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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

SUBCOMMITTEES ON
WASTE MANAGEMENT
AND
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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

SUBCOMMITTEES ON

WASTE MANAGEMENT AND METAL COMPONENTS

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

Friday, October 25, 1985

The meeting of the subcommittees reconvened at 8:30 a.m., DR. Paul G. Shewmon presiding.

PRESENT:

DR. DADE W. MOELLER, ACRS Member

DR. PAUL G. SHEWMON, ACRS Member

DR. CARSON MARK, ACRS Member

MR. HAROLD ETHERINGTON, ACRS Member

PUBLIC NOTICE BY THE

UNITED STATES NUCLEAR REGULATORY COMMISSIONERS'

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

FRIDAY, OCTOBER 25, 1985

The contents of this stenographic transcript of the proceedings of the United States Nuclear Regulatory Commission's Advisory Committee on Reactor Safeguards (ACRS), as reported herein, is an uncorrected record of the discussions recorded at the meeting held on the above date.

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P R O C E E D I N G S

DR. SHEWMON: Good morning. This is the second day of the meeting. So I don't have to tell you to speak loudly and intc the microphone so the recorder can hear you.

This morning, we're reviewing primarily the research and technical assistance programs having to do with marterials. Basically, this means the waste package, I believe, but we'll learn more about that today.

I have no other opening remarks, unless somebody else has something they wish to bring up or question, I'll call on Frank Costanzi.

MR. COSTANZI: Thank you, Dr. Shewmon.

Let me introduce myself. I'm Frank Costanzi, the Chief of the Waste Management Branch, Division of Radiation Programs and Earth Sciences, Office of Nuclear Regulatory Research

The Commission's Waste Management Research Program is essentially managed out of the Waste Management Branch, although there are a number of very significant portions of the program being administered by the Earth Sciences Branch in the same division. One of those programs will be discussed this morning.

That's the program on Rock Mass Ceiling.

I would like to begin with just kind of a

statement of objectives.

(Slide.)

I'm going to tell you where we are and where we're trying to go with our waste package program.

Our waste package program is, of course, one part of our waste management research program. We're going to hear about high level waste this morning.

There are other portions of the program that deal with hydrology and geochemistry of waste management, both high level and low level and another entire section of the program that deals with questions of performance.

We will talk about those this morning only as voiced concerns which are addressed in those programs which cover or affect the kinds of experiments and test considerations that one gets in a waste package.

That is to say, how they determine or influence the environment in which the waste package is supposed to work?

The objectives of our waste package research are four. The first is to identify likely failure mechanisms in the waste package for each of the repository environments, the kinds of repositories which DOE is looking at. We wish to have some idea of what is the likely method by which the steam will be lost. What is the likely controlling factor with regard to the release of radionuclides thereafter?

We looked at the waste package itself. We identified the environmental determinants of the waste package failure. That is to say, what are the conditions on the outside which influence the way waste packages are in the repository. We tried to identify the critical tests and assumptions that are needed to demonstrate the long-term performance of the waste package. That is what DOE needs to do to demonstrate that they abide by the Part 60 and the controlled release requirement.

Lastly, we performed selective experiments to test these assumptions, the assumptions being what goes into the demonstration.

(Slide.)

Our approach is circumscribed by the circumstances in which we are in. Mainly, that we need to track DOE decisions and not go off investigating materials and such to which DOE is paying no attention, so the materials that we looked at our the materials that DOE is leaning towards for the waste package in each of the repository environments which DOE is considering, which means, of course, that we also try and tailor our investigations to be realistic, in terms of what kinds of waste packages we're going to examine.

DR. SHEWMON: Do you think that waste package is pretty stable by now?

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MR. COSTANZI: It depends upon which media you are considering.

The basalt carbon steel seems to be the most likely candidate right now. The tuff site. DOE is giving serious consideration to stainless steel. There are certainly investigations of other materials. DOE has not made a decision yet, saying that this will be the waste package material, the overpack material for a particular site. So we're just following what they're investigating.

DR. SHEWMON: The salt?

MR. COSTANZI: I believe carbon steel is in salt, is what they're leaning toward right now. They had been, at one point, leading towards titanium, but some of our research indicates that there were some serious questions about titanium in salt.

MR. ETHERINGTON: I'm afraid I haven't followed the waste disposal problem at all. Materials is one step down the line from me. I would like to know something about what the package looks like, what the heat flux is from the outside and that kind of thing.

Is that possible?

MR. COSTANZI: I do not have that information at my disposal immediately.

DR. SHEWMON: There is a blackboard. Can somebody draw?

DAVbw

MR. ETHERINGTON: Just the dimensions would help.

DR. MC NEIL: I can draw a few things. I'm going entirely from memory. I see someone there who is perhaps more expert on the salt side than I am. I would be happy to have corrections from the audience.

As I understand it, you have a right circular cylinder of appropriate thickness, and this cylinder is at present generally considered to be cast, although there are good arguments for using rock products instead.

You have tops and probably bottoms simply welded on.

DR. SHEWMON: Mike, why don't you back up and say what's inside of the cylinder.

DR. MC NEIL: All right. You have either waste reprocessing gas, which is a silicate glass or else rods of spent fuel, which are first contained in what is called a canister, which is a right circular cylinder in stainless steel. It is simply designed to facilitate handling. It's not intended at present to represent a barrier to corrosion. This is simply a commercial grade of stainless steel, fairly thin gauge, welded up, so you can just make objects of uniform shape.

MR. ETHERINGTON: These would be about 18 feet high?

MR. COSTANZI: Something on that order, I think. Maybe 14 feet.

MR. ETHERINGTON: And the diameter?

DR. MC NEIL: The picture that I've seen, they're rather long and thin.

DR. MOELLER: Is Mr. Etherington thinking the entire rod is put in there? Is that why you said 18 feet? I assume it's all cut up.

DR. MC NEIL: Excuse me. Dr. Parry?

DR. PARRY: Generally speaking, it's planned that the fuel elements will be disassembled and that the individual fuel rods would be close-packed in a container that would be sealed separately. Now they're talking about doing that away from the repository.

DR. SHEWMON: Can I change your nomenclature for a minute and say that the subassembly will be disassembled, but the rods will not be destroyed or broken up in any way, if they can avoid it?

MR. PARRY: That's correct. They will take the cages off, though.

DR. MC NEIL: Good reason for not, at least in the tuff site, because we know more about the tuff sites than we know about the other sites. At least at the tuff site, there's good reason for this, because they are hoping to claim some credit for the actual zircalloy. I think it

is on the rods, in terms, not of the containment requirement but the controlled release requirement.

DR. SHEWMON: These fuel elements are usually 14 feet long, about?

MR. COSTANZI: I think so.

MR. PARRY: The overall length is about 14 feet. The active length of fuel material in the rods is on the order of 12 feet.

DR. SHEWMON: So the 14-foot plus ends, I think where Harold was coming from.

Okay. Onward.

DR. MC NEIL: In any event, this package here is now placed, for salt and basalt is now placed in the carbon steel overpack. The way in which the carbon steel overpack is fabricated is still somewhat in doubt. We will be discussing this later, in terms of our container manufacturing work.

It is welded shut. This is then placed in the depository. This so-called "open pack," which is of carbon steel thickness, varies considerably, depending upon whose answers you use.

I think DOE hasn't settled on a final answer.

MR. ETHERINGTON: These are loaded at the site, then?

DR. MC NEIL: That is my understanding.

MR. COSTANZI: It is not clear. There is some discussion about the overpacks, the entire waste package being assembled at the MRS and shipped to the repository.

DR. SHEWMON: What's an "MRS"?

MR. COSTANZI: I'm sorry. Monitor retrievable storage. This is the facility which the DOE now proposes to take spent fuel from power plants, consolidate the fuel elements and, presumably, at this point, actually assemble the waste package for assembly and disposal at the waste site.

Many of these plants, you have to understand, DOE is still exploring its options. They have not made decisions on design, to the point where know the exact dimensions of a waste package, the exact thermal loading, the exact materials, the exact manufacturing techniques.

We are tailoring our program to what we believe is the best information we have on what DOE is actively pursuing during the following months.

DR. SHEWMON: Pardon my asking. This has changed some, I know, in the last year. You asked about heat flux. Harold, maybe you won't get an answer on that today.

MR. ETHERINGTON: I think this is enough.

DR. MC NEIL: There's one interesting point I would like to, before we leave the thing, point out. The tuff site, the tuff plant is entirely different. The plan

for tuff was explained to us at a recent meeting at Lawrence Livermore. It is entirely different. They're primarily focusing on spent fuel solutions there, And what they intend to do in most cases is to use a single, very carefully prepared container, either of a special stainless steel chosen for its resistance to various forms of environmental degradation or perhaps a high nickel alloy.

The advantage of high nickel alloys being that they're much less vulnerable to transgranular chloride cracking.

In the case of the tuff site, it is not at all clear whether the DOE people are going with stainless steel or nickel alloys. So at present in the tuff site, unlike the salt and the basalt sites, we are having to consider two completely different alloys. We're in the salt and basalt sites at the moment, as of, I say, the last year and a half or so.

There were some Armco iron tested, some 1018, I think some 1005. But basically, they're going at the iron-rich end of the iron-phased diagram.

They're dealing with plain carbon steels, and barring some future convulsions out of the DOE side, the situation there seems to be settling down a bit, but in the tuff site, there is a fundamental question of container material selection that has not been resolved.

Can I offer you more?

MR. ETHERINGTON: That's fine.

MR. COSTANZI: I might remark again that the materials specification and the design are DOE's choice. DOE is charged under the Energy Reorganization Act to develop the repository and deposal for waste. The NRC is charged with licensing that disposal. Consequently, it's a DOE responsibility to demonstrate that it has safely disposed of the waste.

It is not NRC's responsibility to make DOE's case. So we don't design the repository. We do not select the materials. We do not design the waste package.

What we do need to do is know what the properties of the materials and the designs which DOE chooses are. Not only do we need to know how the waste package, in fact, how the whole repository works. We also need to understand what constitutes a demonstration that it works. That is to say, we not only need to know what the properties of the materials are -- carbon steel, stainless steel or nickel alloys. In a repository environment, we need to know what kind of testing program is going to demonstrate that the waste package in that environment is going to perform as is required by DOE in containment for a period 300 to 1000 years, and controlled release of radionuclides thereafter.

(Slide.)

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Given that, we talk about the four areas that we're going to discuss today -- waste form, overpack, packing backfill materials and seals, and then the waste package environment, primarily conditions of the groundwater, which will be attacking the waste package and the wastes themselves.

The materials we're looking at, in terms of the waste form are HLW glass and spent fuel. The assumptions we're making is that idle waste glass, the boron silicate glass of composition which has been prepared, are the materials characterization center at PNL. The spent reactor fuels are PWR and BWR fuel. Overpack materials. What we're focusing on.

DR. SHEWMON: Stop there for a minute.

Though the system for years has talked about putting reprocessed fuel in here, you will be limited to civilian wastes and no civilian fuel has been reproduced.

So is it true that for the foreseeable fuel, we'll be dealing only with spent reactor fuel?

MR. COSTANZI: Under the current DOE plan, as provided in the Nuclear Waste Policy Act, it limits the amount of defense high level waste, which is glass that can go into the repository. So the bulk of the first load in the repository will be spent fuel. I think it's something like 70 percent or more.

DR. SHEWMON: I thought it was excluded. It's just limited.

MR. COSTANZI: It's just limited.

DR. SHEWMON: Thank you.

DR. MOELLER: Where does the West Valley material fall?

MR. COSTANZI: I believe the West Valley material is commercial material, and it will be vitrified according to current plans. That I believe would fall under the category of commercial wastes.

DR. SHEWMON: Is it clear where it will be vitrified?

MR. COSTANZI: I believe it's in West Valley.

DR. CARTER: In West Valley. They are building a plant.

MR. COSTANZI: That's the current plans.

In overpack materials, low carbon steel, stainless steel and high purity iron.

DR. STEINDLER: I think part of the DOE program is also looking at copper. At least at two sites, and there is a nontrivial amount of dollars that I think are going to be spent on copper.

Have you excluded copper for some reason of your own?

MR. COSTANZI: I don't believe that DOE has

indicated to us that at the salt, basalt or tuff sites, the three mediums DOE is presently looking at, that they are currently, are seriously considering copper.

I think it would be premature for us to look at that right now.

DR. SHEWMON: He just completed chairing a committee which reviewed the research which DOE is doing in this area. So it may be they haven't communicated with you, but in reflection of what they're spending, I suspect he knows whereof he speaks.

