over here on the right include the travel times for each of those cases. So there is a travel time associated with each flow path, and the flow paths are determined by the hydraulic properties of each layer in the boundary conditions.

MR. MOELLER: Martin.

MR. STEINDLER: Does the hydraulic conductivity enter into the travel time as a logarithm?

MR. LOGSDON: No, sir.

MR. STEINDLER: When you take the mean of an exponent, what are you doing?

MR. LOGSDON: It is geometric.

MR. STEINDLER: If it is linear, is that --

MR. LOGSDON: We are taking deterministic values of each of the inputs. This is not a probabilistic sensitivy study. We have taken deterministic sets of parameters and applied them separately. When we use the data provided in the SCR for the pore flow modeling and apply it to our 16-layer

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case, we calculated groundwater travel time in excess of 43,500 years. This is in, we think, reasonably good agreement with the conclusions of the pore flow modeling that the groundwater travel time is to be in excess of 30,000 years for those cases. When we varied the parameters for that single conceptual model, it is possible to calculate groundwater travel times which extend downwards to a low of 51 years. Both the conceptual models that are considered and the full range of hydraulic parameters measured by the Rockwell group are considered -- the groundwater travel times could range from lower than 20 years to greater than a million years.

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The NRC Staff does not believe that we know the pre-emplacement groundwater travel time at the basalt waste isolation project site, nor do we believe that we can say that the current Rockwell estimates are incorrect. Rather, we consider that because of the current high level of uncertainty about the hydrogeology of the site, no significant deterministic statements about the flow path or the travel time can be made at this time.

(Slide)

MR. PHILBRICK: Let me ask you that question. Would you go ahead and investigate the site?

MR. LOGSDON: Yes, sir, I would. I think that there is some reason to believe that a layered sequence of low

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permeability materials offer certain distinct advantages conceptually for isolating a waste system, and on the basis of the information that is available to us, I think that DOE should certainly proceed with their site characterization of this site, subject to recommendations that we have for perhaps other ways to pursue that characterization.

The recommendations to DOE are essentially the same as the recommendations presented by Dr. Verma. We believe that DOE should consider using sensitivity studies to evaluate alternative conceptual models and to help design their field test program to emphasize those hydraulic parameters which are of particular concern.

That is the end of my presentation.

MR. MOELLER: And do you have a sense of cooperation in response from DOE in terms of your particular concerns?

MR. LOGSDON: To a measure, yes. We have to evaluate the methodology proposed yesterday by Dr. Baca. The staff has attempted to do some MONTE CARLO simulations ourselves. We have encountered certain conceptual difficulties in applying it rigorously to the site and we look forward to the opportunity to discuss that and other rigorous methods of incorporating uncertainties.

MR. MOELLER: Any other questions? Well, thank you.

We will move on, then. Is it Roy Williams who will be next?

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MR. LOGSDON: That's it for us, sir. They are just here to help answer questions.

MR. MOELLER: Are there any other questions, then, on the groundwater section?

All right, then thank you.

Then the next item -- excuse me. Bob Axtmann.

MR. AXTMANN: In asking for a little more detail on that last statement, Dr. Verma, there is one conclusion that I could figure out on his last bullet, additional site characterization proposed by DOE may not produce hydrogeologic information needed by licensing time. That is not a deterministic statement.

I wonder if you could put a probability on that.

MR. LOGSDON: It depends on the sort of approach that DOE takes. Our position would be that if they pursue the line proposed in the SCR, it is unlikely that they would be able to provide the quality data that we would consider adequate for site characterization at licensing assessment time.

This is Dr. Roy Williams.

MR. WILLIAMS: I would like to comment on that. I live on the grande ronde. My town draws their water supply from it, 2000 gallons a minute, a depth of 1400 feet, so I am kind of interested in making sure that all the tests that are appropriate are conducted. It's about 120 miles east of

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the site.

One of the comments we made on this subject is -- it is a good point that you raised -- the Site Characterization Report alludes to the same types of things that Steve Baker talked about yesterday, but the plans for detailed analyses are sufficiently nonspecific that you can't tell what the additional tests will yield. Everything he talked about yesterday in terms of additional parameters that will be obtained appears to be within the realm of t he plan described in the SCR. However, almost anything would be included in the plan described in the SCR. It is a very,very general group of statements.

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MR. MOELLER: Thank you.

Well, then let's move ahead.

The next presentation is on geochemistry, and that will be made by Philip Justus. Roughly what is your time target?

MR. JUSTUS: Twenty minutes.

MR. MOELLER: Okay, fine. Proceed.

MR. JUSTUS: I'm Phil Justus, Siting Section leader and group coordinator for the geochemistry team, which I am representing today.

We have four members of the team standing by as needed. Don Kellmers from Oak Ridge National Laboratory is representing our principal geochemistry. David Brooks is next to him. Julia Corrado and John Stormer from the NRC Staff. In the audience we have Mel Siegel, who helped review aspects of the SCR and SCA from Sandia.

Rushing a presentation is a little bit like shaving quickly: you are likely to wound yourself.

(Laughter)

(Slide)

So I have elected to leave a little stubble on the slides here and there. We can always shave the points a little closer later if we have some time and interest.

(Slide)

During the presentation we will review the importance of geochemistry, discuss the Staff analysis of data and plans in the SCR and summarize suggestions to DOE.

(Slide)

The scope of geochemistry in the national program has been essentially to include the composition of the source of radionuclides: obviously, the source term, which is some mix of spent fuel and vitrified waste, as far as we know now; the composition of the principal transporting medium, which is, of course, groundwater; composition of the geologic materials that the radionuclides in the water will come in contact with, clay, host rock, fracture and core fillings.

Also, the environment of the waste package must be determined in this program in geochemistry investigations and we must understand the dominant reactions which involve radionuclides wending their way to the accessible environment.

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We agree with DOE that the most credible mechanism of radionuclides that we are most interested in regulating is by solution transport in groundwater.

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Therefore, the quantities of radionuclides that reach the accessible environment in 10,000 years depend principally on several processes: how much of each radionuclide will precipitate out as new solids, how much will remain in solution, how much of each radionuclide will be absorbed or adsorbed by surface absorption or ionic exchange processes. The end result is how much is released to the accessible environment in 10,000 years and at what rate do they leave the engineered system.

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DOE has recognized that solubility absorption of radionuclides are important factors to be considered in meeting the standards. The relevant parameters are identified. Plans and investigation have been enumerated in the SCR. The fate of any radionuclide is controlled by interactions along the groundwater flow paths as temperature and pressure varies with time, as the kinetics of the reactions change, and as the reactivity of the various host rocks and minerals present are encountered.

For example, with regard to temperature, the data are fairly clear although there is a little uncertainty that the range is as low as 50 C. -- that is ambient -- and the variations at the upper end, which depends on the thermal loading of the facility.

Regarding pH, DOE considers the principal pH is in the alkaline range, more particularly in the SCR, 8.8 to 10.1 to a mean of 9.5 Yesterday we heard, though, that based on experiments at higher temperatures, that the pH in hydrothermal systems being investigated may be as low as 6, and we would encourage that the range of considerations be extended to that which is meaningful. Redox conditions are considered by DOE to be very reducing at the site. The uncertainties on this parameter are fairly large, we think, in the sense that when we compare the measured values ranging from slightly oxidizing of .2 volt to about -.2 volt measure to calcuated redox conditions based on the presence of co-existing minerals that buffer the system of -.5 to about -.55, this is sufficiently large to cause us some concern. I will get into more specific aspects of these uncertainties in subsequent slides.

Regarding the host rocks, the lava flows themselves have been fairly well-defined mineralogically. We have, we think, in hand a tight control over the variation in the various lava flow units. On the other hand, there is much less quantitative mineralogic data from certain portions of the lava flows, and certainly from the interbeds. And as Mark Logsdon pointed out, and others, radionuclides in groundwater is likely to spend a good deal of its time in flow tops and possibly interbeds if they get that high. So we would like to see better control over the mineralogy of those host zones.

Groundwater composition used in chemical experiments is a composite. It is a composite of six sample localities throughout the Pasco Basin. A lot of these were taken over a two-year period. None of the sample localities were within the RRL, and perhaps all of them may not have been representing portions along projected flow paths and therefore the uncertainty of this matter of what is the chemistry of groundwater that may be encountered along the flow path is relatively large. Actually, I should say we don't know.

DOE has identified in the SCR the perturbations and the chemical reactions that will be imposed by emplacement of the waste.

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The solubility studies in the SCR of four radionuclides -- uranium, neptunium, plutonium and americium -- were the basis for the statement on solubility. The statement allows that the rate of release of radionuclides is sufficiently slow, the accumulations are sufficiently low that the NRC and EPA standards are met, at least in general. However, there were some simplifying assumptions made that we believe render this statement less general and less definitive than at first glance.

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With regard to source term, for example, we don't yet know what the mix of fuel and borosilicate glass would be that would contribute particular nuclides and ancillary substances that might be associated with the waste form and the waste package, such as borate and various metals that enter into the soup that might be generated.

Solubilities of radionuclides can be calculated or they can be measured directly. If they are calculated, we have to rely on thermodynamic data. The data for radionuclides, actinites in particular, are scarce. They are concentrated in the 25 degree C. range. Therefore, the data base, if this approach is taken to generate solubility data needs to be strengthened.

Concerning the speciation of radionuclides for any particular radionuclide there may be several forms in which it may occur upon being released. It may occur as a carbonate or as a hydroxide, an oxide. The solubility of each of these different species is different, and when projecting as to what the solubility of a particular radionuclide might be, it is very important to know what the most soluble igneous species will be and what the most soluble solid phase would be. Calculations that we have received so far don't consider all of the possible species. For example, in carbonates we have done a few calculations ourselves so that you can review them in more detail in Appendix U, Part 2, of the DCSA, which indicate that when carbonate species are considered, the stated solubilities for a particular radionuclide may increase by several orders of magnitude.

Concerning the redox conditions, as I have already mentioned, there are a few measurements, a few calculations with some discrepancy. The point, though, is that the solubility of radionuclides is strongly decreased by decreasing the redox potential.

So it sounds good, awfully good. It is good for the site or any site that conditions be found to be reducing or have a lower redox potential, but we will get to redox a little later. We don't know everything we should know. We don't have it pinned down so well on these calculations to arrive by indirectly or secondarily responding to redox.

The role of colloids, admittedly, in the SCR was not considered, but it remains to be considered what are the transporting capability of some colloids that may be formed perhaps by radiation effects on close-in. In the dynamic system of the underground facility near where precipitation of a particular radionuclide may be predicated to occur, can we say -- how can it be demonstrated that precipitation will actually occur? Flow conditions may be acting against what we have reviewed in laboratory considerations. It may be that by high groundwater flow rate in a particular area, sufficiently high mixing or dilution may come into play, and that, of course, would enable the solubility of the concentration of radionuclides not to reach its solubility limit, and the radionuclide may be able to transport greater distances than we might think from lab experiements, and prior to their eventual precipitation, perhaps.

The plans for validating solubility calculations were not sufficiently defined in the SCR for us to make judgments on their adequacy. That is a statement we also have for absorption matters.

The suggestions we have presented in the SCA are enumerated here. I will consider this briefly. I have already mentioned some of them.

MR. AXTMANN: Back in the colloids. What colloids have been identified?

MR. JUSTUS: I don't know of a particular colloid outside of in general organic colloids. They have only been mentioned as one possibility. Let me call upon our geochem team.

MS. CORRADO: Investigators working on the Nevada test site have identified the potential for formation of a colloid, so the potential formation of that colloid has been established by investigators at Los Alamos.

MR. AXTMANN: A pseudo-colloid?

MS. CORRADO: The pseudo-colloid would behave in a manner analogous to a colloid in that it would be transported as a particulate and not retarded as a dissolved solid, so it would behave in a manner analogous. I am not an expert in the area of the distinction between colloids and pseudo-colloids, but the analogy is there.

MR. STEINDLER: Are you planning to leave that list of uncertainties?

MR. JUSTUS: I was going to absorption.

MR. STEINDLER: Let me ask a couple of questions. You are challenging Rockwell's comments on redox conditions.

Are you saying that, especially fractured basalt, for example, hasn't been demonstrated to be a reducing medium for groundwater as it travels through it, or what is the nature of the challenge? It seemed pretty obvious, actually. It appears to me that some of the things you claim are uncertain, I thought they may have done fairly well.

Just as an example of our concern is this. Regarding the measured, or rather the calculated, very reducing potentials based on pyrite, magnetite colloids acting, that was the postulation to explain or offer in evidence a very reducing condition. When we looked for pyrite, when we looked at magnetite in a thin section, for example, we were looking to solve two parts, two aspects of the investigation. In order for redox couples to be activated, the two minerals of interest have to be exposed to groundwater, and the groundwater has to be moving between the substances.

Now, we found very little pyrite. There is some pyrite, secondary pyrite throughout the basalts, and certainly very little magnetite, but what we did see were not exposed or at least in the thin section were not presented or portrayed as occurring along fracture walls or lining vesicles or other likely flow paths. They were withdrawn from interacting, and in our opinion, it is doubtful as to what the efficacy of that particular mineral pair is in actually demonstrating or supporting highly reducing.

MR. STEINDLER: You are challenging contact and not kinetics in the presence of couples somewhere within the ronde.

(Slide)

MR. JUSTUS: No, let me jump ahead a little.

MR. STEINDLER: Oh, I'm sorry. If you are going to get to it, I will wait.

MR. JUSTUS: That is quite all right. Let's stick with redox. We are challenging this matter of kinetics. It has been proposed that soon after dewatering and deconditioning of the depository, the reducing conditions, such as they are, would be quickly reestablished. On the other hand, we don't find any basis for that statement because the kinetics of the reactions that would need to occur have not been portrayed. We haven't seen them nor, obviously, reviewed them.

Similarly, at face value the capacity of certain mineral pairs that may indeed be present -- and I would agree to that -- their capacity to buffer a large system, original system -- that also remains to be demonstrated. Also, it would need to be demonstrated that we can transfer from the laboratory to this complicated underground requirement; that if a particular radionuclide, technicium-7, could be reduced to technicium-4 by the lab under certain conditions, how will it be demonstrated that that may occur in the presence of many other substances within the dynamic system that would be extant?

That kind of uncertainty does not allow us to consider the matter of redox, for statements of redox to be a closed issue at this point.

(Slide)

MR. FOSTER: Quick question. To what extent will the backfill material used to fill up the mine eventually influence the kind of considerations you are giving here?

MR. JUSTUS: Backfill material could have a considerable effect on redox conditions. It has been proposed by them that carbon or charcoal be included as backfill material. That might generate some reducing conditions readily. It has been shown that the mixture of crushed basalt and bentonite generates a generally reducing environment as well.

So yes, backfill can be influential in this regard.

The last item here, I would like to point that out. The matter of publishing for peer review. There were several references to Eh, to a particular paper in the SCR on Eh. That was an abstract and it was a key document in stating or supporting the reducing condition assertion, and it is difficult to review supporting documents that are not in our hands soon after they are developed even in preliminary form so that we can begin to determine the adequacy and timeliness of these points.

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Let me say a few words about sorption. It is indicated in the SCR that sorption can be effective in every component of a multi-barrier system, but the sorption discussion in the SCR does not reflect some uncertainties in parameters. Similarly, as with solubility, the species of a particular radionuclide might, in fact, change with time; but nevertheless, the different species of a radionuclide have different sorption properties. We are not sure that the dominant species of a particular radionuclide is the one that is being studied or the one for which a particular sorptiveness, sorptive value, sorption value is being generated.

In sorption experiments to generate reducing conditions, a hydrazine chemical was incorporated, and apparently there are some side effects, maybe some complication that may interfere with sorption results, and these would need to be evaluated.

Further, back to an earlier point on the availability of the host rock along the flow path, in sorption it would be very important that experiments be conducted on materials that the radionuclides will encounter. In the backfill it may very well be freshly-crushed basalt. In flow tops we are not sure exactly what it is. The vesicle fillings in flow tops, where water may indeed spend quite a bit of time, are more variable than the primary metals, and few, if any -- in fact, there were no such flow top or interbed materials used in sorption experiments presented in the SCR. There may be some and they may be getting such data later.

So the statements --

MR. BROOKS: Can I make a clarification? There was little flow top and interbed material used in the presentation in the SCR. The emphasis was on the basaltic minerals and the dense interior, but they have addressed these, they just haven't emphasized them.

MR. JUSTUS: Thanks, Dave. I stand corrected on that. I didn't want to exaggerate the point by being inaccurate.

There has been some difficulty in reproducing sorption values such as distribution coefficients. It would be important for sorption data as they are used to have an uncertainty attached to it when they are applied to performance assessment determination. Perhaps interlaboratory comparisons, reruns and such would enable error bars or uncertainty limits to be established for sorption data.

There were various techniques used and there is no standard procedure, as I say, and that in and of itself introduces some uncertainty into the data utilized.

So we would emphasize again the use of representative materials. If sorption is not linear, then the sorption isotherm methods would need to be used. The effects of redox conditions would need to be reevaluated regarding sorption or the influence on sorption because most of the sorption work has been carried out under oxidizing conditions, interestingly enough.

MR. AXTMANN: If the groundwater flow times were really nailed down, wouldn't such activity be irrelevant? Nailing down abscrption rates?

MR. JUSTUS: If the release rate and the EPA standard or cumulation standard could be met because of, let's say, very slow groundwater travel time, would we need to concern ourselves -- do we need to give any credit at all or have to give credit to sorption or solubility or any other retardation factors for purposes of meeting the standard for release rate criteria? No, the answer is no.

On the other hand, we have to define it. Some geochemistry work is needed to define the environment of the waste package. That is, the lifetime of the package is dependent, of course, on the environment that it is going to live in for some thousands of years, and similar kinds of experiments that are being conducted now or will be conducted to establish solubility and sorption values would still need to be carried out to enhance the design or optimize the design of the waste package.

Furthermore, there are thermal-chemical effects that may influence the design of the mine opening itself or the pillar structure or something of that sort. Chemical changes may occur that need to be evaluated.

(Slide)

The uncertainties involved in the transferability of information from short-term lab-scale experiments to the assessment of long-term repository behavior also remain to be evaluated. As a minimum, here we should demonstrate quantitatively how the natural analogs briefly described in the SCR apply to assessments of long-term chemical stability at BWIP or any other analogs that may be forthcoming. It is not clear from the SCR what kind of field tests are planned. Yesterday it was pointed out that field tests are planned. Nevertheless, we need to be able to assess whether the tests will be adequate to make the long-term assessments. We need to know how the results will be used in long-term assessments.

(Slide)

The level of uncertainty of each important geochemical parameter still remains to be determined, in our view. We are glad that tests will be done using actual groundwater compositions and conditions such as may be simulated in hot cells, but we need to know the relation of these tests to unresolved issues, the description of the test and instrumentation, the limits of the data, and how they will be used.

Also, the tests are presented without too much of a rationale behind them. There is a large judgmental factor in identifying an experimental approach -- an approach, experiment, lab-type calculations, field experiments, field studies, perhaps. There is judgment to be exercised in making simplifying assumptions and what specific tests to be run.

The uncertainty of the site-specific conditions is large because a reference horizon has not been determined yet. Much of the data that was presented was for Umtanum. The Cohassett flow unit differs from the Umtanum in chemical composition. The Cohassett is a high magnesium flow compare to the Umtanum. It remains to be shown as to whether the kinds of results gotten for Umtanum or Pomona or other specimens can be applied to the more newly-designated candidate horizons. Changes in mineral and solution chemistry and other changes cause us to ask this question: What will the releases be, the radionuclide releases be when multi-component experiments are run? Would it be the same as the simplified -- same or similar results as the perhaps necessarily at this stage simplified experiments using single-component simplified systems?

The use of retardation data in performance models was not spelled out very well. If a single value suchas a K_d is plugged into a transport code but yet very sophisticated experience, perhaps using absorption isotherms and other techniques are utilized, have we bought anything, have we learned any more?

So a reconciliation of the levels of uncertainty that are needed to be established to optimize the testing and evaluation program in a geochemical system still need to be spelled out. Such assessments would enable us to strengthen our recommendations for what might be more timely or more optimal runs, or perhaps even certain bounding conditions might be acceptable.

(Slide)

Nevertheless, Dr. Axtmann, DOE has stated that they appear to wish to give some credit to solubility and sorption, or at least retardation of radionuclides so far towards meeting the EPA standard release rate. Which lab or theoretical or field test DOE is going to rely on to convince any of us, anyone, that the system is going to work the way it is portrayed in 10,000 years we don't know yet. This was a point that Dr. Orth mentioned yesterday.

Until these uncertanties are delimited, the Staff will have much less confidence in key statements in the SCR regarding geochemistry than appears in DOE documents to date.

That concludes my presentation.

MR. MOELLER: Questions? Martin.

MR. STEINDLER: Do I gather that the commentary that you made about wanting to, in a sense, influence optimization of their experimental plan is a method of achieving a primary goal, which is different than, in a sense, dealing with their plan, namely, that you are really interested in the ability to evaluate the data and to ensure that the data they are going to obtain is acceptable quality to you.

MR. JUSTUS: Yes, sir.

MR. STEINDLER: Why is it that you are interested in talking to them about their experimental plans at all, other than the charitable notion that if they don't tell you what their plans are and they go off and do something, they find out two years later that you can't use their information?

MR. JUSTUS: We need early interaction for several reasons. Again, we do not need the information to turn around and instruct DOE on how to proceed. On the other hand, the outlines of plans and early data are useful for discussing the amount of time it may take for resolution of certain points, and that influences our own program. Some tests, I suppose tracer tests might be utilized. These might need a lead time of a year or more before results are forthcoming.

On the other hand,the results may be impinging on performance assessment considerations of transport. And too, at the same time we are independently trying to determine the uncertainties limits of whatever data we are going to get. We are not concerned over whether the conditions are in fact reducing or oxidizing or what the actual values are. That is for DOE to get. We must, however, get a headstart, an e arly start to be an effective critic of the data, to allow the time joy8

to generate independent programs that will assist the uncertainties of a particular method or appropriate strategy.

Does anyone on the team have anything to add?

MR. MILLER: I think we start from the proposition that with these complex geochemical reactions, as DOE said yesterday, we are going to be forced to find ways to bound the problem. You cannot make all the measurements you would like to make to know it precisely, and I think the extent to which we are talking about learning the strategy, if you will, that is being employed in the DOE program, it is to, you know, judge for ourselves whether those simplifying assumptions are good ones or not. Do you have to make simplifying assumptions to get into a program as described yesterday? We just want to make sure that they are early. We have got an opportunity to see that and give advice.

MR. STEINDLER: I don't want to prolong this thing, but I guess the thing that troubles me is there are some recommendations made to DOE about what they ought to do. What I guess I would be interested in looking for in documents produced by the NRC is an identification of the data that is required and the quality of the data that you believe necessary at this point in time that is required.

For example, if you are worried about solubility limits, then what you ask for is not what you have in fact listed: namely, determine the thermodynamic constants, which is a multi-year, megabuck program of some significant difficulty if you look at the past history of how long it takes to get it. .ou don't really want the thermodynamic value. That is just a means to an end. It strikes me that the kind of things that ought to be requested from DOE are, hey, fellows, determine to a particular level of precision and accuracy the solubility of Americium in a carefully constructed ground layer. Then at least you can argue about whether or not the requirements you have asked for make sense -- I'm sure there is going to be an argument on the subject -- and then let DOE arrive at some mechanism of going about getting those bits of data.

Now, if their choice is the only way we can do that is to determine thermodynamic formation constants for the particular species in question, okay, so be it; but you are explicitly calling out in your Vu-graph here the determination of missing thermodynamic constants, which are just one way, but certainly not the only way of getting at the information you find you do want.

MR. JUSTUS: You are quite right. We would agree with the approach you have outlined, and I do not take the statements called considerations on this Vu-graph as something that we want or must have. Therefore, if a particular approach is to be used, if solubilities are to be calculated from thermodynamic data, then show that to 150 degrees the thermodynamic data points that you used, that you took from a table that was derived at 25 degrees -- that the extrapolation was accurate.

And perhaps if it was misleading that we were actually asking for thermodynamic data, we are asking if it is used that certain points be confirmed to perhaps allow for extrapolations to constrain any uncertainties based on a dearth of thermodynamic data.

MR. MILLER: We made an attempt when we first got the SCR to do an overall sensitivity study of the system and to try to get a handle on what we needed out of each sub-element of the system so we could begin to do the sort of thing you are talking about, give quantitative guidance measured to this level of confidence, solubility and all the other parameters that would add up to the geochemistry performance, and likewise with hydrology and so on.

That effort fell on its face because there was so much uncertainty with regard to groundwater that we didn't think we had a good enough integrating model at this site. Yet the time to be very definitive about the required performance in each of these areas -- we are striving for that. We are trying to -- and in fact, engage DOE, as Mel Knapp will describe later, in doing that along with us.

But I'm afraid in fact any ideas the Committee would have on how we might approach this and not get into the trap that DOE has often accused us of being in of prescribing numbers and not taking a systems approach -- we are all ears on. It's something we are grappling with, but I think with the uncertainties that exist in these other elements -- ydrology right now -- we just can't see our way to doing that. But your concern is one that we share.

MR. STEINDLER: I would not send anybody into the lab to do thermodynamic calculations. I would not believe the thermodynamic calculations even if the data were right in the sense that their applicability to the groundwater system is challenged, and until somebody does it in an honest-to-god groundwater system, including attention to things which haven't been paid attention to, which are indicated, such as organics and carbonates, the data are only of questionable applicability, precision notwithstanding. In that sense, I must confess I did get confused when you said DOE should consider the following, and then you come back and tell me, well, you don't really want DOE to consider the following. It is in that category that my comments should be taken.

MR. MOELLER: Well, thank you, Mr. Justus. I think with those remarks we will recess one hour for lunch.

(Whereupon, at 12:45 p.m. the meeting recessed, to reconvene at 1:45 p.m. the same day.)

END T 9

AFTERNOON SESSION

(1:45 p.m.)

MR. MOELLER: The meeting will come to order.

For guidance for those who need to plan your schedules, we are going to take up now the subject of facility design and underground testing by John Greeves. Then it was my intention after we finish this presentation to call upon the DOE representatives in the Rockwell-Hanford group for any responses they have to the NRC presentations of this morning and the first one this afternoon.

Then we will do the report, the status report on the long-term performance of waste packaging materials, and then pick up the last two NRC items, and that will complete today. Then tomorrow the Subcommittee, to repeat, will formulate our opinions in executive session but open to the public if anybody desires to attend.

So we will move ahead, and John, you are first, and I understand you will then cover those several items during your presentation.

MR. GREEVES: Yes. Actually I would like to start by making corrections to do statements that were made this morning. Dr. Verma asked to clarify his statement that 100,000 gallons of mud was lost in RRL-2 was a mistake. It was actually 25,000 gallons. That was estimated in the RRL-2 report which we had received recently.

Dr. Wright also wanted to correct the location of this mud loss which he referred to as a fracture zone, and that is at a depth of 3,822 feet in the RRL-2.

(Slide)

First I would like to just briefly introduce Dr. Cork with the Bureau of Mines and Dr. Raj Rajan with International Mining and Consulting Firm.

(Slide)

These are the four areas that I had intended to address. Stability of repository openings. Obviously, the shaft and the placement holes, performance of barriers and backfill component. That is, control and transport of radionuclides, sealing of shafts and boreholes, which are the major penetrations of the repository horizon. Last, retrievability of waste.

(Slide)

The Staff's evaluation is in the Chapter 6 of the draft Site Characterization Analysis and is concerned with evaluation of the DOE program in terms of EPA and NRC requirements. In the design area, Part 60 stresses four conditions: controlling adverse site characterization effects, for example, limiting disturbance to the exploration of an exploratory shaft; limiting releases from the engineerged system to limit the release rate to 10^{-5} per year of the inventory present.

The sealing of shafts and boreholes. The basic requirement is to not compromise the ability to meet the overall performance objective. And then finally, preserve retrieval option, basically to preserve the retrieval option for up to 50 years after initial emplacement.

Chapter 6 focuses on the geo-engineering aspects of underground facility design.

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You have seen a number of slides on this already, so I am not going to dwell on this. Basically you have two candidate horizons, Umtanum and the Middle Sentinel Bluffs, vertical shafts, as you have seen some of the charts yesterday. This is the conventional shaft and pillar layout.

(Slide)

The waste emplacement method is the horizontal concept that was addressed in detail yesterday. The shape, size and pitch or spacing of the placement holes is based on a 2:1 stress ratio. Backfill is bentonite and crushed basalt.

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The first issue I wanted to address was the stability of underground openings. As shown by Bob Wright, Paul Prestholt and many others, these basalt things can be very complex. The continuity of joints would be very important, and since you stress the strength, it can be varied by large amounts.

This will affect the rock mass strength and stability of openings.

In our opinion, the geologic variability is not adequately identified in the SCR. A lot of this has been gone over, so I won't dwell on it. The thickness of the flow tops and the elevation of those. We believe the stability analysis needs to be identified and presented. Only a single value of rock strength is used in the conceptual design. Detailed reference analysis was referenced, but we need to take a look at some of this information.

Our other comment is we need to prioritize what they key parameters are: for example, fracture density, continuity of joints, and rock strength.

In summary, a realistic stability analysis taking into account the range of rock mass strengch and the deformation characteristics of a jointed rock mass is needed.

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The second issue is the release rate of the engineered barrier system. Release of radionuclides should be a gradual process which results in a small fractional release to the geologic setting over a long time-frame. The performance objective for this is a release rate of 10^{-5} per year.

The Staff is concerned about the apparent premature commitment to horizontal waste emplacement. Some studies have shown that flow in tbe near field would not take advantage of the backfill within the room under this configuration.

Some of this was articulated yesterday by the BWIP group. Some studies have shown that backfill within a room can contribute significantly to the waste isolation. I believe Mike Smith mentioned this in his presentation.

Another point that we noted was it is going to be difficult to provide quality control and high placement density along these horizontal boreholes. I think Dr. Philbrick picked up yesterday the density that is expected is rather high, and this is something we are anxious to look into in the future with the test program that they have identified.

The Staff recommends analytical sensitivity studies be performed which consider a range of waste emplacement configurations and backfill properties, and that is much of what you heard yesterday.

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The third issue is sealing. Sealing of shafts and boreholes is important to help prevent them from becoming preferential pathways for radionuclides. Taking a moment to comment from yesterday -- on a comment that was made yesterday, there is no prohibition on boreholes at any proposed site. The regulatory criteria indicates that you should take care and avoid emplacement areas, put boreholes into pillars and shafts if you can identify where you are going to drill those. So there is no prohibition against drilling. It is a common sense situation: don't drill a bunch of holes and make a problem for yourself.

MR. MOELLER: Where did we then hear that there might be a prohibition? It seems to me -- you know, is this just some folklore that grew up or did we read that somewhere?

MR. GREEVES: Well, I was a little bit surprised at the statement yesterday, and I just wanted to mention it to clear the record on that. Historically, about two or three years ago we commissioned Gold Associates to take a look into this and make some recommendations to us, and their recommendation was that sealing shafts and boreholes is going to be somewhat of a problem, and one of the recommendations that they made was, you know, common sense indicates put these things down where they will not be encountered, either in emplacement rooms or drifting to the extent that you can.

I think with a reasonable amount of planning, you can accomplish that.

MR. MILLER: Also, there is in the regulation an explicit statement about the need to have concern for doing things during site characterization like penetrating the site with lots of holes, from the Lyon, Kansas kind of experience. So it is indeed in the regulation. It is not a categorical prohibition. I think this is one of the sort of things we are available for consultation on.

MR. MOELLER: Thank you.

MR. GREEVES: I would offer to read that section, but in the interest of time, I will skip it.

MR. MOELLER: Well, that is where I recall it.

MR. GREEVES: It was in that report and it was put into the regulation.

MR. MOELLER: It was more a precaution than a prohibition.

MR. GREEVES: Correct. As mentioned by Bob Wright, details on the exploratory shaft are not provided in the SCR but have been submitted recently and are under review by the Staff and our consultants. In our opinion, the SCR places a bit too much emphasis on modeling and not enough on testing. The approach in the SCR was just to meet EPA standards, and we observed that the testing did not start until late fiscal year 1984.

We also observed that there appears to be a lack of detail consideration of longevity and long-term stability in the seals.

In summary, the Staff recommends that an iterative process between data collection and modeling studies be taken to address sealing concerns.

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The fourth issue is the retrieval option. The Staff is concerned with constructing and maintaining the stability of these 27-inch diameter, 200-foot long holes and jointed holes under thermal loading. Considering the unique requirements of the horizontal emplacement concept, the Staff considers timely demonstration of retrievability to be necessary.

MR. PHILBRICK: Would you be satisfied if they drilled a few horizontal holes up on top of the hill on Gable Mountain?

MR. GREEVES: I would not be.

MR. PHILBRICK: And you wouldn't be satisfied if they went underground and drilled the holes.

MR. GREEVES: I would be satisfied if they went underground and did the test program, which included a number of these horizontal concepts at that depth in that stress regime and in that environment. That is what I think is necessary.

MR. PHILBRICK: Have you made an analysis of what the stresses would be on those 30-inch holes?

MR. GREEVES: No. The Department has made an elastic analysis which we reported in the SCR.

MR. PHILBRICK: You really think the 30-inch hole would break?

MR. GREEVES: I don't know what itwould. We have consulted with the Bureau of Mines, the best consultants available to us, and there is quite a bit of concern about how these things are going to perform under the conditions under that environment at depth over a time frame -- a long time frame.

MR. PHILBRICK: Can you make any parallelism between the horizontal -- with the strength of coal and the shallowness of copper and come out to any modeling relationship at all?

MR. GREEVES: No, I can't. This particular media that we are in, basalt, is one that there just isn't any mining experience at this depth.

Another concern we had, a rather minor one, is the priority of these retrieval work elements. We would like to see them moved up in priority.

MR. STEINDLER: Could I ask -- there appears to be I guess a difference of opinion of what constitutes adequate retrievability based on the fact, the statement you just made that Rockwell or DOE doesn't consider it very high on their priority list of things to get done. What is it that drives you folks to attach such importance to retrievability?

MR. GREEVES: This is one of the areas we will have to make findings on at the license application, whether the design they have -- make sure that it will not preclude the retrieval option.

MR. STEINDLER: I guess my question is that it will not preclude -- Sandia did a study some years ago in which I think they pointed out if you are willing to spend enough money, you could retrieve anything from any place you put it into. Obviously, that is perhaps an overstatement, but not by much. So precluding the retrievability option, one that some of us feel has a very low probability of ever being exercised, strikes me as being elevated in your case to an importance that may not be shared by anybody else.

How is it that you arrive at the kind of importance that you attach to it in relation to other aspects of the system?

MR. GREEVES: It is one of the performance objectives in 10 CFR 60. The reason we attach importance to it in this particular instance is largely because of this horizontal concept. As the Project pointed out yesterday, they admitted they recognized a concern if they were to go to the backfill concept, backfilling it and then having to go back and retrieve something from a backfilled configuration.

MR. STEINDLER: I thought the words then were it would be more difficult; I didn't hear anybody say that precludes the ability to retrieve. I guess all I'm saying is eventually DOE is going to have to respond to your suggestions, and if you keep saying move this, that and the other thing up to a higher priority, I think you are giving those folks a rather strange view of how they run their program, with limited resources and whatnot.

MR. GREEVES: It is merely intended to have them take a look at it a bit more seriously than is presented in the SCR, and I think they agreed to that yesterday.

MR. MOELLER: Retrievability is in 10 CFR 60. Is it in the EPA standards?

MR. GREEVES: I can't say.

MR. MOELLER: I should remember, but can someone tell me?

MR. KNAPP: Retrievability as such is not in the EPA standard as proposed in December. They have made a requirement which at one point was guidance that the waste be recoverable for a long period. That is different than the retrievability concept in Part 60. But the underlying philosophy in both the regulation and the standards that we not make an irrevocable commitment is this.

MR. PHILBRICK: John, let's go back to this business of whether the hole stays open or not. You are down there at something like 3800 feet if you go to the Umtanum, and that gives you a compressive stress of 3300 psi.

MR. GREEVES: Vertically.

MR. PHILBRICK: That's right, and that's the only one that makes a difference to you.

MR. GREEVES: But the horizontal stress is twice that.

MR. PHILBRICK: It isn't going to be that high. Otherwise, the stuff would be jumping up in the air. It is going around in a circle, and I don't see why somebody doesn't make some numbers on this thing.

MR. GREEVES: Some numbers have been calculated in part by the Department of Energy. They are based on purely elastic analysis, and that does not account for the deformation characteristics of the underground openings. This is one of the points that we have identified, that realistic analysis using some of the empirical approaches and some of the numerical techniques, needs to be applied to these underground openings, including the core holes.

That, coupled with a limited amount of underground testing and demonstration, would answer these questions.

MR. PHILBRICK: Well, some of these holes have been opened up. Somebody bored a hole down there and they did it with mud, and then they started putting water in, so the mud must have come out of the hole somewhere. And all they did was put water in the hole.

MR. GREEVES: There is mud in the hole.

MR. PHILBRICK: And mud in the hole. They are never going to clean it up.

MR. GREEVES: In many instances the mud never comes back to the surface; it is just a complete loss. Now, they do clean these holes out when they do hyrologic testing.

MR. PHILBRICK: My concern is have you had any holes at that depth with nothing but water in them?

MR. GREEVES: It is my understanding that when they do these hydrologic tests, you get clean water in the hole, but it is in packed-off interval.

MR. PHILBRICK: So with 1500 pounds of water in the bottom of the hole, the hole was still open.

MR. GREEVES: In those intervals.

MR. PHILBRICK: Okay.

MR. GREEVES: I might point out there has been some core loss on these holes of significance. That was of concern to the Staff.

MR. PHILBRICK: That could occur in lots of ways.

MR. GREEVES: Yes, it could.

MR. PHILBRICK: That's all.

MR. GREEVES: In summary, the Staff recommends the performance of a thorough analysis of the retrieval problems and suggests demonstration of drilling, emplacement and retrieval of canisters be conducted early in the field test program.