MR. COSTANZI: It is sometimes difficult to understand which way to go, because we do get a good deal of mixed signals from DOE.

DR. STEINDLER: I am sorry to hear that, but I think it might be well to find out what DOE might be doing in copper, not only in the basalt program but tuff and basalt.

MR. ETHERINGTON: What is meant by high purity iron?

MR. COSTANZI: Just that, iron which has very few impurities.

MR. ETHERINGTON: Very low carbon steel, is that right? An open hearth product?

MR. COSTANZI: I am not sure. An open hearth product?

DR. MC NEIL: No, this is not the old ingot iron. It is vacuum arc remelted. What you do is you take an ordinary steelmaking argon oxygen decontamination system and just run pure iron through it.

MR. COSTANZI: For the packing materials, the backfill materials, and the sealing materials, DOE has been investigating bentonite or a combination of bentonite and basalt and of course cement and various compositions of cement, bentonite, cement, basalt, for the sealing materials.

Lastly, of course, the waste package environment -- this has primarily to do with the groundwater, the chemistry of the groundwater, and its

interaction with the waste package, and with the basalt groundwaters and tuff groundwaters and the brines of basalt.

We will begin to start looking at the chemical compositions of the groundwaters in granite.

MR. ETHERINGTON: The environmental problems come from the outside, not from the inside?

MR. COSTANZI: I am not sure I understand the question.

MR. ETHERINGTON: What is the inside environment, inside the containment?

MR. COSTANZI: Inside the waste package itself? We have done some research looking at the inside of the waste package, essentially corrosion of the canister from the inside out, and we determined that that really isn't significant compared to what is coming from the outside of the waste package.

MR. ETHERINGTON: That is based on the environment being what inside -- just air, oxygen, or what?

MR. COSTANZI: In the case of high level waste glass it would just be a stainless steel can in which the glass has been solidified, and in the case of spent fuel it will be again a stainless steel can in which the consolidated fuel pins are placed. There may be an encapsulating matrix, there may not.

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MR. ETHERINGTON: And the spaces are just filled with air, is that correct?

MR. COSTANZI: They may be filled with some inert gas, argon or nitrogen. We don't expect there would be a tremendous amount of space, anyway.

DR. SHEWMON: With regard to the fuel, is the Nevada site -- well, there is a possibility one could take credit for the zircalloy clad.

Is there anything ongoing on cladding?

MR. COSTANZI: There are discussions for taking credit for the zircalloy cladding as an inhibitor to leaching; that is to say, it provides fewer sites for groundwater to get to the spent fuel.

I don't believe that there has been any serious discussion about taking credit for the cladding for containment, to meet the containment requirement, although that is certainly possible. That is a conceivable thing.

We are looking at the properties of the cladding, but presently our plans call for looking at the leachability of the spent fuel under different conditions of cladding integrity.

DR. SHEWMON: Okay. What about -- yesterday there was some talk about high pressures or stability. Do you assume this will collapse, for example? In the glass you could get a fairly tight fit, deformation. If you had a

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bunch of rods in there, attacking density would be appreciably lower.

Is the design of the can -- or do you call it the overpack -- usually such that it will withstand and maintain its shape under the ambient pressure, or do people assume that it will collapse and fill around?

MR. COSTANZI: As far I am aware, I am not aware that the design for the overpack is intended to take into account any stress from the environment.

DR. SHEWMON: The guys we had here were the same agency but a different branch. They talked about lithostatic pressures and hydrostatic pressures, and they calculate the hydrostatic pressure at 3000 feet. I got 1500 psi.

It may be a problem. On the other hand, if you indeed have 3000 feet of rock sitting on your head, that is a heck of a lot of pressure.

MR. COSTANZI: Yes.

DR. SHEWMON: And in basalt, that salt is rather plastic, as you know. So it seems to me there is at least a question as to whether in several thousand feet of salt you can get enough relaxation and build up enough pressure to buckle things or whether that is a "no, never mind."

It seems to me it is a question worth considering.

MR. COSTANZI: A distinction should be made between what we are following -- that is to say, current DOE designs -- and what sorts of things can happen to the waste package, which is another part of our program.

The question to which I have responded, are these waste packages for taking those kinds of pressures? To the best of my understanding, they are not. I don't believe that is a conscious part of the design.

Whether or not they will be subjected to those kinds of pressures and, if so, when in the lifetime of the repository is a separate question.

The question, for example, of the flow of basalt. Basalt is a plastic material. Whether that will exert pressures that the overpack will not be able to withstand, that is a question which DOE needs to address. They need to demonstrate that either the overpack is designed to take care of that or that that would happen.

DR. SHEWMON: And apparently you don't know what their thoughts are on that, and currently the effective plastic stream is not part of your program?

MR. COSTANZI: It is not part of our program.

Mike, do you know of any design?

DR. MC NEIL: I don't know of any conscious effort that has been made on this subject. I would point out the fact, as Dr. Etherington has pointed out, we have

considerable heat flux. In fact, the inside of the overpack is hotter than the outside of the overpack. It means that the overpack stands to have a tensile heat stress.

Quite frankly, I would think that a certain amount of hydrostatic pressure would not be a bad thing. Crushing a steel container several inches thick purely by hydrostatic compression would take some truly enormous pressures.

Think about submarines.

MR. ETHERINGTON: What about the collapse of the surrounding material? It might not be hydrostatic.

DR. MC NEIL: The real problem one has to look out for, as far as I can see in practical terms -- DOE ought to look out for -- is a roof collapse in a mine imposing a sudden severe buckling stress on these. But I believe this practice that most of the DOE people are talking about, or were talking about the last time they told us what they were up to, of basically placing these holes reduces the chance of a roof fall imposing such a sudden buckling.

MR. ETHERINGTON: Getting back to the heat generation for a moment, what temperature are we assuming?

DR. MC NEIL: We are assuming surface temperatures -- depending upon the details and depending upon the age of the container -- we are assuming that they will be no higher than about 260 degrees centigrade. That

is the surface of the overpack. In general, we think most of them will be considerably less because a lot of this spent fuel they are putting in there is pretty old.

MR. ETHERINGTON: Thermal stress will be very small, then?

DR. MC NEIL: No, that is not true actually.

If you do the calculations, you will find you will get some quite severe heat stresses.

MR. ETHERINGTON: What temperature differences do you have then?

DR. MC NEIL: As I recall, this temperature difference from the center -- I am having to reconstruct a calculation done by someone else from memory, but as I recall, the temperature difference from the center of the load to the outside of the overpack could be in excess of 100 degrees centigrade.

MR. ETHERINGTON: That is only about 20,000 psi maximum?

DR. MC NEIL: Only about 20,000 psi, but by the standards of hydrostatic pressure, that is a lot of pressure.

DR. SHEWMON: I don't remember what the mechanical engineers call these things, but the temperature gradient, if you get a little bit of creep you can relieve the stress; whereas, if you have got a hydrostatic pressure

a little bit of deformation doesn't relieve the stress at all.

So the strain potential is a good deal greater in the hydrostatic stress case than it is in the thermal stress case.

I think secondary and primary is what the mechanical engineers call it.

DR. MC NEIL: I don't know.

DR. STEINDLER: Isn't the issue in part what the temperature gradient is across the thickness of the container rather than from the center of the waste to the outside?

DR. MC NEIL: But I happen to remember the figure from the center of the waste to the outside.

DR. STEINDLER: That is what I am saying. The gradient across the thickness of the overpack is fairly modest.

DR. MC NEIL: Well, we had some discussions of the question of the stresses at the time. One of the consultants to Battelle Columbus did a back-of-the-envelope calculation, and the heat stresses do come out in thousands of psi as opposed to hundreds. I don't remember the exact figure. It is not as high as 20,000.

MR. ETHERINGTON: It is typically 100 or 200 degrees for each degree of temperature.

DR. MC NEIL: I don't know.

DR. PARRY: With respect to the design of the canisters -- and I am sure Marty will clarify if I go astray -- as far as BWIP is concerned, they do plan on hydrostatic pressures that will be existent on reflooding of the mine.

As far as tuff is concerned, there is no expectation of any load at all, since it is in a dry environment and there is only water barely passing through it.

Salt is another matter, though.

The structural integrity of the canister -- of the overpack is considered a major design factor, and heavy hydrostatic pressures are planned on, and the canisters or the overpack is now being designed to allow for erosion of the canister down to a certain final critical diameter of wall thickness, which collapse is assumed to occur instantaneously, assuming uniform corrosion of the whole surface of the pack, which is a question I won't go into.

So there is heavy design effort on providing for sufficient metal to withstand the lithostatic pressure in that one project.

DR. STEINDLER: Let me just add one other thing, and that is neither glass nor spent fuel will completely cover the inside of the waste. In fact, in the case of

glass there is a significant amount of freeboard which would be within simply the overpack. It is that significant freeboard and the lack of sensible density in the case of spent fuel, even if backfilled with sand, that causes the need to pay some attention to the effect of stress.

MR. ETHERINGTON: Well, what is the glass?

DR. STEINDLER: What is the glass? It is boron silicate glass. It is a boron silicate glass.

DR. SHEWMON: Please proceed again.

MR. COSTANZI: I would like to just mention briefly the particular phenomena which this research program is focusing upon.

(Slide.)

The waste form devitrification, glass of course, leaching glass, and spent fuel, overpack materials. We get pitting corrosion, stress corrosion, hydrogen embrittlement, and the concerns which arise from the variability of manufacturing techniques.

We have also done a little bit of work on general corrosion, enough to convince ourselves that we don't believe at this point that we need to do any more work in that area; that is to say, we feel we can review any demonstration concerning general corrosion you would care to make.

With regard to the packing backfill and seal

materials, we are looking at diffusion, hydrothermal alteration of those materials, and the parameters which determine the effectiveness of the shaft and fracture ceiling of the rock.

With regard to the waste package environment itself, we are looking at the chemistry of the groundwater, and we have a project which gives the short end of coupled processes. It is looking at what chemical, thermal, and hydrological interactions take place within a repository to report the performance of the repository, and we will talk about that program today insofar as that applies to the waste package environment.

DR. SHEWMON: Let me ask one question on that. I assume we will go over all of these again today.

Is somebody measuring what the oxygen content is of this water down there -- X thousand feet or so in the basalt site, for example?

MR. COSTANZI: We are doing experiments on using water which was prepared by recipe, the recipe coming from DOE. DOE has made measurements on the chemical composition of the groundwater.

The question of the oxygen content of groundwater, however, is a very difficult one to settle.

DR. SHEWMON: But very important?

MR. COSTANZI: Extremely important. The reason

it is so difficult:

One, it is difficult to measure when you take water samples if the water is anoxic and there is a great likelihood of them being exposed to the air, and of course if you change the oxygen content, and moreover the question of the nature of what happens when you excavate the basalt, what the oxygen content of the groundwater becomes after you have opened up the mine and exposed it to air, also the nature of the fractures that produce fresh fractures in basalt.

We have some experiments which indicate that the groundwater, as measured, as determined as best we can in the DOE measurements, and the current composition of the groundwater do not relieve equilibrium.

So we realize the importance of the question, and we don't know.

DR. SHEWMON: One other point that was brought up again recently -- I had always thought that somehow the water must have got down a thousand feet or so and just didn't communicate with the atmosphere at all. Dr. Kassner pointed out to me last night that some very good organizations have been trying to put out underground fires in Pennsylvania -- coal this is -- for a good many years, and they have come back with what they have termed a bent lance. They simply can't close off all the air that gets

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down there.

So there may be appreciably more avenues for communication of air down through the earth than you know. As you probably know, there is some interesting scientific work -- well, more practical -- on how much radon comes up through the earth and what this reflects of either ore bodies or changes that go on in the earth down to an appreciable depth.

So the permeability of the earth down to these depths seems to me a legitimate question. If there is some way to get at, indeed, what the active oxygen potential is down there, it would be of interest for several reasons.

MR. COSTANZI: Yes. DOE, of course -- again, it is their responsibility to characterize the site, to determine, among other things, what the oxygen content of the groundwater is. That is something of which they are aware and something we are pursuing.

We are pursuing the question of what happens with varying contents of oxygen in groundwater. Part of the work we will discuss is going on at Battelle Columbus, looking at the chemistry of the groundwater and how changes in that groundwater chemistry affect the performance of the overpack and in fact how the overpack affects the chemistry of the groundwater.

DR. BIRCHARD: George Birchard, NRC staff,

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research geochemist.

I would like to just give you a little bit more detail on that. That is that BWIP -- the oxygen contents have been measured in groundwater but they are below the measurable limits.

The argument and the question is just how low below that measurable limit is the oxygen content.

DR. SHEWMON: Even though what the measurable limit is, just so somebody who knows what measurable limits mean agrees with it.

DR. BIRCHARD: It would below the part per million level, a tenth of a ppm.

DR. KASSNER: They can go down to at least parts per billion.

DR. BIRCHARD: Yes, but the theoretical levels that the BWIP people are talking about are way below that. I mean, you are talking about, I think, like 10 to the minus 20 or some extremely low theoretical amount of oxygen.

So that is what the argument is. They can't measure the theoretical level, and they never will be able to measure the levels that they theorize.

So obviously at NTS there is no problem because you have unsaturated stone.

MR. ETHERINGTON: This would have been on the location where there might have been any organic material

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that would have consumed the oxygen?

DR. BIRCHARD: At the basalt site there is reduced iron that would react with the oxygen. So at salt sites organic material may be important.

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MR. ETHERINGTON: What about CO-2? I suppose that is present.

DR. BIRCHARD: There is a large amount of bicarbonate in most of these waters. So it is dissolved. There is also methane at the BWIP site.

DR. KASSNER: Yesterday and today we have identified a number of failure modes for the canister. I wonder, associated with any or each of those failure modes, if you decided on what a reasonable failure criterion is; for example, in the case of a stress corrosion cracking.

If you had a throughwall stress corrosion crack a couple of inches long in a two-inch thick canister, if that were considered to be a failure criterion, it wouldn't seem to me to be very drastic because if you got water ingress through that then the corrosion product would probably plug the crack and it would sit there for some very long period of time.

So what I was wondering is if today and in your program you consider what the failure criteria are associated maybe with each of those failure modes.

The same thing could be said about a pit. You had a small pit through the containment wall. How much transporter leaching could you ever get through, let's say, one pit in a very large container?

MR. COSTANZI: Your point is extremely

well-taken. Unfortunately, I have to respond by saying that we do not have a failure criterion.

The reason for that is that we are not really sure at this point what the consequences of failure are. We are not sure what the differences in terms of the consequence of the overall repository behavior of a small crack or a slightly larger crack or perhaps several cracks are, one pit penetrating the overpack or two pits or 100 pits.

The consequence of failure would depend on a number of things. It would depend on the design of the repository itself. It would depend on the local hydrology, the hydrology inside the underground facility itself, and it would depend on the chemistry of the groundwater.

There needs to be consideration of things like if you have a premature failure. However you define that, does it have an effect on the "downstream" waste packages that will spread like a cancer?

These are all questions which we are trying to grapple with. We do not have definitive answers. So we do not have clearly defined criteria which say that, for example, 5 percent of the surface area of the overpack penetrated through by pits constitutes a failure. We don't have anything like that yet.

DR. KASSNER: That would seem to be a major

factor in calculating the release by other mechanisms -- transport, let's say, off the site?

MR. COSTANZI: Absolutely.

Now, there are programs going on. I believe one of the programs we will discuss this afternoon deals with models of waste package performance. There is a waste management technical assistance contract where they are addressing this.

Our research program is primarily addressing mechanisms at this point. We obviously have something worthwhile to say to the Division of Waste Management contractors about the importance of various failure mechanisms.

DR. SHEWMON: It is mechanisms, yes. Whether they are failure mechanisms or not, it seems to me you have said now they were. But two minutes ago you said you weren't, or at least that you hadn't defined anything yet.

Is that what you said?

MR. COSTANZI: Okay. What we are talking about, we talk about failure mechanisms. What we mean is if we are looking at the question of how will the overpack be breached, at this point we are talking about a true pit or a crack in the overpack. We are not really talking about how many pits can you tolerate before you now say this waste package has "failed." We are just talking about the

mechanisms of failure -- pitting corrosion, stress corrosion cracking.

DR. SHEWMON: Deep pit penetration, then?

MR. COSTANZI: Yes.

MR. ETHERINGTON: But if you had a through crack in a fuel cask, would you have a criticality problem?

MR. COSTANZI: No. I don't believe so, no.

DR. SHEWMON: One assumes not, but that is not this group's effort.

MR. ETHERINGTON: It might depend on whether a through crack was important or not.

DR. KASSNER: Wouldn't this also have some implications as to the choice of materials?

For example, I would be happier to see stress corrosion cracks in a canister that wouldn't dissolve forever rather than take a relatively slowly dissolving canister that will essentially expose the whole waste form in some intermediate period of time.

MR. COSTANZI: Remember again that it is DOE's responsibility to make the demonstration that the waste package will fail, containment will be lost by a particular mechanism, establish why they have concluded that or when it will be lost -- perhaps is a better word -- how they have come to that conclusion, how they are substantiating that conclusion, and what are the consequences of it in terms of

the overall repository performance.

That means both in terms of meeting the EPA high level waste standard and the controlled release requirement, if we are talking about failure of containment, and then of course the consequences with meeting the controlled release requirement, and so on.

DR. STEINDLER: In the case of the overpack, you have up there in effect three potential mechanisms of failure.

First off, why did you pick those three?

And, secondly, DOE, I gather, is spending a significant amount of effort looking at uniform corrosion as the target failure mode. Yet I don't see any attention on your part, if that list is complete, to that potential failure mode.

Is there some reason for that?

MR. COSTANZI: Yes. We have looked at general corrosion earlier in this program, and we have concluded that we understand a sufficient amount about general corrosion in the repository, the expected repository environment, that we would understand how DOE would make the argument that that would be the waste package failure. We feel we can review that argument and say, yes, we agree or, no, we do not.

The holes in our ability to review DOE's waste

package performance demonstration are with the methods of failure that we are dealing with now.

We don't feel we know enough to say, no, this won't fail by this method or, yes, there is going to be a failure.

DR. SHEWMON: Tom, those are good questions. We have got side-stepped this morning, perhaps quite legitimately. It seems to me that whoever is constructing models which they would like to sell to the NRC on contract, or are selling on contract, would be a good person to bring it up with again.

DR. MC NEIL: Could I say one word to Dr. Kassner with regard to what one might term corrosion allowance materials, particularly the irons and the carbon steels?

Even if they corrode severely, neither magnetite nor hematite is particularly soluble, particularly not in a basalt groundwater. Even if you assume extremely rapid general corrosion, you are dealing with a sample that is several inches thick. A several inch thick piece of hematite by itself is a significant barrier to the motion of groundwater.

I am not saying this -- I am not pointing this out. I realize this is not entirely relevant to the discussion. I was offering it as sort of a bit of comfort to your concern.

DR. KASSNER: With regard to the comfort, I looked at some of the work that was done in that area, and over a period of, let's say, 8000 years they essentially fitted an equation to it which came out parabolic, and if you guys can find a material that exhibits parabolic corrosion out through those times, you ought to patent it.

DR. MC NEIL: I am assuming linear.

DR. KASSNER: You get breakaway corrosion after some period of time, and I think it is very optimistic to use parabolic.

DR. MC NEIL: We are not using parabolic fittings. We are using linear fittings. And here is an example of 1800 years old from Scotland. This is a nail, and you notice it is still there, and this was not put in a carefully prepared repository. This is something that some Roman just dropped in the ground in the lowlands and stayed there till the people at the University of Manchester came around and dug it up.

DR. KASSNER: I would like to show you my '69 Chevrolet.

(Laughter.)

DR. MC NEIL: Maybe the Romans made their nails better than General Motors makes their cars.

DR. SHEWMON: I would just comment between you scientists that both of these oxides occupy appreciably

more volume than the iron they replace, is that correct?

DR. KASSNER: Yes.

DR. SHEWMON: So that, too, will tend to restrain flow very substantially, it would seem to me.

DR. KASSNER: The only thing you do consider, if you do get a perforation in, let's say, an iron canister and you get ingress of water to the inside, you get oxidation and corrosion and you get the swelling of the film. You get what happens to my car. The thing essentially falls apart due to the oxide stresses, and the canister then can rupture and the corrosion process is off and running at a very accelerated rate.

DR. MC NEIL: We assume spallation, which is basically in our linear extrapolation scheme.

MR. COSTANZI: Now, I have given you a picture of the sorts of things we are doing in a very general way and why we are doing them.

Turning now to the individual programs --

DR. STEINDLER: Excuse me. Before we do that, I guess you rang a bell, largely because of some of the past sessions that at least Dade's subcommittee has had, when you said you had some difficulty in communicating with the Department of Energy and trying to find out where it is that their program is going, and so on, and so forth.

What mechanism do you currently use to track DOE

plans, not the results of their work, which presumably come out in some kind of a periodic reporting system, but the planning for the direction of the various programs?

MR. COSTANZI: The mechanism which we are finding the most effective for the purposes of our program is our active participation in the NRC/DOE interface meetings which are going on as part of the licensing process.

Whenever possible, members of the Research staff attend those meetings. We think it is to the advantage of the staff. It has been a very effective mechanism of keeping us up to date and keeping our program as focused as it is.

DR. STEINDLER: I would assume there is a significant delay line between the time you find out there is a program delay and a change in program emphasis and the time you are able to implement a sensible research effort on some portions of this new direction.

What would you estimate the length of time is that you folks have to have notice for in order to be able to move in that area? Does it take you a year to react to changes to DOE emphasis on the program, six months?

MR. COSTANZI: It really depends on the magnitude of the change. If we are talking about making small changes in the composition of materials, changing the carbon content of the steel or to a high nickel allow from stainless steel,

that kind of change probably could be accomplished within a quarter or two quarters, provided that the mechanisms you are talking about, the phenomena, are transferable.

If we are talking about going to a material change in which the entire mechanisms are different, where we don't know anything about the effect of the environment, we are not building on past understandings, that could take as long as a year.

Essentially, you would be running a new research program for that portion of the work. You would have to consider what are the problems, how do you identify what the problems are, what kind of experiments do you want to do to pin that down and what do you want to do after you have done those. It could be a major perturbation.

DR. SHEWMON: A related question: do you work primarily -- or do all your projects have to be approved by a user need letter from over what would be the NRR side of the reactor?

MR. COSTANZI: There is a user need letter which outlines the areas in which John Davis believes that research is needed for his waste management program. Within those areas, individual projects are coordinated with the Division of Waste Management.

Let me take a few moments to describe the coordination process.

The members of the Research staff and the Division of Waste Management staff fortunately reside in the same building. That facilitates a lot of interaction. They see each other and talk to each other every day.

The cooperation and the closeness of the work I think is remarkable. So we know what they are worried about; they know what we are doing.

When there is an indication that some work needs to be done, that is usually brought to the attention of management, either Research management or the Division of Waste Management management. There is management discussion as to what kinds of things ought to be done, both ad hoc and on a periodic basic.

So there is not only planning that takes place where I sit down regularly with Hub Miller from High Level Waste and Leon Higginbotham and talk about the sorts of things that we ought to be doing, but also when things are generated spontaneously by the staff. We talk about should we do this, what sort of resources should we expend.

When something looks like it is a good idea and ought to be added to our program or our program ought to be modified, then a statement of work is generated. It is reviewed internally within the Office of Research, or at least within the branch. Then it is sent down for formal review by the Division of Waste Management.

Prior to going to formal review, again these discussions on the Waste Management staff and people on the Research staff have been ongoing, so there are no surprises, or at least few surprises.

There is one more point I would like to mention. After that is done, there is a second level of review called the Waste Management Review Group. That group is specifically charged by the EEO with the responsibility of assuring program continuity and all Division of Waste Management technical assistance programs, all Office of Research waste management programs are reviewed by that group. It is an interoffice group, remember. It is not only Office of Nuclear Materials Safety and Safeguards Research, but also NRR, I&E, and Division of Contracts.

To assure consistency of the program, all programs, both Division of Waste Management and Technical Assistance programs and Office of Waste Management programs in the Office of Waste Management must have Waste Management Review Group approval to be funded.

So besides the interdivisional coordination, there is also an interoffice coordination and program coordination.

Now, the question is now how to proceed with these individual projects. I could give a brief introduction to all of them and then come back to the

questions. I could introduce them project by project, and if you have questions we will dispose of questions on that one and then move on to the next one.

How would you like to proceed?

DR. SHEWMON: I don't understand the procedure. We would like to go over each project and get some idea of what you are doing and how you think you have reached the results which you do.

MR. COSTANZI: I will just give you an overview of each program. Then you can ask questions.

MR. ETHERINGTON: May I ask just one general question?

DOE has good documentation of their reasons for selecting these materials, don't they?