I might add that we have had contractors take a look at this and look into the problem associated with it, and one of them is the over-coring problem. There is equipment available to overcore some of these, about 20 feet.

MR. MOELLER: Martin.

MR. STEINDLER: Back on your point, if I understood it, NRC in 10 CFR 60 does call for retrieval, and EPA in the standards says, "Disposal systems shall be selected so that removal of most of the waste is not precluded for a reasonable period of time after disposal." So in a sense the NRC is committed to retrievability. So what John is saying, I guess, is get on with the tests that will demonstrate that retrievability will be possible.

MR. STEINDLER: Fine. Aside from my personal feelings on retrievability, which I would like to set aside, all I'm saying is it strikes me there are a lot more important things on this whole issue at the moment than retrievability. If you keep moving retrievability priorities up, pretty soon you have got your focus on the wrong thing.

MR. MOELLER: And the Subcommittee may very well want to say that. Thank you.

(Slide)

MR. GREEVES: The SCR presents some information on in situ testing in Chapter 17 of the SCR. The Staff has made the following general observations. The basis for --

this is not presented, for example, the sensitivity studies to identify important parameters either are not presented or they just aren't complete yet. Important test details, as we have talked, in a number of areas are not presented. It is difficult to follow some of the timing and the priorities of the test activities.

The Staff recommends full-scale room excavation tests in consideration of large-scale thermohydrologic testing.

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In summary, the Staff recommends the following: that the Project complete sensitivity studies to identify relative importance of geo-engineering design parameters -- some of the work elements mention this, but apparently they are not complete yet; provide details on the in situ tests and test plans; analyze alternative emplacement configurations, and some of this was mentioned yesterday by the Project; integrate laboratory and field testing at an early time in the sealing program; provide details on construction and quality assurance for the exploratory shaft -- and we are presently reviewing material recently received on this matter -- and increase the priority of retrieval work elements and plan on an early demonstration of the horizontal concept.

MR. MOELLER: Questions for John Greeves?

MR. AXTMANN: Question for the audio staff. There is some feedback in here. It's a C sharp, I believe.

(Laughter)

MR. MOELLER: Okay. Well, thank you very much.

I think, then, now we will move to the DOE -- back to DOE representatives and call upon them for any comments they may have in response to what they have heard from the NRC this morning.

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Go ahead, then, with your comments.

MR. FREI: This is Mark Frei with DOE. I have a few remarks to make and then I will turn this over to Dave Squires of Richland and then Larry Fitch of Rockwell.

I would like to point out for the record that the Project has had a rather limited time to read and evaluate the contents of the SCA. The document has been out a few weeks. Unfortunately, we have had trouble getting the document out to Hanford.

Today was helpful for all of us here on the Project to really get a better insight into what is in the SCA and the rationale for their statement. Certainly some of their concerns have been expressed earlier in the SCA workshops and in earlier workshops than that, and hopefully we have left the impression from yesterday's presentations that plans are under way now in the program since the SCR was issued that will start to address a number of these concerns.

I might point out that the concerns are not only from NRC but concerns that we have within DOE and Rockwell as well as comments we received from our own overview committees.

As far as where we go from here, we do plan and anticipate on meeting with NRC in the near future to lay the groundwork for hopefully a series of meetings with them one to one where we can start to work through their concerns and find out what plans we are going to change, adopt and implement and start addressing those concerns, and hopefully when we issue an SEP later this fall, we will have additional data and additional plans in there, and hopefully the SCA will not have to be 1100 pages but can be considerably shorter than that.

Thank you.

MR. MOELLER: Thank you.

I might ask before you move to the others what you see as your needs from the subcommittee and the full committee, the ACRS. What would be most helpful to you in the way of a report from us?

Of course, our report -- let me put on the record too -- if my memory serves me, the Advisory Committee on Reactor Safeguards received a letter from Chairman Palladino saying that DOE was issuing the SCR, that the NRC Staff would be reviewing and commenting on it, and he asked us to review the NRC's review and to offer comments.

So I am simply saying in exercising that function are there any particular things that would be helpful to you if we could do them or work them into our report?

MR. FREI: Well, considering the expertise of the Subcommittee and the Committee at large, I would view any comments you could make on either the SCR or NRC's analysis as almost an independent peer review of both documents, and we would appreciate any feedback on either document.

MR. MOELLER: Fine. Well, you are correct, you cannot review one without reviewing the other. So our comments will be on both the SCR and the SCA. Thank you.

Okay. David Squires.

MR. SQUIRES: Mr. Chairman, this is David Squires from the Richland Operations Office. A couple of comments I would like to make.

Number one, I guess I agree with what Mark said in general; I would just like to enlarge upon that a little bit in terms of the comments and the observations made by NRC. We certainly agree with the comments made by them. We need to take a look at their recommendations in terms of where does the program go from here. And as Mark said, we believe that closer interactions one-to-one, one-on-one at Richland or at Silver Spring, as Hub Miller said this morning, whatever it takes, but we need to get the staffs together to better outline what we need to resolve and how they need to be resolved in terms of addressing the issues.

I would like to make one statement. I was a little bit disappointed, I guess, to hear a number of the NRC Staff say that there was poor cooperation or lack of cooperation by DOE, and I guess I felt that we had a good relationship in terms of meetings, getting together, setting up, making arrangements; and if there is a lack of cooperation on DOE's part, this is something that Mr. Miller and Dr. Wright and others and ourselves at Richland need to resolve because we feel very keenly that we need to have closer relations with the Nuclear Regulatory Commission.

Thank you.

MR. MOELLER: Thank you.

Larry Fitch.

MR. FITCH: This is Larry Fitch. I have just a couple of comments, and I think -- I really have welcomed this presentation today and our opportunity to talk to you yesterday. I think it has pointed out one thing to me that kind of hinges on what David just talked about, and that is not a lack of cooperation but a lack of understanding. I showed a slide yesterday in my brief presentation that showed a listing of about 10 or 12 man-days worth of inter-play -- excuse me, 10 or 12 days of interplay between the NRC and our staff, getting them prepared to receive the SCR.

That was an exceedingly busy time. It was very hectic. There was a lot going on, and it is probably some very small fraction of the amount of input and interplay we are going to have to have with the Staff so that they, one, better understand what we are doing, and I think when we do that, the differences that you have seen between their understanding of our SCR and ours I think will diminish, at least I clearly hope so.

One other thing I would like to do that David spoke to yesterday morning is I would really like to extend an invitation to your subcommittee to come out to Hanford whenever you could, and I would like to give you a little hands-on as to what is going on out there and show you some of the testing apparatus we have and some of the laboratories we have, and you may be able to get a better handle on what the basalt project is doing.

I talked yesterday for just a moment about writing the SCR was about an 18-month process. That document was given to the NRC in the middle of November of last year, so as of today, the document was started over two years ago, and SCA has now been written, at least in draft form. It took the NRC some several months and a lot of manpower to put together. I think it is a very nice document, but they are reviewing things that we did a year and a half ago, and that is an exceedingly ineffective way for us to continue doing business, I think.

I understand the requirement for regulated formalism between the applicant and the regulator. I understand that wholeheartedly. But I think if we are going to have this early dialogue between the NRC Staff and the DOE during characterization, we are going to have to find a method that is just a heck of a lot more real time than what we are having now.

I don't how the basalt project is going to be able to support it. We are screaming for time to get the things done as it is. But if the dialogue is going to go on and there is going to be correlation between what the NRC thinks they need and what we can produce, and to get the differences dealt with I think we need a mechanism that is a heck of a lot more real time than the one we have been having. I wouldn't like to recommend what it is, but you can well bet that we are going to give it just a heck of a lot of thought and know that the NRC is as well, and we hope to continue that dialogue.

MR. MOELLER: Thank you. And anything the Subcommittee can do to foster communications, we will be more than willing to try. And indeed, we certainly can plan a subcommittee meeting at Richland and bring the NRC Staff with us out to your place. Yes, we will plan on that.

MR. FITCH: We welcome you.

MR. MOELLER: Hub, did you have anything to say before we move on?

MR. MILLER: The letter that transmitted this to the Department of Energy was from Mr. Davis, and I will be repeating a bit, but it did indicate that interacting through the mail with thick documents, as we both agree, will not work. I would only say one comment about Dave's impression of lack of cooperation. I think we have definitely had the joy 7

feeling that the DOE organization at Richland has tried and we recognize that they have been under a lot of time pressure, and I think it is not until you get to putting out a document like we have, and in the workshops we find specifically what kinds of things we need to see to do our job. That makes the case very strong for the kinds of interaction Larry is talking about.

I would like to ask Bob Browning if he would have any other comments he would like to make.

MR. BROWNING: I would just like to say that I think all the indications we have gotten is that DOE clearly plans to respond to the Davis letter, which supported the site characterization process in a way which will encourage prompt receipt of data and prompt communications. There clearly is a limit in the time it takes to generate data and handle it and get it out. We have both got to work on a scheme that will allow us to pull that timeframe down.

DOE has another problem in addition to keeping us happy. They have also got to keep the applicant states informed and happy, so their job is an extremely difficult one to deal with. But we are going to do everything we can to make it easy for them.

MR. MOELLER: Fine. All right. Well, that was helpful.

Why don't we pick up, then, with our agenda, and the next item -- I think we have plenty of time before the high level waste packaging material. We have time to take up waste form and package by Robert Cook.

Were there any questions by the Subcommittee consultants or members?

While Bob is getting ready to start, it will be June, you know, the earliest, before the Full Committee would issue any kind of a report However, we will be formulating our Subcommittee's report tomorrow and there will be official minutes of the Subcommittee meeting, and you need not wait. You should have those and we ought to be able to send those to you in three weeks, at the most, so we will do that.

(Slide)

MR. COOK: I am with the waste package group. There are several other people that helped on the waste package analysis: Mel Siegel from Sandia, Michael McNeil from the Office of Research, who is the metallurgical expert for us, and several people from Brookhaven also worked on this assessment of the SCR.

(Slide)

The objective is to try to focus on issues rather than the things we agree with, to make this short. I don't think there is large disagreement -- I wasn't sure that there wasn't to begin with, but I know that at least in one area on the analysis where we were worried about getting the good analytic methodology identified, which was amenable to a reliability analysis, it looks like BWIP is going along those lines. The details we are not sure of, but that was quite encouraging to hear them yesterday say that reliability analysis was on its way.

So I want to talk about that a little bit and I want to talk about the reasons why we feel reliability is important and the type of analysis is important and talk about some of the chief modes of failure in the waste package we are worrying about and whether they will be addressed in the BWIP testing, particularly pitting and corrosion, and say a little bit about the hydrogen concerns we have with carbon steel and talk a little bit about failure mechanisms if we have got time in the packing material, and then I will conclude.

(Slide)

Just as a brief semantics thing, Al referred to packing material. It has been refer... to as backfill material in the past. Packing is part of the waste package in this connotation. It is the bentonite-basalt mixture. Just a brief overview. This was presented yesterday so I won't go through it.

The idea of a can in a can with the waste form or a single can with spent fuel inside is the design of the package that we are considering, 18 or 20 packages in a row, and a 200 length borehole surrounded by packing material.

(Slide)

I will skip over that design of the waste form. The key features of the waste package that are important, it seems to us, are the annular region in here, which will be filled with packing. Obviously, the overpack or the corrosion barrier, which is carbon steel, and some of these other artifacts in this design, like these pins which this sits on, which will not be filled with packing but are carbon steel or some other material, preferential degradation of those relative to the packing material. Also the ability to install the material as was discussed, with a single pipe. Filling in all the voids around here and getting to the other side of the package uniformly to get a material which is in the engineering sense reliably known and have good properties seems to be a tough problem. It will come out in the testing, though, I think, that is planned to demonstrate how well you really do know that configuration.

(Slide)

Going on to our objectives, in the analysis area we talked briefly about -- Dr. Wright talked briefly about the concept we have of water coming in into the engineered system at this point. In the case of the design the BWIP people have presented, it comes directly into the waste package. There isn't any other engineering barrier, for the most part, because the waste package is right up against here in this case. Going through the water would come through the packing, attack the barrier waste form and transport materials out over a period of time.

The objective is to describe or specify or whatever you want to call it some sort of analysis which takes into account the processes that occur with the water coming in and the water going out with its load of radionuclides or lack of radionuclides.

(Slide)

So what have we stated or have we stated in the SCA as to the objectives of the analysis, and how does this relate to reliability analysis is the question I want to address here. We wanted to get at the failure of the container, failure of the package to allow the release of radionuclides, this being relative to the containment criteria in 10 CFR 60. We want to look at it all the way out to 10,000 years because the packages may in fact last that long, and if they contribute to the overall system performance, you certainly want to be able to do that.

That is not to imply that there is a limit or any criteria out at 10,000 years. It may come back early in time, depending on the heat loop that the packages actually have. So we are looking for a statement as to the containment time statistically for the 30,000 packages or so that we are going to have in the system. We are also looking at the release rates and we are looking at release rates for each radionuclide because they will differ greatly in how they transport on through the geologic media.

So when we talk about the nuclides, since the rule limits or looks at limiting radionuclides specifically, we want to get an analysis which addresses each of the radionuclides. The analysis which we have talked about here is a fault tree/event tree type of construction, being something which would fit the objective of coming up with a reliability analysis, and we would use conditions with statistical distributions of the conditions that are boundary conditions on each of these waste packages, 30,000 waste packages.

The processes would be typically engineering type of equation for whatever process is pertinent.

(Slide)

MR. AXTMANN: Do you ever call for the results of the risk analysis?

MR. COOK: The objective is to quantify the uncertainties. We are not going to take into account --

MR. AXTMANN: No, no, no. You are just doing what risk analysis started out to be, components and so forth. I am talking about do you have a goal for the integrity?

MR. COOK: The objective is to try to quantify the uncertainties, and the reliability analysis is a comprehensive method of doing that, of quantifying the uncertainties.

MR. AXTMANN: So you don't care what you get.

MR. COOK: Sure, you do care what you get. I mean if you get a very low reliability, that is probably not going to fly. I mean I think you have to look at what is reasonable from an engineering standpoint. So I get into what we might expect -- we did some brief analysis just on one component to look at scatter data to see what we could expect in terms of reliable statement.

MR. AXTMANN: Well, there are some statements about the desirability of the containment, right? That it will last 10,000 years or whatever?

MR. COOK: I think it says right now it is variable from 300 to 1000 years. It is going to depend on the -- and you want reasonable assurance of that, and in the 300 to 1000 years it is going to depend on the temperature that you get. If you have got a mundane waste package which doesn't give off much heat, then we would look at reducing that containment requirement from the 1000 years on down to, I think, a minimum of 300 is what it says.

So the idea is to take into account or provide flexibility and adjust this containment to reflect the heat output of the wet waste packages.

MR. AXTMANN: It seems to me to make the exercise uesful you would have to have some kind of quantitative target.

MR. COOK: What we have done in the SCA is to say we agree with that, until you look at the whole system, the geology and engineering system, and to say, okay, here is how I'm going to break up my overall objective of meeting the EPA criteria, I want this part of the system to do this much for me and the engineered part of the system to do this much for me. In terms of reliability and performance, you really can't say what it should be.

We have asked DOE to at least, in a normal design control sense, to establish objectives, design objectives for reliability. They should sit down and say, okay, here is how this whole system is going to work, I'm going to assign this reliability to this part of it and I'm going to go about getting my data so that I can show that.

We are having a hard time seeing how, at least for the engineered system and waste package, you can design a test program without knowing how reliable you want the components to be, and you can't do it, it's just impossible.

MR. MILLER: Dr. Axtmann, it goes back to what I was saying this morning in response to a question. We at this early stage -- there is difficulty in stating the uncertainties of the natural systems, and at this stage we cannot prescribe to the DOE hard and fast probability or reliability goals. We are asking that DOE -- sort of giving them the running room to at least tentatively identify some of their own.

Now Dr. Knapp is going to spend a good deal of his talk on this whole business of how we envision this licensing assessment being done.

MR. COOK: The type of curve or the type of result we are looking for is -- this is representative -- is a curve that we put here for a typical corrosion barrier -- a canister. This is what we believe the curve will look like in the long run, and that is based on looking at a number of different systems, piping systems, pressure vessels, how do systems that depend on corrosion as a failure mechanism really fail when you have a large, large number of them, and you look at that number together. You get typically some early failures, which is in these curves, called infant mortality, and then you have some random failure that you really can't account for, and then if the thing starts to wear out like you expect it to statistically now, though we don't have them all failing at one time, and then of course the curve has to bend over out here as you go out in time because it goes to zero again as everything fails, but this curve may go way out in time, particularly for carbon steel if it is thick enough and contribute very significantly to the long-term release of this system, just having full containment for a long time.

So we are anxious that we not cut off analysis at 300 years or 1000 years but to look at the total capability of that barrier in providing release rate control throughout the 10,000-year period we are evaluating. So that would be a typical curve.

I could give you another one for each of the radionuclides, the same sort of failure rate or release that that you would expect in changes with time, as an example of what we are trying to get.

(Slide)

Now, let me just go through briefly because I'm not sure there is any disagreement with all these modes of failure, but we have listed a number of different modes of failure for the canister that we consider should be included in the analysis, in the fault tree analysis. Some are much more important than others, and I want to stress the Item D, the pitting corrosion and the hydriding question for carbon steel.

(Slide)

I haven't said much about waste form failure modes, but matrix dissolution was mentioned yesterday. There are five or six different modes that we are interested in looking at specifically and trying to analyze in any failure fault tree analysis. Hydration is one which would occur if you had leaky packages.

(Slide)

I don't think there is a disagreement on all these. On the packing modes, one of the things we are worried about when it comes to packing material is that it is subject to change. It is in a very complex chemical environment. You have got methane and carbon dioxide, chlorine, fluorine, all sorts of other components in this groundwater. You have got a temperature gradient and you have got radiolysis going on, so the chemical reactions that occur from 50 degrees to 300 degrees C. in the packing material make it very difficult to say what the properties of that packing material are going to be from the time when it has got to work out to 10,000 years.

So since it changes with time and it can change with time, it is not a very good engineering material. One of the things we are worried about is the propensity for washing mud away. It's not a structurally sound material. If you have a fissure or if you have a local spot in one of these 200-foot sections where the water runs more readily than other sections, is it going to wash the mud away? I mean that is basically what it is. It's a mixture of basalt and gravel.

MR. PHILBRICK: Let me ask you where that stuff is going.

MR. COOK: The same way that RRL-2 goes.

MR. PHILBRICK: When they have got that stuff in the placement rooms, they are all filled.

MR. COOK: Yes.

MR. PHILBRICK: So then this material is supposed to come out of the hole and run into the placement room. The placement room is filled. Where is it going?

MR. COOK: You have got 120 miles of tunnel which are right up against the rock.

MR. PHILBRICK: Sure, you fill it up.

MR. COOK: And you have got six inches there between the rock and the package, and you have got an annular space there.

MR. PHILBRICK: No, you don't, because you have filled it.

MR. COOK: I'm saying if you have got cracks in the rock, depending on how frequent they are and what the flow is in those cracks, you are going to wash the mud -- the bentonite right out of the --

MR. PHILBRICK: If it is going to wash the bentonite out of someplace, the bentonite has to be carried someplace, so you have got to have two cracks. The probability of having two cracks in there instead of one is a little odd. In the second place, the only outlet you have for that is through these placement rooms.

MR. COOK: You mean through here?

MR. PHILBRICK: Yes, and they are all plugged.

MR. COOK: Well, you may be right. I'm not -- I don't know what the cracking density is that we are going to have to contend with in these holes, whether you can expect none for ten holes or one for each hole or what; but I agree, if this is -- if the packing material has to go out through the length of that hole --

MR. PHILBRICK: It's not going to go anywhere, so let's relax about that one.

MR. COOK: I would agree with that. The concern is that it does go directly into the dense rock, through a fissure of some sort.

(Slide)

The other questions on the backing were the Item B cracking, what happens -- and this again comes back to this question of how does the packing material change with time. Does it cement itself together with silicates or whatever? Really what happens to it chemically over the 10,000-year period if you don't have the washout question? Can you depend on it remaining ductile over that whole period of time so it stays as a diffusion barrier for you? One crack through that will defeat the purpose of it being a diffusion barrier. It will swamp the --

MR. PHILBRICK: It will if it dries out, but where is the moisture going?

MR. COOK: Well, we are not too worried about it drying out, I don't think.

MR. PHILBRICK: All right. Then let's quit worrying about the cracking, then.

MR. COOK: We are worrying about it cementing and silicates forming in it. Like I said, it is going to be a complicated chemical situation when you get radiolysis in that picture for 500 years.

MR. PHILBRICK: Let's strike out B and C. If we get enough of them struck out, you can go home and go to sleep without worrying about anything.

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MR. COOK: The point I think we are making is we do have to put these things to bed, so we do look at them and that doesn't come up as an issue late in licensing and lo and behold here is something we forgot to think about.

(Slide)

Conditions that we are most worried about in the -- and again, this one is not -- this is, I think, where the biggest concern is that we have with the BWIP approach, is not properly worrying about the conditions that are pertinent to the failure modes, particularly the pitting corrosion.

When it comes to the chemistry of the systems, we are worried that the testing that goes on includes all the components in the system, including the radiolysis and the temperature gradients, to make sure that the chemistry, if it's local chemistry within the waste package now -- that you get the right chemistry identified because the pitting corrosion or general corrosion or whatever the limiting mode is when it comes to corrosion of these packages is going to be very important. It is going to be based on the conditions. And I will show you that relative to the pitting here. This is what I wanted to get into.

(Slide)

I think I will skip this other one on the processes. Let me get into this pitting question.

(Slide)

The design of the waste package, the conceptual design that the BWIP people present is basically based on a general corrosion scheme as being limiting. We think that the pitting corrosion is going to be more limiting than the general corrosion, particularly in the anoxic environment that is hypothesized. And typically what you get when you have a pitting corrosion attack is a cathode and an anode area on the middle that is being attacked with some sort of barrier or corrosion products, some more permeable than others, depending on the conditions that you have, whether they are due to bacterial colonies or due to just these various iron hydroxides, iron oxides. They are more or less permeable to the F8++ ions. If they are less permeable, the pitting doesn't go on very fast; if they are more permeable, as you get, apparently, in anoxic environments, you get these ferrous hydroxides in the shells over the pit. It will go on faster, and I have got some data to show where the pitting rates in anoxic environments can be quite high.

So the idea here again is that the local chemistry is very important when it comes to the pitting corrosion correlation that you have to worry about.

(Slide)

What we have done -- and most of the data -- this was a mistake. It is cast alloys, not case alloys on your Vu-graph. What we have done is take a look at a lot of NBS data, and there is a tremendous data base when it comes to carbon steel and low alloy steel corrosion. I wanted to put this slide up here to show you for what we have been looking at -- there are some 12,000 different specimens that NBS has studied, and about 50 different systems, from very anoxic to oxic, not many with radiolysis in them. That is one problem, and I will address that in a little bit, but just a host of data that we are going to have to contend with, it seems because there are correlations for pitting for that data that are well established. They are a function of oxygen in the environment and a function of chloride iron concentrations and a function of temperature more than likely.

So this is to show that there is a lot of data available on pitting corrosion.

(Slide)

This is an attempt to show how the data varies. Most of it fits in the equation where pit depth, some constant time to the exponential power. I don't know. There were 47 different systems and all different kinds of loams that NBS has looked at for up to 22 years or 27 years, so there is pretty good data base, and pitting has gone on for some period of time. Granted, it is not at the temperature that we are worried about, it's at a lower temperature, but it is in an environment that is not unlike the environment that you get in the repository. In fact, it is an environment where you have a close contact with clay up against metal, which is not very conducive to good corrosion, by the way.

But you can see here that the K's vary all over the lot. This constant that goes in front of the time varies all over the lot, from about 20 -- and the units aren't pertinent here. I'm trying to show relative variation. From about 20 to 107. So there is a factor of 5 difference, basically, in the K that you are going to have for different systems. And it is real, because if you look at the sigma, which is the standard deviation on these data points for a given system, the sigmas are pretty small for the K's. So the variation in the K that fits any given specimen is pretty small. It shows that there truly is some sort of correlation as a function of the environment. The K and the N as a function of the environment that you can come up with to try to handle the pitting in the repository.

(Slide)

One of the parameters that seems to be important -- and this is not pitting corrosion, so this is a little bit off base. This is weight loss corrosion, but I think the pitting will show a similar relationship. You can see here this is soil resistivity, the electrical resistivity. If you measure the resistivity of the clay/bentonite/basalt mixture, you would expect some fairly significant differential in the pitting rates, depending on what that resistivity was, probably related to chloride iron concentrations, so you may be able to calculate clearly this is something we want to measure in whatever tests we run so we know where we stand.

(Slide)

I mentioned before the business on the anoxic conditions.

MR. THOMPSON: Are you suggesting that the data in soils is applicable to the repository?

MR. COOK: Yes.

MR. STEINDLER: Why?

MR. COOK: Because the variable in the soils, the oxygen level, the electrical conductivity, the temperature --

MR. STEINDLER: You have got the same mechanisms?

MR. COOK: Yes. I think the same mechanisms are going to apply.

MR. STEINDLER: Do your friends at DOE agree with that?

MR. COOK: I'm not sure. We haven't really discussed it that fully with them. I think Dave Stahl is going to talk to that after us, really. In fact they are trying to work on correlations which take into account some of those parameters, but the thought is that the oxygen level, the conductivity, the temperature are two variables. Radiolysis changes the oxygen level, and it is in that sense that it affects the corrosion mechanism or the corrosion rates, so you have to calculate that and you go into the systems. That is why we want to set up the test properly so we do determine these environmental conditions and the chemical conditions locally and properly in the test because that is where it is going to tell you where you fall, where your case at the ends are in that previous equation.

This shows some data from the NBS report which shows the variation in the bit depth, so the slope of this line is the pitting range as a function of oxygen level, and it goes from very poor, high pitting rate to very good, low pitting rate. So there is a function of oxygen in the system, and it may be bad to have anoxic environment when it comes to pitting corrosion in carbon steel. In fact, I think a lot of people would conclude that.

(Slide)

That is one of the things we want to get the test focused on. The reason I am highlighting this is this is the disagreement. The tests are not focused on limiting mode failure in the carbon steel.

(Slide)

Here is another example of variation depending on the type of environment you have. I just put that in there to show how the -- I don't have the key parameter on here. There has been a proposal that you could take and multiply the general corrosion by some factor to get the pitting depth, okay, for carbon steel corrosion. Well, it is probably true if you have an oxic environment and you have got a significant general corrosion, but when you get an anoxic environment, you can get very large pitting factors, which is a multiplication of the number that you have to use against the general corrosion to get the right pit depth; 4 isn't enough, 11 isn't enough. We don't really know what is enough because we don't know what the environment is truly in the repository, but you can get a big variation. You cannot just take general corrosion and multiply it by a factor to get pitting.

(Slide)

Okay. What is the punch line and what do we conclude? We didn't but one of our consultants did, took an example of one of these 47 sets of data out of the NBS data and looked at -- did a statistical analysis, reliability analysis and tried to derive that failure rate curve that I showed you a little while ago and came up with some confidence probability statements as to what you would expect in terms of mills. This would be 4.009 inches of metal that you would have to go through under mundane conditions, room temperature conditions, basically, to get some different reliability confidence probability statements on the reliability of the containers. And granted we didn't know what PK to the

TN to use. We picked one that we thought was reasonable, but the key thing was the scatter data for a given system, which was probabl going to be less because those systems were pretty well defined. They had a very localized situation, so the scatter in that data was well defined.

This is a typical engineering confidence reliability zone of about 4 inches for carbon steel for 1000 years. We would be pushing it less if it were 300 years. So this was to get our own idea of what probability you would have, and you can make it. You can make a reasonable engineering judgment with carbon steel. We concluded that carbon steel is a viable material.

The punch line is we are not sure from pitting corrosion and the thicknesses that they have identified that it is going to be satisfactory from our assessment here. Granted it is rough, not having a knowledge of the detailed conditions but knowledge of the typical scatter in the data we have which we have to contend with from probably better-controlled systems.

(Slide)

So that is really the punch line. So in summary, we need to get together with BWIP on the analytic methodology on this fault tree so we can see the failure modes they are considering, look at the programs that they are using, if they are using programs, agree, yes, this is the mode of failure that you should be considering, or if not, argue about it and try to figure out what it should be. It is going to be amenable to reliability analysis if you do it that way. You put your variables in in a statistical fashion and reliability analysis will bear it out.

We think that, addressing Dr. Axtmann's issue, that they need to establish reliability design requirements. This is consistent with what we think are good design control methods. ANSI in their quality assurance provisions for design control are now suggesting that you have reliability design numbers established. It is not a requirement but they suggest it is a good idea for engineering systems. DOE's general orders for projects require that they have reliability design requirements identified, so this is not anything that's new, it's an old engineering method of design control and assessment.

We think this needs to be done and give a focus for the project to work toward. Not to say it can't be changed. I mean if something comes up where the number that they have selected -- you can't meet it or you want to improve it, you can improve the system and get a lot more confidence out this part relative to that part of the system and you can do it. This is a flexible number but it is needed to focus testing. It is needed to focus testing as well as these analytical methodologies. You want to know what your method is so you can go off and get the data that will fit into the methodology. The potential environmental conditions are the key problem area, particularly the chemical conditions for corrosion and for packing material integrity.

The conclusion was it would not likely work because of pitting corrosion and potentially, as well, hydrogen embrittlement. I don't have the data to give you a good story on that one for carbon steel. You need to have a thicker material and a thicker canister if you are going to use carbon steel. If you picked a material that had less variability, like copper if it didn't have other problems -- you see that the scatter goes way down. If I went back to this last curve --

(Slide)

This one. We did look at copper to see what variability we would get. You can do with a lot less copper because it is much better behaved as an engineering material. It is a pure material. You don't get problems in material variation in it. There has been some thought that pure iron would be better because it has less variability in its performance, and I don't know whether Dave Stahl is going to talk about that or not, but that is an idea that we want to parlay to DOE too. It is not any more costly. It may improve the performance.

That is all I have.

MR. MOELLER: Dick Foster.

MR. FOSTER: I'm trying to get a little better feel for how much of this is generic versus how much of it is site specific. I have "what if?" question. If we had been listening here today to characterization of, let's say, a shale site instead of a basalt site, how many of your Vu-graphs would be different than the ones we saw today?

MR. COOK: Not many. They would be about the same. I think the corrosion mechanism, particularly if you were to use the design where you have a clay packing material, you wouldn't see much difference. The bentonite clay in the packing creates a crevice and a local chemistry -- it would differ if you had a lot different chlorides, and it would differ if you didn't have any methane in the water. I think there is something like 700 ppm methane. You get that in a radiation field and you are going to have very nasty chemistry that you are going to contend with in the packing material. So although it may not affect the corrosion, and I'm not sure that it doesn't affect corrosion in the long run, it is probably going to affect the packing material, I think.

MR. FOSTER: What I understand from what you are telling me is that this is something which is characteristic of how you build a canister and it has got not too much to do with whether the site is here or someplace else.

MR. COOK: That's right. Analysis methods apply across the board. What I said for carbon steel applies across the board. The same sort of correlation is going to apply, I think. A steam environment might be different. A dry environment could change it.

MR. FOSTER: Another question which is quite a bit off that track. You mentioned radiolysis quite a bit. I am wondering if anybody has looked at the potential gas generation from radiolysis over a long period of time and whether that is going to be enough to create any kind of a gas pressure inside this repository that is going to keep the water out.

MR. COOK: The Swedes have done that. The Swedes have run test bentonite with the radiolytic environment, and I think it was within a year you got hydrogen gas bubbles forming in the bentonite, discrete hydrogen gas bubbles. So the temperature is somehow going to affect the diffusion of the gas out of the system. You might have thought hydrogen would diffuse out quickly, but it apparently doesn't. It is going to be an effect that changes this chemistry. That is all the more reason why it is very important to get the proper environment in all these tests that are corrosion tests, so-called, because if you don't have the right environment, you might as well forget about all the tests. If you do them in pure water, that is not going to be representative of what you have when you have got clay up against the material. If you don't have radiolysis -- the same Swedish program in testing shoes that the g factors when you have bentonite present are 30 percent above. That's from the various hydrogen peroxides. And it shows that they are 30 percent above normal water g factors because of the better transfer of energy, apparently. That is one of the arguments. When you have got the bentonite and water in very close communication with each other, the radiolytic energy is transferred better to water, apparently, but you have got to do the test to get the conditions with the actual materials that are there to define where we stand, and radiolysis is going to be a problem, there is no doubt about it.

MR. FOSTER: I am thinking here just beyond gas in the water. I am thinking whether you create enough gas to create a great big balloon down here so that you aren't bathing things in water, you are really bathing them in gas.

MR. COOK: I don't think you get that. I think people could have a feel for how much gas you form. I don't think you get that much gas generated.

MR. FOSTER: You have got lots of time.

MR. COOK: But you can integrate how much energy you have going into the water so you don't get that much. I don't know if anybody could comment to that. I'm not positive.

MR. MOELLER: But he is asking over 10,000 or a million years or something, what do you get.

MR. COOK: Well, the temperatures are going to be important because they will drive gases away, particularly once you get out into the rock, you know, into water, whatever cracks or whatever water you have. In the clay it is a little different, particularly in the short term. Radiolysis goes away, for all practical purposes, about 500 or 600 years.

MR. MOELLER: Martin.

MR. STEINDLER: I put up this probability confidence calculation that you did on the steel. Are you suggesting, then, to DOE or somebody that the 99 percent confidence, 99 percent probability should be a target?

MR. COOK: No, I'm just saying it's a common engineering number. What they have to do is look at their whole system. This is one component in the system. I think if it were 50-50, I would say no, that's not going to fly, but --

MR. STEINDLER: What have you done to confine the 1000-year term?

MR. COOK: You mean whether it applies or doesn't apply?

MR. STEINDLER: No. What does it mean?

MR. COOK: Any way you can get a nuclide out of a package.

MR. STEINDLER: Any out of the 25,000?

joy 15

MR. COOK: Yes.

(Slide)

Now, if you look at what we said here --

MR. STEINDLER: So you are telling DOE that none of their canisters can fail on the first 1000 years.

MR. COOK: No.

MR. STEINDLER: You are not?

MR. COOK: We are saying that if you have a canister fail, that is a canister for any given one. You are bound to have some. If you looked at that first curve, some of them are going to be failed when you put them in there, we believe. So you are going to have a failure rate, and the key thing is you have 95 percent of them work, you have 90 percent, 50 percent. What is the number? It is going to depend on the goodness of the rest of the system -- it's the systems approach here -- and how good you want the overall system to be.

So we are not trying to dictate any particular number. We wish that DOE would establish some engineering target for the people so they can go and have a meaningful way of setting up your test programs.

MR. STEINDLER: I guess I'm not making my point. Have you established for somebody, either for your own planning or for DOE, how good you want the system to be.

MR. COOK: The overall system?

MR. STEINDLER: Yes.

MR. COOK: I don't think we have done that other than in the qualitative terms of the rule.

MR. STEINDLER: It says it shall contain reasonable assurance.

MR. COOK: Right, and that's a variable time period and it depends upon the temperatures, basically.

MR. MILLER: I think it might be useful now for Mel Knapp to say a few words about the course we are on.

MR. MOELLER: Why don't you do it in your presentation, Mel. I have one question. I hope it can be a quick one.

Yesterday we asked the question: before the backfill is put in, you might have the canisters in the steam atmosphere at elevated temperature. I believe we were told that that was a pretty good situation, or at least it wasn't a bad situation.

Now, do you have any questions there?

MR. COOK: Well, if you just have steam, it is probably not too bad. Steam corrosion, I think, is generally better, certainly better from a pitting standpoint. The question, though, of the conditions is a key question, and if you have got water that has got 700 ppm solids drifting into this, some of it 2 liters per year and some of it 100 liters per hour insome of these holes -- that is the reason we are interested in this variation, this statistical statement, so you know what to expect statistically. But if you have got 700 ppm solids, chlorides, fluorides, whatever, dripping into the hole and it evaporates, I think on one of these things I calculated, if it is up to snuff it will vaporize 130 gallons of water a day. That means it turns it from water to steam. You could get like a couple kilograms of salt being deposited in here per day.

So you can get a very salty environment. That is another issue when it comes to using this horizontal borehole design. What is the condition in the hole when you go to put your backfill in? Does it have 40 tons of salt deposited in it? I mean you are going to have to go in and inspect it -- it's not a very good engineering system -- to know what you have when you start and hope to go forward with it. So the salt question is a big question, and I'm not sure that the naive position with steam is right. If you have got water dripping in and salt is in it and it evaporates, you are going to have salt left. So it could be a lot worse.

MR. PHILBRICK: But that stops when you get it back.

MR. COOK: Fifty years later, but you have got to contend with the salts then. You backfilled all these salts in and you have covered them up and they are right up against the package, so the environment you have could be quite a nasty environment.

MR. PHILBRICK: Where are they going to put the canisters, at the floor level or above it?

(Slide)

MR. COOK: They have got it above the floor level.

MR. PHILBRICK: Are they going to fill up the space from the floor level to the canisters?

MR. COOK: Here is what it looks like. This is the canister. Here is the floor and this is a 27-inch or 30-inch diameter --

MR. PHILBRICK: Where is that with respect to the floor of the placement room?

MR. COOK: The placement room is off over here, and I don't know what the distance is between the floor level and the placement --

MR. PHILBRICK: How are you going to get the salt up to the canisters?

MR. COOK: If you have got water coming it, it's going to drip down on these high canisters that are above 100 degrees C., and the water will evaporate and the salt will be left right on the canister or on the bottom, whatever the temperature is of this thing. It is probably going to remain in the canister because if you get enough evaporation on these rocks, it is going to cool them down somewhat.

MR. PHILBRICK: Maybe you ought to backfill the thing at the beginning.

MR. COOK: If you are going to backfill at all, I would think it would be better to backfill from a performance standpoint right away, and the idea of having another uncontrolled set of conditions to contend with for a long term is a problem. For 50 years is a problem, I think. So you would be better keeping the salt out, probably. Then again, the retrieval is a problem.