MR. COSTANZI: When they select those materials, material selection will have to be documented in the license application.

MR. ETHERINGTON: You mean they haven't made any tentative selections yet?

MR. COSTANZI: They have made a number of tentative selections, but they have not made any definite selection.

MR. ETHERINGTON: So you are looking at materials independently. You are not just looking at DOE's planning?

MR. COSTANZI: No. We are looking at the same

materials which DOE is considering, and we are following DOE's lead.

DR. SHEWMON: But learning which way they are leading is sometimes not as easy as one might expect.

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MR. COSTANZI: Precisely because they have not made definitive statements.

(Slide.)

The first project I'd like to speak about is the project entitled Container Manufacturing Parameters and the contractor is the Manufacturing Sciences Corporation. The objective of this project --

DR. SHEWMON: Is that in Colorado?

MR. COSTANZI: This is in Colorado. Manufacturing Sciences Corporation is an adjunct of the Colorado School of Minds. It's my understanding now that almost all the work is actually being done at the Colorado School of Mines.

DR. SHEWMON: Who are the principal investigators?

DR. MCNEIL: The principal investigator for Manufacturing Sciences Corporation is a Dr. Alan Liby. However, the Manufacturing Sciences Corporation has in fact been acquired by another firm and their headquarters has been moved. Almost all the experimental work is in fact taking place at the Colorado School of Mines under the guidance of Professor Robert Frost.

He was one of Flemming's people.

DR. SHEWMON: Thank you.

MR. COSTANZI: The objective of this project is

to assess the effect of variabilities in the manufacturing techniques of the performance of the waste package.

Particularly what they're doing is they are producing samples of overpacked material which are then analyzed at Battelle Columbus.

MR. ETHERINGTON: What is overpack?

MR. COSTANZI: Overpack is the material that essentially gives the corrosion barrier to the groundwater. It protects and contains the waste form, the glass and spent fuel during the containment period.

DR. SHEWMON: Why do they feel they have to manufacture it instead of buying it to the specifications used commonly?

MR. COSTANZI: The question we're investigating here is how close do those specifications have to be. DOE will have a specification on material which will -- that specification will determine both its composition and the variability of its composition, and the way it's manufactured, its history.

The question we're investigating here is how good does that control have to be in order to have confidence that the waste packages will perform as designed?

If the specifications are extremely loose on the quality control program, or the quality control program is not well-defined and not well-managed, that will diminish

the confidence that we would have that the waste packages has performed as designed.

But, in order to make any sort of judgment as to how much that would diminish our confidence, we would need to understand how sensitive the performance of the waste package is to those variabilities.

DR. SHEWMON: We're talking about cast steel. The containers you're talking about are cast steel? These would be centrifugally cast probably or sand-cast? What's your assumption?

DR. MCNEIL: Let me answer that. The DOE documents that we have do not specify at present whether they need to be centrifugally cast or not. In fact, we, that is, the people at the Colorado School of Mines, are of the opinion that ultimately, if they use a cast structure, which they're still talking about but it's not unequivocally clear that that's the best way to go, that if they use a cast structure, they will in fact probably want to centrifugally cast. But, certainly, a lot of sand castings and static chill castings are being used now as samples.

MR. ETHERINGTON: You would have a centrifugally cast cylinder and welded on heads?

DR. MCNEIL: Yes, precisely. There are centrifugally cast parts. Professor Frost, in fact, when he was in Massachusetts, worked at Watertown arsonel where they

have considerable experience at centrifugally casting large gun tubes, which are not all that different in fact from some of these overpacks.

DR. SHEWMON: Are you working primarily with chemistry or microstructure?

DR. MCNEIL: Both. But, of course, they combine. One of the important things is the way in which the steel is handled and killed before. We take basically an AISI spec, or they take an AISI spec that DOE is using and examine the question of how the addition of various components in the casting process controls the structure, not only the microstructure but also what I might term the gross structure, as in a sample which I will pass around.

This sample, incidentally, was part of a blank. We took the basalt site specs and we told the people at the Colorado School of Mines pretend the lowest cost foundry you're going to make something that will technically satisfy the DOE specifications. But you will in fact take no special precautions, just as they were trying to minimize cost.

Here is a cross-section of the appropriate thing; the sides that don't have any holes in them are the ones that are exposed to the air that you would see on visual examination.

DR. SHEWMON: Well, it will pass for a piece of

Swiss cheese in case anybody wants to know, except it doesn't seem to be made out of goat's milk and cow's milk.

Let me come back. What you are using is entirely then a chemistry spec. Sorry --

DR. MCNEIL: A chemistry plus casting technique.

DR. SHEWMON: The question is what is the spec you gave them? Is it a process spec? A chemistry spec?

DR. MCNEIL: We gave them what we got out of the basalt document. It's purely a chemistry spec. It was to be statically cast out of such and such steel.

DR. SHEWMON: There's nothing about porocity in it, obviously.

DR. MCNEIL: There was nothing in the spec as we had it. I was talking yesterday to someone from the BWIP site, who informs us that now they are in fact imposing some specs on their materials.

I sent them another cutting of that incidentally. I sent one out. They are now imposing some quality assurance specs on their materials, which will prevent this sort of thing from happening, they say.

But since we haven't had a joint NRC-DOE review of the waste package work in the basalt site for more than two years now, the details of precisely what they're doing are wrapped in mystery.

DR. SHEWMON: I can't imagine that the people who

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use cast steel in commerce don't have a specification which avoids disasters like you're passing around.

DR. MCNEIL: Of course. The next thing we told them, you know, when you're making samples for Battelle, use normal casting practice and produce sound castings.

DR. SHEWMON: That wasn't my question. The question was, is there a specification which what you now call normal castings are made to.

DR. MCNEIL: I do not know of a specific appropriate specification in the sense of an ASTM spec for casting procedures for low carbon steel.

DR. SHEWMON: But since a fair amount of tonnage is used successful, I have difficulty believing it's made without specifications. But that's your professional opinion from your work in the subject. Is that right?

DR. MCNEIL: My background is not in solidification. I have been told that there is no ASTM specification.

DR. SHEWMON: I don't give a damn whether it's ASTM or not.

DR. MCNEIL: There is a general knowledge according to my understanding among foundarymen of how you produce sound casting, and it involves various precautions that everyone in the sense of people in the business take to make sound castings, but which were not included in the DOE

documents we received on the subject.

DR. SHEWMON: I'm sure you're right, but I would also ask you for all the money you are spending at Colorado School of Mines if they would also see what specifications are used by the military, for example, or are used by the railroad industry or put out by the Foundarymen's Society, or Lord knows what. People closer to foundary practice than ASTM is, to see what can be found other places.

DR. MCNEIL: I will ask that.

MR. ETHERINGTON: Sand castings are typically 35 carbon. Are you trying to get much lower carbon?

DR. MCNEIL: That is either 10-18 or 10-25. It is in fact what DOE are using.

DR. STEINDLER: Let me make one other comment or question. If my understanding is correct, you're looking at the details of the manufacturing process for alloys that DOE has not yet specified. And the manufacturing processes are still up for grabs. In some cases, you probably don't even know the environment very well in which the corrosion action is going to take place.

Doesn't it seem like maybe this program is a tad premature?

MR. COSTANZI: I think the picture that you paint is a little bit bleaker than it actually is. While it's true that DOE hasn't made any decisions in terms of saying

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that this is our design for this repository, we do follow the materials on which they are leaning and on which they are doing research.

We do, for example, understand that the overpacks will be welded. It is part of this program we are producing welded samples. We do have some understanding of the general conditions, for example, of the salt repository. Much of our program is to determine how sensitive the performance of the waste package designs would be to changes in those assumptions, changes into the casting techniques or the welding techniques, or changes into how the program changes in the chemistry of well water.

DR. STEINDLER: I guess the problem that I have is I think perhaps it's unreasonable but it seems to me, for a lot of reasons, DOE has not made a firm decision on its alloys because it's their testing program. It's not complete. And, presumably, DOE is not totally satisfied that the alloys that currently represent the reference on which they're doing most of their testing will in fact be qualified as suitable to meet the standards of both NRC and EPA.

If, in fact, it turns out that a year from now that turns out to be a real problem, surely, DOE will then pick another alloy backup, or what have you, and effectively a fair fraction of the work that you're doing might very

well be interesting but not particularly germane.

MR. COSTANZI: In terms of the fact that DOE might take a backup material, which, incidentally, to the extent that we know what they are considering as backup materials, they aren't even investigating. But the primary material might be abandoned, or the primary design might be abandoned in favor of something else.

What you say is absolutely correct but let me remind you that we are under a legislative mandate to be ready to review and to review for a three-year period DOE's license application. We cannot afford to start our research and develop an independent technical base for that review until after DOE has made all its decisions.

We feel that we have no choice but to track, albeit with some delay, track what DOE is doing.

We are making some judgments as to what are the most likely avenues which they will pursue, and we are trying to follow them down those paths.

If they turn out to be blind alleys, I'm afraid that's kind of where we end up. We don't feel we can afford to wait. We're not trying to lead DOE but we don't feel we can wait until they make their decisions and then do all our necessary research to have an independent technical base for our mandated review.

We have to follow that.

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DR. STEINDLER: To what extent is DOE doing the kind of research that you're doing here?

MR. COSTANZI: Dr. McNeil,?

DR. MCNEIL: As I stated, in both the salt and the basalt cases, the amount of information we obtained from the Department of Energy is slight and we obtained that with delays in terms of years instead of months.

Insofar as I know, DOE is doing very little, if any, work on welding and solidification research. I know they are doing something on carbon steel because I have friends at Armco and I happen to know they are buying high purity iron from Armco. But that's the sort of information that I have.

That's the answer. The tuff site, the people in the tuff site are pretty open, but the other two sites, we have a great deal of difficulty. I just found out accidentally, for example, about these new specs on the containers at the BWIP site. It happens I was teaching a short course and there was someone from BWIP there. The subject came up and they just sort of tossed this out.

DR. CARTER: Let me make one comment. I guess I'd like to follow this a little bit because we've heard some cases, I guess, of extreme cooperation, I guess, between NRC and DOE. It sounds like to me that you're making a case for the fact that they're withholding

information from you on a deliberate basis.

DR. MCNEIL: I didn't say that.

DR. CARTER: You didn't say that but the implication could well be that you found out from friends.

DR. MCNEIL: I will give you one example of something that happened. I came from DOE to NRC in 1982. At the time that I left DOE there was available in DOE headquarters in draft a very interesting document having to do with radiation damage in salt.

After a while of being at NRC, I thought it would be nice to have a copy of this. Well, I discovered I couldn't get one. Well, nobody knew where it was.

I finally managed -- this was 1983. It took me two years to get a copy of a report that was circulating in DOE headquarters in 1982.

On the other hand, in the tuff site, basically, any information that I want, Dr. Vieth makes available. We have no serious problems at all with the tuff site on the sharing of communication.

I have found obtaining metalurgical information from the other two sites extremely difficult.

DR. CARTER: Well, what's been done to try to rectify the situation, if it's a problem, and I gather it is from the remarks that I've heard.

MR. COSTANZI: I'd like to address that.

DR. CARTER: Please do.

MR. COSTANZI: One of the reasons why these NRC-DOE prelicensing meetings are taking place and taking place quite regularly is to identify early on the issues which need to be addressed either now or certainly at the time of licensing, so that we will minimize the surprises that come up when licensing takes place.

Now, in the course of doing that, in our discussions about who is doing what, what kinds of things DOE is doing, what sort of avenues of research they're pursuing not just in this area but also in terms of geology and hydrology and geochemistry of their sites.

Those are the topics of discussion. That's the meat of those meetings.

DR. SHEWMON: Why don't you, for example, then, tell us about the overpack materials and when they are prepared.

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MR. COSTANZI: Those are regularly scheduled. The information as to when the meetings are appear on both the DOE and NRC hotlines.

DR. SHEWMON: Tell me about when the last overpack meeting was and the next one will be, then, on each of these m terials that is the subject of discussion.

MR. COSTANZI: I don't have the information at hand when the last one was.

DR SHEWMON: Let me tell you when the last one was then.

As I understand, the one on tuff was last June, is that right?

DR. MC NEIL: That is correct.

DR. SHEWMON: And the one which was to occur on salt and basalt was flipping from both coasts so fast one couldn't follow it last month when it was scheduled, and now maybe they will be scheduled the first quarter of next year if it can be arranged but it is not at all certain.

Is that a fair statement?

DR. MC NEIL: That is a fair statement.

MR. COSTANZI: That is correct. That is my understanding.

DR. SHEWMON: So what he is saying is in general true, but in this area it just isn't working.

DR. MC NEIL: As I said, the last meeting on the

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basalt waste package was 1982.

DR. SHEWMON: You say it is working, but when I ask you when it has occurred, it hasn't occurred and it isn't occurring but it may occur someday.

MR. COSTANZI: I don't think that is exactly a fair characterization, at least the part that it may occur someday. It will occur.

DR. SHEWMON: I have faith, too.

MR. COSTANZI: It is not just a question of faith. You have to remember that the waste package is a fundamental part of the repository but it is not the only part of the repository, that right now the questions which DOE is facing in terms of the kinds of decisions that they need to make today -- or I should say in the next few months -- are siting decisions. They need to make decisions with regard to which sites to characterize.

They have the environmental assessments which they have done and published for comment. They are reviewing those comments. Final environmental assessments need to be prepared.

DR. SHEWMON: But relative to Dr. Carter's comments, you are committing money now on research which is supposed to provide answers, and he asked about the possibility of liaison and coordination, and that is the question. And that is at best being delayed, I think, is

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the answer to your question.

DR. CARTER: It sounds to me, if I interpret correctly, here is a report apparently that Dr. McNeil would like to have had and would have found useful, I presume, but he says it took him two years to get a copy.

DR. SHEWMON: That was two years ago. What are we doing now?

DR. CARTER: That is correct. What are we doing now?

MR. COSTANZI: We have on numerous occasions brought to the attention of the DOE management at headquarters the difficulty of getting information.

Both Mr. Browning, the Director of the Division of Waste Management, and Mr. Rusche are aware of the difficulty. It is not something that we are ignoring.

The point I am trying to make is simply that in the area of waste packaging right now we have not solved all of the problems of communication. We realize in general that communications ought to be complete and thorough, but there are other things which are going on besides the area of waste packaging.

There is communication among the people who work in the DOE program and the Division of Waste Management and in the Office of Research. The formal communications, the formal lines, published reports aside, are primarily with

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the waste package meetings.

The recent difficulties encountered in scheduling the waste package meeting at the BWIP site again come from pressure of other responsibilities which both the Department and the NRC have. It is not that anyone is really trying to forestall discussions or forestall decisions.

DR. SHEWMON: What you are saying is having a meeting has a low priority right now, or at least not a high priority, and that there seems to be no other way to get this to occur except that you hope it will happen at the meeting when it occurs?

MR. COSTANZI: I would say that the press of DOE's responsibilities, in my judgment -- this is my opinion -- seems to be their highest priority.

DR. SHEWMON: Everybody seems to agree. Do you want to pursue it further or shall we go on?

DR. CARTER: No.

DR. SHEWMON: Any other questions on the manufacturing project?

(No response.)

MR. COSTANZI: Perhaps the next several projects -- Dr. McNeil is the principal investigator -- it might be convenient if I would ask him to come up here, if that would be all right.

(Slide.)

The next project is the pitting corrosion chemistry at Brookhaven National Lab. The objective of this project is to assess the confidence available in extrapolating short-term laboratory tests to long-term pitting rates.

What the specific program is trying to do is observe the rate and chemistry of pitting in artificially produced pits, pits that are produced to represent what we expect an aged pit to look like. Specifically, what we are trying to get at here is the basis for assessing what will be DOE's extrapolation of short-term pitting behavior to long-term pits.

We would expect that pitting corrosion would be something that DOE will do with us, regardless of how it believes its failure rate will be for a waste package. There will be short-term tests to measure pitting rates and a question as to how believable are those tests in terms of the stability of the pitting process over the long term.

DR. MC NEIL: I am waiting for questions.

DR. KASSNER: I have one then. I would just like a little short discussion as to how the short-term pitting data is being used, in the sense that a pit usually starts out very small, grows rapidly, and most models say that it dies. Then it either reinitiates and goes again.

As I looked at some of the material in the

handouts that I have read recently, I got the impression that they might have been taking a pitting equation and essentially applying it to a pit that might be two inches in diameter and eventually two inches deep, growing by either a parabolic or cubic rate.

I wonder if that is at all realistic. In other words, if you get a pit that is two inches in diameter, that is like uniform corrosion. Maybe you have to reinitiate a pit on a pit on a pit, and therefore the kinetics might be much faster than essentially growing a two-inch diameter, two-inch deep pit over 8000 years.

What I am really asking is how is the short-term pitting data being used in the models to assess penetration of the canister?

DR. MC NEIL: We have two different issues here, one of which is the behavior of very deep pits that have a high aspect ratio. We are doing experiments there, basically creating artificial pits with a high aspect ratio and trying to determine how these grow and how this correlates or does not correlate, in fact, with appropriate models. You have the question identifying the rate controlling step. Is it in fact the rate controlled by the availability of cathodic reaction or is it controlled by the precipitation of salts to the bottom of the pit?

The second question is when you have these pits,

like you get in carbon steel and pipelines, the very low aspect ratio, deep and wide pits, how does that pit grow?

And the answer that we seem to be coming out with -- and we are not really ready to go out publicly with this because I have told them to confirm. You know, we want a lot of confirmation -- is that the traditional models of pitting, which are oriented toward high aspect ratios, are utterly inappropriate for this.

DR. SHEWMON: What is a high aspect ratio?

DR. MC NEIL: A high aspect ratio is a deep pit, deep and narrow. Low aspect ratios are the ones typical of pipelines that are, you know, two to three times as deep as they are across as opposed to 50 or 60.

Dr. Bibers believes that he has a new model which is appropriate to these pits. We are doing a lot of experiments because we have such questions as to what extent -- you know, where is the cathodic reaction taking place, where is the current going?

He thinks he has really interesting new ideas on this subject, but I have told him to be pretty sure before he publishes because I don't want any muddied waters in this program.

MR. COSTANZI: I might interject that the purpose of this program is not to develop a new model of pitting corrosion. The purpose is to have some confidence and to

establish what confidence you can have that once pitting starts you can measure it in the laboratory. What is going to happen several hundred thousand years down the line is going to be reflected by what you measure over the short period of time in the lab.

DR. SHEWMON: If you can do that with any confidence, you are going to have to do original scientific work.

MR. COSTANZI: Exactly. And we need to do that with confidence in order to understand whether or not you can do that and to what degree.

DR. MC NEIL: The issue here is, you see, everybody takes -- well, most people -- people worry about pitting in stainless steels. They take models that work for stainless steel, like Alkeier's work at Illinois, and if you are not too perceptive -- well, you have pitting in carbon steel, let's go take this model that works for stainless steel and just apply it simplemindedly, and it doesn't work.

DR. KASSNER: The danger here is that you will get a very nonconservative calculation if you take some of these simple models and just apply them to these release things.

I just wondered if that was being evaluated.

DR. MC NEIL: That is a major source of concern.

MR. COSTANZI: Let me stress, we have nothing against using the simple models provided that you know how wrong they are. If a model that is used is nonconservative, the degree to which it is nonconservative can be quantified, therefore, in some sense, compensated either by design or by environment.

Fine, that is no problem. We cannot afford to be ignorant of just how far off the simple models might be.

MR. ETHERINGTON: I have seen them with a three-quarters inch diameter surface, but I gather we are talking about something much bigger.

What is the biggest pit that you have seen anywhere?

DR. MC NEIL: The biggest pit I have seen anywhere -- I gather other people may have seen some bigger ones -- is in a sample of pipe at Battelle Columbus Laboratory and is approximately three inches in diameter.

MR. ETHERINGTON: Was this made artificially?

DR. MC NEIL: No, no, this was a problem that Battelle was taken in on on a failure. It is sort of a right circular cylinder. It doesn't narrow down very rapidly.

Excuse me, while I have got the microphone I would like to correct an answer to a question I gave you earlier. I gave you an answer that was garbled on the

subject of the pure iron samples.

We are using samples from a heat of pure iron that was produced by vacuum arc remelt owned by the Department of Energy. We are using samples of that because it is nice to have one heat so everybody knows what they are drawing from.

The commercially available iron now is argon-oxygen. If you ordered commercial iron now, it is produced in an AOD furnace.

MR. ETHERINGTON: How long have you owned that?

DR. MC NEIL: I don't know. It has been owned since we were AEC. My guess is probably that it was from electrodes of some sort.

DR. SHEWMON: You said AOD. What is the carbon?

DR. MC NEIL: As I recall, you can get to 0001 easily. You can pull another factor of 2 or 3 off of that if you really want to.

DR. SHEWMON: If you don't cast it in air centrifugally?

MR. ETHERINGTON: Are they filled with magnetite?

DR. MC NEIL: They are normally filled with magnetite. For experimental purposes, in checking some of the models, we have tried making artificial bits and packing them with other materials to determine to what extent there are -- we are concerned about cathodic reactions within

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these very large pits.

DR. SHEWMON: Tom, did you have other questions?

DR. KASSNER: I am just saying I wish you guys would get high aspect ratio pits because when they perforate the amount of transport of the species out through that type of pit will be minimized. One can then take advantage of it.

DR. MC NEIL: I know, but unfortunately if you make things out of carbon steel you tend to get these very broad shallow pits. You have to speak to God about the behavior of carbon steel.

DR. KASSNER: Is there any chance in getting some long-term data to see what the shape of these things eventually will be? I mean, is it well-know that they are going to come out?

DR. MC NEIL: All we can do is look at things like old pipes. We tried to consult people who have some experience with dealing with things like perhaps water pipe systems in New England. In these cases you are talking about sort of archaeololgical metallurgy. You just have to take what is available, unfortunately.

DR. STEINDLER: What sort of materials are you looking at in this program?

DR. MC NEIL: Almost entirely carbon steels.

DR. STEINDLER: Are you doing this with

distilled water?

DR. MC NEIL: We have several different experimental systems. We take basalt groundwater, according to a recipe which we get from DOE.

DR. STEINDLER: What sort of control do you exercise in the oxygen content?

DR. MC NEIL: We do generally some of them simply with atmospheric levels of oxygen and others sparged with argon, low oxygen. We do not believe that these ultra low oxygen figures -- I do not believe these ultra low oxygen figures being cited at BWIP for two reasons.

The first is that the theoretical arguments they advance having to do with the CH-4-H CO-A-3 minus equilibrium are quite pointless.

I think the arguments advanced having to do with the thermochemistry of freshly cracked basalt surfaces are inappropriate because you may make arguments on thermochemistry based on basalt that has been cleaved in vacuum. But the basalt down there is not cleaved in vacuum. It has been broken up in mining processes and exposed to the air for years.

And the third thing is because you have got radiolysis.

DR. STEINDLER: So you are picking on one of the alloys in the case of basalt, that the basalt is

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investigating?

DR. MC NEIL: We are also doing tests in pure iron.

DR. STEINDLER: I am trying to get at the solution chemistry that you are looking at, since that seems to be one of the driving forces.

Do you know anything about the EEH of that solution that you are doing?

In other words, I am not interested in exploring technical issues. I am trying to figure out whether or not the results of this work will indeed be extrapolatable to the real world and include sufficiently decent data so that the NRC and/or DOE will be able to defend itself against some group of intervenors who have hired some pretty decent corrosion people.

DR. MC NEIL: Well, we try to do the tests in the most anoxic conditions we regard as believable. Now, of course, that is very much a judgment call.

We try to do it in rather oxic conditions. We regard equilibration with that oxygen. It is pretty oxidizing.

We occasionally do tests with peroxide, and then we do these scans, you see.

(Slide.)

The polarization, where we can drive the

reaction potentially up to extremely oxidizing conditions. Well, that is not extremely oxidizing, but the thing is look at the pitting we got.

Now, this isn't much better.

DR. SHEWMON: Show us the pitting on that one, since you admonished us to look at it.

DR. MC NEIL: I am sorry. Here is the anodic branch. This is the potentiodynamic scan. This is the anodic branch of the curve. Forget about this part here.

When you begin to crank up the potential -- I will show you the equipment first. That might help.

(Slide.)

Here is a schematic of the experimental setup, a very crude schematic of an experimental setup. Here is a reference electrode which could be hydrogen, but is in fact usually calomel for practical purposes.

You have an adjustable voltage. You are connected to a working electrode, which is your sample. The auxiliary electrode merely serves as a way of dumping off the electrons. You are going to use this adjustable voltage to drive the working electrode to make it corrode faster than it wants to, in some sense, and you need some place tc take away the electrons.

And this is the auxiliary electrode. You measure the voltage with a salt bridge and the device called an

electrometer, which is an extremely high impedance potential measurement.

So we get an accurate measurement in the difference potential here and we get a current.