MR. PHILBRICK: If you didn't have any drip, there would be no reason for backfill, so it is a condition that you develop as you build the repository. It doesn't have to be solid now.

MR. COOK: Well, you certainly have to anticipate what happens if you come down there and you have got this design all worked up, and now you have been anticipating this particular design and you have run all your tests and you get your data together and that doesn't turn out to be right, and then you are back to ground zero starting to run tests again in different environments.

MR. PHILBRICK: You are just like any other guy doing the job. He finds conditions are different, so you modify your stuff to do it the way conditions are,and it's the site-specific thing and it ought to be in the hands of the guys that are operating the repository. You shouldn't get into a whole international problem with it.

So I have got a little salt coming on top of the repository. I fill up the hole and that is done. I don't have any salt coming in. I don't have to worry.

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I don't think we ought to solve all these damned problems now. One, two, or three generations after, what are these guys going to do for fun if they don't have the problems to work on?

(Laughter)

MR. MOELLER: Okay, with that --

(Laughter)

MR. MOELLER: -- let's take 10 minutes and then we will resume with the Battelle Columbus report.

(Brief recess.)

MR. MOELLER: The meeting will resume.

The next item on our agenda is the status report on the long-term performance of high-level waste packaging materials and the research underway at Battelle Columbus. And to introduce this topic will be K. Kim, the project manager for NRC.

MR. KIM: This project just passed the 1-year mark since it started, and I thought that it would be a good idea for the contractor himself to present the program. So I will turn over this microphone shortly to Dave Stahl, who is the project manager at Battelle Columbus Laboratory.

Before I do that, I would like to make a couple of comments. First of all, the presentation that you will hear is not site-specific. His presentation will be organized more or less according to the type of material that he is handling more or less along with the project structure, which is experimental analysis of the methodology for research and for the matrix and the waste package modeling. So his presentation will go along with that line rather than the site-specific fashion.

The second one is early in this project, before the selection of the contractor, ACRS made a recommendation. And specifically, they made two recommendations. One is getting a peer review from outside to make sure the program is going soundly, and also more involvement of in-house technical staff.

Some response to that ACRS recommendation, this project has been reviewed by a peer review group, a small group, and early in the program last July and Professor Doramus and Professor Russell served as the panel for the peer review. Doramus is known for his glass work, and Russell is known for his metallurgical research work.

At the beginning, I was the project manager, and then we had a couple more staff added to our branch. And now we have two additional staff who is involved in this project. Dr. McNeil will handle the metallurgical part of the projet, and Dr. Randall is following the modeling part of the program. And I continue to coordinate the whole project as well as follow the host matrix part.

So with these remarks, I would like to turn this microphone over to Dr. Stahl.

(Slide)

MR. STAHL: Can you hear me?

MR. MOELLER: Yes. Very well.

MR. STAHL: It's a pleasure for me to present an overview to you on our Nuclear Regulatory Commission program, which is called Long-Term Performance of Materials Used for High-Level Waste Packaging.

This program is being run at Battelle Columbus Laboratories as opposed to the Battelle project management division. So we are separate and distinct from that operation.

(Slide)

Today's presentation is going to be an overview, as I said. We are going to be dealing with several of these items or most of these items here in detail and some of them just in passing.

Of course, the objective of the program is strategy that we have taken, the structure of the program, certainly our accomplishments in the first year, and what we plan to do in the second year. And I will, of course, attempt to summarize the program for you.

(Slide)

This is an informal presentation, so if members of the committee have questions, please interrupt as we go along.

MR. MOELLER: Don't encourage them too much.

(Laughter)

MR. STAHL: Well, I have been involved with review committees for many years while I was at Argonne, providing the reactor safety review committee. So I am very familiar with the review process.

This is the objective of our program to develop a predictive methodolgy that can be used by NRC to evaluate and license candidate waste package systems that will be used in a repository for a long period of time; that is, 1,000 years.

This methodology will be used with other codes and will be interfaced with other codes that the NRC is developing elsewhere.

(Slide)

For example, the work at Lawrence-Berkeley on geochemistry and the work at Sandia on the SWIFT Code. This strategy is basically a simple one. The first point is use of available data. We don't want to reinvent the wheel. We would like to take full advantage of the Department of Energy and the NRC-sponsored work in waste management as related to waste packages and its environment. So that entails certainly a very vigorous review of the literature, which we have accomplished during the first year of the program.

And then using that, the second part of the strategy is to identify those critical areas where we have to perform experiments and do model development where no models exist. Again, it's part of the strategy: Don't redo what's been done before, but we need to evaluate it and make certain that it meets our criteria and that it can be supported.

There are two parts to the modeling effort. One is to evaluate the condition of the barriers as a fucntion of time in the repository and at any time of failure to be able to identify what the radionuclide inventory is and its release.

As I mentioned, we do both experiments and modeling in an interactive way so that one can benefit from the other. I show that schematically on the next viewgraph.

One of the major milestones in the first year was to provide first-generation system model, and we have completed that and we have just issued a draft version of the annual report.

The focus in the first year was to develop simple models. And as we go through this loop several times it will be more comprehensive. Models that will be developed. And as we show in the next bullet, in addition to taking deterministic phenomenological approaches in the early going, later on we will be factoring in some probabilistic approaches, much like Bob Cook mentioned earlier.

We must look at what the probabilities are of failure and look also, as was pointed out, on the birth defects and the general degradation of the barrier as a function of time.

And lastly, we must be able to validate that model and issue a final report which the NRC can use as ionput to the licensing process.

(Slide)

Briefly, this is shown schematically here. It is just a simple loop. We want to be able to use that information to develop those tests that we think are critical and needed, perform the experiments and develop those models, come up with some results which we can evaulate on the basis of the known data. If it does a very good job of defining everything, certainly we can go to the validation stage and out.

Our plan is to go through this loop several times until we can get a consistent and meaningful methodology which would then be able to validate and conclude the program.

MR. MOELLER: Can you remind us of the overall schedule?

MR. STAHL: Yes. I was just going to mention this is a 5-year program, and as you will see later on one of the other viewgraphs, some of the major activities as they go along with time.

(Slide)

This is just a partial listing of some of the candidate materials in the host matrix and cannister/overpack that were considered in the first year of the program. Some of these were considered before the program actually began.

Certainly, we knew that a borosilicate glass was going to be considered, and early on it was a high silica glass and Synrock, which was the backup to the reference borosilicate glass.

Later on we certainly recognized that the high silica glass was not going to do it or be ready in time, so that the program switched to looking at the commercial and defense standard at least at that time borosilicate glasses and Synrock-C.

On the cannister/overpack side again, these were the materials that were identified early on, perhaps as much as a year and a half ago. So the materials which DOE would be utilizing in the cannister/overpack, those got somewhat more solidified certainly in the inner cannister which would be used to hold the waste glasses. The type 304L stainless steel, that has been pretty consistent. One of the cast steels that we thought could be used is ASTM 65-35, which is a low carbon steel approximately equal to a 1020 steel. And lastly, of course, TICODE-12.

Now, again there have been some changes to that.

MR. MOELLER: Why do you put the word "overpack" on there? Help me. I understand that these could be the cannister metals.

MR. STAHL: I will show that schematically further on. I think it will be a little clearer.

(Slide)

MR. STEINDLER: How important is the selection of specific examples for steels in waste form to your ability produce a generically useful methodology?

MR. STAHL: We would certainly like to be able to focus in as closely as possible to the materials that DOE is using. And we have chosen, as you will see in this next slide -- so thank you for the lead-in --

(Slide)

-- current materials. But what we will do in the program is to perform sensitivity analyses and some experimental work around the composition of interest, so that if there are small changes in composition, the code will still be able to handle it, certainly, if there is a dramatic change. And certainly the model and experimental programs will have to shift in order to accommodate that.

MR. MOELLER: I guess I was thinking when you first began, or as my memory recalled, I was thinking you were looking simply at the cannister. You are looking at the system.

MR. STAHL: The emphasis is on the waste package interior to the backfill or the packing interior to the packing. However, as I show over here, there is a very strong influence on the packing material in the repository.

MR. MOELLER: On the performance of the cannister.

MR. STAHL: That's right. Certainly, it's going to influence it.

Now, let's just list the present candidates. And basically they haven't changed too much. The borosilicate glass instead of the 77-270, I think we had on the last slide, there have been some problems with that particular glass. So they have gone back to the 76-68 glass, a slightly different composition.

Savannah River is still looking at 131, but they're modifying that slightly and looking at what they call Savannah River 165. So again, small modifications in composition that the model should be able to adjust to that.

Interestingly, of course, in the host matrix list is spent fuel as opposed to Synrock. Synrock is essentially dead, at least in the DOE program, and it's very likely that spent fuel will be placed in a repository perhaps before any reprocessed material is. So that will be included in our program, and some activity will be initiated on looking at spent fuel.

The cannister/overpack materials are basically the same. The packing materials reflect the three standard repositories as far as the basalt at the top and the basalt repositories. Certainly, we will be looking at granitic type materials when that part of the DOE program gets going.

MR. MOELLER: Now, is the waste matrix primarily of importance in terms of the retention of the fission products or important in terms of interaction with the cannister, or both?

(Slide)

MR. STAHL: Mostly the former. There is a little bit of interaction. This is a slide showing the cannister for waste package concerns. It's in your handout, as I mentioned. The repository and the backfill character are going to be important inputs to the model because that will establish the chemistry at the groundwater. And certainly, the temperature, the pressure, the pH and the Eh as well as the chemistry are going to be important parameters in the model and certainly in the waste package performance.

The radiation field is also important. We are using the standard unshielded package right now, assuming that we will see radiolysis in the groundwaters.

This is a fairly standard listing of the degradation processes. I don't want to go into detail. Bob and others I am sure have covered it. Let me just mention what our approach has been in the near term. And that is that we have looked at general corrosion and we have got some models there, and we will get into that in a little bit more detail. Piiting and corrosion and hydrogen embrittlement. Those have been our near-term thrusts.

As far as the waste form degradation processes, dissolution has been our major concern. And we have also looked at radiation damage and thermal aging to some extent.

Now, when these barriers are degraded, then of course groundwater and brines will come in contact with the waste form. Then we are concerned with turning on the release module of the code. And we are dealing initially with solutions, assuming that all the radionuclides are in solution. But in later phases of the code we will consider colloids and precipitates. And we hope to get very strong input from the Lawrence people who are working in that area.

(Slide)

This you also have. It's an indication of the structure of the program. It's indicated on here, as Dr. Kim mentioned who is the project officer coordinating the entire effort. This is the initial structure when we began the program. And the project manager reporting to Dr. Kim and also to Dr. Milford, who is director of reseach at Battelle Columbus.

We also have some quality control or assurance type activites on the technical side through the project advisory team, which is made up of senior department people, and the research council, which are senior people at Battelle.

MR. MOELLER: How frequently do they meet with you and in what manner?

MR. STAHL: The project advisory team meets roughly on a quarterly basis to hear our input, and they certainly receive from us on a monthly basis all our monthly reports. The research council, on the other hand, just sees our reports. We have not made a presentation to them as yet.

I am the project manager, assisted by Neal Miller, who is somewhere in the audience. Neal and my staff of technical peole of various disciplines approrpiate to the activities in the program.

The thing I want to mentionabout this particular viewgraph is that all the people who started on the program are still actively involved after one year.

(Slide)

Now, as Dr. Kim mentioned, we had a change or slight reorganization, so we have adjusted the structure of the program accordingly. And this reflects the fact that when you have three project officers on three generic tasks -- the waste form task under Dr. Kim; cannister materials task under Dr. McNeil; and the modeling task under Dr. Randall. And these three task leaders essentially in that position initially interface directly with the NRC project officers and, or course, are monitored by myself and Neal Miller as well as Dr. Kim.

I think there is good communication throughout the structure.

MR. MOELLER: What percent of your time are you spending on this project?

MR. STAHL: I spend about 50 percent of my time in the program.

I should point out this box on separate effects and statistics is a very important area, and that's why I highlighted it here. That's why Dr. Markham is the separate effects expert, and Dr. Feder is statistics and accelerated testing expert.

MR. MOELLER: What percent of the time do the three BCL task leaders spend on this project?

MR. STAHL: They spend also about half time on it, I guess. But they have additional staff working for them that might be involved in smaller or greater amounts.

(Slide)

The program, as you know, is about a million-dollar-a-year effort, so that would give you a rough handle on the number of people who are working on the program.

I don't want to dwell too long on this. It's a standard work breakdown structure showing the new activity where we have the waste form area, the cannister material area, and the system modeling area.

(Slide)

Now, this -- I am sorry, you won't see in the back, but it just gives you a flavor from there, at least, of the three major tasks in the program. It is ceratinly clear in the handout. And I will get to some of the more specific activities within each area on the next set of viewgraphs so that you can see what is happening particularly in the program.

What I just wanted to point out here are some of the extent of each of the activities. For example, down here, when we get to system modeling, we provided the first cut, the first-generation system code here, and we will upgrade this on an annual basis until we get to the validation stage in the last year.

(Slide)

This again is an overview chart, and I won't dwell on this because I have individual breakouts of each major activity. The purpose of showing this is to just kind of indicate the kind of interactions that go on between each of the tasks.

For example, here when we have developed a kinetic glass dissolution correlation that goes to the system modeli input at the end of the first year to put into the model, and of course it's upgraded on an annual basis.

(Slide)

Let me now go through some of these near-term past and future activities in each category. This is the waste form area, as you can see. The timing, this is the first year, the second year, and of course on to the end of the program.

As I mentioned earlier, the major emphasis was put on looking at the literature and evaluating what was there on the models and the data. We needed to evaluate separate effects, particularly in the waste form. We were concerned with dissolution, as I mentioned, radiation damage and devitrification and stress. Those are the four principal areas.

We need to look at each of those and later on to try to determine the interaction of each of those effects. We designed some experiments for leaching. At the same time, we looked at developing an equilibrium glass dissolution correlation, and we have done that. That is a simple silicate control model.

We have also developed an initial glass devitrification correlation using a standard correlation that has not been input to the system model at this stage because basically we don't see a problem with devitrification. But we are looking again at that and at radiation damage and other factors that may enhance devitrification so that hopefully in the second year we will be looking at inputting a devitrification model if it were necessary.

One of the things that we have done in the near term is to look at thermal effects using a TRUMP code, which is a standard type thermal analysis code. We have determined conservatively the temperature of a cold or cooling waste form as a function of time, and we are using that in these devitrification models to determine the degradation basically of the glass lattice. And as I said, this will be used in this devitrification correlation, which will then be input into a dissolution type model to determine what that interaction is.

MR. STEINDLER: That isn't the major problem in the case of devitrification of glass. It can drop fission products that now have much higher solubility and a high rate of dissolution. What are you going to do with that information on the silicate lattice? Why is it applicable? That's question one.

Question two: In the more generic sense, I guess, you talk about developing a kinetic glass dissolution correlation.

MR. STAHL: Yes.

MR. STEINDLER: It's I think generally conceded that, number one, glass doesn't dissolve in a homogenous fashion.

MR. STAHL: That's right. That's why the simple thermodynamic model is more of an equilibrium model. In other words, you reach saturation.

MR. STEINDLER: The point I am making is that that saturation is reached at different levels depending upon which particular material or fission product you focus on. Even more importantly, you have detected that 76-68 borosilicate, that it may have no real relationship in actual kinetics to whatever it is that you or anybody else is going to see. How are you going to be able to address your --

MR. STAHL: Let me answer the second question first. Along with these approaches we are also developing a topoligical model which will describe the performance of borosilicate -- classic borosilicate glass as a function of time, so that whenever a particular chemistry is established we will be able to go to that model and be able to pick out some performance information.

That is, I think, a general response to the question. I don't know if I can be more specific.

Do you want me to get back to your first question?

MR. STEINDLER: It's solved by your second answer.

MR. STAHL: Okay. Thank you.

(Slide)

That is our activity in the waste form area.

This is the cannister material area -- oh, I am not sure I addressed your comment early on. Let me get to that other figure. Before I go on to that, let me talk about this.

(Slide)

This is the waste form which is the borosilicate glass. Now, that is contained in type 304L stainless-steel cannister. It's usually the definition of that material. Now, the cannister would be contained in an overpack or an overpack support, and that may or may not be clad with an overpack of TICODE-12. So we will be looking at all of those materials in the corrosion part of the program.

(Slide)

Okay, now we can get back to this. Several activities here have been going on at the same time. We have been doing some autoclave testing, for example. We have done model development, some slow spring rate tests, some hdyrogen embrittlement tests. And we have also performed some preliminary glass-steel tests to look at internal corrosion. Let me start from the beginning again.

We needed to look at the information that was available from the literature, and there is a great deal of it, but unfortunately a lot of it is not either well-documented or predictive of actual repository conditions.

So that's a starting point. Certainly, as far as models are concerned, there aren't too many very well accepted models for general pitting and corrosion, although there are some, and we have been following those very closely.

As Mr. Kim had mentioned earlier, we have been using some consultants on the program, and Profesor Doramus has consulted with us in regard to the waste form program, and we have used Dr. Dick McDonaldson from Ohio State University in modeling general empitting corrosion. So he has helped us out tremendously there.

Let me talk about the autoclave tests. As I mentioned early on, we started with very strong input that TICODE-12 would be primary reference material either as an overpack support,a thicker member of titanium alloy. So we started out looking at TICODE-12 and various brines, brine A or brine B, from Sandia chemistry. And we did some standard autoclave tests, 250 degrees C., presurized tests, 2,000 hours. And basically as a baseline we were able to reproduce the data that was available in literature. That was an isothermal test.

Then we went to a test which provided a thermal gradient across the specimen both axially and radially. We took a cylindrical specimen -- in this case of TICODE-12 -- put an internal heater in it and partially submerged this in brine, the same brine at 250 degrees C. But there was at least a 25 degree C. gradient along the axis of the specimen.

One of the things that we found out, a little bit to our surprise, is that that gradient was enough to cause vapor phase attack. This is a titanium-lined 276 autoclave. And the vapor, which we found out later was hydrogen chloride, came from a decomposition of the magnesium chloride salt which was splashing onto the upper portion of the specimen which was above the liquid and producing deposits of magnesium hydoxide, so that the pH of the system which initially started out at about 7, I believe, went down to about .65.

Now, we did not see a great deal of corrosion, general corrosion of the TICODE-12 that was submerged, which agreed with the isothermal data that we feel that if indeed a similar system had used a carbon steel, for example, it probably would have been wiped out very quickly.

So as I said, that was a surprise here, and it just reflects the comment I made earlier, and that is, you have to be very careful when you do your corrosion experiments. For example, a lot of the work that was done at Sandia on salt was done in an isothermal environment, and if you don't consider any gradients or vapor phase activity, you may be mistaken in your program.

We have also done some crevice/corrosion tests with TICODE-12, and there are some minor effects there which basically support some of the results that the Brookhaven people found and people at Sandia found, although I guess they intend to deemphasize their observations at Sandia.

As I mentioned, we have developed the initial water chemistry and general corrosion correlations and are now working on developing a pitting correlation.

One of the things I want to point out to amplify Bob Cook's comments, again you have to be very careful here in looking at pitting corrosion and general corrosion as well that you have to consider these concentration effects if you start out with a dilute salt or brine or even a basaltic groundwater, that you could have concentration of the salts in water as a function of time due to evaporation and condensation mechanism. Bob touched upon that.

But it certainly is going to influence very strongly pitting reactions, and that is something that we will be looking at later on in the program.

With regard to slow strain rate testing, there are no surprises there. Basically, we reproduced the fact that it was steam conditions that we chose, there was no strain rate problems, no stress corrosion cracking.

We have also initiated a program on hydrogen embrittlement. The basic purpose in this portion of the program is to compare wrought and cast steels with the same composition, basically 1018 steel. So we have cast large quantities of 1018. We have cut those castings in half. Those have been wrought. Specimens have cut so that we can compare cannister wrought structures.

We have also looked at doped and clean steel. We have got some preliminary tensile data, which is about the kind of results that you would have expected in hydrogen as far as embrittlement of the cast and wrought products. We will be factoring in our results with the results that will be obtained at Brookhaven using an ARMCO-type iron, which is a very pure iron. So we should be able to get a very clear indication of what normal impurities in steel and higher levels of impurity will do to hydrogen embrittlement.

We will also be factoring in the effect of radiolysis later on in the program with some work that we do and the work that is done at Brookhaven.

END 7.

MR. STEINDLER: If you look at phase -- do you control the redox potential?

MR. STAHL: We control it. We measure eH and pH as a function of time at temperature.

MR. STEINDLER: You have got a way to measure pH at 250?

MR. STAHL: Yes. I can give you the details later. I'm not the expert in that area.

(Slide)

Okay, let me go on to the modeling area. Again, we have reviewed and evaluated the available system models, and as you can see, I don't draw any lines from here to our work, basically because there isn't much to draw upon. We have looked at the barrier in the WAPPA code and we feel the approaches that they take are not consistent with what we are looking for. We felt that as a first cut we thought we would try to develop our codes independently of theirs and then take another look at it later to see what contributions that coding can make to ours.

As I mentioned, we were developing consecutively -- or concurrently, excuse me -- barrier degradation models in radionuclide inventory and release calculations. For the inventory and release calculations we are using an origin of a '79 formulation which we modified to eliminate all of the inputs which have to do with the reactor, so we are taking the back end of the code to reduce running time, basically, and we grouped the radionuclides in their various categories for ease of running the codes, and that has worked out fairly well.

We have tested that against the full '79, so we get very good results with that and very much reduced running time. As I mentioned, we have input these three or four, at this stage, simple separate effects models into the code which has been formulated to accept that as a function of time, some of those as a function of temperature as well, but we are going to, as I say, have more comprehensive models later on in the program.

We have coded that flow chart and I show that in the next Vu-graph, but I don't intend to go through it. Next year one of the important things we are going to do is try to improve that release rate calculation by looking at colloids and precipitates, and we will need to have a very strong input from the Lawrence-Berkely work, and certainly we are going to be inputting from our own work the year two separate effects correlations which I mentioned, particularly in the waste form, and we will also be developing a thermal and radiation transport model for the code, taking in, as I mentioned, radiolysis effects and also thermal gradient across the waste form in the package.

Another important item which I had mentioned at the

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outset was that we needed to interface this code with other NRC codes. It is written in FORTRAN-5, which I think is fairly standard and will be able to be used with other codes as a source term. Certainly we need to upgrade that as a function of time, and as I said, we will be going through that on a loop to reach the completion of the program.

(Slide)

MR. MOELLER: What is the percentage distribution of your work that is theoretical versus experimental. You, of course, are doing both.

MR. STAHL: Sixty percent experimental and 40 percent analytical.

MR. MOELLER: So there is a lot of supporting experimental work.

MR. STAHL: That's right. Now, this chart you can look at at your leisure. This is reduced, and certainly you are not going to be able to see that. I can't even read it from here. But what I just want to say about this is that we do have various loops in the system that look at the barrier condition as a function of time. For example, the containment condition at zero condition is that it is unbreached. There is no way that the fluids from the repository backfill can get into the waste form. Condition 1 is that we have penetrated the barriers and there are diffusion equations then which are used -- coupled diffusion

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equations which are used to move materials back and forth across the various volumes. Condition 2 is when you complete degradation and there is no barrier to migration both in and out. Various packages will be input into this loop, the general corrosion, pitting corrosion, hydrogen embrittlement and so forth, to establish the containment condition as a function of time.

Then if the barrier is degraded and goes from zero to one, for example, then it can go out into the rest of the code to determine whether radionuclides are released. For example, in this branch it will calculate the radionuclide inventory and the release as a function of time for particular nuclides in this grouping, as I mentioned, and I will go through that loop, until we will be able to output a print and at that point be able tostay up with the nuclide release.

(Slide)

MR. MOELLER: In the box over at the side that says restart, what does that mean?

MR. STAHL: That is a user convenience. If you run through, for example, and you want to pick up on a particular aspect of the code, you don't need to start to scratch. You have a restart code which picks it up at that particular point, and then you can modify any particular parameter. It will be a user-friendly type of code.

I have covered these four bullets and now I will go on to the summary.

(Slide)

I think you can see that from the back. Basically, our year one milestones have been met. The first generation system model has been prepared and we have issued a draft of the annual report. The experimental program is well under way, and as mentioned, we have done some interesting results in the first year.

We noted earlier that the literature that is out there is incomplete. It is inadequate, perhaps, and biased toward particular materials. For example, TICODE-12 appears in the literature to be better than we find it.

We have also seen that the system model can help prioritize research needs and identify those areas we need to concentrate on. I have defined the year two program activities. We will be generating, of course, the second generation system model, and we will be delivering several technical papers and reports of the work that we have completed during the year.

And lastly, we have built confidence that the program will meet NRC objectives and licensing needs.

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MR. MOELLER: In terms of your peer review group and your consultants, what has been their major input or what

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changes have they brought about?

MR. STAHL: Well, early on in the program, the peer review was, I think, very important because they had a different perspective than we had in the Project, so I think that is a very useful process and should be continued.

MR. MOELLER: Well, are there any specific things, any specific research you have undertaken?

MR. STAHL: Yes, particularly in the waste form area, which Professor Doramus made some very useful suggestions on how to approach waste form dissolution, and that was one of the reasons we decided to utilize him further as a consultant to the program, to help guide that particular area.

MR. MOELLER: Then you said that the data base is biased, and you mentioned TICODE. Why is that?

MR. STAHL: Well, I don't want to speak for the Sandia people, but they helped develop that particular material, and I think that perhaps not looking at it as objectively as they could.

MR. MOELLER: Go ahead, Marty.

MR. STEINDLER: To what extent are you tracking the various DOE projects and their output to make sure that your results continue to be, although generic, still be within the frame of reference that they are using.

MR. STAHL: Well, we try to get as much information as we can on the status of DOE programs, but we don't have

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any inside track. We have information available to us basically that is available to yourselves and the general public, and that is published reports of attendance at technical meetings. So using that approach and site visits, we try to stay on top of their program as far as what they consider viable candidate materials for the waste package.

MR. STEINDLER: As various things change -- for example, you mentioned Savannah River moved to 131 and 165 -- have you been able to track that fast enough so that whatever changes you need to make, you can make?

MR. STAHL: Yes, I think that's correct.

MR. STEINDLER: So you don't have any major problems in that area?

MR. STAHL: Not at the moment. But, for example, the TUFF area -- T-U-F-F, I should say -- they are still looking at various materials. They are just still undertaking or have under way a screening program. They have reduced their 17 or 18 candidate materials down to 4 or 6, and they still need to focus down on just a couple of materials that they would do further experimentation with, so we are watching that carefully, but we certainly have not input, at least into the code at this point, any particular material with the TUFF environment.

MR. MILLER: Yesterday I mentioned that we were getting assistance from folks within the research organization

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and research staff. Part of the reason for having people from the research organization on those teams, in fact, is to have them be able to participate wi:h us in the workshops that we have with the sites in orde: to come to DOE in a least burdensome way, if you will, in our data extraction and also to have this kind of research be in the mainstream information flow from DOE.

MR. STAHL: And by the way, we are very much interested in work that Argonne is doing on the recirculating system where they are able to monitor, I think it is, seven or eight points in the system. It is a very interesting work and we are following that closely.

MR. MOELLER: Are there other questions? You say that the system model can help prioritize -- it can help us on priorities, too, research needs. Can you give us any "for instances" that you have already developed? Where is it shown?

MR. STAHL: One of the things that we found, for example, is that in the radiation damage area, we haven't -- there is a broad spectrum of experiments that have been performed, but none of those are really prototypic, and we are viewing that from the modeling standpoint. We have been able to identify specific more prototypic experiments that should be performed to try to put to bed radiation effects on the waste form. In fact, one of the things that will probably

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come out in the next six months, if not an experimental program on our part, at least some recommendations to the Department of Energy of experimental programs that should be followed.

MR. MOELLER: You mentioned earlier that you spend roughly 50 percent of your time on this. What types of projects do you spend the remainder of your time on?

MR. STAHL: I have been working mainly in spent fuel integrity programs, oxidation as a function of time.

MR. MOELLER: This is during storage.

MR. STAHL: Yes.

MR. MOELLER: So it is closely related. Thank you.

Okay. Well, thank you. It is a delight to have a presentation that actually finishes a little bit ahead of time. It is most unusual for us.

MR. STAHL: Thank you very much.

MR. MOELLER: Thank you for coming and appearing.

We will move, then, immediately into the last agenda item on the day, or for the day, and that is performance assessment, by Malcolm Knapp. This will be continuing the NRC review of the SCR.

MR. KNAPP: My remarks on performance assessment are taken in large measure from the performance assessment chapter in the Site Characterization Analysis.

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I will be talking about four concerns today, and hopefully I can also address some of the questions that the ACRS members and consultants have raised about some of the concerns on reliability, and I would also like to speak briefly about capability. I would like to talk principally about how performance assessment relates to 10 CFR Part 60 and what the NRC perspective on performance assessment is.

Based on this foundation, I would then like to discuss the performance assessment issues identified by DOE in the SCR, and I would also like to highlight a few of the recommendations that we made in the site characterization analysis.

One portion of our definition of performance assessment that I would like to mention right now is that we see it as contributing not only to a determination of compliance with numerical criteria in a regulation, but also as supporting the developing of a license application, in this case as supporting the site characterization program. I will speak more about that shortly.

With respect to 10 CFR Part 60, performance assessment is principally related to the performance objectives in the regulation, both to performance objectives addressing the operational phase, exposures to workers as well as releases beyond the control area, and the maintenance of the retrieval option.

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After closure, performance assessment will be very much involved in determining compliance with EPA standard, minimum waste package containment time, maximum radionuclide release from the barrier system, and the minimum pre-emplacement groundwater travel time.

As we look at these, there are several points about the relationship to Part 60 that I think should be made. Perhaps the first is a statement which is contained in the rule relating to reasonable assurance. I will paraphrase it here. Proof of the future performance of geologic repository systems not to be had in the ordinary sense of the word. What is required is reasonable assurance, making allowances for the time period hazards and uncertainties involved, that the outcome will be in conformance with these objectives and criteria.

The point here is that we recognize that a factor of reasonable assurance is what is required to look at compliance. We recognize that the rigor of a reliability analysis which might be deemed necessary for a reactor application may be simply impossible to achieve here if we were attempting to predict performance over intervals as long as 10,000 years.

We have also recognized this problem from a somewhat different perspective from the way we have defined finding, a finding being a determination of compliance or noncompliance.

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But we suggest a finding will be reached after weighing the results of the reliability analysis as well as expert opinion, empirical studies and other sources of information. It is a recognition that a reliability analysis by itself may not do the job in arriving at findings, and we want to be in a position to take advantage of expert opinion.

I think this might be a good time, perhaps, to talk briefly about the question of what we mean when we have set some of these standards in the performance objective, such as what we mean when we say a waste package shall last 300 to 1000 years. I would like to reemphasize what Bob Cook said, that it is not our intent that all waste packages last a minimal time. It is rather our intent that containment by the waste packages be substantially complete. That is the text of the regulation, and that the reasonable assurance provisions, which I mentioned earlier, apply.

Now, that leads to the question of what do we mean by reasonable assurance and substantially complete. Can we attach specific numbers to these concepts? At this point we are not prepared to put specific numbers on those things. We share the concerns raised by the ACRS as well as by DOE that specific numerical values would be good to have. I am inclined personally to think that would be pretty limiting on flexibility at this point. I think what is appropriate here is what I would call a common sense approach, both in

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our development of the numbers and in our discussions with the public and in our responses to public comment as well as in our analyses of EPA standard.

Our view of reasonable assurance meant that if one made the presumption in subsequent analyses that this criterion was met, that reasonable assurance would be close enough to compliance that that assumption would remain valid. It is another way of saying if we do an analysis on compliance with the EPA standard, which presumes a 500-year containment interval, then if 90 percent of the packages in fact contain the waste for 500 years, then that assumption will hold up pretty well in the analysis we have done for compliance with the EPA standard. If only 50 percent of them held up for that interval, then that assumption would become pretty weak.

Now, exactly where we would draw the line, I think, is a decision that the Commissioners perhaps themselves, and certainly with the Licensing Board, will want to exercise their judgment in; but I believe that if one looks at reasonable assurance that these requirements will be met, then a common sense approach will suggest that 50 percent is clearly too low, and 99.9 percent is clearly going to be too high, but probably somewhere in the neighborhood of 80, 90, 95 percent is a reasonable target.

Now, that puts to my mind a question that I would

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like to ask of DOE. As I said, we share your concerns in recognizing that a large fraction of the containers being in compliance or a large fraction of the target values being what we are seeing. If in your work you find that it would substantially alter your program if we went from, say, 80 percent to 95 percent confidence, if that would make a big impact on what you would be doing in your testing program and the way you would run your analyses, then I would be grateful, for my part, if you would bring those to our attention early in these planned workshops, and we will attempt to emphasize those particular areas and address them early on.

My guess is --

MR. MOELLER: We have a question.

MR. STEINDLER: All you have done is traded "reasonable assurance uncertainty" with "it will probably work" uncertainty. I guess I still can't claim that you have enlightened at least me by telling me that reasonable assurance mixed with common sense will give you some idea whether that assumption will probably hold. So "will probably hold" is now the new operative set of words.

I would guess the other criterion you have just used, namely, is there going to be a substantial difference in the DOE program if you tighten up that requirement, will ultimately translate into a dollar question because I don't

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see from my limited vision, I don't see anything that cannot be solved by any lesser or very large increase in effort to accommodate to whatever statistics are required, for example. It seeems to me, however, that if the Savannah River facility is going to turn out encased glass which is then going to end up at BWIP in some fashion or another and BWIP has to bear the burden of interaction with the producer of the canister and give them some kind of criteria for how good their QA has to be, et cetera, et cetera, and what kind of sampling they have to go through and so on and so forth, I don't think those folks are going to talk to each other by waving hands at each other. So I think it is encumbent, or at least -- let me put this way -- don't you think it is going to ultimately be encumbent on you guys to turn out a hard number against which somebody on a zero order approach can begin to design their processes and all the other ancillar operations that are going to be required, specifically QA and the things they give to outside vendors?

I don't know, but I would guess it is going to make a heck of a difference whether you require 80 percent compliance versus 95 percent compliance in terms of the kind of effort it is going to take to produce at the same reliability that kind of a package.

MR. KNAPP: Well, I certainly couldn't tell you that that would not be the case, and that prompts my request

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of DOE in those areas where there will be a substantial impact on their program. We would like to emphasize those first.

MR. STEINDLER: In times when people cut budgets, what do you mean by substantial? They have already been hit by a document that says to them, and in some cases justified and in other cases not, that hey, guys, you just turned out an SCR that has lots of holes in it. Holes are plugged by putting effort into the particular areas in question, and effort is directly related with the dollars.

So they are going to have to, like everybody else, simply assign a set of priorities on where to put their dollars, and the question is is it going to be substantial change in effort if you go from 80 to 95 percent? Itis going to be translated by some program manager either in Rockwell or someplace else into how much money is it going to take for us to satisfy these folks.

MR. MILLER: We have really struggled hard with this question of reliability, put some numbers in there, take a stab, which is what I think you are suggesting we do, and then try to go justify those under all of the practical and real world conditions and uncertainties that we face and certainly a manager running a program will face, or give him the opportunity to pick a number. I guess what we did was --

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MR. STEINDLER: I don't think that is pertinent. You are the licensing folks; he's not. To give him the opportunity -- I don't want to prolong this because I think it's an almost undoable statistic. First off, EPA has the very same problem. They were asked to provide a numerical value to the societal risk available to the public at large. They fussed around in whatever process they used. They finally had to come to grips with a number. Now, they caught all kinds of flak for that number, but they chose a number and they gave you a rationale, which was moderately eloquently explained in the Federal Register and the other documents that I am sure you have seen.

There is no reason within the context of the uncertainty given by the EPA folks for the selection of their number. You cannot provide an equivalent number for what you mean by reasonable assurance and be able to defend it with at least the same degree of vigor that the EPA is going to have to defend, their one death in whatever it is, 1000 years or 10,000 years.

What I am saying is that I think you have some guidance as to what the range is within which you can operate, and the term "reasonable assurance," you have had it given to you, in a sense, by the model that the EPA laid out for you. I don't think it is impossible to do.

I am a little bit puzzled why there is such a real

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timidity in turning out a number. You guys surely are not new to the notion of catching flak. You catch flak all the time. I don't think this would be an enormous increment.

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I think it would be a most useful thing for the whole business to be able to start quantifying the design criteria against which these codes are going to have to start to operate.

MR. KNAPP: Shall I continue?

MR. MOELLER: NRC, of course, has tackled the difficults subjects such as these. Appendix I is one. They did it there. And in the safety goals they have taken a lot of flack, but at least they have put out some numbers. I can understand both approaches. I mean let DOE come up with numbers and come back to NRC and prove that these numbers are acceptable and that that's the major burden on DOE.

MR. MILLER: If I could just make one observation here. If you look at what took a long time in the effort, what made the rulemaking stretch out over a number of years -- and I think you can ask Mr. Knapp and Mike Bell -- it was the numerical performance objectives. And if DOE would go along with whatever numbers we would come up with, I think that would be very useful. But I think that in the real world, in terms of trying to pick numbers and make them stick, you're talking about a several-year process. But maybe that's what we should do. I think we would welcome the committee's comments along these lines.

MR. MOELLER: Indeed, it would require years. I mean looking at Appendix I or looking at the safety goals, it's going to be -- pick a number -- 4 or 5 years before they get them. You know, it's something -- well, we can talk about it. But it's a basic point that needs discussion.

Dick.

MR. FOSTER: There is somewhat of a risk that you don't get in a mode of chasing your tail, your own tail here, in setting some of these numbers. Perhaps a little more specifically is a caution on how you arrive at the numbers you pick in the first place.

I have a very uncomfortable feeling that some of the numbers which are being set here are being set very much on an ALARA principle; that is that we have looked in the past at what we think a candidate site can achieve and therefore a reasonable group of standards for a site set in this particular fashion should be achievable. Those numbers then may later be looked at when site characterization occurs.

If you look at and in fact find that you can meet those that you visualized earlier in the game that were achievable, if you don't look at, you go the opposite direction and you find that, holy moses, I set my original goal and something I think I could achieve and now when I really get down to the nitty-gritty, I find that I can't.