Now, will you put back the other little thing? Here is an example of a measurement. We start off, let's say, here in a quite reducing thing. This is against calomel, so you are up 250. You have got 250 between here and the hydrogen electrode. Here is the current density, logarithmic, going out there.

So as you crank up the potential, you make it more and more anodic. The current goes up and up. This is uniform corrosion.

Now, you have a fall-off here. It is beginning -- some alloys, you see, would go way down here, but this is Ferrovac E, a commercial iron, commercial pure iron, and it doesn't passivate very well in this groundwater. You see it passivates a little bit down here, and then it goes along here, and you keep driving it, and finally it starts pitting.

So this current is pitting.

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Now one of the reasons I have this -- shows on hysteresis, one of the nasty things about this curve from the point of view of assurance of noncorrosive behavior, as you see, it had to go up here to make it start pitting.

But once it starts pitting, when you back off, you see it comes all the way back down here before it goes back down. What you're showing here is a situation in which it's very hard to make the sample start pitting. But once it starts, it can grow in a much more reducing environment than the environment you have to create in order to get it started.

DR. SHEWMON: Fine. Go back to the next question. You asked the question again. Have you got an answer to the question?

DR. STEINDLER: The focus, I gather, is primarily on basalt, the basalt container material.

DR. MCNEIL: Yes. It's not entirely on basalt but I would say more than half the effort is on basalt.

DR. STEINDLER: Do you have programs that work in the environments defined by the other repositories?

DR. MCNEIL: We have a significant effort in the salt. We have very little in tuff at the present.

DR. STEINDLER: In the case of salt, how did you choose the solutions?

DR. MCNEIL: There are two brines, brine A and

brine B with appropriate impurities which are used on common tests between DOE and NRC.

Basically, we just try to stick to what DOE seems to feel are brines typical of their repositories. It's a question of the oxygen concentration. That's a very significant one there, particularly considering that you have radiolysis of basalt as well as the brine.

All I can say is we're trying to do something on that.

DR. STEINDLER: Presumably, then, you recognize that those results will be more generic than specific in light of the fact that the actual groundwater is not defined.

DR. MCNEIL: In all cases, well, we are doing one thing in the basalt case. We have a number of different components that we know will be present. We, that is, Battelle, have a number of components that are known to be present in the basalt groundwater. And a very large number of these experiments are being done.

And varying the concentrations of these partly because we feel that the concentrations in the groundwater probably will vary from one part of the repository to another. Quite apart from the variations in the average composition. And we are attempting to determine by a scheme of statistically-designed experiments which of the first

order and second order interactions are the most important. For example, a trivial question that one has to answer is how important is silicate; if the concentration of silicate ions is increased by 4 times, does this make a fundamental difference to the important parameters here, or is it down in the noise?

The second order question is, if you increase the amount of silicte ion and you increase the amount of, say, borate ion, is there a second order effect?

It just basically depends on the product of these two. So we're doing quite a large program of experiments to identify not only just to collect a lot of data on what I would term believable basalt groundwaters in the sense of basalt groundwaters whose compositions are more or less consistent with the information we've got, but also to determine which ions are the most important.

In other words, if it turns out that everybody was grossly wrong about the borate composition, does it really matter?

That's a question we really feel we have to have answers to because the groundwater is not that well-defined.

DR. STEINDLER: Are these data going to be used in validating the model?

DR. MCNEIL: These data, I don't know that I can

answer that question. I think these data will be used to analyze DOE's submissions.

MR. COSTANZI: I think that if the Division of Waste Management determines that it needs a specific model for pitting corrosion and chooses to develop one at this level of detail, then this data, certainly data collected from these experiments will be used.

Right now, it's a question of what kind of cases do you need to make in that? How do you view that? Again, as part of this Aerospace contract, they are looking at that waste packge. I'm not sure they're into that level of detail.

DR. SHEWMON: Okay.

DR. MCNEIL: I think Dr. Parry had a question.

DR. PARRY: The basalt groundwater that you've used there, is that saturated?

DR. MCNEIL: We have two different varieties of basalt groundwater; in the sense that we vary the ranges, yes, some of them go all the way to saturation.

But we have two standard basalt groundwaters, as well as what I might term the artificial ones we use for the statistical experiments. One is a 1-Xed. That is the straight basalt in groundwater as specified by DOE. And then the second one is simply basalt groundwater concentrated 10 times.

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The reason you concentrate it 10 times is because the repository is going to be physically hot. The cesium and strontium produce heat and so it's our perception that as the first groundwater comes back into the repository after it's been excavated, filled and sealed, it will be converted to steam which will escape leaving the soluble salts behind.

And so we think it is quite plausible. By the time we actually have liquid electrolite in contact with the containers, it will in fact be significantly enriched in all the soluble salts.

Could I say one more thing about this curve in terms of the statistical experiments?

It is often observed. Suppose you do these curves twice and you do them once slowly and the second time fast? It is often observed that if in fact you have a very peak here down in the general corrosion area, the difference between the fast and the slow scans is a great deal. As in this case, you see I have penciled in.

Suppose this were the slow scan and you got a lot and then you had this in the fast scan? You got a sizable difference between the slow and fast scan. In this loop, this type of behavior seems to correlate with stress corrosion cracking.

And so, in doing the statistical experiments

also, we're in a sense doing a screening in the places where we have to worry about stress corrosion cracking.

DR. PARRY: Is that a high magnesium groundwater that you're using in the system?

DR. MCNEIL: This is basalt.

DR. SHEWMON: Tom, do you have a question?

DR. KASSNER: Just a quick one. Did you make any in situ measurements of the redox potential?

DR. MCNEIL: You mean, in the actual sites?

There has been considerable experimental work at the basalt site. It's been giving very contradictory numbers. This was work done by DOE. They have got a very wide range of values going from plus 200 on hydrogen down to about minus 200.

DR. KASSNER: That's the kind of information that would be.

DR. MCNEIL: When the information is all that broad, it's not all that useful. We do what we can given the information that we get. In this case, I do not think DOE is concealing. I think it's just a very, very difficult measurement to do. Ph measurements are not all that easy to do in the lab. When you try to do them down in the ground somewhere, I can appreciate some of DOE's difficulties.

DR. KASSNER: Those potentials are quite a bit higher than what you're looking at.

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DR. MCNEIL: Put that back. That's calomel, remember. So, in fact, minus 200 hydrogen has got minus 450 calomel, which is around there.

DR. KASSNER: So those environments were up in the pitting range then?

DR. MCNEIL: We are observing pitting in basalt groundwaters.

MR. ETHERINGTON: You mentioned radiolysis of salt. Can you quantify that relative to the normal chloride ion concentration in brine?

DR. MCNEIL: It can be done. I don't have the calculations here.

MR. ETHERINGTON: Is it 10 times as much?

DR. MCNEIL: No. We're talking here about conversion of NACL into sodium metal, which then reacts, and chlorine CL2. So far, what has been detected is CL2. Personally, I think if it's done in one of these situations where you have all this brine around, what you're going to get is some sort of oxygenated chlorine atoms, chlorides, chlorate, or something like that.

There's not been adequate experimentation in that area.

MR. ETHERINGTON: Wouldn't it be a chloride ion before it became chlorate?

DR. MCNEIL: No. The evidence from the actual

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experiments. These reactions take place in solid salt, not in the solution.

MR. ETHERINGTON: Okay.

DR. SHEWMON: One of the bylaws of the ACRS says that for a subcommittee chairman to maintain his post, he has to have a break at least every two hours. So why don't we take one.

(Recess.)

DR. SHEWMON: Can we begin, please?

One of the questions I wanted to get your comments on, Mike, had to do with -- after a look at the program, it would seem to me that you have concluded that uniform corrosion you can handle and that pitting corrosion is the most important parameter that you should be able to handle, but you didn't have enough information on to handle.

DR. MCNEIL: I think that it is going to be relatively easy to judge uniform corrosion data.

DR. SHEWMON: You can answer yes if you want to.

(Laughter.)

DR. MCNEIL: The answer is that uniform corrosion, I don't think NRC needs to do a lot of work on. Stress corrosion cracking, we simply have to avoid. Stress corrosion cracking, as far as I can see, cannot be coped with over a multi-hundred year basis as a corrosion

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allowance thing.

Stress corrosion cracking will propigate. They will proprigate at crack velocities that make corrosion allowance unreasonable.

So, in that case, you just have to avoid pitting corrosion. Pitting is something you can allow for and discuss the rates of. That's, I think, why there seems to be a particular emphasis on pitting corrosion, because it's a situation in which we in DOE will probably agree. Yes, it happens but what is the rate going to be over a thousand years?

DR. SHEWMON: Do you then feel that anything coming out of the programs we've talked about so far, which I guess is Battelle and maybe Brookhaven, but we haven't differentiated them, will give NRC the information that's needed to try to predict what kinetics are over these longer terms?

MR. COSTANZI: The important thing again to realize is that what we're trying to do is be in the position to make an intelligent evaluation of DOE's demonstration.

DOE's demonstration says pitting at such and such.

DR. SHEWMON: They may come in and say it's all uniform corrosion because you have to be able to make the

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prediction whether they comment or not, if you think it's important.

MR. COSTANZI: We have to be able to evaluate.

DR. SHEWMON: Let's say they come in with uniform corrosion. You say it's all pitting corrosion. You can't go in without anything to back that up.

MR. COSTANZI: That's right. So we really don't need to know what the conditions are, which would say that pitting corrosion --

DR. SHEWMON: Okay, would dominate. But how do you get to say it dominates? You've got to say it's faster. If you say it's faster, you've got to have a rate equasion. So let me come back to the question.

DR. MCNEIL: I believe that this work will permit us to estimate, I prefer the word "bound", because I think that DOE will not claim that the pitting rates, let's say in the basalt groundwater, are below a certain curve.

I believe that our program will permit us to judge whether claims on pitting corrosion are believable or not.

DR. SHEWMON: How do you get that out of short-term tests that you were just talking about? Or, don't you?

DR. MCNEIL: One of the things that we're doing is trying to deliberately create old pits. That is, very

large pits. If you start a pit now, it is never going to get to be big enough. If it takes 200 years to make a pit the size of my thumb, we are making artificially very large pits which one would project from short-term data would be what one would have in any number which is convenient to you experimentally.

So you artificially make the pit. Then you study how the artificial pit grows, so we can set up a pit that is four inches deep and has whatever aspect ratio you want and then study how it grows.

DR. SHEWMON: You can create that geometry but can you also create a chemistry?

DR. MCNEIL: Yes.

DR. SHEWMON: How do you do that?

DR. MCNEIL: We take the groundwater, enrich it in chloride, pack the container with a slurry of groundwater enriched with chloride and magnetite, which appears to represent fairly well what seems to be found in long-term pits.

DR. SHEWMON: With a low aspect ratio pit, you've got uniform corrosion in the valleys.

DR. MCNEIL: It tends to sort of spike out the picture; the overall effect in some of these things tends to be that it tends to grow relatively uniformly. But, in the short-term, you tend to have spikes and irregularities

developing during the corrosion process.

In other words, even though the long-term effect of the pitting corrosion over the very long period may be uniform, if you do the measurements in a matter of weeks down in an old pit, you find considerable irregularities.

DR. SHEWMON: So low aspect ratio pits, I have difficulty understanding. After you've chewed the hole in it, it knows it should behave like a pit, instead of like uniform corrosion. Something must have started that pit there in the first place. That's by definition different from what goes on around it.

DR. MCNEIL: You see, it's being choked off down on the bottom. It still is enriched with chlorides and it tends to acidify because you do get reactions down in the pit. It tends to acidify. A deep, a low aspect pit, a very low aspect, clean pit is a contradiction in terms.

I had a viewgraph of one of them around here somewhere. These pits like we were talking about, the ones that look like this, they're never clean. This is in fact experimentally, when you find this in part, this area is invariably full of bits of corrosion product. It has to be because otherwise the hydrodynamics would make the electrolite down here like the electrolite out here. You wouldn't have any more force for pitting.

It's the fact that the pit chokes up and so you

have what amounts to sort of super crevice corrosion all over the bottom of the pit, is what keeps this type of pit going, we think.

DR. SHEWMON: Do you think you know enough about what chlorides and oxides and so on to put in to indeed emulate the behavior of the pit?

DR. MCNEIL: I don't but my contractors do.

(Laughter.)

DR. STEINDLER: You indicate a strong interest in magnetite. In some cases, particularly when you use sensible groundwater, the layers have not much to do with magnetite. You in fact get stealites and silicates that precipitate as corrosion layers on top of the metals.