You can only come up looking bad or denying reasonable sites by this process. So I guess my caution is: Be very careful and have a good basis for setting those original numbers rather than basing them strictly on early information on what you can achieve.

MR. STEINDLER: Let me add one other point. I don't think the focus is on the specific reasonable assurance issue that deals with any one of the portions of this whole waste system. The NRC staff has just gotten done chewing on DOE for not having adequate quality data in a whole host of areas.

And the whole question of what constitutes adequate quality is tied back into reasonable assurance for that particular aspect.

This is not an isolated issue that deals with corrosion testing. I think this is an issue that deals with the whole reliance that you want to place on various aspects of the performance that comes out in the performance assessment.

At the moment, I don't see that there is a basis of judgment, for example, on what constitutes a good enough chunk of information on, for example, the solubility of sodium hydroxide. I haven't seen a mechanism for somebody to say, hey, I know it, three orders of magnitude, that's good enough.

And it isn't clear to me where in the process of evaluating either DOE's data or the world-at-large literature how you are going to decide that that's good enough or not good enough.

Bob Cook says it's going to take 4 inches of cast iron or steel. I guess DOE says an inch will do or half an inch plus a factor of 2.

I don't know what is good enough in this performance assessment. It's a prevailing and pervasive sort of a question that keeps coming up and ultimately gets back to Dr. Philbrick's point that he made sometime back: When are you going to quit and be prepared to tell the applicant, hey, guys, we need all we need to know at this stage of the game?

For heaven's sake, if you've got a limited budget, don't go do that, do something else. You have talked about priorities. You have said, you fellows, why don't you look at retrieval priorities a little bit more. They will come back and say, well, we think your guys are going to require a little bit more information on solubility. You can't tell them yes and you can't tell them no in a more concrete fashion other than, I have a gut feeling that you don't know enough about solubility.

The whole question of reasonable assurance is tied into this point of when are you going to be able to tell somebody, yes, we have enough information on this aspect of the system, so it's not just an isolated thing about knowing two or three significant figures what your pitting corrosion rate is. That's not the limiting aspect of my point.

MR. KNAPP: I wouldn't disagree with you, and I certainly don't mean to say that this isn't an important point. But at the moment I can say with assurance that no vertical conductivity measurement is not enough. The point I want to make is perhaps 20 will do, perhaps we will need 30 or 40 depending on how the 20 will turn out. I couldn't answer that question.

But I have tried, with a number of stochastic analyses, to address this kind of a question, and at the moment we don't know enough to be able to address it well. I honestly think that's something we will have to do is to approach this with DOE in the workshops as our understanding and the modeling improve.

I don't disagree we are going to have to address this question before licensing. I am just not sure at this moment it's not premature to do more than recognize it as a goal and aim for it.

(Slide)

I would like at this point to talk a little bit about our perspective on performance assessment, and I would like to thank Dr. Stahl, if he is still here, for his viewgraph, which was prepared independently but which follows the logic very similar to the one that I have here. So his must be sound.

(Laughter)

Our view is the way to apply performance assessment process is to begin by establishing what is currently understood and, based on that, identify what the performance issues are. And here we are talking about the relationship to compliance with the NRC criteria.

From this step one may move rather smoothly through the development of assessment methods, identification of means, and one can then establish tests, generate data. And then we turn in the loop to study the sensitivity of the system or, for that matter, subsystems or components, improve assessment methods and iterate again.

Now, that is the iterative modeling development data collection process that we need. However, it strikes us that there is also another step; that is, establishing component requirements that can be undertaken earlyon.

Now, initially, this will have to be done with simple models and substantial judgment. But this could be a basis for developing component requirements now. And again, this would be a way that DOE can provide targets for things like the waste package containment time, to begin to do some initial work to identify information needs in parallel, to avoid a complex looping process before they can actually identify component requirements.

Obviously, once some initial data has been generated and some analyses have been undertaken, it would have been appropriate to revisit and refine those appropriate requirements.

Finally, when this process has been taken to the point that DOE is confident they can support an application, they would then bring it before the NRC. We would reach findings by reviewing what they have done here in terms of the assessment methods they have developed and how they use those with the data which they have taken.

The concept here of both establishing component requirements early on and interacting between development of assessment methods and site characterization follows in some of the comments that we have on performance assessment portions of the site characterization report.

(Slide)

In that part of the report, DOE identified three major issues which I have paraphrased here. The first are preemplacement, groundwater traveltimes, and compliance with NRC criteria. That concern has been discussed at length by the hydrology folks earlier today.

I would just note one or two points from a performance assessment or perhaps a numerical modeling perspective. The computer results -- that is, both the documentation of the computer codes and of the actual runs that were made -- require -- excuse me, the results require complete documentation. Both the codes need to be documented and the data needs to be documented so that we can independently repeat this work if we choose or at least be able to follow the logic used to arrive at the conclusions.

It is also worth noting that much of the DOE work was based on groundwater travel from the underground facility to the accessible environment. Our regulation regulates groundwater traveltime from the disturbed zone to the accessible environment which has beyond the underground facility.

MR. PHILBRICK: Earlier today accessible environment was shown as a circle with a 10-kilometer diameter.

MR. KNAPP: Excuse me, what is your question?

MR. PHILBRICK: What is the accessible environment? Is it 10-kilometer or 10-kilometer radius?

(Slide)

MR. KNAPP: First let me move ahead to a viewgraph about probably two pages ahead in your handout.

MR. PHILBRICK: I beg your pardon.

MR. KNAPP: That's all right.

For an underground facility such as this, accessible environment is currently defined by DOE in their proposed standard as the surface of the earth as well as the atmosphere, surface waters and oceans and those portions of the lithosphere more than 10 kilometers horizontally from the initial location of the placement of the waste.

MR. PHILBRICK: So then it's a radius.

MR. KNAPP: It's a radius.

MR. PHILBRICK: So that's what your performance assessment is, a 10-kilometer radius.

MR. KNAPP: That's correct. I would prefer to say up to 10 kilometers. That is, through the definitions in 10 CFR 60, what we have said. And certainly, if we can meet the EPA standards at any point up to 10 kilometers, it follows we would meet them at 10.

This, in our view, gives DOE the flexibility to control a smaller area if it's practical to meet the EPA standard at the boundary of the smaller area and thereby achieve any cost savings they can by having a smaller location.

MR. PHILBRICK: Which is not possible at Hanford.

MR. KNAPP: Frankly, I am in no position to say that at this point. There is enough uncertainty at Hanford that I think it is possible the smaller area could be set aside.

MR. PHILBRICK: And controlled? How are you going to control the motion of groundwater?

MR. KNAPP: It's not out intent to control the motion of groundwater.

MR. PHILBRICK: Unless you do something that nobody has talked about yet. Right? You might suggest that upstream of the direction the groundwater is coming down it might be a direction in which less than 10 kilometers would be satisfactory.

MR. KNAPP: That's absolutely right. And I guess I don't quite follow your remarks about controlling the motion of groundwater.

MR. PHILBRICK: You made it.

MR. KNAPP: Then I am sorry. I was very much in error. It was not my intent that we would attempt to control groundwater. If we can understand the motion of groundwater and find which way is upstream with great confidence and by analyses show that the likelihood of radionuclides going upstream is nil, there is little point in attempting to control -- and by that I mean not the groundwater but the surface area above it as we would have to do if that area were set aside.

Part of the provision of the regulation are that

that area that is within this region, known as the control zone, has got to be protected by virtue of permanent markers, records of ownership.

MR. PHILBRICK: You just said ownership, didn't you?

MR. KNAPP: That is my current understanding. The regulation is now before the Commission for their consideration. How it will read when it is promulgated, I do not know.

MR. PHILBRICK: This means a fee simple ownership?

MR. KNAPP: I am not a lawyer; I don't know what that means.

MR. PHILBRICK: Well, you own it; nobody else can take it away from it. You don't have a mortgage on it; you don't have any leases or easements. It's yours. Is that the type of thing you want complete control? You can do what you want to and nobody else is in there?

MR. KNAPP: I am sorry, I just am not involved in that part of the regulation. It is before the Commission, and I don't know how that will finally be resolved. I simply can't say. I would be happy to visit this with you or have Mike Bell, who knows this better than I do, get back to you in a week or so. But I can't answer you right now.

MR. PHILBRICK: I think it's a fundamental problem in this whole thing. How far out do you have to control, how completely do you control, what rights do you have, what rights does anybody else have?

MR. KNAPP: I can only tell you that that has been visited at length by ELD in their support for our preparation for the regulation, and it's my understanding that that problem has been resolved to their satisfaction. Exactly how it has been resolved as to the exact nature of the ownership, I cannot say.

MR. MOELLER: As I recall, 10 CFR 60 says that the site has to be government-owned, does it not?

MR. KNAPP: That is not clear to me. It has to be under government control. Whether that means owned by DOE or set aside in some other manner which would essentially be federal ownership perhaps through the Department of the Interior or something else, I don't know. I just don't know the details of it. That's my difficulty in responding to the question.

MR. MOELLER: I understand what you are saying. Dick.

MR. FOSTER: I guess this is the first time I had heard that the accessible environment and a control zone were one and the same. Is that the intent?

MR. KNAPP: That's the intent of 10 CFR 60.

MR. PHILBRICK: I thought the accessible environment was out beyond the control zone.

MR. KNAPP: I meant the boundaries are contiguous.

MR. FOSTER: I hadn't appreciated that before.

MR. MOELLER: Say again what is it, what's the point?

MR. FOSTER: That EPA's definition of the accessible environment, this 10 kilometers, also then determines the control zone.

MR. KNAPP: The definition of accessible environment is made by EPA. The NRC in the regulation has defined the term "control zone." The idea of the control zone is to reduce the likelihood of inadvertent intrusion at some time in the future into this area, which is not as well protected as the area beyond this boundary in the accessible environment.

MR. FOSTER: And you are telling me that the regulation says that the accessible environment boundary and the control zone boundary are one and the same?

MR. KNAPP: I will say yes, there is a nit in there but plus an FDS.

MR. MOELLER: Okay. Go ahead.

(Slide)

MR. MOELLER: While we are mentioning accessible environment, I might ask was the definition of the accessible environment in the site characterization analysis the defintion that the three agencies had agreed upon?

MR. KNAPP: To the best of my understanding, it was -- well, that definition was taken from 10 CFR 60 in the version which was published last July. You will probably recall there is a linkage between again accessible environment and control zone.

That definition was consistent with what I believe all three agencies accepted at the time that EPA published their standards for public comment. What EPA's position will be after they have heard the response I cannot honestly say.

MR. PHILBRICK: Do you think the public has any idea what you are talking about about the size of the control zone?

MR.KNAPP: My impression is that a number of the members of the public do as a result of the comments which I understand they are making to EPA.

MR. MILLER: We have had quite a bit of contact with the States in connection with this, and the assessments we're doing at the other site and some contact with the public. And a key issue with the States is the question of the boundaries of the accessible environment and that whole issue. They are keenly aware of this as being very significant to their waters, and for example at Hanford. And so I think the answer to your question is, at least the States are very much on to this.

MR. PHILBRICK: Thank you.

MR. MOELLER: Well, I have heard the answer on my question, but I am not sure still, because the definition of accessible environment that the three agencies agreed upon was roughly December, I believe. I mean it was very recent, and I guess your SCA was written since then. But I wasn't sure when I read it that the definition given here was the one that had been agreed upon.

MR. PHILBRICK: I just think that's one of the most awful things that ever came out of Washington.

MR. MOELLER: Your definition is on page 9-4 here, and let me just look at it and see. I will read what it says. It says, "The atmosphere, land surfaces, surface water, oceans, and a portion of the lithosphere that is outside the controlled area, the overall system performance for the geologic repository is calculated at this boundary."

Well, see, that says nothing about 10 kilometers.

MR. KNAPP: If you now refer to the definition of controlled area just below that, I believe that completes the concept.

MR. MOELLER: Okay. It's outside the controlled area. Then the control area is the surface location to be marked by suitable monuments extending horizontally no more than 10 kilometers in any direction from the underground facility. Okay, I see your point. Then by combining the two, you are all right. I am glad you pointed that out to me, because I missed it.

MR. KNAPP: Let me continue to the second point on the viewgraph. What is the maximum release rate from the engineered barrier system. Again, this has been addressed with some specificity by John Greeves in one of his remarks and a part by Bob Coe. I would just note one or two points.

The anlayses used to investigate the engineered barrier system, we considered there was some nonconservatisms which DOE might visit in their work in preparing the site characterization of the plan; specifically, the porosity of the adjacent host rock and the groundwater velocity we think may be nonconservative.

The third bullet that I would like to discuss are potential releases to the accessible environment in compliance with EPA standards. This is, I think, the one that concerns me the most. We considered that DOE should in their site characterization plan look at this point with a great deal more care. Obviously, EPA had not promulgated their standards at the time the site characterization report was written. And they have now been promulgated for public comment. And I think it provides a good opportunity for DOE to visit the question of how they will be addressed.

This is of concern to me because the standard is probabilistic, and therefore DOE will have to look at how they are going to deal with the probabilistic standard.

MR. PHILBRICK: If you had an absolutely water-tight site at depth, then you wouldn't have any problem with this situation. You would have zero, wouldn't you?

MR. KNAPP: If it could be guaranteed.

MR. PHILBRICK: Then it would be zero?

MR. KNAPP: Yes.

MR. PHILBRICK: All right. If you can prove it's zero, then you get the probabilistic situation which gets quantified when you find out what the rate of flow of groundwater is under the heads. So then the next thing you have to do is get right back to the business of putting in the wells with the observation wells and pumping and establishing what the conductivities are in the various layers.

Now, I think that's what your performance assessment should be. You cannot assess until they have been done, and I wouldn't worry about that last one. I would just admit we don't have enough data to talk about it.

MR. KNAPP: There is some point to what you say.

MR. PHILBRICK: I think there is a hell of a lot.

(Laughter)

MR. KNAPP: The NRC staff has spent approximately 3 chronological years and a number of staff years at the Sandia Laboratory dealing with the probabilistic nature of the EPA standard. If DOE defers for 2 or 3 years dealing with this problem, they will not be able to address it in their application.

I can tell you from experience it is not a simple thing to do. The calculations are not simple to understand or perform. And I think it would be very wise for them to ask early on how they are going to do it. I think it would be playing with fire to presume they can avoid doing that or to put it off.

MR. PHILBRICK: I didn't say that.

MR. KNAPP: Okay. I misunderstood you.

MR. PHILBRICK: I said they have got to put in the pump and well, and then they have got to put in enough observation wells radial from the pumping well in whatever directions they want and run a pump test and find out what the permeability is of the various aquifers or layers and finally the repository level. And when they have got that done, then you can begin to compute what the performance assessment is, because we have all agreed that the radionuclide releases are going to be transported by water.

END T. 15 (15A fols.)

T-15A
Amy/
Pat

If they stay in the repository, why do you care? But they are not. They are going to move. And they won't move and we won't be able to calculate until we know what the permeabilities are.

This is the most critical thing that has happened today, that everybody recognizes now that we don't know what the K is.

Now, you can go ahead with performance assessment if you wish, but I don't think you can do it rationally until you have those data.

MR. KNAPP: I guess I misunderstood your point. I am not recommending that we do a performance assessment now. I am recommending that we look towards the capability of doing it, that we recognize that data in cluster wells or anything else will have uncertainties associated with them.

We have to be able to treat them in a way that will address the probabilistic nature of the standard and withstand the scrutiny at licensing. That's my only intent. It would certainly be pointless to attempt at this time to try to assess whether or not you have compliance with EPA standards.

In fact, I think it would be very premature. But it's not premature to ask how the data that is taken from these cluster wells will fit into an overall performance assessment strategy which can then be applied to reach a finding of compliance or noncompliance. Otherwise, we run the risk at licensing time that data will be a loose collection of parts that will not give DOE the information to support their assertion that the EPA standard will be met. That's my concern, that we look to licensing now that we attempt an assessment.

MR. STEINDLER: How far are you along in that process to be able to answer the question how would you go about identifying compliance with EPA standards?

MR. KNAPP: We have just completed -- I believe Malcolm Siegal has just completed a six-volume tome at Sandia dealing with a number of issues related to reaching compliance with the EPA standard, including some suggestions as to how achievable it is. I will be happy to supply copies of that to the ACRS next week.

MR. STEINDLER: Let's hold off on six volumes. There has got to be a simpler way.

MR. KNAPP: I will send you the executive summary.

MR. STEINDLER: That's much better. Can you identify for DOE, for example, those parameters which are most critical in determining compliance at this stage of the game?

MR. KNAPP: We can identify some parameters which are important, but at this point many of those parameters can be identified using common sense and other means.

Mark Logsdon in his discussion of vertical permeability identified two.

MR. STEINDLER: Let me ask a question. Can you identify or can you make some comments about the DOE program where they are apparently getting information which you feel is going to be totally unimportant in an assessment of compliance?

MR. KNAPP: I could arrive at answers today to that question for about 25 parameters. However, the model that implies those parameters our geologists believe to be pretty primitive at this time. This is the initial version of Appendix D which fell on its face because of our inability to convince ourselves that we describe the geology properly.

I consider one of our principal tasks in the coming hopefully months but certainly in the near future to improve this model. In fact, Sandia has given us a generalized version of the computer code to address that very problem.

I would hope that within a year we can make some intelligent statements about the importance of solubilities and retardation of specific radionuclides. There will be a great many caveats associated with that, but I think we can identify those areas which we think might be most important. And of course, this is something I think we would very much have to do in concert with DOE to take advantage of their data and any suggestions they would make.

MR. STEINDLER: It sounds like that's one of the ways to get at the old question how much is enough? If the requirement for precision and accuracy with particular parameters appears to be on the basis of less sensitivity to the final assessment, those requirements are fairly loose, then I think -- I am sure the guys in the program would like to know early on that that is an area that they don't really have to spend a lot of time on.

MR. KNAPP: Absolutely. That's the whole concept, as a matter of fact.

(Slide)

Beyond this viewgraph that I put up earlier -- and it's exactly this interaction between assessment methods and data where we hope that assessment methods which DOE uses will give them a basis for saying that they have a right to ignore this particular radionuclide because for appropriate reasons in the data it's a --

MR. STEINDLER: You expect your situation to be reasonably well in hand for a first cut at this problem in a year?

MR. KNAPP: That's a goal. Now, bear in mind this would be a recommendation for -- it's essentially sensitivity analysis, but I think that is a reasonable goal. I think it would depend on what kinds of resources we have to be able to do this. It would have to be updated. We might come up with somewhat different answers 2 years from now, but I think that's a reasonable target.

MR. MILLER: You have got to look at the next year to 2 years to be critical here. And we will, as I said before, continue to look at that overall system of study, and I am sure we will fall flat on our face for a while longer. With respect to the hydrology and geology, it is basically constrained by that because until you can get a good overall far-field groundwater flow picture, you don't have a good model to integrate all the other inputs like geochemistry and so on.

MR. STEINDLER: Have you interacted with Rockwell at all on this so far? Have your Sandia folks been in touch with them?

MR. KNAPP: They are certainly knowledgable about what goes on. Our performance assessment people,including the Sandia folks, have visited Rockwell on a number of the workshops.

Performance assessment has not until recently relatively been singled out for an area of attention. We looked last summer for guidance under hydrology or geochemistry. I would like to see us put more emphasis on it.

MR. MILLER: We expect the performance assessment picture will be picked up at the same time as the groundwater, Dr. Steindler, in the first of the meetings that we are going to have. That would be our recommendation.

(Slide)

MR. MOELLER: Yes, Dick.

MR. FOSTER: A little along these lines, do we know enough about the site at this particular time so that we could say that there are certain features that you are going to be looking at that if you came up with values that were so bad that these would be fatal to the site? In other words, can you provide information that says there are a half a dozen things that we could take a quick look at if they turn out to be that bad, it's a no-go situation?

MR. KNAPP: I am not in a position to identify any features like that. One of the unattractive aspects of an overall sensitivity analysis is that you can frequently rationalize, well, if the groundwater traveltimes are very long, then the geochemistry doesn't have to be all that good to retard radionuclides.

On the other hand, if the geochemistry is great, we can have shorter groundwater traveltime. It's sort of a balancing act. And I think that if those sorts of fatal flaws are identified, that that identification will come from the geologists based on their understanding of structure or from the hydrologists first. We may provide some confirmatory evidence, but they will be the first one to identify.

MR. WRIGHT: I think the answer to your question is no.

MR. PHILBRICK: what is the question? That there is no single element in a site so poor that could cause the site to be --

MR. WRIGHT: Which might upon investigation prove to be fatal flaws to the site.

MR. PHILBRICK: Well, who lost 25,0000 gallons of fluid someplace?

MR. WRIGHT: That was lost in one of these zones that has a permeability of 10 to the -6 or 10 to the -5 or 10 to the -7, presumably a flow top. That's towards the lower part of the dense interior, the permeability values given for that zone are anywhere from 10 to the -4 meters per second to 10 to the -5 meters per second. That is dense interior.

MR. PHILBRICK: And if you have that type of permeability and that quantity of flow, would you still build the site?

MR. WILLIAMS: The problem is the tests are so short that you can't tell how extensive a zone like that is. And further testing they reveal it's just a small feature that has no problem.

MR. PHILBRICK: There was nothing in the site which would cause the site to be thrown out? If you have this thing, I think you have no business building it. I don't know if that's the point.

MR. WRIGHT: Keep in mind, sir, that that's in the Umtanum, and we understand that the preferred horizon at the present time is no longer the Umtanum, it's the Cohasset, which is some 800 feet higher, so that this zone that is fractured in the Umtanum does not necessarily affect the Cohasset.

MR. PHILBRICK: I think you are wise to go up.

MR. WRIGHT: It's not my choice. It's the wisdom of Rockwell and DOE.

MR. MOELLER: Go ahead, Malcolm.

MR. KNAPP: I believe I can run through my last two viewgraphs rather rapidly because I think we have covered most of these points.

These are a view of the recommendations that we made to DOE in the site characterization analysis. The performance assessment framework needs to be described and it should address the iterative process between modeling and site characterization. I think we discussed my interest in that already in my answers to some questions.

The idea here is to -- it would be very helpful to us to have a linkage from Part 60 particularly, including that provision of Part 60 that requires compliance with the EPA standard, back through the data gathering program which addresses how this modeling and data gathering will iterate in whatever stepwise fashion DOE chooses.

END 7.15A

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The second point, which is one I am not sure has been addressed today, risk analyses should be performed to identify structures, systems, and components which should be identified for safety prior to the closure of the underground facility.

That phrase is one which will very much affect those operational items that John Greeves and the undeground facility folks will have to address. And I think that DOE should visit exactly what the systems are going to be, and I think that risk analysis is probably a sound way of identifying those systems, components, and structures that are important.

My third bullet is that performance assessment terms should be defined and reviewed with NRC. I think that defined is important, and perhaps the meaning of some of these definitions should be established.

(Slide)

That is to say how the definitions are implemented should be established. The one in particular, having already talked about accessible environment, of concern is the disturbed design. The disturbed design is the portion of the control area whose properties have changed as a result of the underground facility construction or the thermal effects of the emplaced waste such that the change of properties will have a significant effect on the performance of the repository.

Now, the object of the disturbed zone is to be able to identify a region beyond which we can say that the measurements and inferences that we make now will with some confidence be extrapolated over the next 10,000 years and that some of the complexities that have been mentioned by some of the other speakers in the last 2 days about the effects of heat and chemistry and rock mechanics will not have to be dealt with in complex modeling exercises that might have a great many uncertainties associated with them.

This boundary is one that we are going to have to discuss at length with DOE, and we would like to do that.

MR. STEINDLER: You include the thermal effects in this definition. Doesn't this in effect cause an intersection of the disturbed zone on the surface?

MR. KNAPP: Well, that's why we have the words "significant effect" in here. Well, now, seriously you were absolutely right. And the point is at what point can you say that's an no-never-mind and that's where the boundary lies?

MR. STEINDLER: Well, when you define reasonable assurance, then you will get an answer to that.

(Laughter)

(Slide)

MR. KNAPP: My last viewgraph I hope I can treat very briefly. As I mentioned earlier, we think that performance assessment can be a very valuable tool to guide site characterization at this point. And I certainly think it's premature to attempt to make any statements about the performance of the repository system at this time.

I think that that is in agreement with what Dr. Philbrick has said earlier.

I would again note that it's necessary that in a site characterization plan, that all the computer results be documented well enough to enable independent evaluation of them.

And finally, I would like to stress that DOE's plans for code evaluation and documentation, particularly evaluation, should be described in somewhat more detail. We hope they will be in the site characterization plan.

Code evaluation is kind of a tricky problem when you are attempting to predict 10,000 years into the future and to reach a consensus that involves DOE and NRC and yourselves, among others. We are going to have to pay a lot of attention to that, and we look forward to seeing more detail in their plans.

Now, that summarizes my remarks, with two exceptions.

(Slide)

First, in the site characterization report, DOE has said that they will be presenting plans and programs for performance assessment in this fiscal year. We look forward to reading this and discussing them with DOE.

Second, although I was not able to be here yesterday morning to hear the DOE presentation, it's my understanding the bulk of our comments in Chapter 9 were addressed and that DOE was responsive to those comments.

I am very pleased, and I look forward to working with DOE and working with DOE in workshops in the future to work out the details.

MR. MOELLER: Thank you.

Any other questions or comments for Mr. Knapp?

MR. PHILBRICK: I would like to make one general comment. I think during the last 2 days the questioning has ben rather rough, intensively so because the answers need to be achieved, and I think the speakers have displayed remarkable control under conditions which I think were not necessarily good.

MR. MOELLER: Thank you.

Well, at least that wraps up then the formal part.

MR. STEINDLER: I have one question. How many man-days or years or months of effort did the NRC expend analyzing the site characterization report?

MR. MILLER: There are several answers. There is the time it took to prepare for it. That was a number of months, about 69 months, during the period of time that we actually worked on the report, I guess it was about 10 man-years.

MR. STEINDLER: So you spent 10 man-years in preparing NUREG-960, is that what you are saying?

MR. MILLER: Bob, do you remember? I think that's about the number.

VOICE: 10 or 12 man-years.

MR. MILLER: That is all the overhead that we carry. It's a very complete number.

MR. STEINDLER: That corresponds to about -- I thought we heard from DOE/Rockwell an 18-month effort on the part of about 20 people. Is that what I thought I heard?

MR. MOELLER: They could tell us. That's okay. About 30 person-years then.

Hub, did you have additional comments?

MR. MILLER: Yes, just one last remark. And that is, of course the obvious question of where do we go from here. But just for the record I want to point out that Chapter 10 of the SCA was written to form the basis or the key, if you will, at least in our minds, for establishing a mechanism of information exchange.

It's very important to me and to others responsible for this program that the committee and others understand that we recognize that in doing our job we have to do it in such a way that DOE can do their job. I misspoke -- no, it's Chapter 10 -- that we recognize that we have got to work out a scheme whereby we can do our job and they still be asble to do their job.

The key, as I mentioned earlier, I think to doing our job is having timely access to data and timely consultation on the plans and specifics about the tests they are going to run so that we have the opportunity to raise questions in a way that won't be in the critical path and disrupt their program.

In Chapter 10 there is a dissection of this process, and we recommended some spots where we think it would be useful to have release of data where we could have access to it and consider it and weigh it without a lot of bothering of DOE and their staff, but then on a discreet basis consult with them.

So I would call your attention to that because I think it's very important to solving this problem of keeping both DOE and we in a position of being able to do our job.

We appreciate very much having the opportunity to talk with the committee, and we welcome whatever comments you do have on the site characterization analysis.

MR. MOELLER: Martin.

MR. STEINDLER: I do have one final comment. I guess I must say for the record the Rockwell presentation was an impressive display of what you can do when you are prepared. Those folks went through 1,800 pages of convoluted and multidisciplinary types of material and, as Mr. Philbrick said, with some nasty questioning, but in a coherent enough fashion. So I have essentially no trouble understanding areas that I don't know about, which I consider to be a fair achievement on their part. And I really think they ought to be commended for being able to pull this off.

MR. MOELLER: Well, that's a nice tone on which to wrap things up. Does anyone else have comments or desire to speak before we adjourn the formal portion of the meeting?

Well, to repeat, the subcommittee will be discussing these matters in the morning, and we will have minutes, and we will I am sure as a part of those minutes have our summary of some of our conclusion.

However, to repeat, I believe it will be the June meeting before the full committee considers this. Certainly, that would be the earliest at which they would consider it and issue anything in the way of a formal full committee report.

Well, let me also thank everyone for being with us the last 2 days and presenting, as Dr. Steindler pointed out, a vast amount and volume of material in a very summary fashion for our edification and to expand on what we have learned by reading the material.

And let me thank our reporter for getting all of the names straight and all of the words down on paper.

With those words, then, I would adjourn this meeting.

(Whereupon, at 5:35 p.m., the Subcommittee was adjourned.)

END
T.16

CERTIFICATE OF PROCEEDINGS

This is to certify that the attached proceedings before the NRC COMMISSION

In the matter of: ACRS - Subcommittee on Waste

Date of Proceeding: April 22, 1983

Place of Proceeding: Washington, D.C.

were held as herein appears, and that this is the original transcript for the file of the Commission.

Official Reporter - Typed

Ann Riley / JHT

Official Reporter - Signature

PRESENTATION

TO THE

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS
U.S. NUCLEAR REGULATORY COMMISSION

ON THE NRC PROGRAM

LONG-TERM PERFORMANCE OF MATERIALS USED FOR HIGH-LEVEL WASTE PACKAGING

APRIL 22, 1983

BY

DR. DAVID STAHL
PROGRAM MANAGER

Battelle
Columbus Laboratories

OUTLINE

- Objective
- Strategy
- Structure
- Major activities
  - Accomplishments
  - Plans
- Summary

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NRC PROGRAM

LONG-TERM PERFORMANCE OF MATERIALS USED FOR HIGH-LEVEL WASTE PACKAGING

OBJECTIVE: DEVELOP A PREDICTIVE METHODOLOGY THAT CAN BE USED BY THE NRC TO EVALUATE AND LICENSE CANDIDATE WASTE-PACKAGE SYSTEMS FOR PERFORMANCE IN A REPOSITORY ENVIRONMENT FOR A ~1000-YR TIME PERIOD

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STRATEGY

- Use available DOE/NRC information
- Identify and perform/develop critical experiments/ phenomenological models
  - Barrier degradation
  - Radionuclide release
- Integrate experiments and modeling to achieve interactive effort
- Provide first generation system model using simple/available models
- Upgrade system model annually using separate and combined effects models including probabilistic approaches
- Validate model and issue final report

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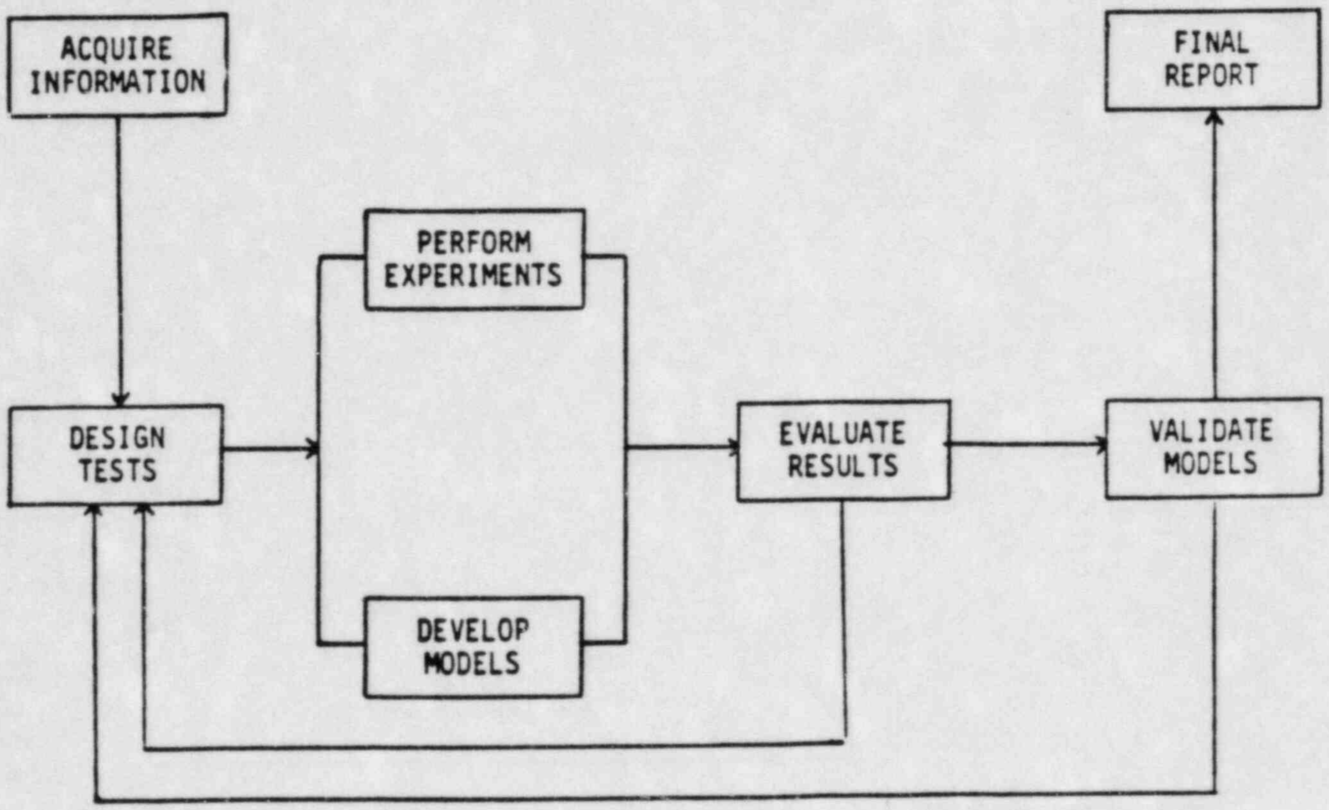
PROGRAM ACTIVITY SCHEMATIC



CANDIDATES

HOST MATRIX

BOROSILICATE GLASS	PNL 77-260 BOROSILICATE GLASS
HIGH SILICA GLASS	SRL-131 BOROSILICATE GLASS
SYNROC	SYNROC-C

CANISTER/OVERPACK

STAINLESS STEEL	TYPE 304L STAINLESS STEEL
NICKEL SUPER ALLOY	ASTM 65-35 CAST STEEL
TITANIUM ALLOY	TICODE-12 ALLOY

PRESENT CANDIDATES

Host Matrix

PNL-76-68 borosilicate glass

SRL-131 borosilicate glass

Spent fuel

Canister/Overpack

Type 304L stainless steel

1018 cast carbon steel

Ticode-12 titanium alloy

Battelle
Columbus Laboratories

Other Laboratories

Packing

Bentonite clay

Zeolites

Salt

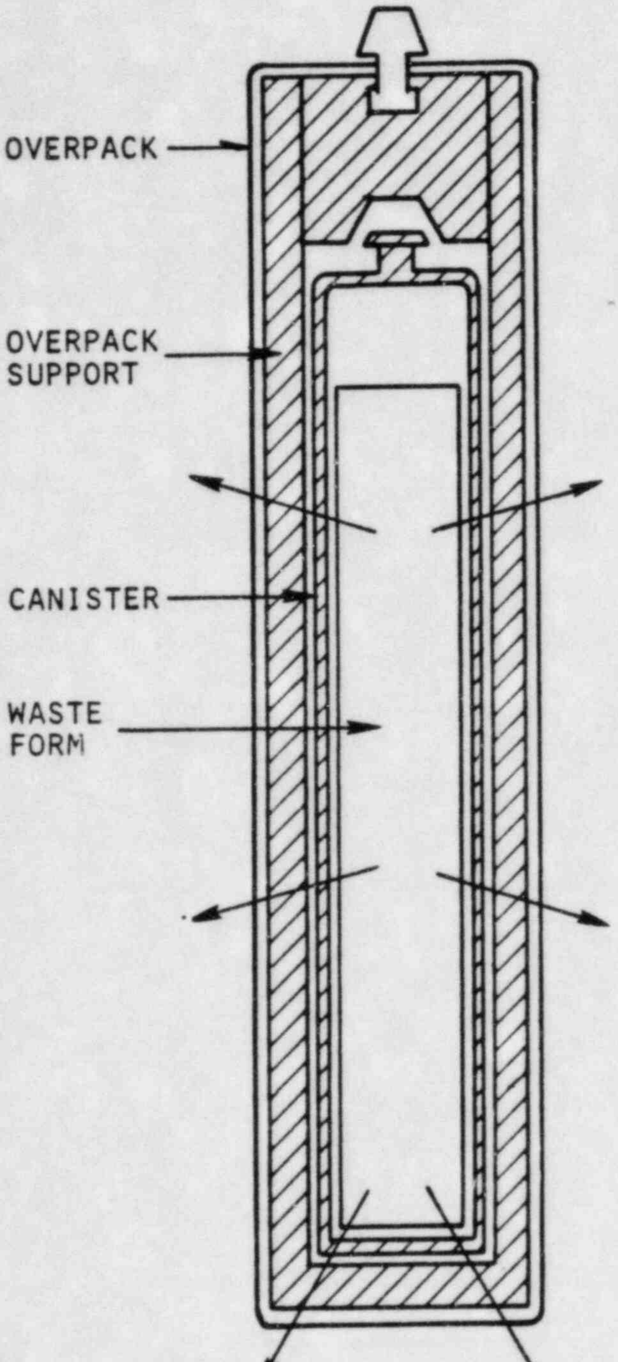
Repository

Basalt (Hanford Reservation)

Tuff (Nevada Test Site)

Salt (Sites Under Consideration)

WASTE PACKAGE CONCERNS



ENVIRONMENTAL EFFECTS

- REPOSITORY CHARACTER
- BACKFILL CHARACTER
- GROUND WATER CHARACTER
  - CHEMISTRY
  - T, P, PH, EH
- RADIATION FIELD

BARRIER DEGRADATION PROCESSES

- GENERAL CORROSION
- PITTING CORROSION
- GALVANIC CORROSION
- HYDROGEN EMBRITTLEMENT
- STRESS CORROSION CRACKING
- MECHANICAL STRESS

WASTE FORM DEGRADATION PROCESSES

- DISSOLUTION
- RADIATION DAMAGE
- THERMAL AGING
- MECHANICAL STRESS

RADIONUCLIDE RELEASE STATES

- SOLUTIONS
- COLLOIDS
- PRECIPITATES

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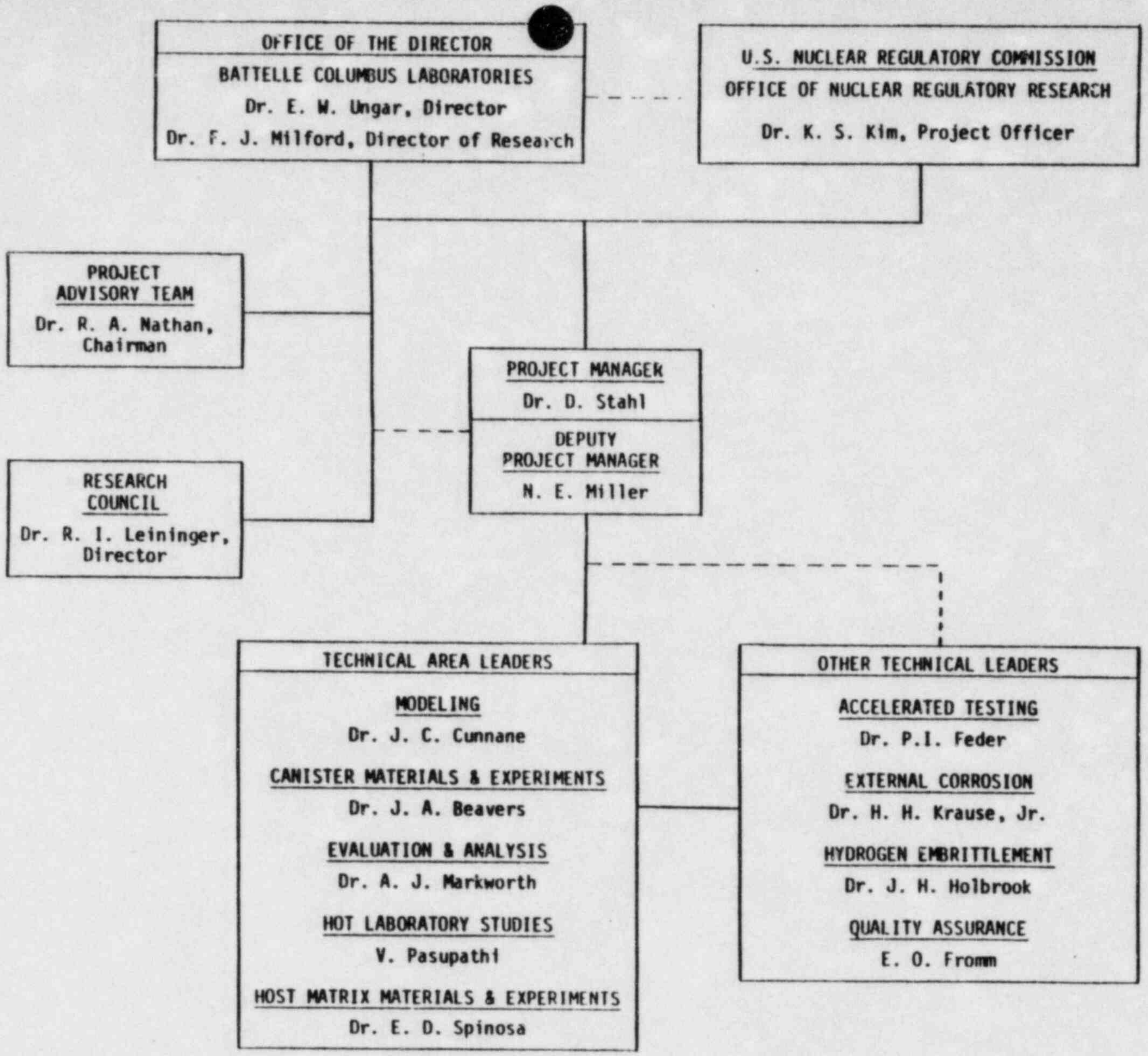
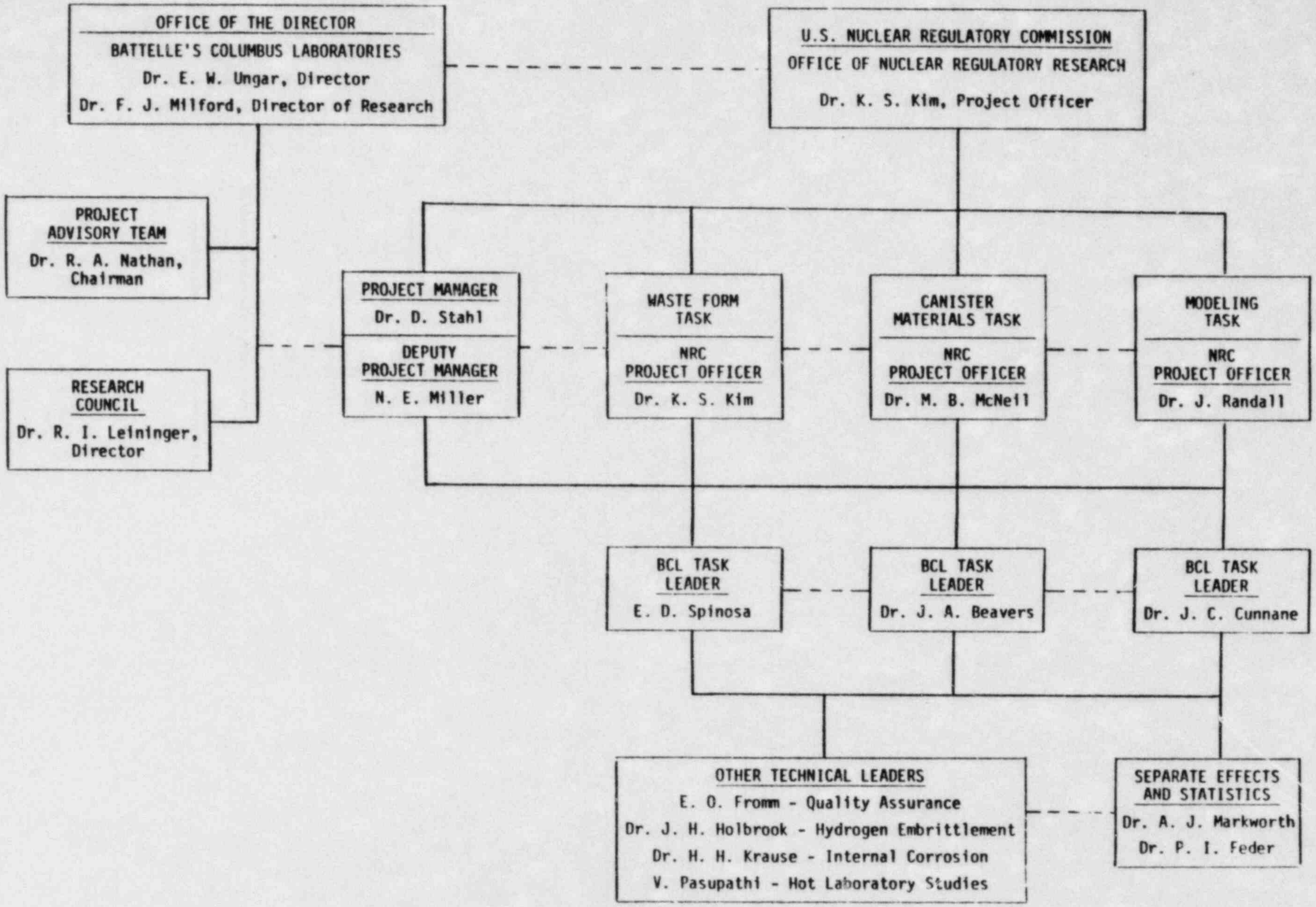


FIGURE 4.1. PROGRAM ORGANIZATION



MODIFIED PROGRAM ORGANIZATION

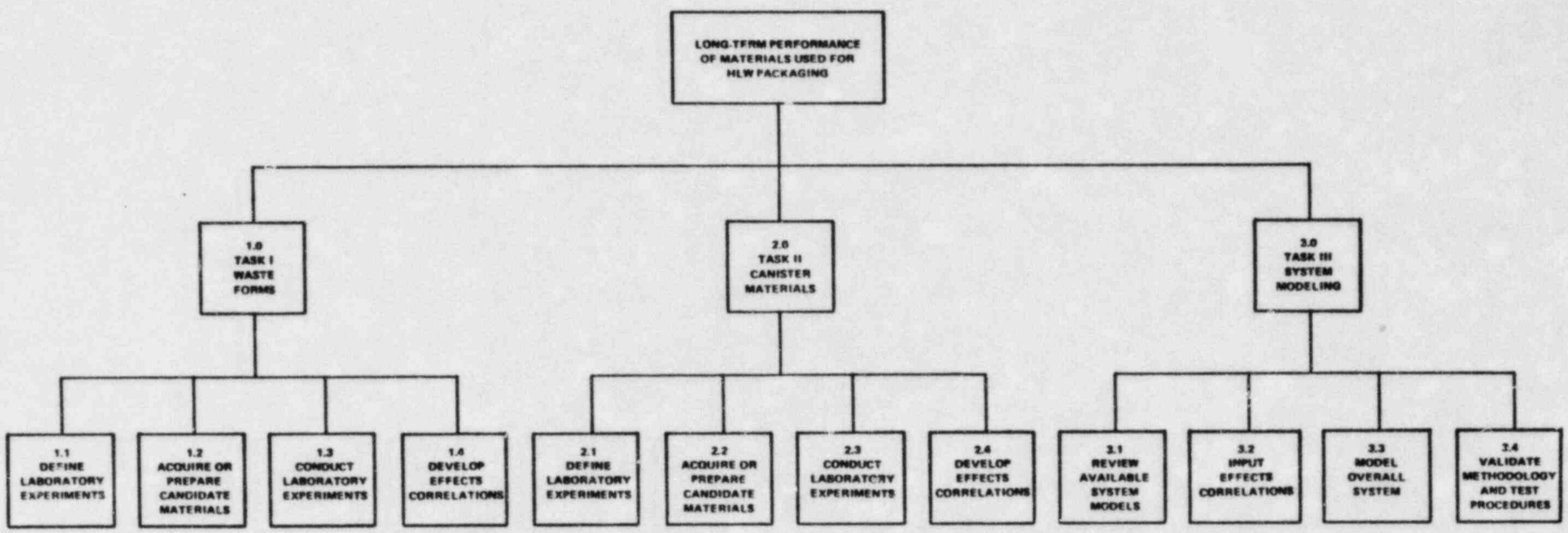


FIGURE 3.3 WORK BREAKDOWN STRUCTURE

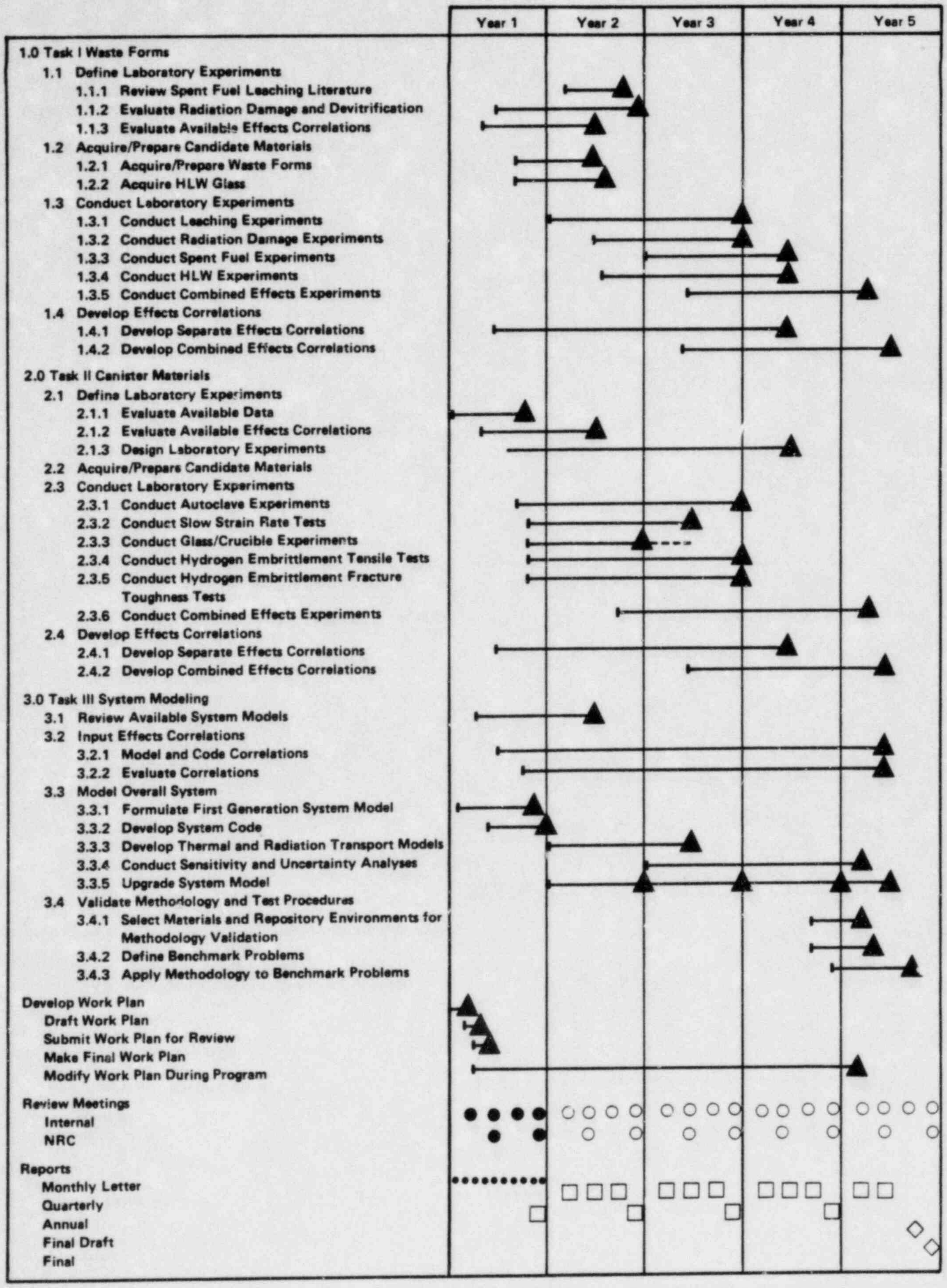
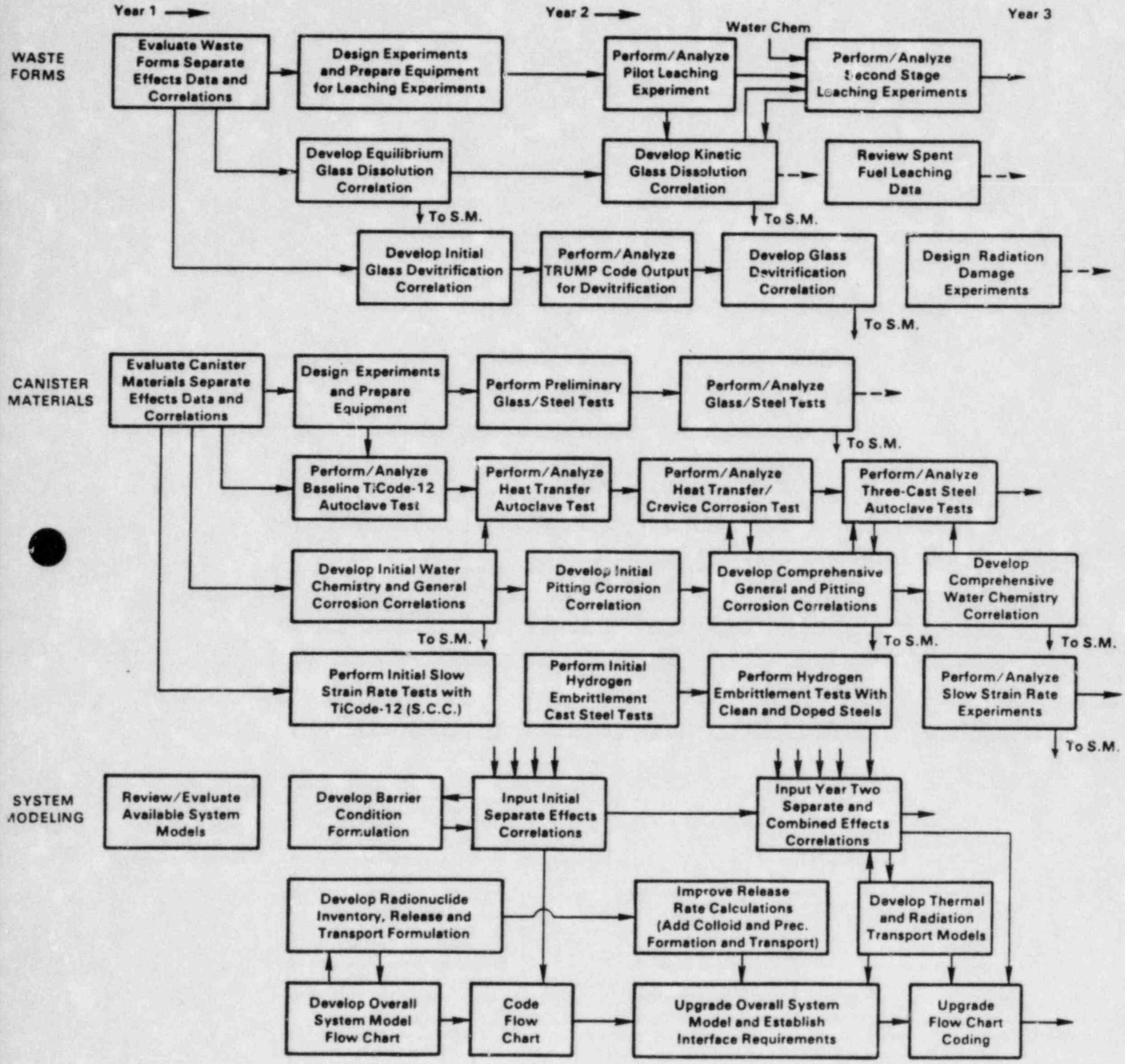
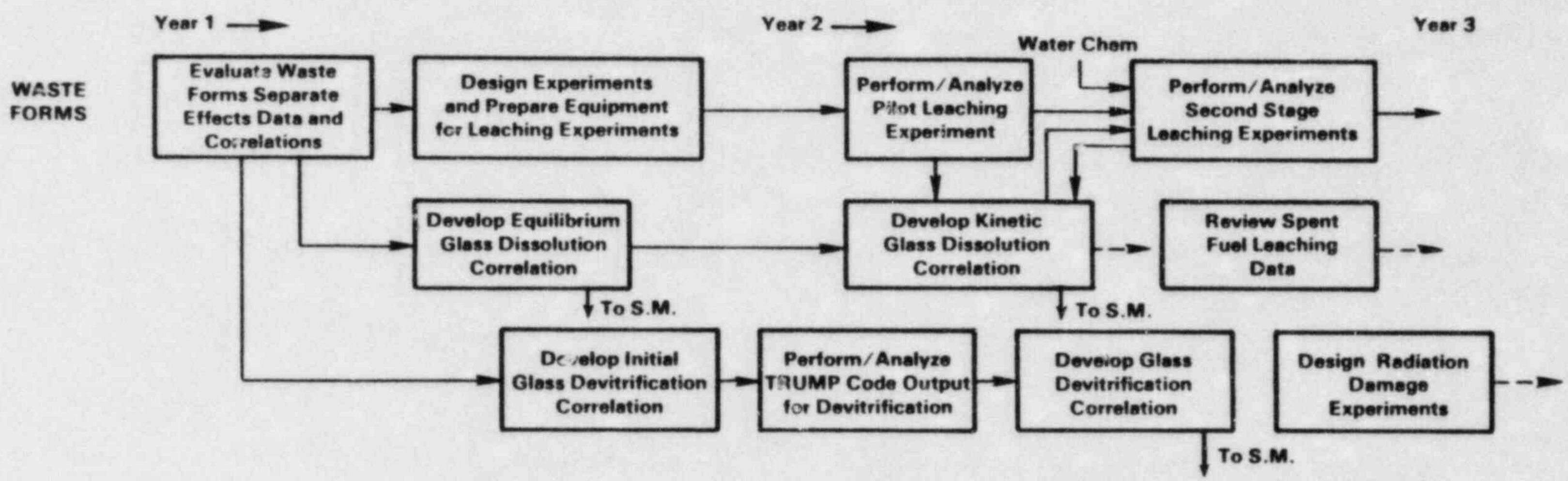


FIGURE 5.1. PROGRAM SCHEDULE

TIMING AND INTERACTION OF MAJOR NEAR-TERM ACTIVITIES





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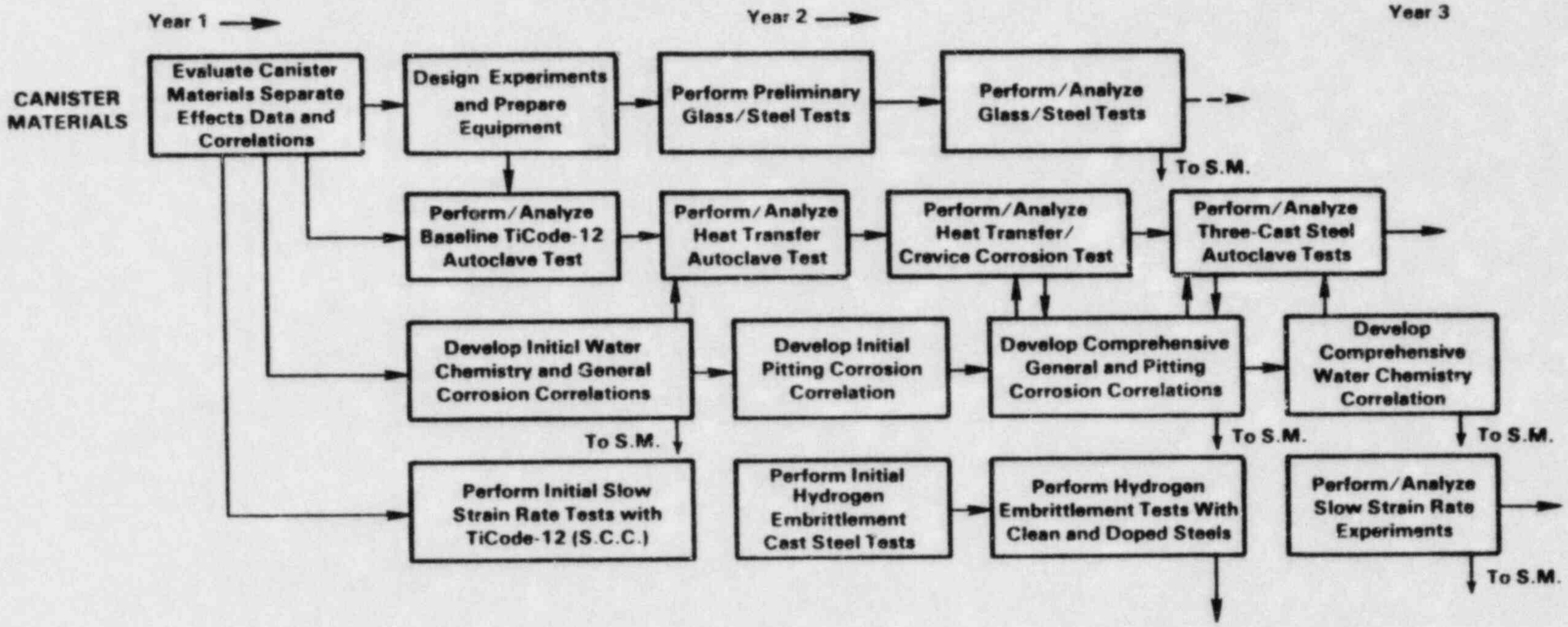

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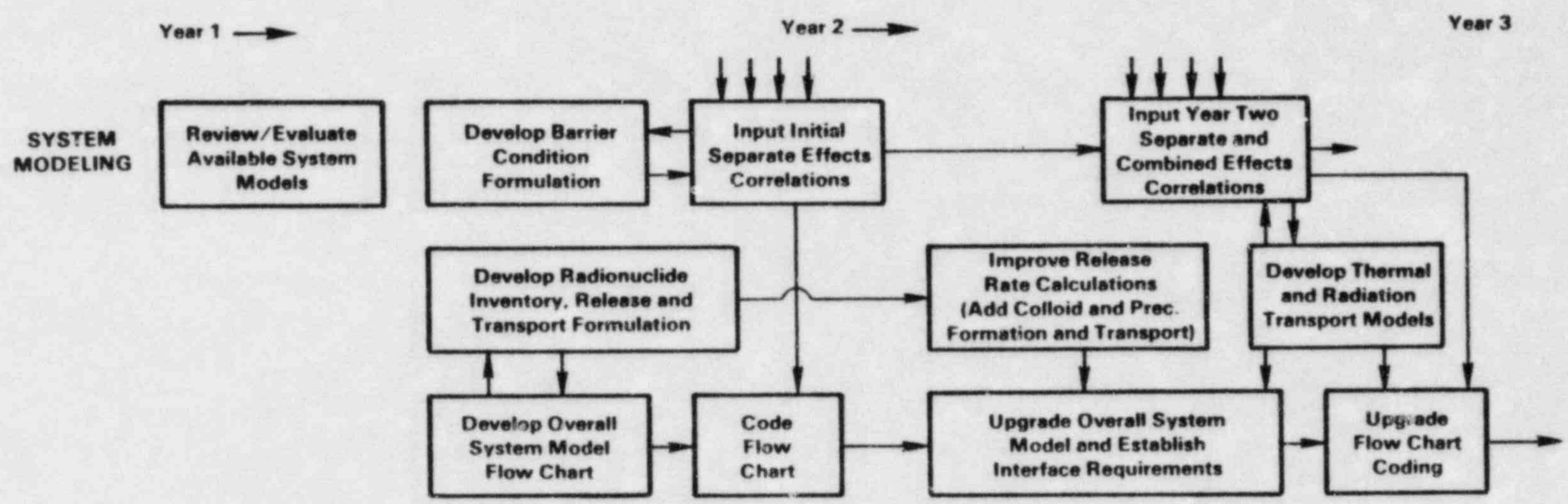

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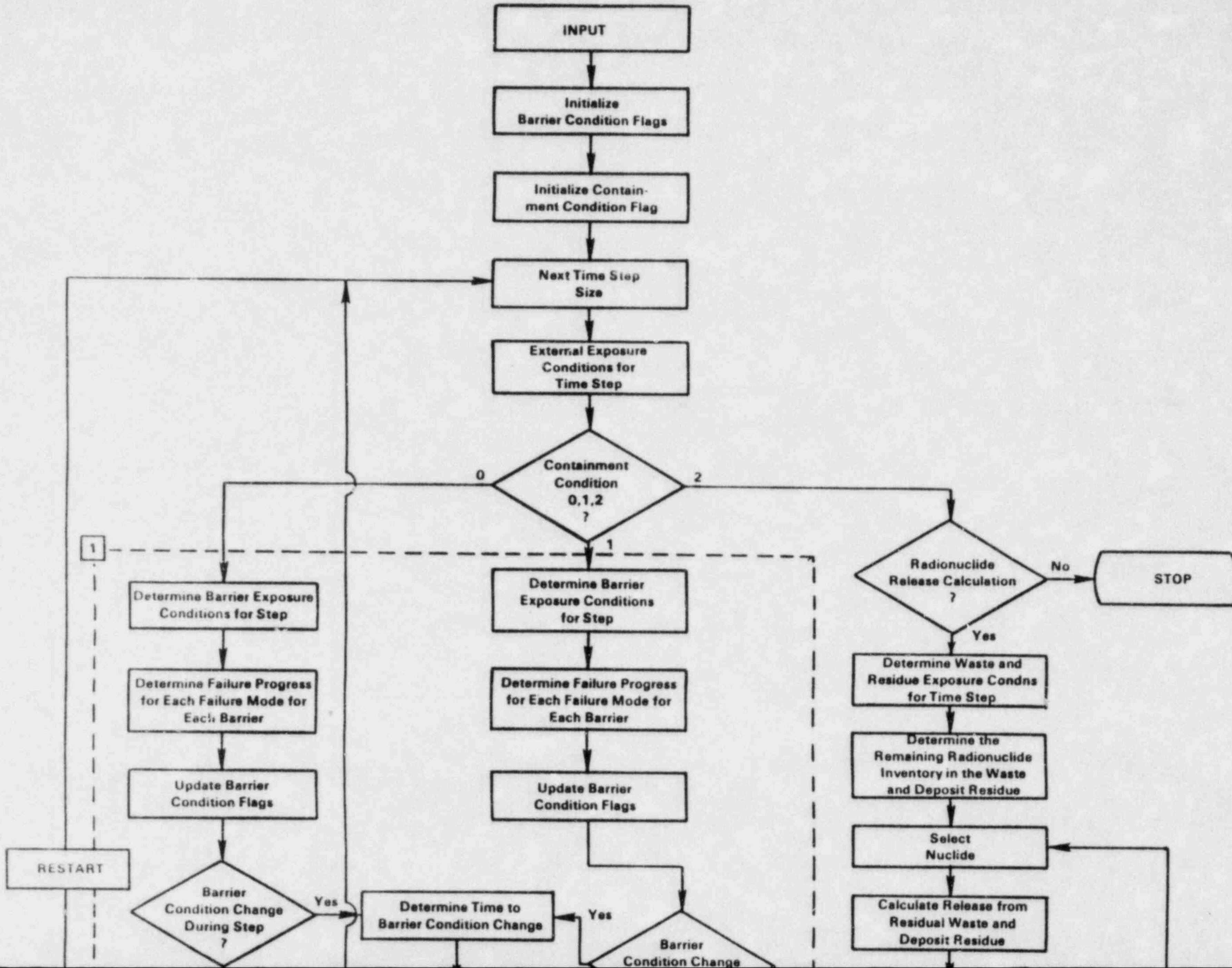
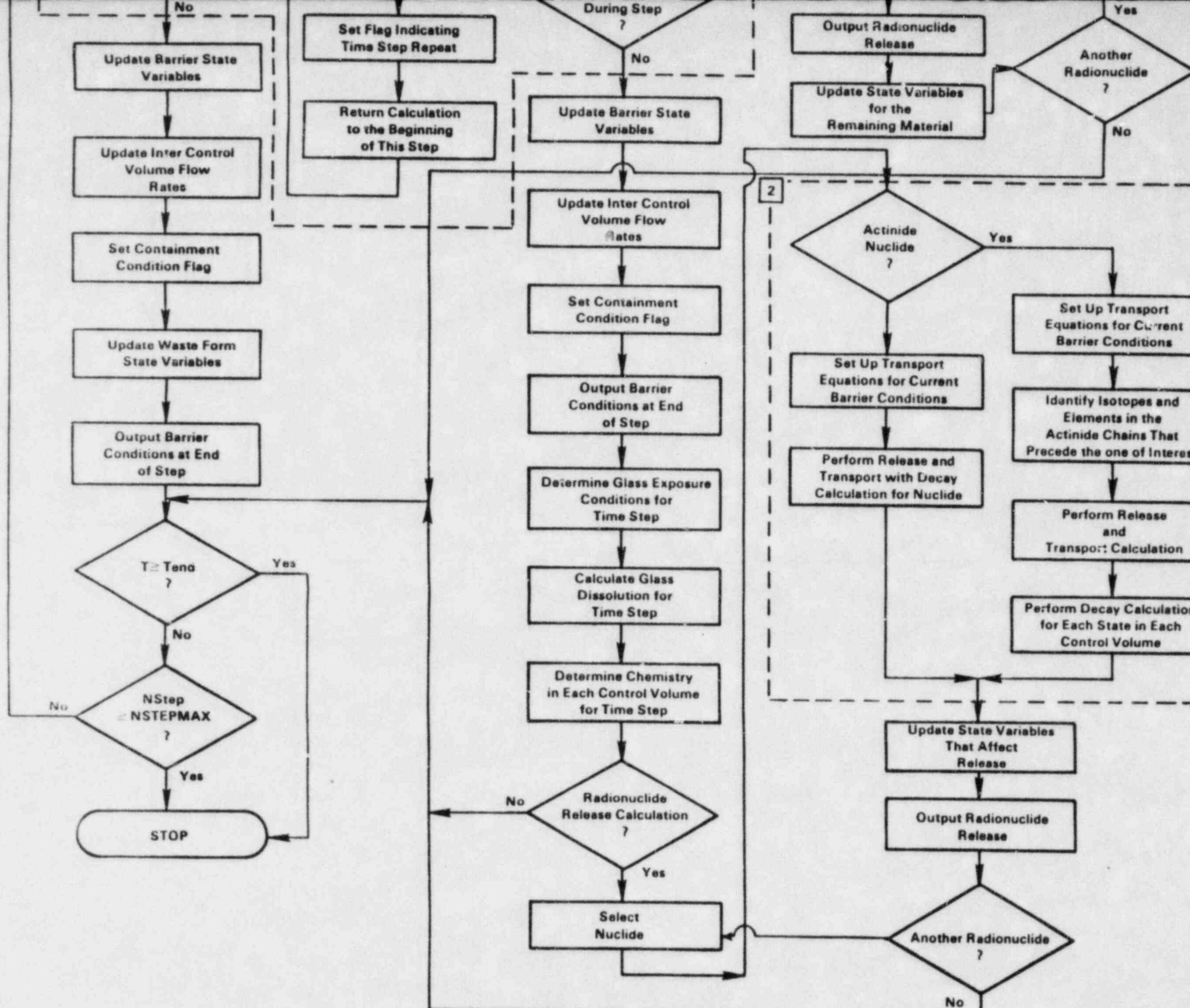
SYSTEM MODEL FLOW CHART





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SUMMARY

- YEAR ONE MILESTONES HAVE BEEN MET
  - --FIRST GENERATION SYSTEM MODEL PREPARED
  - --EXPERIMENTAL PROGRAM WELL UNDERWAY
- CONCLUDED FROM LITERATURE REVIEW AND INITIAL EXPERIMENTAL EFFORT THAT EXISTING DATA BASE IS INADEQUATE AND BIASED
  - --SYSTEM MODEL CAN HELP PRIORITIZE RESEARCH NEED
- YEAR TWO PROGRAM ACTIVITIES DEFINED
  - --SEVERAL TECHNICAL PAPERS AND REPORTS IN PREPARATION
  - --SECOND GENERATION SYSTEM MODEL AVAILABLE
- CONFIDENCE BUILT THAT PROGRAM WILL MEET NRC OBJECTIVES AND LICENSING NEEDS

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PRESENTATION BY THE NRC STAFF

TO THE

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

APRIL 22, 1983

WASHINGTON, D.C.