It seems to me that in fact if that is a more accurate description of what goes on on the inside of your cavity which you have filled, or however you do it, it ought to make a significant difference in the quality of the results you get.

Do you have some way to determine that this artificial pit that you produced in some fashion or another and the rate equations that you obtain from it are in fact something more than just the rate equations for an artificial pit?

DR. MC NEIL: Relating the artificial pit to the real pit is a difficulty, particularly in the presence of mineralization.

The answer is you try to cope with this by studying the pits that grow in groundwater -- of course, these are fairly small pits -- and try new types of junk in here. What I think the answer is.

Our work along these lines has been quite limited. We are hoping in future times to spend more effort.

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We are aware of the fact that there is a detailed connection between mineralization and the corrosion process. We are not deliberately avoiding it. But it is something that we are doing as our limited resources become available.

DR. MARK: I have a very loose understanding of what we are talking about, some of these things here. You are interested in pitting corrosion so that water may get through the overpack. Then starts this famous leaching business at 10 to the minus 5th per year, or whatever it is.

If you have pitting corrosion over 10 percent of the area, is your leach rate then 10 percent of the things you had uniform corrosion over 100 percent of the area?

DR. MC NEIL: No. It is not that simple.

DR. MARK: I was aware it wouldn't be that simple. There would be no room for research if it were.

(Laughter.)

DR. MC NEIL: This question of leaching, of what happens when you have leaching through a failed surface, has been looked at by a number of people. In the particular case where you have a fuel rod after the overpack has failed and you have water in there on the fuel rod, cracks and holes in the fuel rod, the answer is that the work is being done. The answers that I have seen aren't useful.

DR. MARK: There isn't much tendency for the water to be flowing up and down the hole?

DR. MC NEIL: It is clear that there is only one pit through and that not much happens. The question of what happens when you have cracks or what happens when you have more than one pit, I have seen several different answers from different groups, and there doesn't seem to be any particular commonality.

DR. MARK: Anyway, there is some attempt to get some hold on that?

DR. MC NEIL: This is primarily being done by DOE so far as I am aware.

Perhaps Dr. Stahl would like to comment.

DR. STAHL: Dr. Stahl, Battelle Columbus Laboratories.

I am not aware of much work that is being done in regard to the surface area corrosion on the DOE side. We have a study going in the cell on spent fuel, which is addressing this particular area.

But if I may, just by way of reference to some work that has been done by DOE and also by our Canadian friends on oxidation, looking at defect sizes, drill holes, cracks, there seems to be a threshold on the surface that must be made available to the oxygen in order to get a substantial effect, and I suspect that similar analogous

behavior would occur with leaching, that you do need some threshold volume available of the fuel to the leaching.

DR. SHEWMON: Any other questions?

(No response.)

DR. SHEWMON: Who is the PI at Brookhaven?

DR. MC NEIL: Hugh Isaacs.

MR. COSTANZI: If there are no further questions on that one, we will move on to the next project, pitting statistics.

(Slide.)

A program underway at the National Bureau of Standards.

The objective of this program is to assess the variability of pit depths with pit age and environmental conditions. Basically, the scope of the work is to examine fluctuations in passivation current as a possible precursor to the onset of pitting and also to statistically analyze distribution of pit depths and size with pit age.

The results of this work are passed on and correlated with the work at Brookhaven and Battelle Columbus.

Again, this is related to the stability over time of the pitting process in expected repository environments.

That parenthetical statement reflects some of the work that has been done on the pit geometric studies that

have been done, which indicate that the repository environments may very well be in regions where pitting corro ion or general corrosion would not normally be.

Again, Dr. McNeil is the project manager of this program.

DR. SHEWMON: How long has this been going on?

DR. MC NEIL: About six months.

DR. SHEWMON: And the PI is?

DR. MC NEIL: Hugo Bertocci.

DR. SHEWMON: Where do you get your pits or cracks that they are doing statistics on?

DR. MC NEIL: These are done in carbon steel passivated with borates. There is a subcontract with the University of Manchester Institute of Science and Technology.

In fact, if you are going to do counting pits, you do large numbers of samples and you have to do statistics on the pits. You have really got to do it in an institution that has a large supply of cheap labor, of graduate students.

DR. SHEWMON: We still get back to the problem of extrapolating from one month to one century.

DR. MC NEIL: Yes. The answer is "yes." I will show you the sort of thesis.

There are two differen questions -- well, there

are several different questions having to do with statistics and pitting.

One of the trivial ones which DOE is working on -- it is trivial to state. It is not trivial to solve -- I don't have a viewgraph, but I will pass it around.

The pitting potential. Here are data plotted on probability paper for the pitting potentials measured on a number of nominally chemically identical steels.

This work was done by a Dr. Shibara, and it came out of the program of Professor McDonald, which is funded by the Office of Basic Energy Sciences.

If in fact the pitting potential were a state property like heat capacity, all these lines would go straight up and down because there would of course be only one value observable. So that is an issue of pitting statistics, which we are not dealing with because I have full confidence in Professor McDonald and in the Office of Basic Energy Sciences to cope.

We are concerned with two different issues, one of which has to do with how do you detect a difference between incubation times and situations in which you are not going to have any pitting.

Suppose I measure the corrosion current and I will measure the corrosion current as a function of time, and I have two cases. I have one case where the sample is

subject to pitting but it has an incubation time, and if I do it on a gross scale, I will discover that the basalt looks like this.

I have another sample where in fact pitting will never happen, and of course the answer is it just goes along the flat line.

Now, if you do all your measurements up to this point, how do you discriminate?

Well, the answer is that if you look -- if you fix the voltage very carefully with specialized equipment and look at microfluctuations in the current, you find that if in fact you are not going to have pitting at all you have a certain amount of noise in here, that in fact it just looks like this on a blown-up scale.

If, on the other hand, you are going to have -- this again is a severely blown-up scale, goes straight up and then decays -- if, in fact, on a grossly exaggerated scale -- this is microamps per centimeter squared -- if in fact you find that you are having a pattern like this in which you have little spikes that decay from time to time, that is a warning sign that in fact you are not in uniform pitting, that it is just incubating.

DR. SHEWMON: I thought you and Kassner agreed half an hour ago that pits would grow for a while and then stop.

DR. MC NEIL: I will get to that point in a moment. This is one part of the thing.

So we have the question of using the statistics of current, the very fine statistics of current to judge claims as to whether pitting will happen or not. This is a way of addressing the question.

DOE says here is a sample in which pitting will not occur, and this is a way of saying, of discriminating, where we say, okay, yes, we agree that the evidence is that pitting will not occur versus telling them, hey, you didn't run your experiment long enough.

Now, let us consider the following experiment. We take a large number of identical samples, and we put them in an identical environment, and every now and then we pull one of them out and we plot the number of the density of pit depth versus the pit depth.

This is in T-naught. We will find it looks something like that; that is, there will be a lot of very small, very shallow pits that probably will come down here, and there will be a few slightly larger pits. Now, we have T-1. It is greater than T-naught. And we have a further distribution. It may even have maximum.

The question is: as time develops what happens to this distribution?

The pattern in some of the arguments advanced by

the Department of Energy has been to assume in their calculations that all pits have the same depth. That is just a block distribution. And the delta function -- basically, the delta function just propagates bigger and bigger pit depths as times goes on.

This is clearly not the case. There clearly is a statistical istribution pit depth, and it is very important to know.

Suppose we have two cases here, both at T-1, and suppose T-1 is a long time. We can have two distributions that have the same mean, but one of them is very narrowly distributed actually. The second one may have the same mean, but in fact it may have a long tail.

This is what we have to look out for, it seems to me, in analyzing data. If you deal only with average pit depth, you can get a situation in which the average pit depth is still quite shallow, but if the pit depth distribution has a long tail out to long depth, you might wind up with holes anyway.

This would be typical where you had a lot of pits, most of them passivated but a few of them kept growing.

DR. SHEWMON: The question was: how do you extrapolate from a month to a century? Can you answer that?

DR. MC NEIL: I am not capable of saying how I

will extrapolate to a century. I am telling you how I extrapolate from short term to intermediate term, a matter of a few years.

We take this, and we analyze the tails, and we try to determine -- I would like to find a demonstration that there exists a function -- say the steel, water, DT, which I will call phi -- such that the limit, as T approaches infinity and that D approaches infinity, and N of D equals phi -- what I am hoping to do is to find that for all available beta that we can get in short and intermediate terms we can find an asymptotic value for this distribution function and that we can then tie that in in some fashion to our studies on artificial old pits.

DR. SHEWMON: That was written just for you, Carson. Go ahead.

DR. MARK: Thank you.

I think I understood most of that.

Is there a means today of doing a microscopic survey of a surface and saying there will be a pit here and a pit there, not in the middle?

DR. MC NEIL: No, not until they start.

DR. MARK: There ought to be.

DR. MC NEIL: Yes, I agree. We have in fact a technique which can map the microcurrents at a surface and can pick up pitting before you can detect a pit by any

optical means.

I mean, I can artificially say, look, I can tell you, for example, that in certain types of enclosures there are likely to be nuclei for pits. I can tell you that if you take a punch -- given the trivial examples of surface abrasions, impurity concentrations, and so forth.

But given a sheet of superficially uniform carbon steel of uniform chemistry, I cannot tell you where the pits will and will not propagate until we put it in the electrolyte.

DR. MARK: Well, look, I certainly don't mean to argue with anything. On the other hand, a uniform surface will not promptly develop pits.

DR. MC NEIL: It is not quite true.

DR. MARK: It has got to have centers where things are going to get their teeth in.

DR. MC NEIL: That is not actually quite true because the model of why, of how pits initiate is a picture of defects in the surface, atomic scale defects, and when you get -- statistically speaking, you will now and then get an abnormal concentration of defects at this point or that point. That is the point where the pit will start, and it may be entirely identical to the other points on the surface. But it just happens, just from statistical principles, that this is where there were too many

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vacancies at one point.

DR. SHEWMON: The scale on which it is uniform, I think, is the example.

DR. MARK: I was thinking of some of the kinds of things where you examine the variations in crystal structure.

DR. SHEWMON: Let me bring one question up on this. You said inclusions don't necessarily go very deep. Once you have this instability there, the fact that you have run out of inclusions doesn't stop the pits, is that right?

DR. MC NEIL: No, it does not.

DR. SHEWMON: Thank you.

DR. MC NEIL: One other thing I would suggest is that a lot of work has been done on the subject of relating -- you see, pitting is a statistical phenomenon, but the properies of atomic defects are deterministic, and Professor McDonald, who was in Dr. Shewmon's department, has just become head of all chemical research for Stanford Research Institute and has done a good bit of work on demonstrating how you can reconcile the statistical nature of pitting properties, such as those curves I sent around, with the deterministic nature of atomic defects, and I am sure he would be happy to give you some of his reprints.

DR. SHEWMON: Has there been any DOE work in this area that you can try to check and reproduce to see what the

state of the art is on transportability of results from one lab to another?

DR. MC NEIL: Dr. Isaacs is doing some work for DOE. Some of his work on mapping the microcurrents on surfaces is for DOE.

DR. SHEWMON: That is not quite an answer to the question.

Because I suspect what you are going to end up, and what you suspect also, is that between one piece of steel and another you are going to get different results. You are also aware that on experiments like this one laboratory doesn't do the same thing that another one does.

So the fact that the tide is rising on odd days for you and on even days for DOE doesn't help much.

What might come from the West Coast, Brookhaven, or Columbus --

DR. MC NEIL: Well, we are circulating samples, and the group at Battelle Columbus has deliberately attempted to reproduce and succeeded in reproducing an experiment done for DOE by Dr. Diegel at Sandia.

Also, we do try as far as possible to maintain supplies of steels, distribute supplies of steels, so, as far as humanly possible, everybody knows that they are working from the same piece.

DR. SHEWMON: Any other questions on this project?

(No response.)

MR. COSTANZI: The next project in the overpack program is the containment assessment program.

(Slide.)

The viewgraph has a typo in there. It should be VNL, not BCL. This is a program which is finished. The objective of this program was to examine the questions of crevice corrosion and hydrogen embrittlement in titanium alloys and carbon steel overpack materials.

Experiments were performed to assess crevice corrosion in titanium and hydrogen uptake and its consequences in steel overpacks in the sale and basalt repository environments.

The reason for doing this was to assess whether or not crevice corrosion and cracking in a salt repository was a significant problem with titanium overpacks.

The second question was whether or not hydrogen embrittlement is going to be a problem for carbon steel overpack materials. This program has finished.