OVERVIEW

BY

ROBERT J. WRIGHT

OUTLINE OF OVERVIEW

1. GEOLOGIC FEATURES

2. NRC APPROACH TO SCR REVIEW

3. OVERALL IMPRESSIONS OF THE SCR

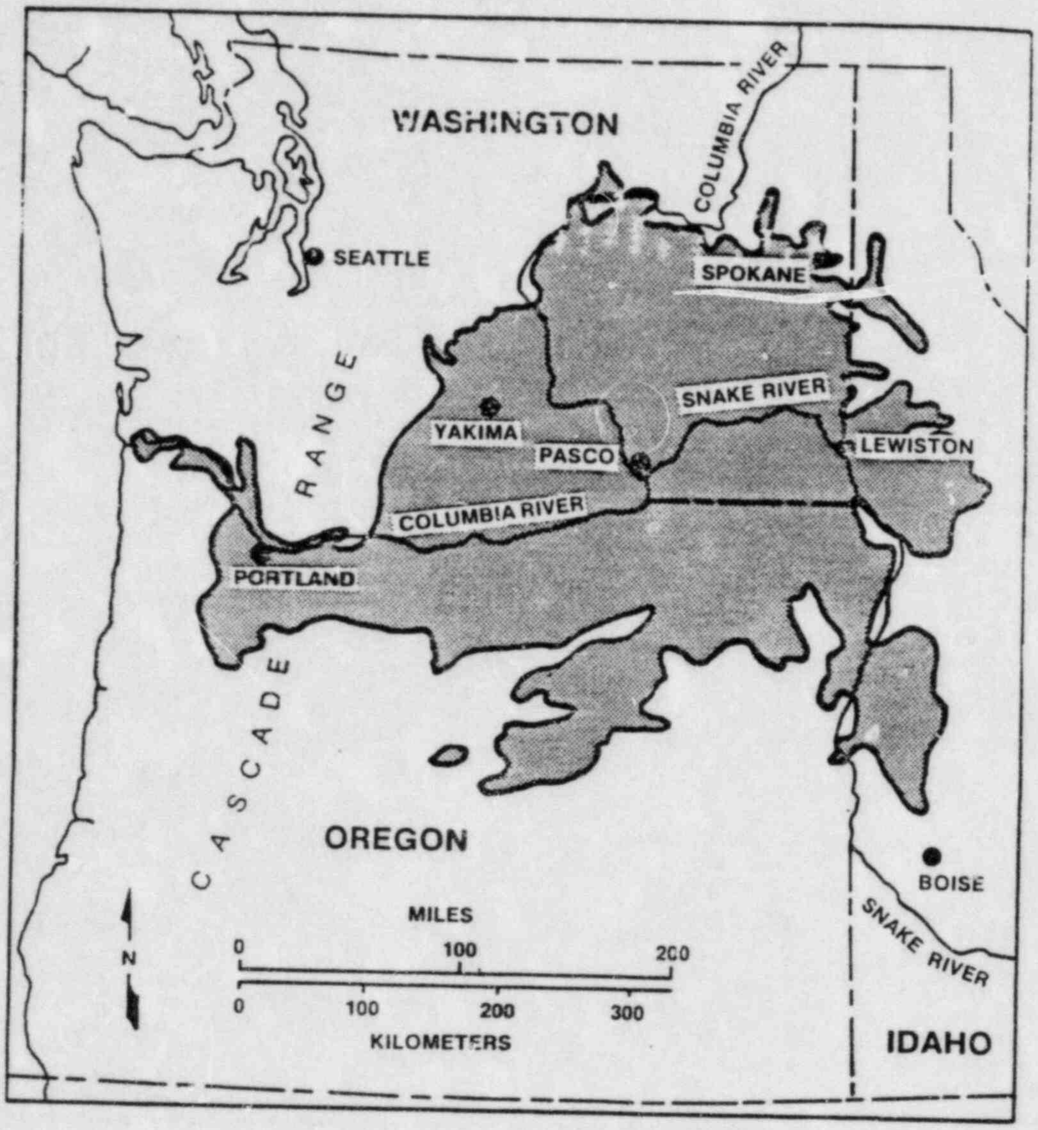
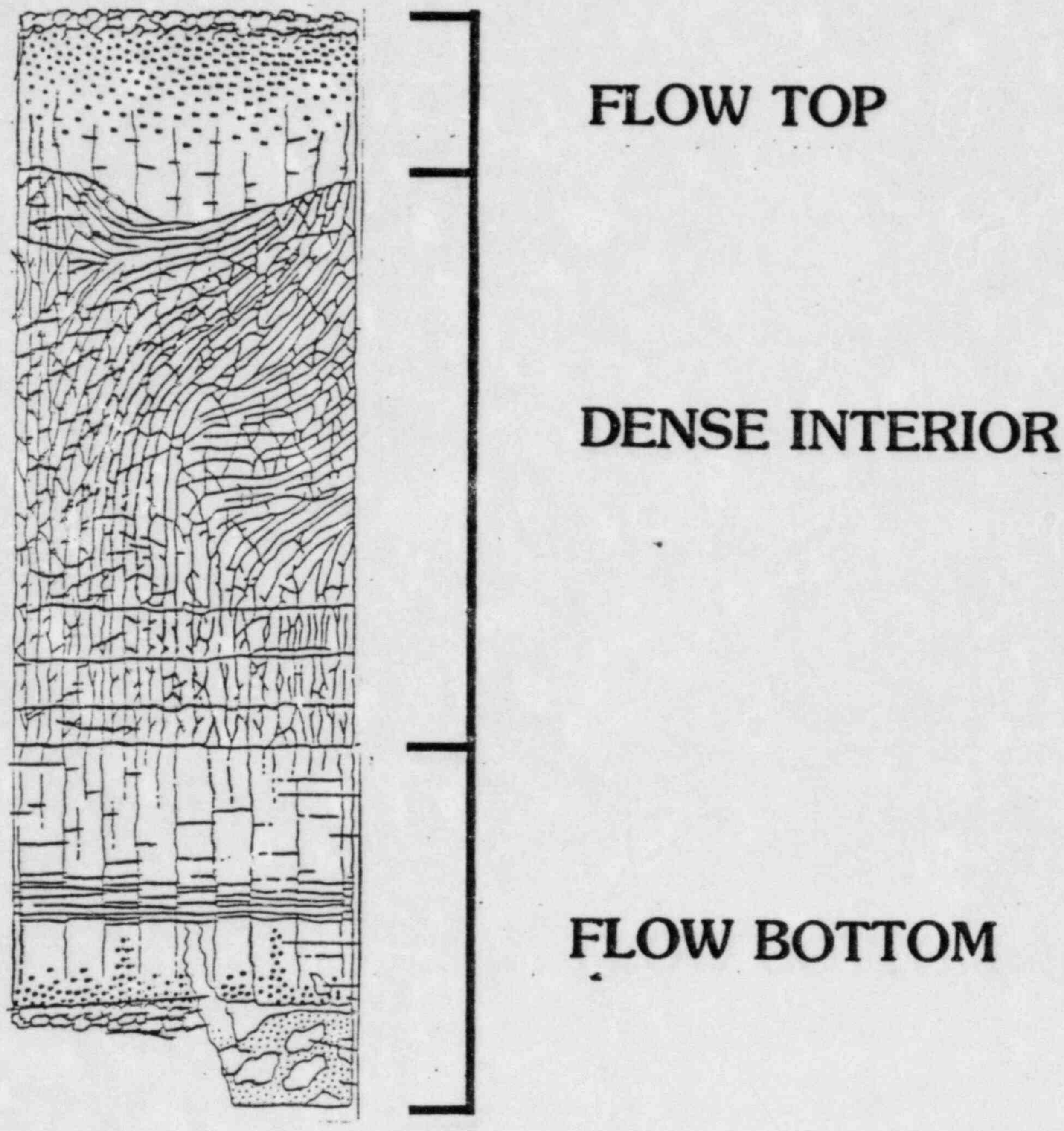


FIGURE 1. Outcrop Extent of the Columbia River Basalt Group (shaded area).

STRUCTURE OF A BASALT FLOW



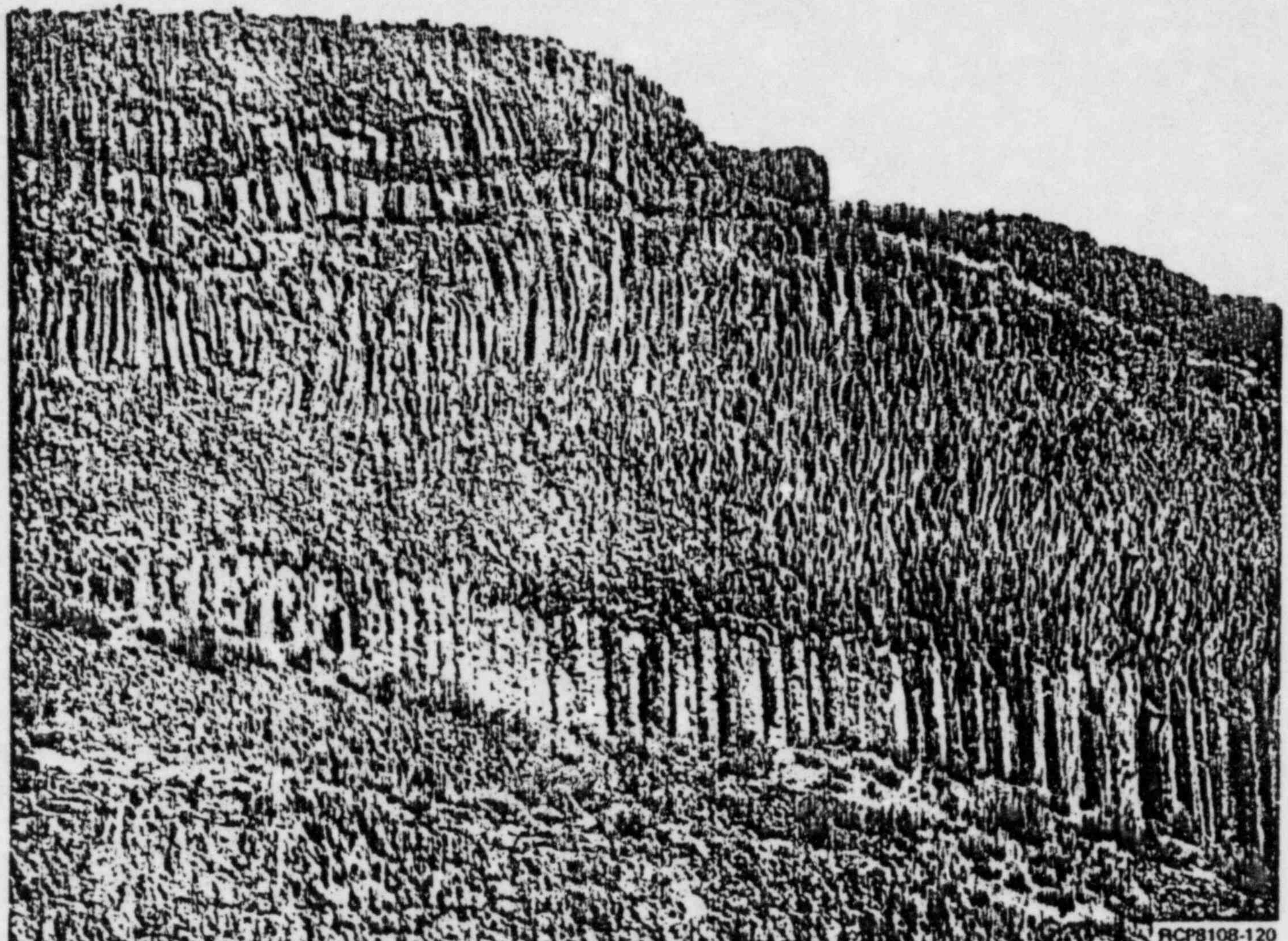


FIGURE 5-4. Well-Developed Upper Colonnade (upper third of exposure) along East Side of Columbia River North of Vantage, Washington. Entablature and lower colonnade underlie the upper colonnade. Height of colonnade is ~5 m.

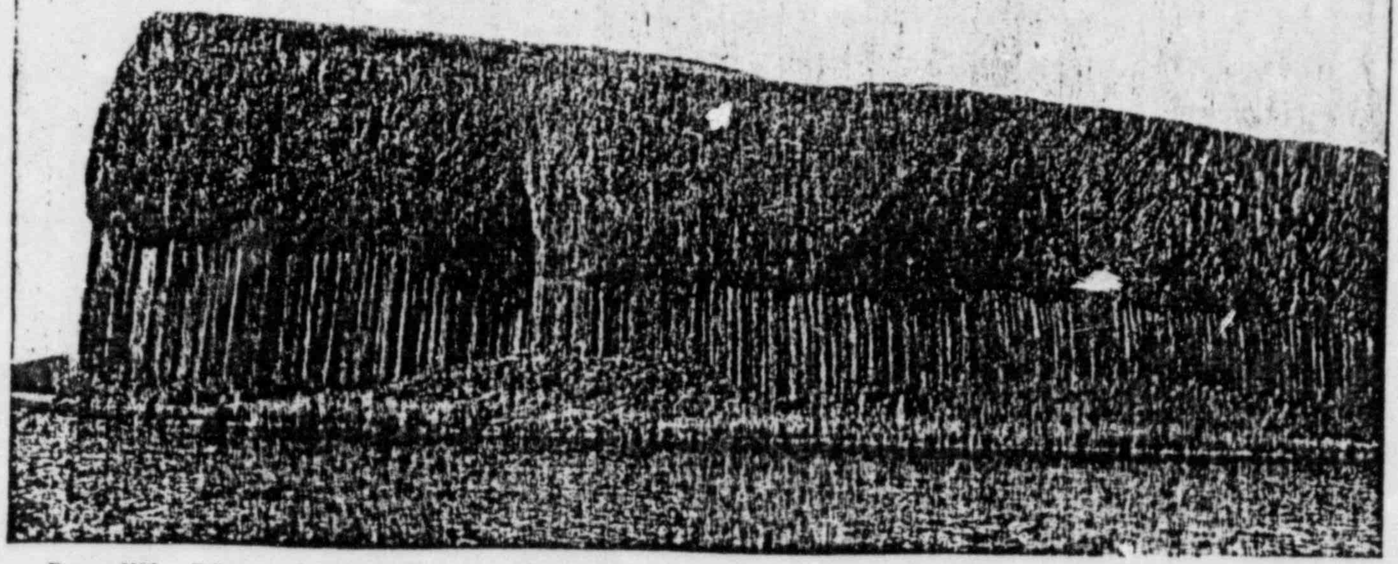


PLATE XII. Columnar Jointing in Lava at Great Face, Staffa, off the West Coast of Scotland. (Photo by K. Fowler-Billings.)

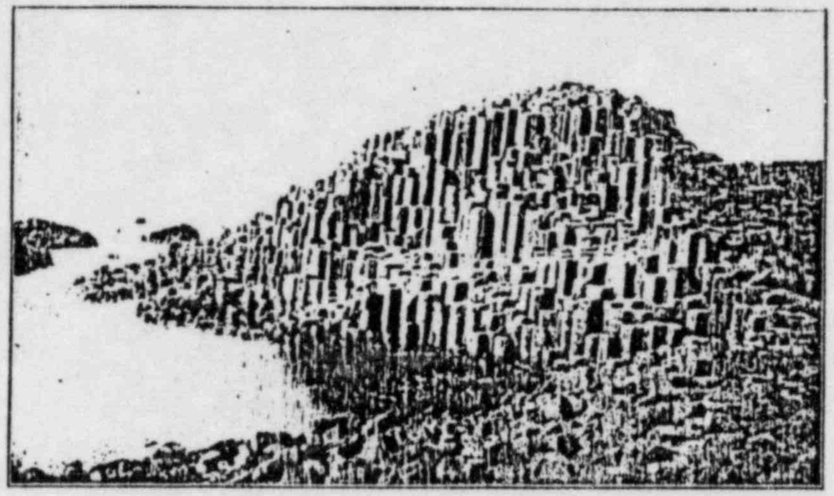


Fig. 223. Part of the Giant's Causeway, northern Ireland. Columnar jointing in basalt, caused by contraction through cooling of lava. Much of the rock has been removed by wave erosion.

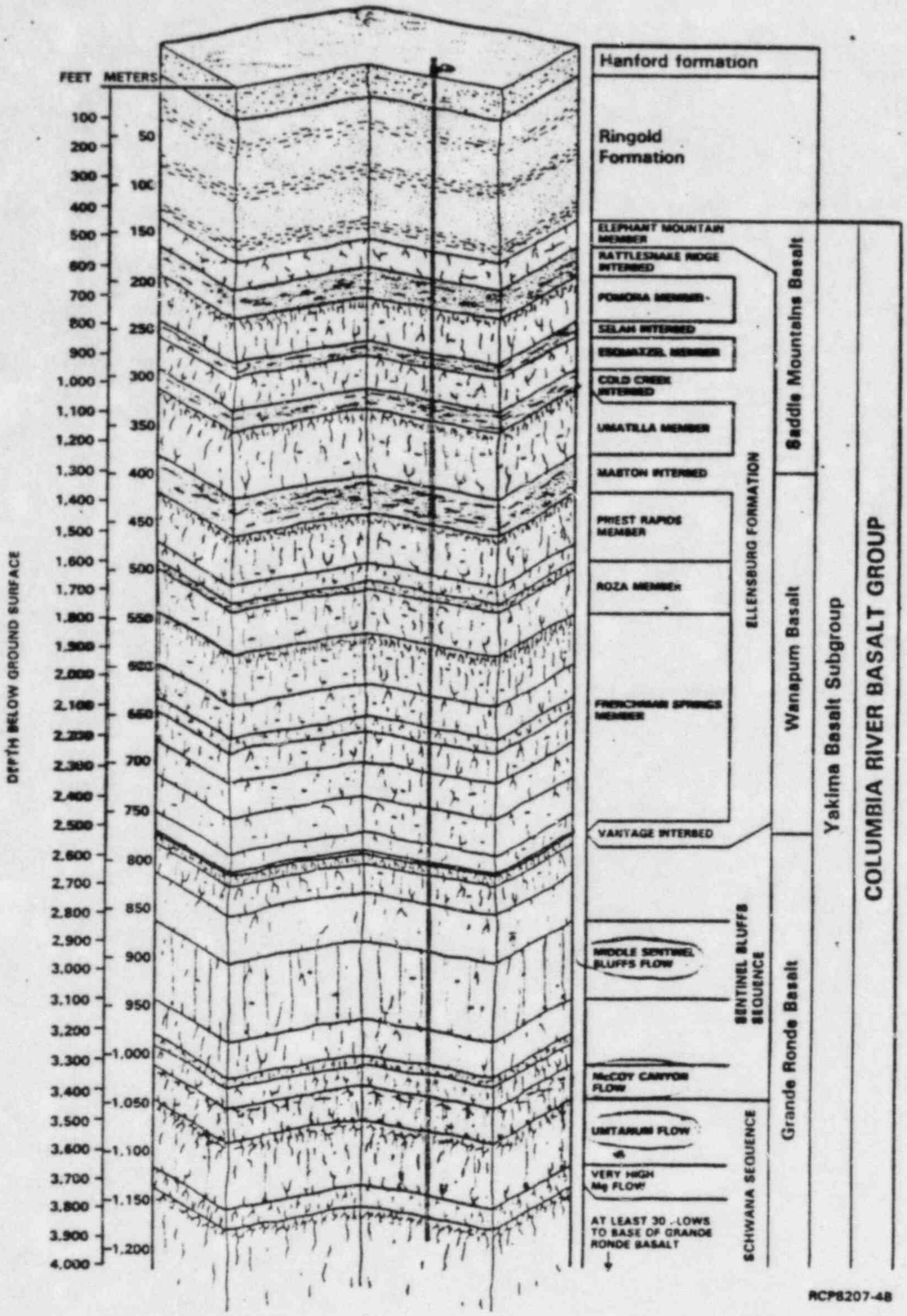


FIGURE 1-4. Stratigraphy of the Columbia River Basalt Group, Yakima Basalt Subgroup, and Intercalated and Suprabasalt Sediments Within the Pasco Basin.

METHOD OF SCR REVIEW

FOUR QUESTIONS WERE ASKED ABOUT THE SCR:

(1) DOES THE SCR CONTAIN THE MATERIALS PRESCRIBED IN 10 CFR 60?

(2) DOES THE SCR ADEQUATELY IDENTIFY POTENTIAL LICENSING ISSUES?

(3) DOES THE SCR ADEQUATELY DESCRIBE THE LEVEL OF KNOWLEDGE AND UNCERTAINTY ASSOCIATED WITH THE RESULTS OF INVESTIGATIONS TO DATE?

(4) ARE THE PROPOSED INVESTIGATIONS ADEQUATE TO OBTAIN THE REMAINING INFORMATION NEEDED TO ADDRESS THE LICENSING ISSUES?

QUESTION 1: DOES THE SCR CONTAIN THE MATERIAL PRESCRIBED IN 10 CFR 60?

GENERAL CONCLUSION. "PROVISIONS TO CONTROL ANY ADVERSE, SAFETY-RELATED EFFECTS FROM SITE CHARACTERIZATION INCLUDING APPROPRIATE QUALITY ASSURANCE PROGRAMS" 10 CFR 60.11(6)(III) IS LACKING

- NRC/DOE COMMUNICATIONS IN ADVANCE OF DSCA PUBLICATION

QUESTION 2: DOES THE SCR ADEQUATELY INDENTIFY POTENTIAL LICENSING ISSUES?

- LICENSING ISSUES - QUESTIONS ABOUT THE SITE THAT MUST BE ADDRESSED BY LICENSING TIME

- GENERAL CONCLUSION. SCR ISSUES AND WORK ELEMENTS SUBSTANTIALLY COVER THE SAME GROUND AS THE NRC ISSUES

QUESTION 3: DOES THE SCR ADEQUATELY DESCRIBE THE LEVEL OF KNOWLEDGE AND UNCERTAINTY ASSOCIATED WITH THE RESULTS OF INVESTIGATIONS TO DATE?

- O <u>GENERAL</u> <u>CONCLUSION</u>. SCR EXPRESSES A LEVEL OF CONFIDENCE IN THE SITE THAT APPEARS UNWARRANTED BASED ON THE NRC STAFF REVIEW OF THE SAME DATA

QUESTION 4: ARE THE PROPOSED INVESTIGATIONS ADEQUATE TO OBTAIN THE REMAINING INFORMATION NEEDED TO ADDRESS THE LICENSING ISSUES?

- GENERAL CONCLUSION. SOME PLANS APPEAR TO BE ON TARGET; SOME PLANS ARE LACKING OR INCOMPLETE; REDIRECTION IS RECOMMENDED FOR SOME PLANS

OVERALL VIEW OF THE SCR

- A WELL ORGANIZED DOCUMENT
- GENERALLY FOLLOWS THE SCHEME PROPOSED IN REGULATORY GUIDE 4.17 - I.E. PRESENT STATE OF KNOWLEDGE, ISSUES, PLANS
- PROVIDES A GOOD BASIS FOR A CONTINUING DOE-NRC DIALOGUE ON HOW TO EFFICIENTLY ADDRESS LICENSING NEEDS

GEOLOGY AND TECTONIC STABILITY

BY

PAUL PRESTHOLT

GEOLOGY/GEOLOGIC STABILITY
DSCA CHAPTER 4

1. INTRODUCTION

2. ISSUES

3. AREAS OF CONCERN

   - TECTONICS AND SEISMICITY
   - STRATIGRAPHIC AND STRUCTURAL DISCONTINUITIES

4. RECOMMENDATIONS

TWO GEOLOGIC ISSUES IDENTIFIED IN THE SCR

- WHAT ARE THE GEOLOGIC, MINERALOGIC, AND PETROGRAPHIC CHARACTERISTICS OF THE CANDIDATE REPOSITORY HORIZON AND SURROUNDING STRATA WITHIN THE REFERENCE REPOSITORY LOCATION? (PAGE 13.1-3)

- WHAT ARE THE NATURE AND RATES OF PAST, PRESENT, AND PROJECTED STRUCTURAL AND TECTONIC PROCESSES WITHIN THE GEOLOGIC SETTING AND THE RRL? (PAGE 13.1-3)

TWO AREAS OF CONCERN

- TECTONICS AND SEISMICITY

- STATIGRAPHIC AND STRUCTURAL DISCONTINUITIES WITHIN THE BASALT FLOWS

TECTONICS AND SEISMICITY

THE SCR STATES

- NO FAULTS HAVE BEEN IDENTIFIED ON THE HANFORD SITE THAT WOULD HAVE AN ADVERSE IMPACT ON A REPOSITORY CONSTRUCTED AT THE REFERENCE REPOSITORY LOCATION...(EXECUTIVE SUMMARY, PAGE 2)
- THE PRESENT CALCULATED RATE OF DEFORMATION POSES NO THREAT TO THE LONG-TERM INTEGRITY OF A REPOSITORY IN A BASALT AT THE HANFORD SITE...(EXECUTIVE SUMMARY, PAGE 1)
- ...A PRELIMINARY QUANTITATIVE ASSESSMENT INDICATES THAT THE TECTONIC PROCESSES WITHIN THE PASCO BASIN DO NOT POSE A HAZARD TO REPOSITORY CONSTRUCTION AND OPERATION OR TO LONG-TERM ISOLATION OF RADIOACTIVE WASTE...(PAGE 3.8-6)

STRATIGRAPHIC AND STRUCTURAL DISCONTINUITIES

THE SCR STATES:

- THE GENERAL STRATIGRAPHIC SETTING OF THE PASCO BASIN AND COLD CREEK SYNCLINE IS WELL UNDERSTOOD, AND THERE ARE NO CURRENTLY KNOWN STRATIGAPHIC OR LITHOLOGIC FACTORS THAT WOULD PRECLUDE THE SITING OF A REPOSITORY IN ONE OF THE TWO CANDIDATE HORIZONS WITHIN THE REFERENCE REPOSITORY LOCATION... (PAGE 3.5-39).

- BASALT FLOWS LOCATED MORE THAN 610 METERS (2,000 FEET) BELOW THE GROUND SURFACE ARE NOT SUBJECT TO SIGNIFICANT EROSION, AND SEVERAL FLOWS MAY HAVE THICK ENOUGH FLOW INTERIORS AND SUFFICIENT LATERAL CONTINUITY TO ACCOMMODATE THE CONSTRUCTION OF A NUCLEAR WASTE REPOSITORY...(EXECUTIVE SUMMARY, PAGE 1).

- ...UMTANUM AND THE MIDDLE SENTINEL BLUFFS ARE THE LEADING HOST-ROCK CANDIDATES WITHIN THE REFERENCE REPOSITORY LOCATION.... BOTH FLOWS ARE INTERPRETED TO HAVE SUFFICIENTLY THICK DENSE INTERIORS TO MEET DESIGN AND ISOLATION REQUIREMENTS... (PAGE 3.1-1).

RECOMMENDATIONS

TECTONICS-SEISMICITY

- REVIEW ALL GEOLOGIC DATA TO DEVELOP A GOOD REGIONAL SYNTHESIS AND DEVELOP ONE OR MORE TECTONIC MODELS THAT ARE CONSISTENT WITH GEOLOGIC DATA

- EXPAND THE FIELD PROGRAM, AS NECESSARY, IN STRUCTURAL GEOLOGY IN AREAS ADJACENT TO THE PASCO BASIN TO SUPPLEMENT WORK IN THE SITE AREA.

- ESTABLISH THE MAXIMUM CREDIBLE EARTHQUAKE FOR EACH SEISMOGENIC STRUCTURE OR REGION THAT COULD AFFECT THE SITE.

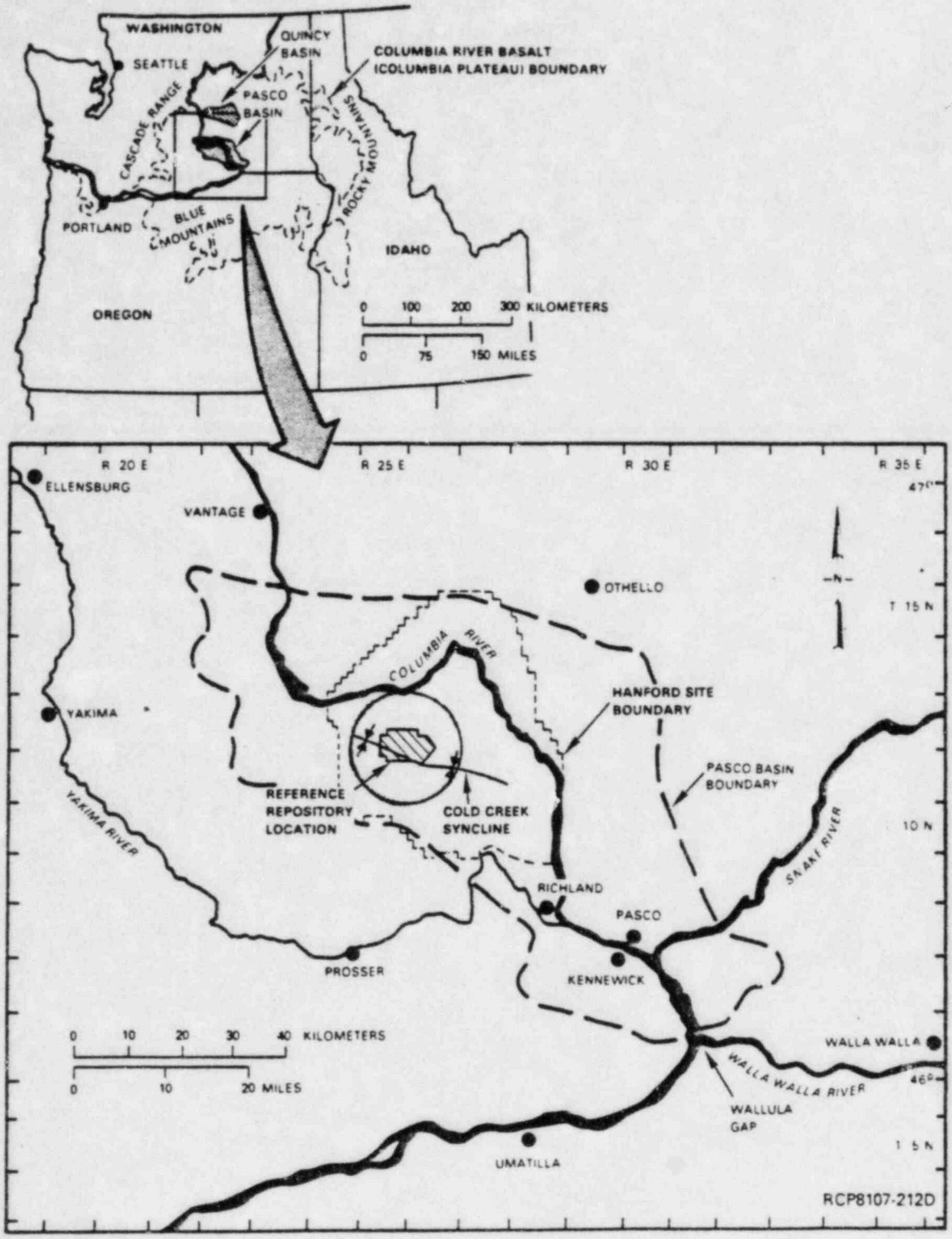
- SPECIFY THE APPROPRIATE GROUND ATTENUATION TO THE SITE.

RECOMMENDATIONS

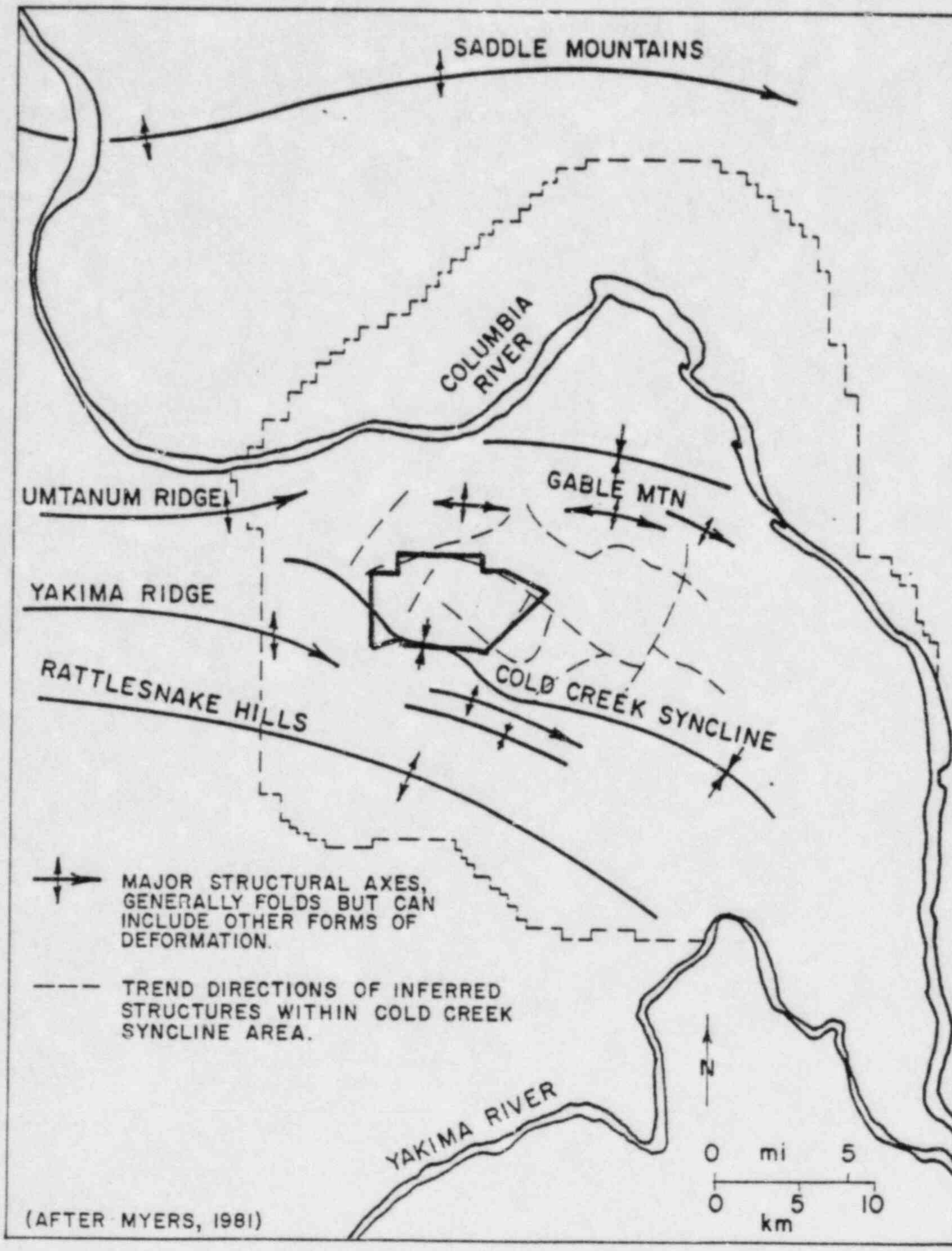
TO ADEQUATELY CHARACTERIZE STRATIGRAPHIC AND STRUCTURAL DISCONTINUITIES

- DEVELOP A WELL DESIGNED EXPLORATION PROGRAM TO DEFINE, TO THE EXTENT NECESSARY AND PRACTICABLE, THE HETEROGENEITIES WITHIN THE CANDIDATE BASALT FLOWS.

- FACTOR THE REMAINING UNCERTAINTIES INTO THE PERFORMANCE ASSESSMENT STUDIES, PARTICULARLY INTO THE CONCEPTUAL GROUNDWATER FLOW MODEL AND UNDERGROUND FACILITY SENSITIVITY ANALYSES.



Location of the Columbia Plateau, Hanford Site, and Cold Creek Syncline, and the References Repository Location (Source: SCR Figure 3-1)



Major structural features of the Hanford site and vicinity

GROUNDWATER

BY

TILAK R. VERMA

GROUNDWATER

- IMPORTANCE OF GROUNDWATER SYSTEM
- MAJOR CONCLUSIONS IN THE SCR
- HYDROGEOLOGIC INFORMATION IN THE SCR
- NRC STAFF'S ANALYSIS
  - ANALYSIS OF THE DATA IN THE SCR
  - ANALYSIS OF THE SITE CHARACTERIZATION PLANS IN THE SCR
- RECOMMENDATIONS

IMPORTANCE OF GROUNDWATER SYSTEM

IT IS GENERALLY RECOGNIZED THAT THE MOST PROBABLE MODE BY WHICH RADIONUCLIDES COULD BE RELEASED FROM A REPOSITORY IS THROUGH THE GROUNDWATER SYSTEM.

MAJOR CONCLUSIONS IN THE SCR

- THE GROUNDWATER FLOW PATHS FROM BOTH CANDIDATE REPOSITORY HORIZONS ARE PREDOMINATLY HORIZONTAL AND ARE RESTRICTED TO THE GRANDE RONDE BASALT.

- STUDIES CONDUCTED TO DATE BY ROCKWELL AND OTHER INDEPENDENT ORGANIZATIONS UNANIMOUSLY AGREE THAT THE MINIMUM TRAVEL TIME FROM THE REPOSITORY TO THE ACCESSIBLE ENVIRONMENT UNDER NATURAL, PRE-WASTE-EMPLACEMENT CONDITIONS IS LIKELY TO BE ON THE ORDER OF 10,000 YEARS OR LONGER. AS A RESULT, CONSIDERABLE CONFIDENCE EXISTS THAT COMPLIANCE WITH THE 1,000-YEAR MINIMUM TRAVEL TIME TO THE ACCESSIBLE ENVIRONMENT WILL BE DEMONSTRATED.

- THE POST-WASTE-EMPLACEMENT GROUNDWATER TRAVEL TIMES FROM THE REPOSITORY TO THE ACCESSIBLE ENVIRONMENT ARE ESTIMATED TO BE GREATER THAN 10,000 YEARS.

HYDROGEOLOGIC INFORMATION IN THE SCR

DOE'S CONCLUSIONS ARE BASED ON:

- DOE'S OVERSIMPLIFIED CONCEPTUAL MODEL OF THE HYDROGEOLOGIC SYSTEM OF THE HANFORD SITE
- SELECTED DATA ON HYDRAULIC PARAMETERS (I.E., VERTICAL AND HORIZONTAL HYDRAULIC CONDUCTIVITIES, HYDRAULIC HEADS, EFFECTIVE POROSITY, DISPERSIVITY AND MATRIX DIFFUSION)
- USE OF HYDROCHEMISTRY DATA FOR GROUNDWATER FLOW INTERPRETATION

NRC STAFF'S ANALYSIS OF THE SCR

STAFF'S ANALYSIS IS DIVIDED INTO TWO PARTS:

1. DATA REPORTED IN THE SCR AND THE USE OF THESE DATA IN CONCEPTUAL AND NUMERICAL MODELING FOR GROUNDWATER FLOW PATHS AND TRAVEL TIME CALCULATIONS.

2. SITE CHARACTERIZATION PLANS PROVIDED IN THE SCR.

1. DATA

- DATA COLLECTED TO DATE ARE:
  - O FROM SMALL DIAMETER SINGLE BOREHOLES
  - O THROUGH DRILL-AND-TEST SEQUENCE
  - O SMALL SCALE POINT MEASUREMENTS OF HYDRAULIC PARAMETERS
  - O HIGHLY VARIABLE, INDICATING VERY POOR CORRELATION
- THESE DATA DO NOT INCLUDE:
  - O MEASUREMENTS OF VERTICAL PERMEABILITY
  - O EVALUATION OF THE EFFECTS OF DRILLING MUD ON MEASURED HYDRAULIC PROPERTIES
  - O EFFECTS OF STRUCTURAL AND STRATIGRAPHIC DISCONTINUITIES ON HYDRAULIC PARAMETERS
  - O LONG TERM MEASUREMENTS OF HYDRAULIC HEADS IN DIFFERENT HYDROSTRATIGRAPHIC UNITS

- FROM EVALUATION OF THESE DATA NRC CONCLUDES THAT:

  - O ALTERNATE CONCEPTUAL MODELS ARE PLAUSIBLE

  - O HYDROGEOLOGY OF THE HANFORD SITE IS TOO POORLY CHARACTERIZED TO DEVELOP OR DEFEND ANY SINGLE CONCEPTUAL MODEL OF GROUNDWATER FLOW

  - O DOE'S ASSERTION THAT THE GROUNDWATER FLOW IN THE PASCO BASIN IS TO THE SOUTHEAST CAN NOT BE SUPPORTED

  - O CONCLUSIVE DEFINITION OF SEPARATE FLOW SYSTEMS IS NOT SUPPORTED BY DOE'S HYDROCHEMISTRY DATA AT THE SITE

  - O REGIONAL-SCALE GROUNDWATER MODELING HAS NOT BEEN USED TO DERIVE BOUNDARY CONDITIONS FOR BASIN-SCALE MODELING PURPOSES

  - O SENSITIVITY STUDIES BY THE NRC STAFF SHOW THAT CALCULATIONS OF PRE-EMPLACEMENT GROUNDWATER TRAVEL TIMES CAN VARY BY SEVERAL ORDERS OF MAGNITUDE.

  - O THE LARGE RANGE OF POSSIBLE TRAVEL TIMES IS THE RESULT OF UNCERTAINTIES IN HYDROGEOLOGIC CHARACTERIZATION OF THE HANFORD SITE

2. SITE CHARACTERIZATION PLANS

- THE PLANS PRESENTED INCLUDE:

  - O A UNIQUE CONCEPTUAL MODEL, WHICH IS BASED ON THE ASSUMED STRATIFIED NATURE OF GROUNDWATER IN BASALTS.

  - O PLANS TO COLLECT NEW DATA ON HYDRAULIC PARAMETERS THROUGH SMALL-SCALE TESTS IN 30 SINGLE BOREHOLES, 4 DUAL BOREHOLES AND 1 THREE-BOREHOLE CLUSTER.

  - O PLANS TO CONTINUE COLLECTING POINT MEASUREMENTS OF HYDRAULIC HEAD DURING THE DRILL-AND-TEST SEQUENCE.

- THE PLANS DO NOT INCLUDE:

  - O REASONABLE ALTERNATIVE CONCEPTUAL MODELS THAT INCLUDE HYDROGEOLOGICALLY IMPORTANT ASPECTS OF OBSERVED GEOLOGIC FEATURES.

  - O LARGE-SCALE MEASUREMENTS OF HYDRAULIC PARAMETERS

  - O LONG-TERM MEASUREMENTS OF HYDRAULIC HEADS OF LOCATIONS NEAR AND WITHIN THE RRL.

  - O USE OF REGIONAL-SCALE GROUNDWATER MODELING TO INFER BOUNDARY CONDITIONS.

- FROM EVALUATION OF THESE PLANS, THE NRC STAFF CONCLUDES THAT:

  - O ADDITIONAL SITE CHARACTERIZATION PROPOSED BY DOE MAY NOT PRODUCE HYDROGEOLOGIC INFORMATION NEEDED BY LICENSING TIME

RECOMMENDATIONS

BASED ON ANALYSIS OF DATA AND PLANS PRESENTED IN THE SCR, THE NRC STAFF RECOMMENDS THAT THE FOLLOWING PROBLEM AREAS BE ADDRESSED BEFORE LICENSING:

(1) REPRESENTATIVE HYDRAULIC PARAMETER VALUES. DOE SHOULD CONSIDER CONVENTIONAL, LARGE-SCALE, MULTIPLE-WELL PUMP TESTS THAT ARE COMBINED WITH CONTINUOUS HEAD MEASUREMENTS IN VARIOUS HYDROSTRATIGRAPHIC UNITS.

(2) EXTERNAL BOUNDARY CONDITIONS. DOE SHOULD CONSIDER DIRECT, LONG-TERM MEASUREMENTS OF HYDRAULIC HEADS TO DETERMINE BOUNDARY CONDITIONS FOR NUMERICAL GROUNDWATER MODELING.

(3) EFFECTIVE POROSITY. DOE SHOULD CONSIDER MEASUREMENTS OF EFFECTIVE POROSITY AT SEVERAL LOCATIONS IN SEVERAL HYDROSTRATIGRAPHIC UNITS.

(4) HYDROCHEMISTRY. DOE SHOULD CONSIDER INTEGRATION OF THE HYDROCHEMISTRY WITH DEFENSIBLE HYDRAULIC PARAMETERS AND HYDRAULIC HEADS, IF HYDROCHEMICAL CHARACTERIZATION IS TO BE USED FOR FLOW SYSTEM INTERPRETATION.

(5) ALTERNATIVE CONCEPTUAL MODELS. DOE SHOULD CONSIDER USE OF THE ABOVE DATA TO CHARACTERIZE THE HYDROGEOLOGIC SYSTEM BY TESTING ALTERNATIVE CONCEPTUAL MODELS IN APPROPRIATE SENSITIVITY STUDIES.

UNCERTAINTY IN GROUNDWATER TRAVEL-TIME CALCULATIONS FOR THE HANFORD SITE

BY

MARK J. LOGSDON

UNCERTAINTY IN GROUNDWATER TRAVEL-TIME CALCULATIONS FOR THE HANFORD SITE

BACKGROUND

PURPOSE

ASSUMPTIONS

DATA

RESULTS

CONCLUSIONS

RECOMMENDATIONS TO DOE

ACCESSIBLE ENVIRONMENT

(1) THE ATMOSPHERE, (2) LAND SURFACES, (3) SURFACE WATER, (4) OCEANS, AND (5) THE PORTION OF THE LITHOSPHERE THAT IS OUTSIDE THE CONTROLLED AREA. THE OVERALL SYSTEM PERFORMANCE FOR THE GEOLOGIC REPOSITORY IS CALCULATED AT THIS BOUNDARY (§60.2).

CONTROLLED AREA

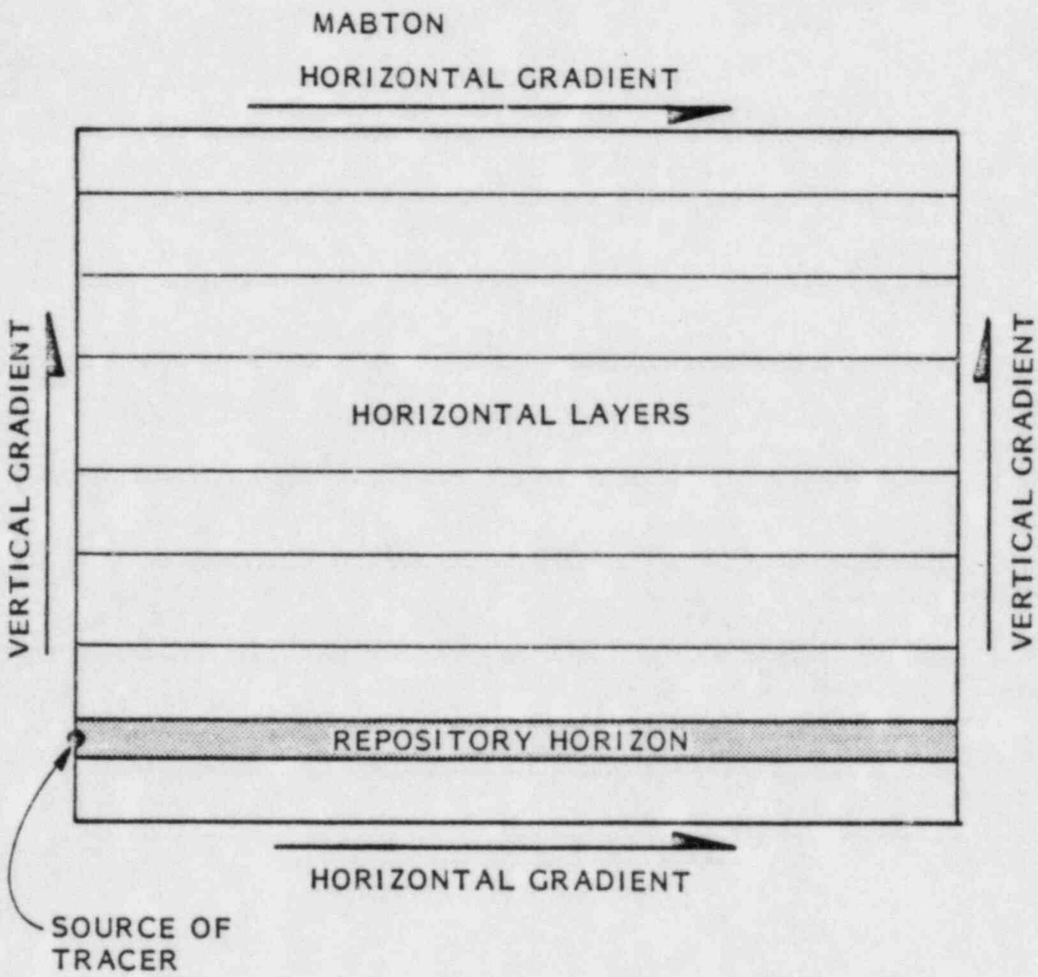
A SURFACE LOCATION, TO BE MARKED BY SUITABLE MONUMENTS EXTENDING HORIZONTALLY NO MORE THAN 10 KM IN ANY DIRECTION FROM THE UNDERGROUND FACILITY, AND THE UNDERLYING SUBSURFACE, WHICH AREA HAS BEEN COMMITTED TO USE AS A GEOLOGIC REPOSITORY AND FROM WHICH INCOMPATIBLE ACTIVITIES WOULD BE RESTRICTED FOLLOWING PERMANENT CLOSURE (§60.2).

DISTURBED ZONE

THAT PORTION OF THE CONTROLLED AREAS WHOSE PHYSICAL OR CHEMICAL PROPERTIES HAVE CHANGED AS A RESULT OF UNDERGROUND FACILITY CONSTRUCTION OR FROM HEAT GENERATED BY THE EMPLACED RADIOACTIVE WASTES SUCH THAT THE RESULTANT CHANGE OF PROPERTIES MAY HAVE A SIGNIFICANT EFFECT ON THE PERFORMANCE OF THE GEOLOGIC REPOSITORY. THE MINIMUM GROUNDWATER TRAVEL TIME IS CALCULATED BETWEEN THIS BOUNDARY AND THE ACCESSIBLE ENVIRONMENT (§60.133(A)(2)).

PURPOSE

TO ASSESS THE RANGE OF POSSIBLE PRE-EMPLACEMENT GROUNDWATER TRAVEL TIME BASED ON CURRENTLY AVAILABLE HYDROGEOLOGIC INFORMATION FOR THE HANFORD SITE.

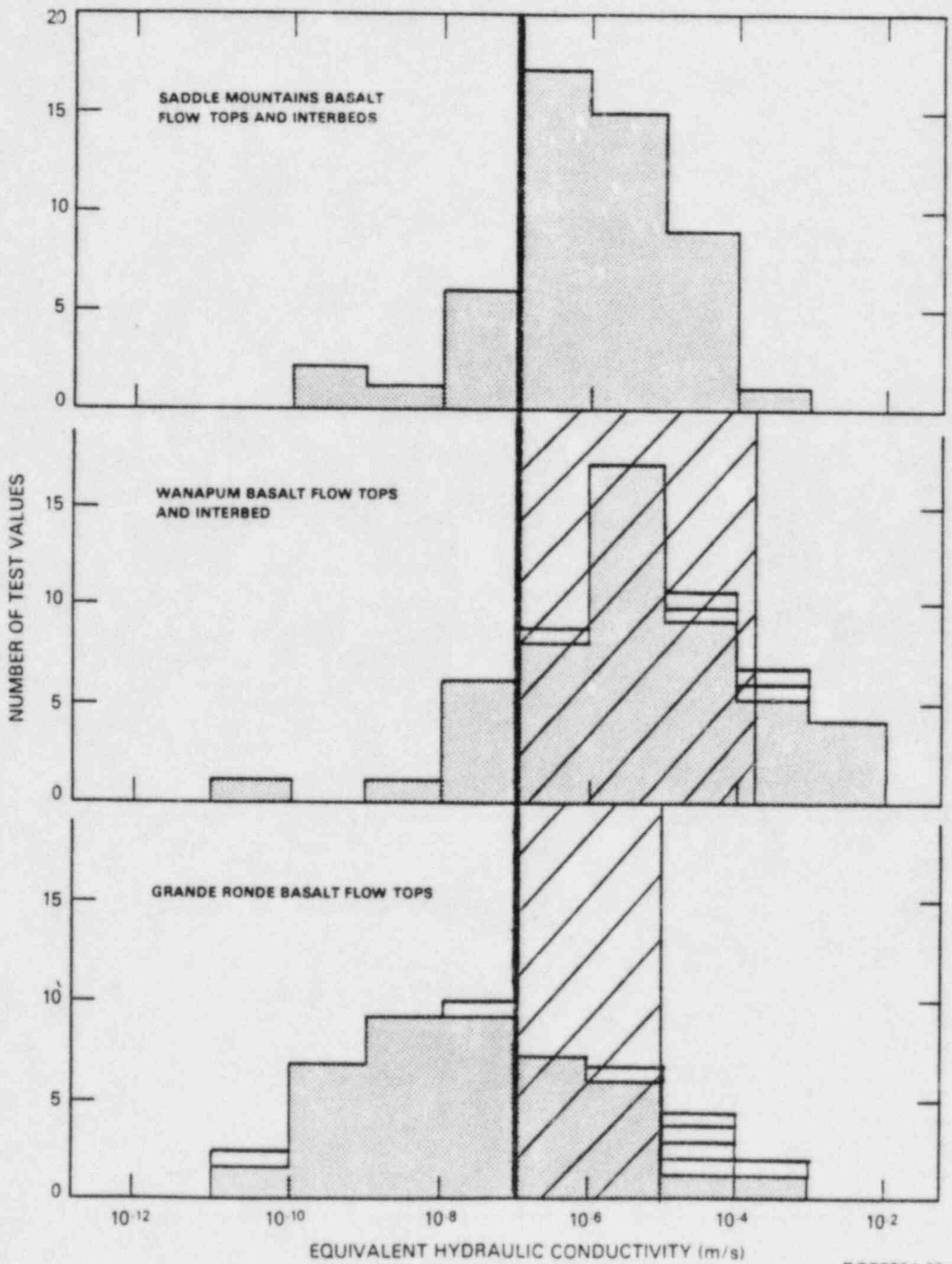


The analysis domain of the study.

GROUNDWATER TRAVEL TIME EQUATION

$$\text{TRAVEL TIME} = \frac{\text{FLOW PATH LENGTH}}{\text{AVERAGE LINEAR VELOCITY ALONG THE FLOW PATH}}$$

$$\text{TRAVEL TIME} = \frac{\text{EFFECTIVE POROSITY}}{\text{HYDRAULIC CONDUCTIVITY} \times \text{HYDRAULIC GRADIENT}} \times \text{FLOW PATH LENGTH}$$



SADDLE MOUNTAINS BASALT FLOW TOPS AND INTERBEDS
WANAPUM BASALT FLOW TOPS AND INTERBED
GRANDE RONDE BASALT FLOW TOPS
NUMBER OF TEST VALUES
EQUIVALENT HYDRAULIC CONDUCTIVITY (m/s)

RCP8204-60

RESULTS

- FOR A CONCEPTUAL MODEL EQUIVALENT TO THE ONE USED IN THE SCR, TRAVEL TIME RANGED FROM 51 TO 43,547 YEARS.

- FOR ALTERNATIVE CONCEPTUAL MODELS AND THE ENTIRE RANGE OF HYDRAULIC PARAMETERS IN THE SCR, PLAUSIBLE GROUNDWATER TRAVEL TIMES COULD RANGE FROM LESS THAN 20 YEARS TO GREATER THAN 1 MILLION YEARS.

CONCLUSION

- UNIQUE, DETERMENISTIC STATEMENTS ABOUT FLOW PATH OR TRAVEL TIMES CANNOT BE MADE WITH A HIGH DEGREE OF CONFIDENCE AT THIS TIME. VALUES OF KEY HYDRAULIC PARAMETERS ARE UNCERTAIN OR UNKNOWN, AND THE APPROPRIATE CONCEPTUAL MODEL HAS NOT BEEN DETERMINED.

RECOMMENDATIONS TO DOE

- DOE SHOULD CONSIDER USING SENSITIVITY STUDIES TO HELP EVALUATE ALTERNATIVE CONCEPTUAL MODELS AND TO HELP GUIDE THE FIELD TEST PROGRAM.

- DOE SHOULD CONSIDER A SITE CHARACTERIZATION PLAN THAT ADDRESSES THE FOLLOWING PROBLEM AREAS:

  (1) REPRESENTATIVE VALUES OF HYDRAULIC PARAMETERS

  (2) EXTERNAL BOUNDARY CONDITIONS

  (3) EFFECTIVE POROSITY

GEOCHEMISTRY

BY

PHILIP S. JUSTUS

GEOCHEMISTRY

- IMPORTANCE OF GEOCHEMISTRY
- GEOCHEMICAL INFORMATION IN THE SCR
- STAFF ANALYSIS
  - ANALYSIS OF DATA IN SCR
  - ANALYSIS OF PLANS IN SCR
- SUGGESTIONS TO DOE
- STAFF CONCLUSIONS

IMPORTANCE OF GEOCHEMISTRY

THE MOST CREDIBLE MECHANISM FOR MIGRATION OF RADIONUCLIDES FROM A REPOSITORY TO THE ACCESSIBLE ENVIRONMENT IS SOLUTION TRANSPORT IN GROUNDWATER (SCR, 1982)

GEOCHEMICAL INFORMATION IN SCR

TWO MAJOR PROCESSES WHICH RETARD RADIONUCLIDE TRANSPORT...

- PRECIPITATION OF RADIONUCLIDES AS NEW SOLID PHASES
  - SOLUTION/PRECIPITATION

- SORPTION OF RADIONUCLIDES ONTO HOST ROCK OR PACKING/BACKFILL MINERAL PHASES
  - SORPTION/DESORPTION

GEOCHEMICAL INFORMATION IN SCR

SOME VARIABLES WHICH AFFECT SOLUBILITY AND SORPTION...

		SCR DATA	STAFF ASSESSMENT OF UNCERTAINTIES
O	TEMPERATURE	50-300°C	SMALL
O	PH	ALKALINE	MODERATE
O	REDOX CONDITIONS	VERY REDUCING	LARGE
O	HOST ROCKS	PRIMARY/SECONDARY MINERALS IDENTIFIED	MODERATE
O	GROUNDWATER COMPOSITON	COMPOSITE	UNKNOWN ALONG FLOWPATH

SOLUBILITY

"BASED ON SOLUBILITY, THE MAXIMUM POSSIBLE RELEASE RATES FOR ALL THE RADIONUCLIDES CONSIDERED WILL BE BELOW THE NRC 10^{-5} PROPOSED RELEASE RATE CRITERION (NRC, 1981) AND THE DRAFT CUMULATIVE RELEASE CRITERION (EPA, 1981)." (SCR, 1982)

THERE IS CONSIDERABLE UNCERTAINTY IN:

- SOURCE TERM
- THERMODYNAMIC DATA BASE
- CHEMICAL SPECIATION
- REDOX CONDITIONS
- ROLE OF COLLOIDS
- PLANS FOR VALIDATION

DOE SHOULD CONSIDER THE FOLLOWING:

- DETERMINE MISSING THERMODYNAMIC CONSTANTS
- USE OF MODELS TO ESTIMATE SOLUBILITY
- EXPERIMENTALLY VERIFY MODELING RESULTS
- DETERMINE IMPORTANCE OF COLLOIDS
- PUBLISH FOR PEER REVIEW

SORPTION

RESTRICTION OF NUCLIDE MIGRATION IS PROVIDED BY SORPTION AND THE SORPTIVE PROPERTIES OF THE HOST ROCK/BACKFILL WILL NOT BE DEGRADED BY HEAT PRODUCED BY THE WASTE MATERIAL. (SCR, 1982)

THERE IS CONSIDERABLE UNCERTAINTY IN:

- O SOURCE TERM
- O CHEMICAL SPECIATION
- O REDOX CONDITIONS
- O USE OF HYDRAZINE
- O HOST-ROCK ALONG FLOW PATH
- O USE OF SORPTION DATA

DOE SHOULD CONSIDER THE FOLLOWING:

- O USE OF REPRESENTATIVE HOST-ROCK MATERIALS
- O DETERMINE SORPTION ISOTHERMS
- O CONTROL OF REDOX CONDITIONS
- O DETERMINE EFFECTS OF SPECIATION
- O PUBLISH FOR PEER REVIEW

REDOX CONDITIONS

THE PREVAILING REDOX POTENTIAL AT HANFORD IS ESTIMATED TO BE VERY LOW AFTER WASTE EMPLACEMENT AND CLOSURE, THE REPOSITORY WILL RETURN TO VERY LOW REDOX POTENTIAL CONDITIONS. (SCR, 1982)

THERE IS CONSIDERABLE UNCERTAINTY IN:

- MEASURED VALUES
- CALCULATED VALUES
- BUFFERING CAPACITY
- REACTION KINETICS
- REDUCTION ABILITY OF SYSTEM

DOE SHOULD CONSIDER THE FOLLOWING:

- BOUND _IN SITU_ REDOX CONDITIONS
- CONFIRM MINERALOGICAL CONTROL OF REDOX CONDITIONS
- DETERMINE REDOX EQUILIBRATION KINETICS
- CONFIRM REACTIVITY OF KEY RADIONUCLIDES
- PUBLISH FOR PEER REVIEW

LONG-TERM ASSESSMENTS

NATURAL ANALOGS STUDIES OF WASTE FORM, CANISTER OVERPACK AND REPOSITORY SUGGEST THAT LONG TERM HAZARDS FROM HLW IN A REPOSITORY IN BASALT SHOULD BE MINIMAL (SCR, 1982)

DSCA COMMENT

- PROPOSED ANALOGS APPEAR INAPPLICABLE

DSCA SUGGESTIONS

- RELATE ANALOGS TO EXPECTED SITE AND REPOSITORY CONDITIONS
- PLAN REALISTIC FIELD STUDIES

MAIN AREAS OF UNCERTAINTY EXPECTED TO BE ADDRESSED BY DOE

- SITE-SPECIFIC CONDITIONS
- SOLUBILITY AND SORPTION DATA
- EFFECT OF REDOX POTENTIAL AND pH
- SYSTEM RESPONSE TO TEMPERATURE VARIATION
- CHANGES IN MINERAL AND SOLUTION CHEMISTRY
- APPLICATION OF NATURAL ANALOG STUDIES AND FIELD TESTS
- USE OF RETARDATION DATA IN PERFORMANCE MODELS
- APPROACH TO CONFIRMATION OF RESULTS

SUMMARY OF STAFF CONCLUSIONS

- UNCERTAINTY ABOUT GROUNDWATER COMPOSITION AND THE GEOCHEMICAL ENVIRONMENT
- UNCERTAINTY IN CALCULATIONS OF RETARDATION
- UNCERTAINTY IN EXPERIMENTAL CONFIRMATION

REPOSITORY DESIGN

BY

JOHN T. GREEVES

PRINCIPAL DESIGN ISSUES IN THE SCA

(1) STABILITY OF REPOSITORY OPENINGS

(2) PERFORMANCE OF BARRIERS (BACKFILL COMPONENT)

(3) SEALING OF SHAFTS AND BOREHOLES

(4) RETRIEVABILITY OF WASTE

REPOSITORY DESIGN
(CHAPTER 6 DSCA)

- O CHAPTER CONCERNED WITH:
  - EVALUATION OF THE DOE PROGRAM IN TERMS OF EPA AND NRC REQUIREMENTS
    - (1) CONTROL ADVERSE SITE CHARACTERIZATION EFFECTS
    - (2) LIMIT OF RELEASES FROM ENGINEERED SYSTEM
    - (3) SEAL SHAFTS AND BOREHOLES
    - (4) PRESERVE RETRIEVAL OPTION
- O FOCUS ON GEOENGINEERING ASPECTS OF THE REPOSITORY DESIGN

DESCRIPTION OF THE CONCEPTUAL DESIGN

- TWO CANDIDATE HORIZONS (UMTANUM, MIDDLE SENTINEL BLUFFS)
- FIVE VERTICAL SHAFTS
- BOW-TIE ARRANGEMENT OF SHAFT PILLAR LAYOUT
- HORIZONTAL WASTE EMPLACEMENT
- SHAPE, SIZE, AND PITCH BASED ON 2:1 STRESS RATIO
- CRUSHED BASALT-BENTONITE BACKFILL

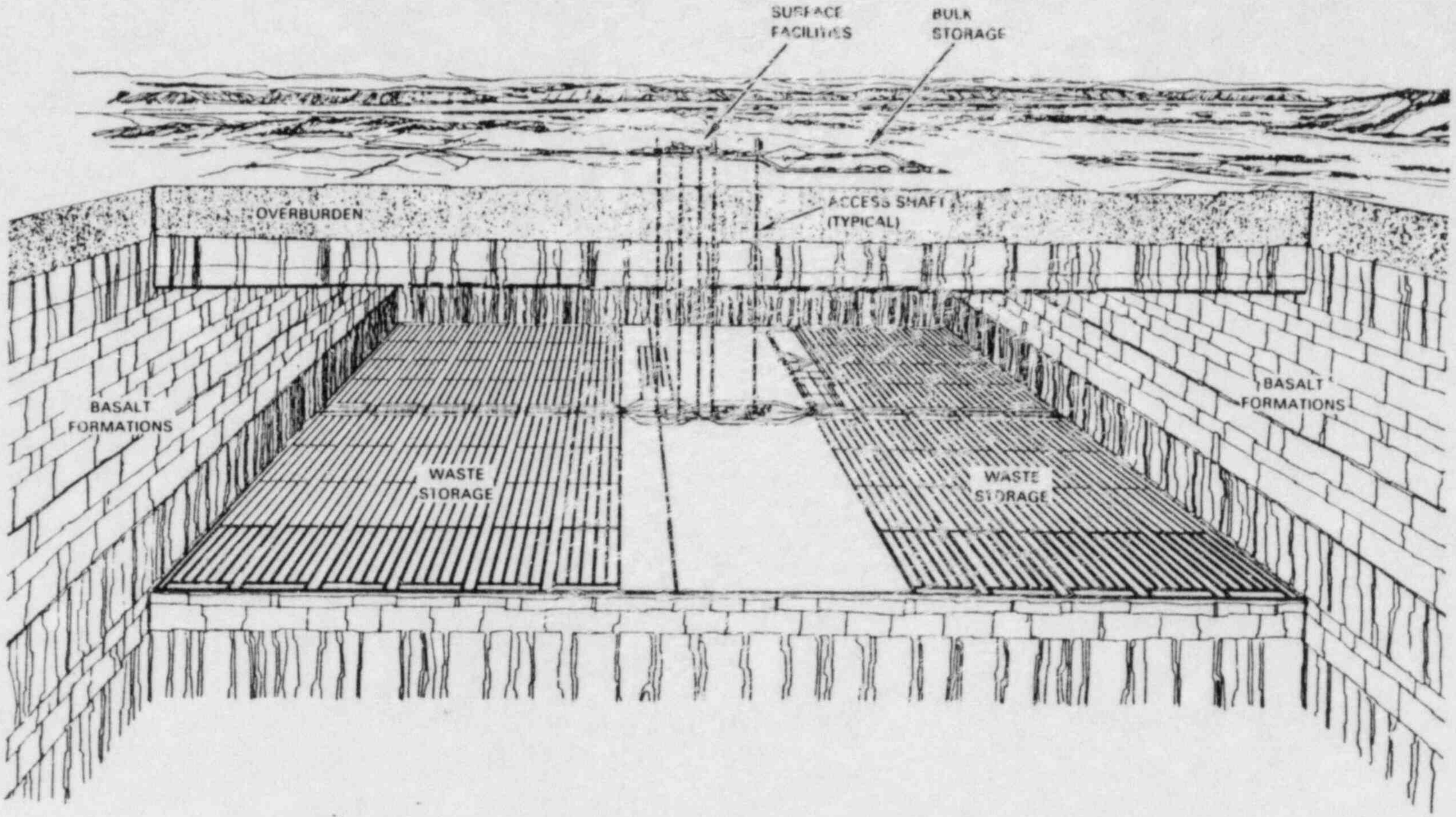


Figure A-14 Repository cutaway (Source: SCR)

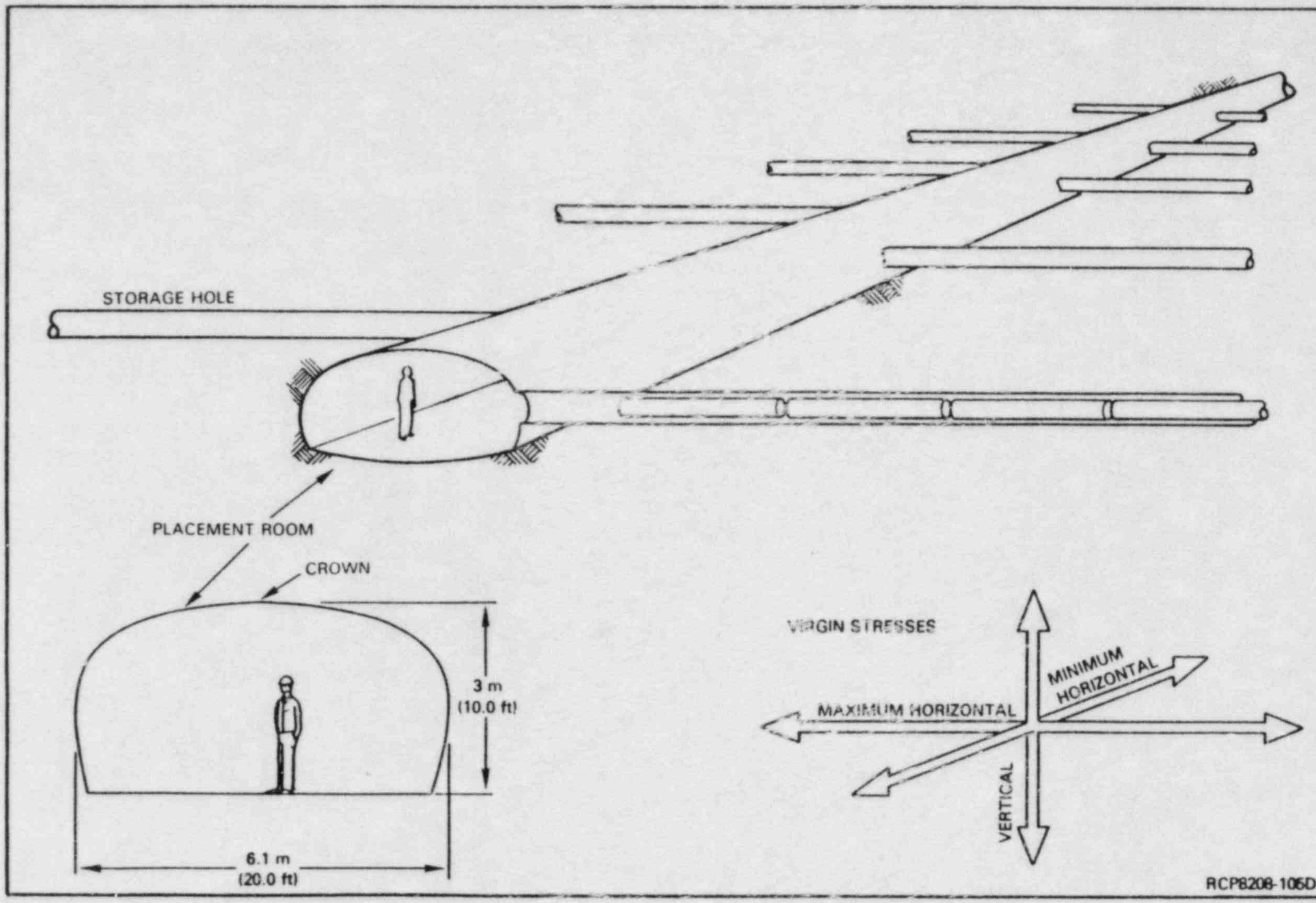


Figure A-18 Orientation of rock stresses and excavations
(Source: SCR)

ANALYSIS OF THE ISSUES

(1) STABILITY OF OPENINGS

- GEOLOGIC VARIABILITY NOT ADEQUATELY CONSIDERED
- REALISTIC STABILITY ANALYSES NOT PRESENTED
- DETAILS OF REFERENCED ANALYSES ARE NEEDED
- PRIORITIZATION OF KEY PARAMETERS IS NOT IDENTIFIED

ANALYSES OF THE ISSUES

(2) ENGINEERED BARRIERS

- COMMITMENT TO HORIZONTAL EMPLACEMENT APPEARS TO BE PREMATURE
- POTENTIAL ADVANTAGES OF BACKFILL NOT CONSIDERED ADEQUATELY
- PLACEMENT OF BACKFILL NOT GIVEN SUFFICIENT ATTENTION

ANALYSES OF THE ISSUES

(3) SEALING

- DETAILS ON CONSTRUCTION AND QA PROCEDURES ARE LACKING
- MODELING STUDIES APPEAR TO BE OVERSTRESSED (NOT ENOUGH EMPHASIS ON TESTING)
- LABORATORY AND FIELD TESTING ARE STARTING LATE
- LONGEVITY AND LONG-TERM STABILITY OF SEALS ARE NOT GIVEN DETAILED CONSIDERATION

ANALYSES OF THE ISSUES

(4) RETRIEVABILITY

- CONSTRUCTIBILITY OF EMPLACEMENT HOLES NEEDS TO BE ASSESSED
- TIMELY DEMONSTRATION OF RETRIEVABILITY IS NEEDED
- WORK ELEMENT PRIORITIES NEED TO BE UPGRADED

EVALUATION OF IN SITU TEST PLAN

- BASIS FOR ARRIVING AT THE PLAN NOT PRESENTED
- DETAILS OF IMPORTANT TESTS ARE LACKING
- TIMING AND PRIORITIES ARE NOT CLEAR
- FULL-SCALE ROOM EXCAVATION IS NEEDED
- LARGE-SCALE THERMAL-HYDROLOGICAL TESTING NEEDS TO BE CONSIDERED

RECOMMENDATIONS

(1) COMPLETE SENSITIVITY STUDIES TO IDENTIFY RELATIVE IMPORTANCE OF GEOENGINEERING DESIGN PARAMETERS

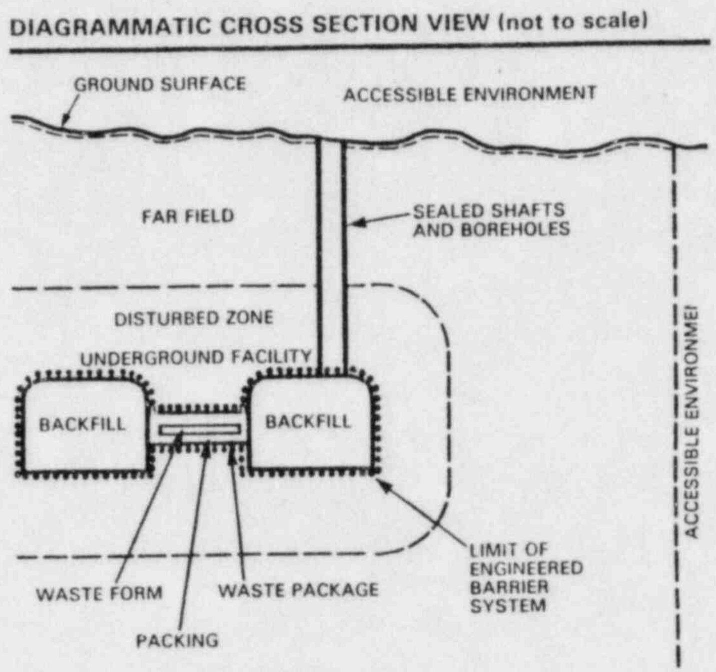
(2) PROVIDE DETAILS REGARDING IN SITU TESTS AND TEST PLANS

(3) ANALYZE ALTERNATE EMPLACEMENT CONFIGURATIONS

(4) INTEGRATE LABORATORY AND FIELD TESTING AT AN EARLY TIME IN SEALING PROGRAM

(5) PROVIDE DETAILS ON CONSTRUCTION AND QUALITY ASSURANCE FOR EXPLORATORY SHAFT

(6) INCREASE PRIORITY OF RETRIEVAL WORK ELEMENTS AND PLAN ON EARLY DEMONSTRATION OF HORIZONTAL RETRIEVAL


DIAGRAMMATIC CROSS SECTION VIEW (not to scale)
GROUND SURFACE
ACCESSIBLE ENVIRONMENT
FAR FIELD
SEALED SHAFTS
AND BOREHOLES
DISTURBED ZONE
UNDERGROUND FACILITY
BACKFILL
BACKFILL
ACCESSIBLE ENVIRONME!
LIMIT OF
ENGINEERED
BARRIER
SYSTEM
WASTE FORM
WASTE PACKAGE
PACKING

WASTE FORM AND WASTE PACKAGE

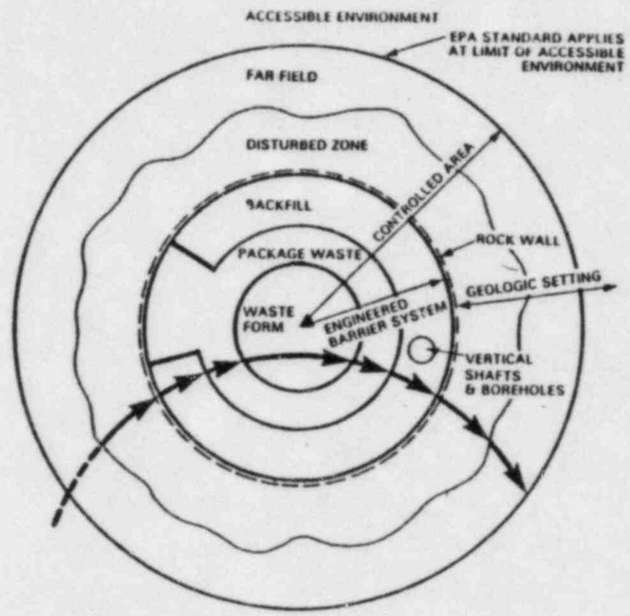
BY

F. ROBERT COOK

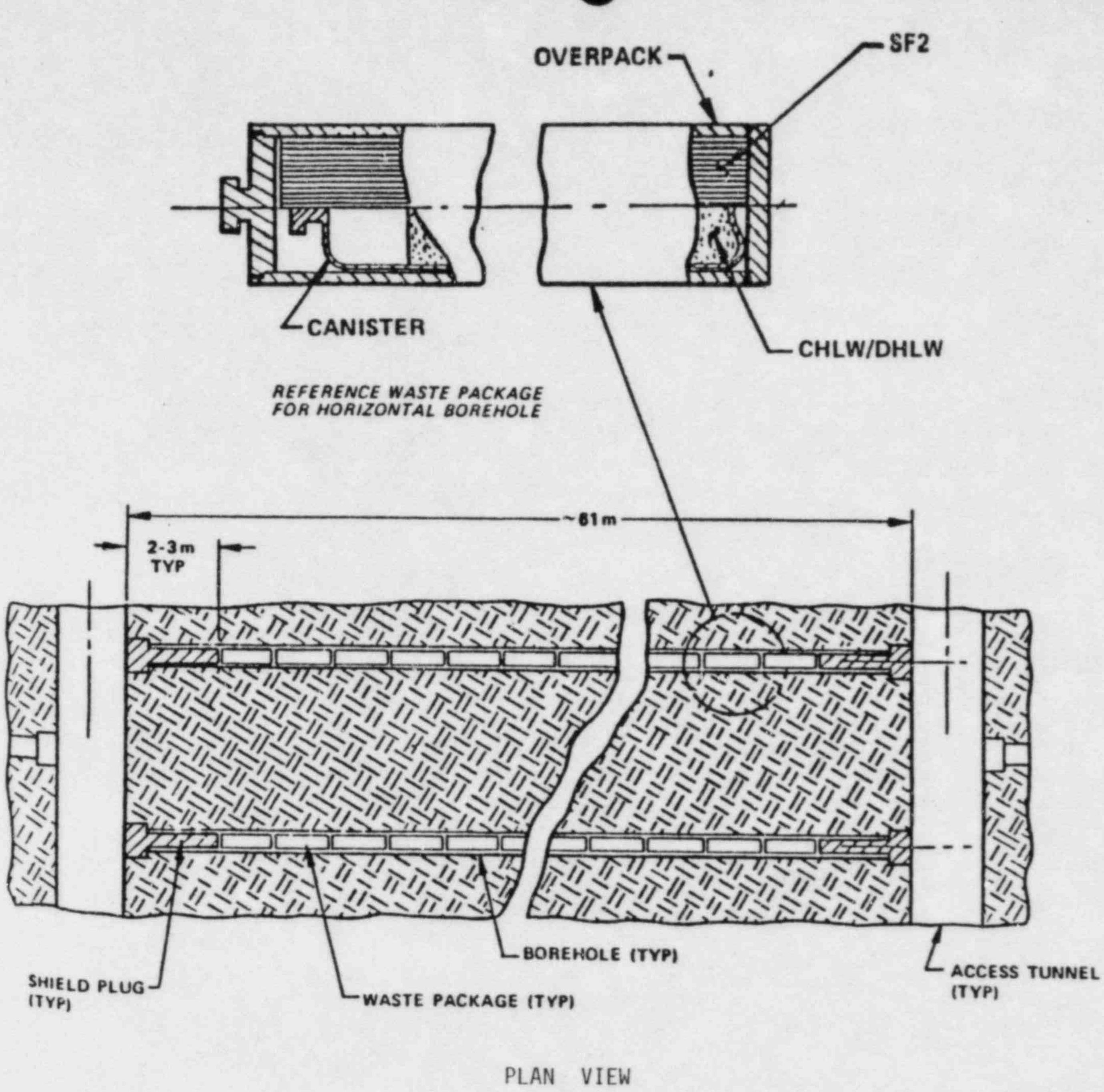
OUTLINE: WASTE PACKAGE ASSESSMENT

1. WASTE PACKAGE DESIGN
2. ANALYSES AND THEIR ROLE IN LICENSING AND SITE CHARACTERIZATION NEED
3. REASONS FOR RELIABILITY ANALYSES
4. WASTE PACKAGE FAILURE MODES, CONDITIONS AND PROCESSES IN THE WASTE PACKAGE
5. LIMITING FAILURE MODE; PITTING ANALYSES
6. THE FAILURE MECHANISMS IN THE PACKING
7. STAFF CONCLUSIONS ABOUT BWIP WASTE PACKAGE DESIGN

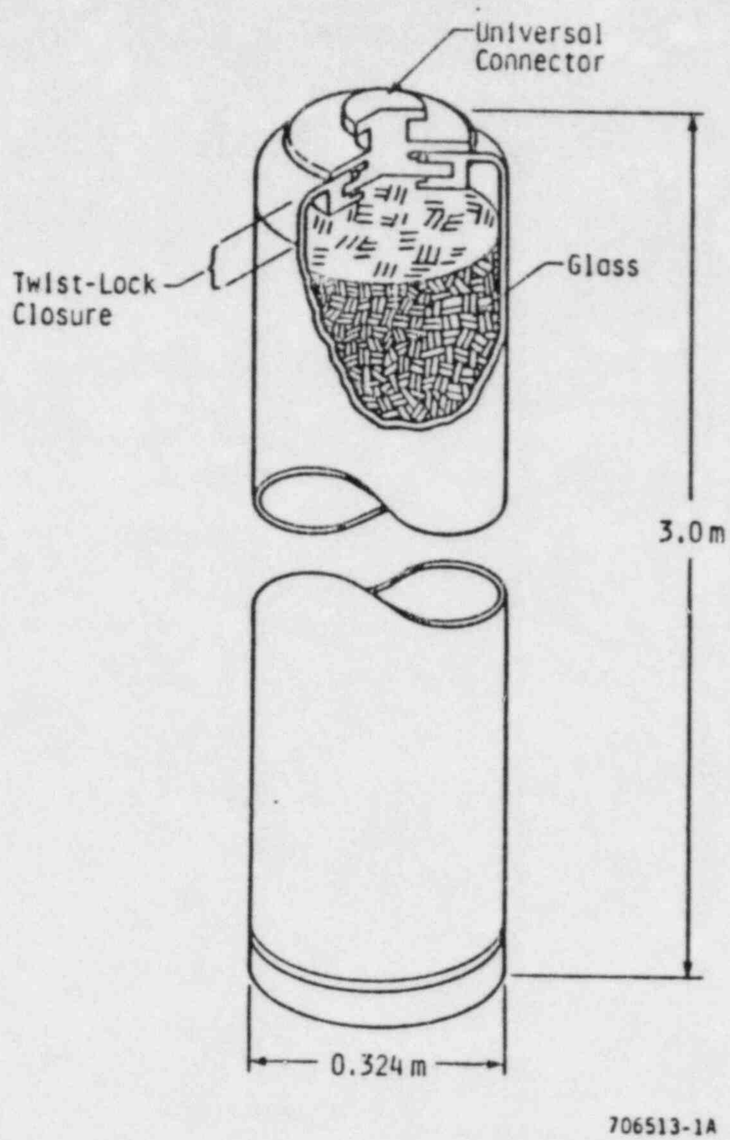
DIAGRAMMATIC PLAN VIEW (NOT TO SCALE)



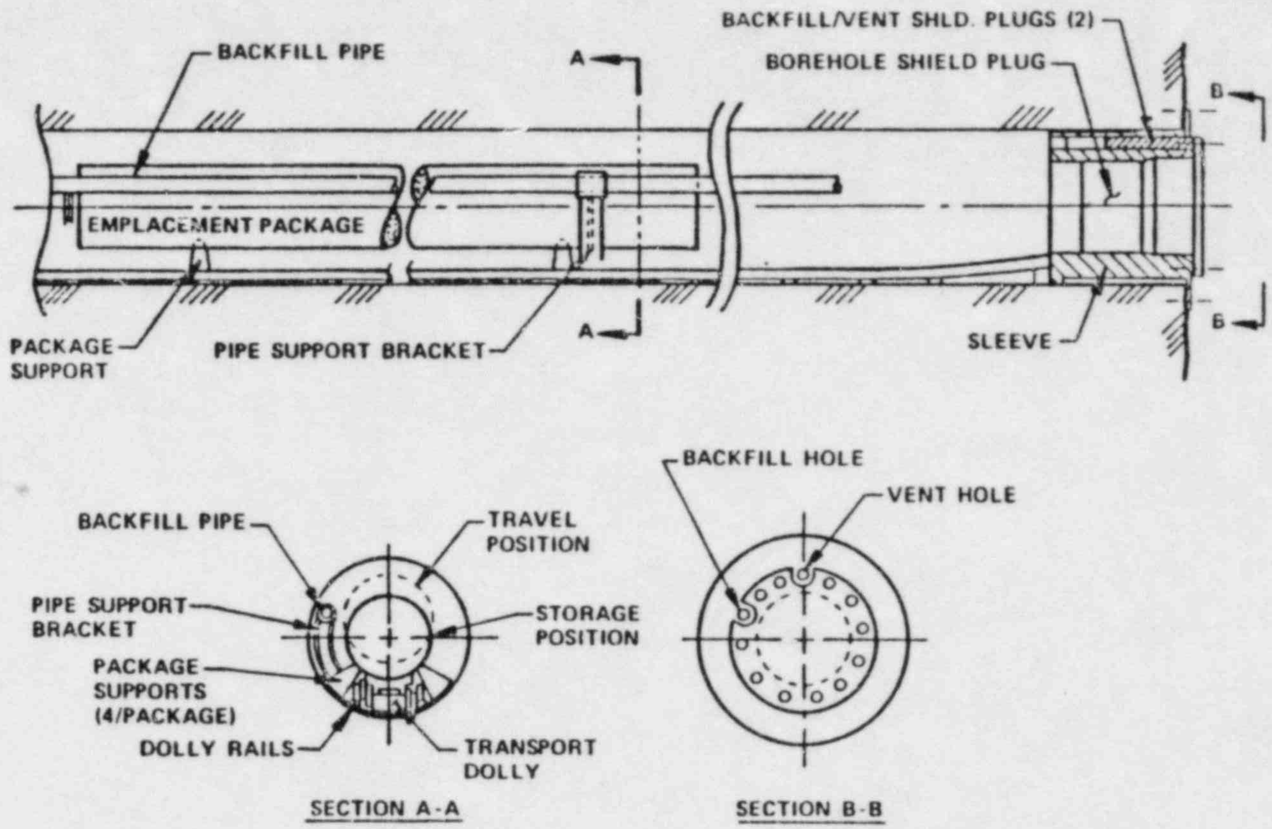
REPOSITORY SYSTEM ELEMENTS AND PERFORMANCE ISSUES RELATED TO LONG-TERM PERFORMANCE AFTER PERMANENT CLOSURE



Reference Borehole Type Package and Emplacement Scheme for Horizontal Emplacement



Reference Commercial High Level Waste Form



Reference Waste Package Conceptual Emplacement Features

FAULT TREE/EVENT TREE CONSTRUCTION TO DEPICT FAILURES OF WASTE PACKAGE AND TRANSPORT OF RADIONUCLIDES FROM THE WASTE PACKAGE TO REPOSITORY FACILITY AND HOST ROCK

DEVELOP A SEQUENCE OF EVENTS GOVERNED BY PROCESSES AND CONDITIONS LEADING TO OCCURRENCE OF THE FOLLOWING KEY EVENTS FOR ANY WASTE PACKAGE:

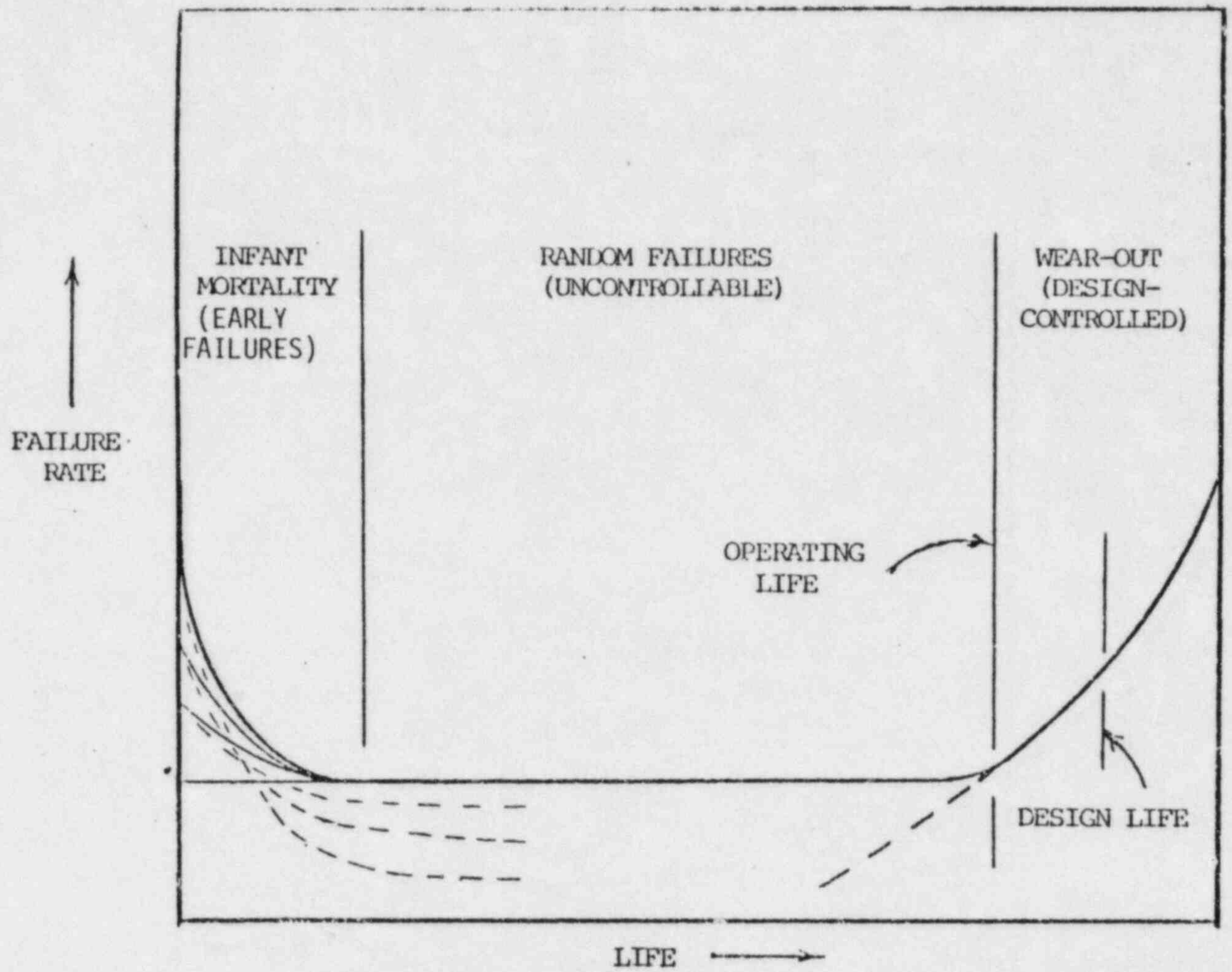
A. FAILURE OF A WASTE PACKAGE AT ANY TIME FROM TIME OF EMPLACEMENT TO 10,000 YEARS AFTER EMPLACEMENT. ANY RELEASE OF RADIONUCLIDES OUT OF A WASTE PACKAGE IS CONSIDERED A WASTE PACKAGE FAILURE.

B. RELEASE RATE FROM A WASTE PACKAGE AT TIME T OF ANY GIVEN ITH RADIONUCLIDE EXCEEDS RI. THE RADIONUCLIDES AND RELEASE RATES (RI'S) WOULD BE SELECTED BY THE ANALYST DEPENDING UPON HIS ANALYTICAL OBJECTIVES (OR UPON LICENSING REQUIREMENTS).

THE CONDITIONS AND PROCESSES RELATED TO OCCURRENCE OF A AND B ABOVE WILL BE SPECIFIED AS FOLLOWS:

A. <u>CONDITIONS</u>: RANGES AND STATISTICAL DISTRIBUTION OF THE CONDITIONS FOR THE SITE SPECIFIC GEOLOGIC (BASALT) REPOSITORY.

B. <u>PROCESSES</u>: CONSTITUTIVE EQUATIONS RELATING PROCESSES TO PARAMETERS WHICH AFFECT THE PROCESSES. THESE PARAMETERS INCLUDE MATERIAL PROPERTIES, SITE SPECIFIC CONDITIONS, GEOMETRY AND TIME.



Conceptual Failure Rate Curve.

I. CANISTER FAILURE MODES

A. THERMAL/MECHANICAL FAILURE MODES

1. FORCE AND/OR TEMPERATURE-INDUCED ELASTIC DEFORMATION
2. YIELDING
3. DUCTILE RUPTURE
4. BRITTLE FRACTURE
5. FATIGUE
   A. HIGH-CYCLE FATIGUE
   B. LOW-CYCLE FATIGUE
   C. THERMAL FATIGUE
   D. CORROSION FATIGUE
6. CREEP
7. THERMAL RELAXATION
8. STRESS RUPTURE
9. SPALLING
10. BUCKLING
11. CREEP BUCKLING

B. CHEMICAL DEGRADATION MODES

1. CORROSION
   A. DIRECT CHEMICAL ATTACK
   B. ELECTRO-CHEMICAL ATTACK
   C. CREVICE CORROSION
   D. PITTING CORROSION
   E. INTERGRANULAR CORROSION
   F. SELECTIVE LEACHING
   G. EROSION CORROSION
   H. STRESS CORROSION
2. HYDRIDING AND HYDROGEN EMBRITTLEMENT
3. COMPOSITION CHANGES INCLUDING RADIATION INDUCED CHANGES

C. BIOLOGICALLY-INDUCED CORROSION

II. WASTE FORM FAILURE MODES

A. MATRIX DISSOLUTION

B. HYDRATION

C. DEVITRIFICATION

D. RADIATION ENHANCEMENT OF LEACHING

E. MECHANICAL FRACTURE

III. PACKING FAILURE MODES/PERFORMANCE CHARACTERISTICS

A. MINERALOGICAL CHANGES

B. CRACKING

C. WASHOUT AND LOSS OF LOAD BEARING PROPERTIES THROUGH MASS FLOW

D. LOSS OF SWELLABILITY

E. LOSS OF IMPERMEABILITY THROUGH HYDRATION/DEHYDRATION

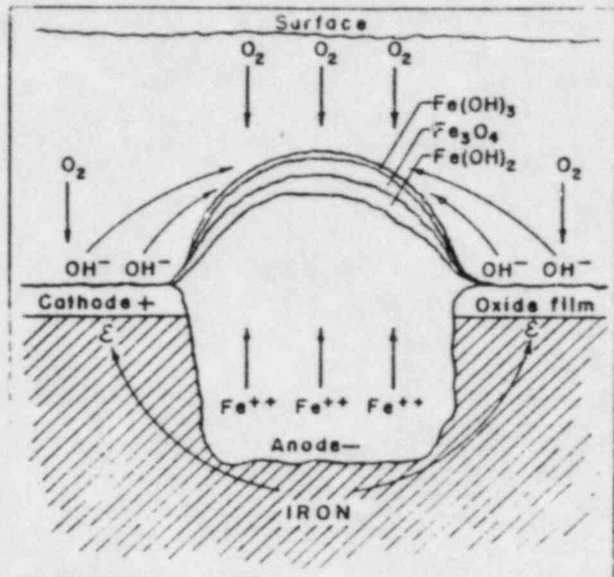
F. RADIONUCLIDE TRANSPORT MECHANISM

CONDITIONS

1. GROUNDWATER FLOW (WATER RESIDENCE TIME)
2. TEMPERATURE
3. HYDROTHERMAL CONDITIONS AT THE SURFACES OF THE WASTE FORM AND CONTAINER AND WITHIN PACKING MATERIALS
4. MECHANICAL LOADS ON CONTAINERS AND PACKING
5. GROUNDWATER CHEMISTRY (INCLUDING EH, PH AND O_2) IN THE VICINITY OF THE CONTAINER AND THE PACKAGING
6. RADIATION FIELD

PROCESSES

1. MECHANISMS BY WHICH WATER PENETRATE PACKING MATERIALS AND CONTAINERS
2. RADIOLYTIC GENERATION OF HYDROGEN, OXYGEN AND OTHER SPECIES DUE TO GAMMA RADIATION IN THE VICINITY OF THE CONTAINER
3. BIOCHEMICAL PROCESSES DUE TO PRESENCE OF MICROBES
4. GROUNDWATER FLOW
5. DEGRADATION OF WASTE FORMS (BOROSILICATE GLASS AND SPENT FUEL)



DEVELOPMENT OF A PIT IN A FERROUS MATERIAL.

SCOPE OF THE NATIONAL BUREAU OF STANDARDS CORROSION TESTS

MATERIAL	NUMBER OF VARIETIES	NUMBER OF SPECIMENS BURIED
FERROUS:		
PLAIN AND LOW-ALLOY WROUGHT	40	8,662
HIGH-ALLOY WROUGHT	12	1,391
PLAIN AND LOW-ALLOY CASE	18	3,539
HIGH-ALLOY CASE	2	668

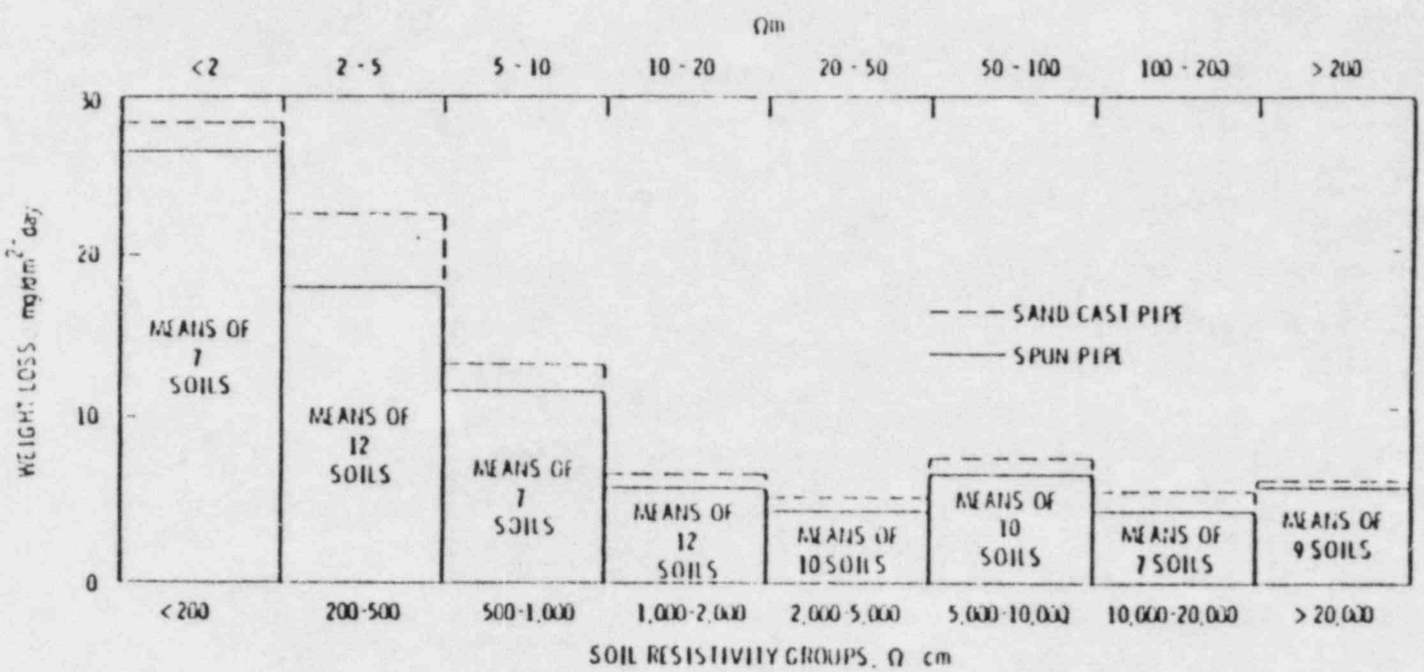
MEAN VALUES OF CONSTANTS A, K, AND N AND THEIR STANDARD ERRORS

IN THE EQUATION $P=KT^N$

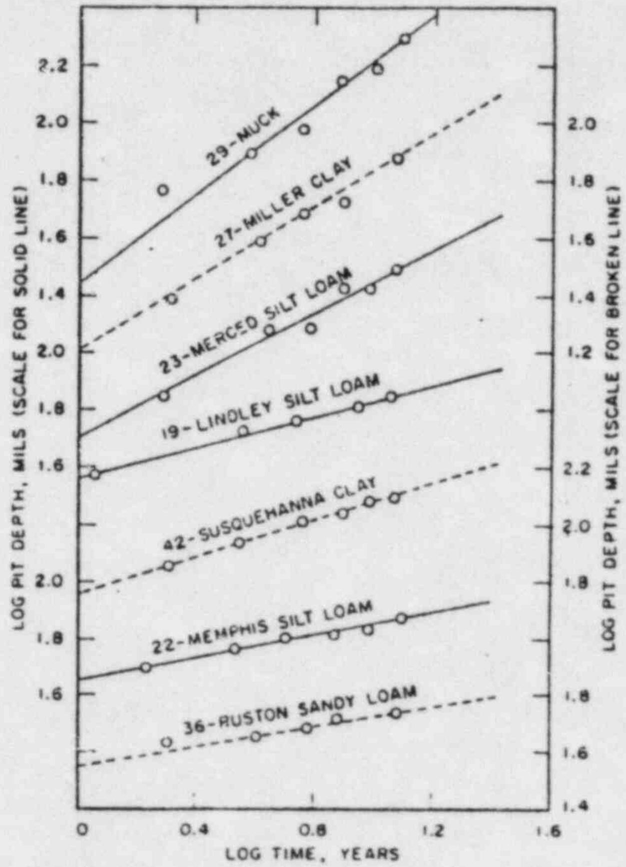
SOIL NO.	SOIL TYPE	NUMBER OF REMOVALS	MILS ON 0.4 FT^2 AT 5.3 YR	$\sigma_{K_{5.3}}$	N	σ_N
1	ALLIS SILT LOAM	6	58.5	2.7	0.49	0.06
6	EVERETT GRAVELLY SANDY LOAM	5	21.7	1.1	0	.05
11	HAGERSTOWN LOAM	6	63.2	2.4	0.5	.06
23	MEROED SILT LOAM	*5	107.3	2.9	.51	.04
28	MONTEZUMA CLAY ADOBE	*4	86.0	13.2	.92	.22
31	NORFOLK FINE SAND	5	40.4	2.0	**-.13(C)	.08
38	SASSAFRAS GRAVELLY SANDY LOAM	5	27.5	0.4	.23	.02
45	UNIDENTIFIED ALKALI SOIL	6	54.3	8.6	.78	.16
47	UNIDENTIFIED SILT LOAM	5	20.1	1.2	.32	.08

*IN THESE CASES, BECAUSE THE PIPE WAS PENETRATED, THE PIPE WALL THICKNESS WAS USED IN CALCULATING K AND N SO THAT THE VALUE OF N AS GIVEN IS SLIGHTLY LESS THAN THE CORRECT VALUE.

**SINCE A NEGATIVE SLOPE ON A LEG PIT DEPTH-LOG TIME CURVE HAS NO PHYSICAL SIGNIFICANCE, THE VALUE FOR N IN PARENTHESIS IS PREFERRED.



Corrosion of gray iron in soils of differing resistivity



RELATION OF SLOPES OF PIT-DEPTH-TIME CURVES FOR FERROUS METALS TO AERATION OF SOIL [106].

SOIL	AERATION	SOIL	AREATION
29	VERY POOR - - -	42	FAIR
27	POOR- - - - - -	22	GOOD
19	FAIR- - - - - -	36	VERY GOOD

VARIATION OF MAXIMUM PIT DEPTH AND PITTING FACTOR
AFTER SIMILAR PERIODS OF EXPOSURE

IDENTI-FICATION (FIG. 14)	SOIL NO.	SOIL TYPE	LOSS IN WEIGHT (OZ/FT^2)	AVERAGE PENETRATION (MILS)	DEPTH OF MAXIMUM PENETRATION (MILS)	PITTING FACTOR
1	47	UNIDENTIFIED SILT LOAM - - - - - - - - -	2.1	3.2	3	1
2	27	MILLER CLAY- - - - -	3.7	5.7	36	6.3
3	20	MAHONING SILT LOAM -	3.0	4.6	34	7.4
4	16	KALMIA FINE SANDY LOAM - - - - - - - - -	4.2	6.5	60	9.3
5	3	CECIL CLAY LOAM- - -	3.4	5.3	63	11.8
6	14	HEMPSTEAD SILT LOAM-	2.9	4.4	107	24.5

CONFIDENCE AND PROBABILITY LEVELS FOR OPEN HEARTH STEEL

TIME (YEARS)	CONFIDENCE/PROBABILITY			
	95/90 (MILS)	95/99 (MILS)	99/90 (MILS)	99/99 (MILS)
13	201	225	229	253
1,000	3,712	4,009	5,211	5,509
10,000	18,297	19,416	30,996	32,116

CONCLUSIONS:

1. THERE IS A NEED FOR THE BWIP TO IDENTIFY THE ANALYTIC METHODOLOGY FOR EVALUATING WASTE PACKAGE PERFORMANCE. THE STAFF CONSIDERS THAT SUCH A METHODOLOGY SHOULD CONSIST OF A RELIABILITY ANALYSIS TO PROVIDE A COMPREHENSIVE AND QUANTITATIVE EVALUATION OF UNCERTAINTIES.

2. IN ORDER TO GUIDE TESTING PROGRAMS RELIABILITY DESIGN INTERIM REQUIREMENTS SHOULD BE ESTABLISHED FOR WASTE PACKAGE PERFORMANCE. THESE REQUIREMENTS SHOULD REFLECT THE INTENDED OVERALL SYSTEM RELIABILITY AND THE RELATIVE RELIABILITY OF OTHER COMPONENTS IN THE SYSTEM.

3. THE POTENTIAL ENVIRONMENTAL CONDITIONS IN AND AROUND THE WASTE PACKAGE WILL BE THE MAJOR FACTORS INFLUENCING WASTE PACKAGE RELIABILITY. HENCE, KNOWLEDGE OF THE STATISTICAL DISTRIBUTION OF PERTINENT CONDITIONS WITH TIME AND POSITIONS IN THE REPOSITORY IS IMPORTANT.

4. THE WASTE PACKAGE DESIGN PROPOSED BY THE BWIP APPEARS INADEQUATE SINCE IT IS UNLIKELY THAT IT CAN BE SHOWN TO PROVIDE AN ADEQUATELY RELIABLE CONTAINER (CONSIDERING PITTING) TO FULFILL CONTAINMENT CRITERIA IN 10 CFR PART 60.

PERFORMANCE ASSESSMENT

BY

MALCOLM R. KNAPP

PERFORMANCE ASSESSMENT

(DSCA CHAPTER 9)

- RELATION TO 10 CFR PART 60
- NRC PERSPECTIVE ON PERFORMANCE ASSESSMENT
- ISSUES IDENTIFIED BY DOE
- NRC RECOMMENDATIONS

PERFORMANCE ASSESSMENT - RELATION TO 10 CFR PART 60:

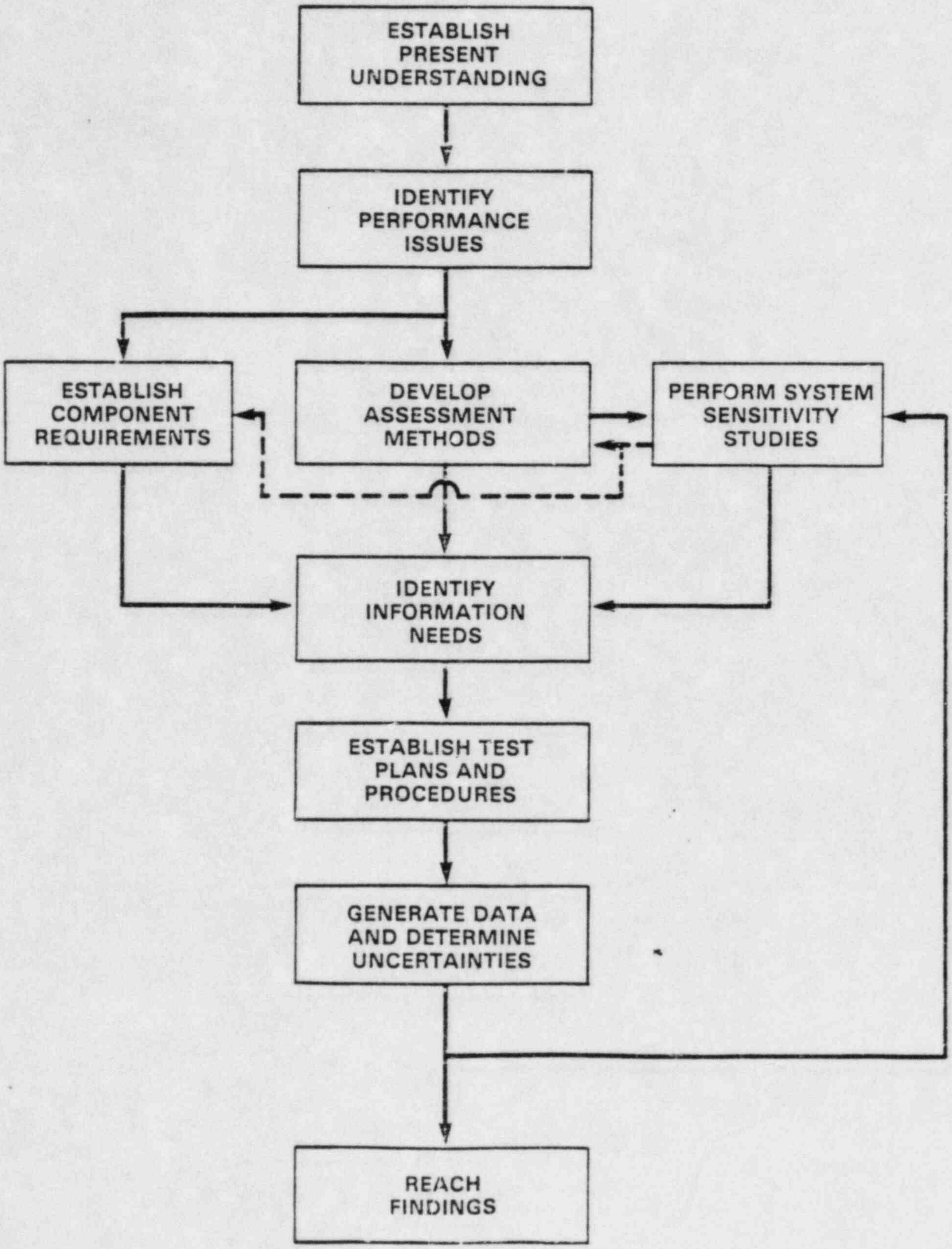
DURING OPERATIONS -

- EXPOSURES AND RELEASES
- RETRIEVAL OPTION

AFTER CLOSURE -

- COMPLIANCE WITH EPA STANDARD
- MINIMUM WASTE PACKAGE CONTAINMENT TIME
- MAXIMUM RADIONUCLIDE RELEASE RATE FROM ENGINEERED BARRIER SYSTEM
- MINIMUM PRE-EMPLACEMENT GROUNDWATER TRAVEL TIME

Performance Assessment - NRC Perspective



PERFORMANCE ASSESSMENT - ISSUES IDENTIFED BY DOE

- ARE PRE-EMPLACEMENT GROUNDWATER TRAVEL TIMES IN COMPLIANCE WITH NRC CRITERIA?
- WHAT IS THE MAXIMUM RELEASE RATE FROM THE ENGINEERED BARRIER SYSTEM?
- ARE POTENTIAL RADIONUCLIDE RELEASES TO THE ACCESSIBLE ENVIRONMENT IN COMPLIANCE WITH EPA STANDARDS?

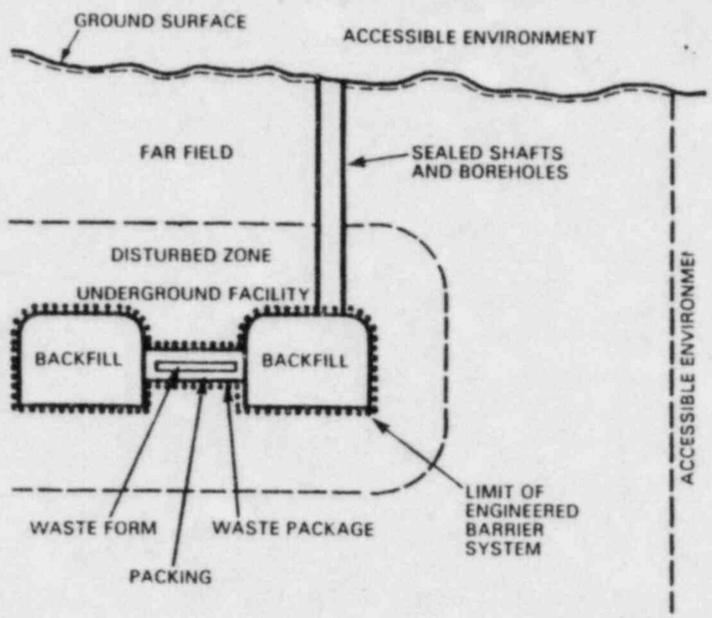
PERFORMANCE ASSESSMENT - RECOMMENDATIONS

1. A CLEAR PERFORMANCE ASSESSMENT FRAMEWORK NEEDS TO BE ADEQUATELY DESCRIBED. THIS FRAMEWORK SHOULD ADDRESS THE ITERATIVE PROCESS BETWEEN MODELING AND SITE CHARACTERIZATION.

2. RISK ANALYSES SHOULD BE PERFORMED TO IDENTIFY SYSTEMS, STRUCTURES, AND COMPONENTS WHICH ARE IMPORTANT TO SAFETY PRIOR TO CLOSURE OF THE UNDERGROUND FACILITY.

3. TERMS CONCERNING PERFORMANCE ASSESSMENT NEED TO BE DEFINED AND REVIEWED WITH NRC.

DIAGRAMMATIC CROSS SECTION VIEW (not to scale)



RECOMMENDATIONS, CONTINUED

4. AT THIS STAGE OF SITE CHARACTERIZATION, PERFORMANCE ASSESSMENT SHOULD BE USED FOR GUIDANCE, RATHER THAN TO DEVELOP ASSERTIONS ABOUT REPOSITORY SYSTEM PERFORMANCE.

5. ALL APPLIED COMPUTER RESULTS MUST BE DOCUMENTED WELL ENOUGH TO PERMIT INDEPENDENT EVALUATION.

6. PLANS FOR CODE EVALUATION AND DOCUMENTATION SHOULD BE DESCRIBED IN MORE DETAIL.