DR. SHEWMON: Why did you do hydrogen carbon steel?

MR. COSTANZI: Originally, we concluded that wasn't going to be a problem. However, some recent work at

Battelle Northwest has indicated that there might be cracks when we get on to the work at Battelle Columbus. It might be a more appropriate time to discuss that.

DR. SHEWMON: We already had Battelle Columbus. There's something here on long-term performance of the waste package. We haven't gotten to that yet.

MR. COSTANZI: I have slightly rearranged the presentation from the listing in order to group the material on overpack. The next grouping will be waste forms.

DR. SHEWMON: Is that the copy of the agenda you're working with? It's not the copy that I have.

MR. COSTANZI: I'm afraid not.

DR. SHEWMON: Go ahead.

MR. COSTANZI: Maybe we ought to move on to that last project.

DR. STEINDLER: Before you leave that one, you indicate, at least the documents that we have that, number one, the program has been closed; and number two, that one of the conclusions seems to have been that titanium alloys are not to be used in civilian high level waste.

To what extent are your results transmitted backwards to DOE on this issue?

MR. COSTANZI: DOE was informed of the results that we derived from this work.

DR. STEINDLER: When did you close this program

out? Or, let me put it this way. When were the results available?

MR. COSTANZI: The results were available essentially almost as they happened. Our topical reports and quarterly reports are made publicly available. So when is the final report of this program?

DR. HSU: Peter Hsu, Brookhaven Labs. I think that the last report was published about a year ago. The program started around about 1979-1980, starting out with Tycho 12 as a principal containment material, because the basalt project people had as one of their prime reference material.

We do publish quarterly progress reports which have national distribution, 350 copies all over the country.

DR. MCNEIL: Let me say one thing though. It's a slight exaggeration to say that Brookhaven said that Tycho 12 was inappropriate. Brookhaven said that Tycho 12 demonstrated crevice corrosion in the repository environment.

DR. STEINDLER: Let me read you from what I've got in front of me. In effect, it says the results of this program, which has been phased out, were used to support the NRC's contention that titanium alloys ought not to be used as overpacks for civilian high level waste.

DR. MCNEIL: NRC may say that but Brookhaven does not say that.

DR. SHEWMON: And the basis for it was crevice corrosion?

DR. MCNEIL: Right. Crevice corrosion was one problem. There was another problem, however, that had to do with high magnesium brines. Does anybody want to know about that?

(No response.)

DR. MCNEIL: Okay.

MR. COSTANZI: The last program on overpack research is the Battelle Columbus program, the long-term performance of high level waste package materials.

(Slide.)

With regard to the overpack, again, the overall objective is to identify the likely overpack failure mod.s under expected repository conditions and what the scope of the work is to observe the rates and conditions for various overpack failure modes. General corrosion, heat corrosion, stress corrosion, pit corrosion and embrittlement.

Of course, what we're trying to do is provide the technical basis for assessing the DOE's demonstration of overpack integrity to contain material for 300 to 1,000 years. The programs that are being done, the research to be done under this program, are the potentiodynamic

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polarization tests, identifying regions where stress corrosion, pitting corrosion or general corrosion will take place in basalt repository environment.

Strainright tests for stress corrosion cracking for overpack materials, pit propigation experiments, using artificially produced pits with various aspect ratios to see in effect how pits propigate hydrogen embrittlement tests. And, lastly, in a modeling program for identifying the rate controlling parameters of corrosion.

MR. ETHERINGTON: Would you say a little bit about hydrogen embrittlement?

DR. MCNEIL: We found the experimental that we had had would be collected on what I would call straightforward samples cut from the appropriate steels. It indicated that although there was some loss of ductility in the carbon steels due to hydrogen uptake, under no believable hydrogen activities for the repositories did this lead to any really significant problems.

Unfortunately, the tests were done without welded samples. When we began to get welded samples and began to get new tests in the heat-affected zone, we found recently catastrophic loss of ductility in some parts of the feed-affected zone due to this hydrogen uptake.

MR. ETHERINGTON: Is this hydrogen uptake or hydrogen that was in the well to begin with?

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DR. MCNEIL: This was a case where we took two identical wells. We treated one of them with hydrogen and the other one -- we did one of them in a hydrogen atmosphere and the other in a nitrogen atmosphere. We found there was a radical difference in the ductility.

So this is pretty clearly due to the post-weld atmosphere.

MR. ETHERINGTON: At room temperature?

DR. MCNEIL: Yes.

DR. SHEWMON: If you held that at 90 degrees C., would you expect to find that had any effect? Usually, hydrogen brittleness stops around 100 degrees C. This is what I carry in the back of my head.

DR. MCNEIL: You certainly know more about that than I do, yes. Hydrogen embrittlement is something of a low temperature phenomenon, but the containers will get cold eventually. Unfortunately, well, we hope to undertake more work of hydrogen problems on areas that have had thermal histories consistent with the heat-affected zone in future but we're doing relatively little on it now.

(Slide.)

Another question that I will mention at this point has to do with hydrogen damage. The question has been raised several times at Brookhaven about whether we have to worry about hydrogen damage, particularly since we're

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talking about very long times. There's lots of time for hydrogen to go into the steels. It appears to me that, except possibly for very heavily loaded containers in the salt repository, you probably ought not to have to worry about it, because if you look here at the hydrogen pressures, this is, of course, Fahrenheit unfortunately.

Even for a really heavily loaded container, a really hot container in the salt repository with lots and lots of hydrogen around, you are really marginal, marginal, marginal in terms of this diagram.

So we may eventually look at hydrogen damage. But we've not done it up to this point simply because we've had more alarming problems to look at.

DR. SHEWMON: Can you have hydrogen potential for a long time? I would think this would only occur in the area of very active corrosion.

DR. MCNEIL: Well, in the salt dome proposal, deganners are decomposing the salt into sodium chlorine. The sodium is reacting with the brine. You've got hydrogen being generated from the brine by just straight radiolysis of the brine, which produces hydrogen peroxide and hydrogen is a single species.

You've got hydrogen being produced by radiolysis of the salt in contact with the brine, followed by reaction of the sodium with the brine itself. And you've got

corrosion hydrogen. So I would say, in the particular case of the salt repository and also the salt repository has this self-sealing characteristic, which means that hydrogen at above 1 atmosphere finds it less easy to leak out than something above 1 atmosphere would from a normal mine.

So I'm not completely prepared to dismiss this as a problem in the salt repository though, as I said. We have not regarded it as a high enough priority to justify any investment of resources up to this point.

DR. KASSNER: In this program, they are also doing some modeling work on the radiolysis of the groundwater.

DR. MCNEIL: That is correct.

DR. KASSNER: Has any conclusion been made over the last year that enough of these projects are generated to have any influence on the erosion process?

DR. MCNEIL: I did some back of the envelope calculations, given what we knew about the site, and concluded that, yes, they were significant.

DR. KASSNER: Do you have any idea which ones?

DR. MCNEIL: Hydrogen peroxide.

DR. SHEWMON: This is just in salt or this is in basalt also?

DR. MCNEIL: My judgment is that in any situation where you've got groundwater in contact with the container,

before the cesium and strontium are all gone, the radiolysis of groundwater will produce significant perioxide.

DR. STEINDLER: Does this conclusion take into account the design of the salt container, which is thick-sectioned material, in order to avoid radiolysis?

DR. MCNEIL: It used the last data I had on the basalt container, and I forget the exact thickness. But, as I recall, it was of the order of 10 centimeters. I don't have the calculation and I do not remember the exact figure, but I was using data that were available from the basalt site and their anticipated design.

I will say that one thing that might happen, however, one option that is being discussed, that DOE research has put forward, I gather, to somebody in the DOE waste program, is the idea of putting into the backfill or into the mineralization some species that catalyze the recombination of the radiolysis products.

Apparently, there are iron salts whose nature I can no longer remember. This came up in a DOE meeting that I attended, that have the effect of in fact catalyzing the recombination of hydrogen perioxide and greatly reducing its stability.

This idea has been put forward to the DOE waste people; what action if any they intend to take on it, I do not know.

DR. STEINDLER: If you have the opportunity, it may be worth your while to engage yourself in a discussion with the BWIP folks on the extent of radiolysis.

DR. MCNEIL: I would love to do so if they'd talk with me.

DR. SHEWMON: The rates for various failure modes that were used to describe this, again, let me harp on on. month to one century or one decade, or whatever. Failure modes indeed mean failure modes, not corrosion modes.

Can you identify failure modes here? If so, what do you think the project will do for the rates?

DR. MCNEIL: In this program at this point it addresses only penetration of the overpack. I will call upon someone from Battelle who may be doing work that I'm unaware of, but I am not aware of any work in the corrosion part of the program which addresses the question of the rate of escape of radionuclides. And, for that matter, the definition of failure.

DR. SHEWMON: I'm taking maybe out of context the words I heard from Costanzi five minutes that there were rates for various failure modes.

MR. COSTANZI: No, I'm sorry. I was talking about what are the rate controlling parameters of the various forms of corrosion.

Failure mode, I'm talking about the mechanism,

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the pitting corrosion, stress corrosion, whatever. Those are the things I'm talking about.

Again, let me emphasize that the definition of failure in terms of a regulatory definition would be, for example, a certain percentage of the overpack no longer being there.

What we are trying to do in our materials research program dealing with the overpack is to identify under what conditions, what kind of mechanism is going to lead to loss of overpack integrity. However you define failure. When you presumably might mean percentage.

DR. SHEWMON: The problem is you still have to get at rates before you can say what will decrease integrity and make it lose integrity.

MR. COSTANZI: The corrosion mechanisms do compete to a certain extent.

DR. SHEWMON: I've heard of that. Zimmer's anyway.

MR. COSTANZI: And if you're in a condition in which pitting corrosion is going to be the dominant corrosion, then what you're worried about is what is controlling the pitting corrosion. You can do that in an environment in which there is no passivation and is going to be general corrosion and you can worry about what's controlling the rate of that.

DR. SHEWMON: It's a very difficult problem and the reason I'm appearing to be hassling you, or am hassling you, I guess, is that I don't think you're going to be able to predict it. And what you're waffling on is the fact that you can.

MR. COSTANZI: I'm sorry. Can't predict what?

DR. SHEWMON: The rates of penetration through more than four inches of steel.

MR. COSTANZI: Yeah, I think we do. We have a model, that is to say, a concept, right now of the stability of the process, or a way to extrapolate through short-term tests to either the kind of corrosion which will end up failing the waste package over time or the stability of that process.

If the rate changes, you're absolutely right, we don't. But that's the point in doing all of this work. We're trying to figure out to what extent can you do that. And to what extent you can do that, what that solution looks like.

DR. SHEWMON: Any other questions?

DR. STEINDLER: I have one. Again, the material, the research summary that we have, has an objective for this program, which looks a little different from the statement you have up there. Is it in fact true that the long-term performance of the high level waste package materials

program at BCL deals only with overpack?

MR. COSTANZI: No. The statement that appears up there applies only to the overpack portion. The program at Battelle Columbus has a much broader program. It's looking at the whole waste package.

So we're looking at not only the overpack but it's also looking at the waste form itself, and it's looking at the waste package as a combination of waste form and overpack in the respository environment.

So the statement before you now is simply a different piece of Battelle Columbus program.

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DR. SHEWMON: When do we get to the other part?

MR. COSTANZI: The waste one is coming up next.

DR. MARK: Could I ask a question here? You are looking at basalt-tuff, basalt, with half the work on basalt at the moment.

You will ultimately get a picture, I suppose, of the different conditions you have to meet in one of these media from one of the other media, and you will even get a picture that it is easier to live in one than another perhaps.

Are the Canadians doing the same thing with respect to granite?

DR. MC NEIL: A considerable amount of work is going on in granite in Canada and the United Kingdom.

DR. MARK: Do you have a way of guessing now -- is that a good or bad bet -- for the kind of applications you are thinking of?

DR. MC NEIL: I think that it is a very significant question that has to do with the basic issue there, which is the greater flow of groundwater. Basically, it is my gut feeling from the work that I have seen that if you have got a good -- and I am putting the word in parentheses -- granite deposit in the sense of water availability, it is not at all a bad situation.

DR. MARK: But it is premature to make such a

conclusion?

DR. MC NEIL: In all these cases, the question of the actual conditions in the mine, particularly the geochemistry and hydrology, are so critical it is like asking me whether a salt site is a good idea. Some salt sites are really terrible. The question is: can you find a good one?

DR. MARK: But you are, in any event, reasonably sporadically informed of the findings these other people make?

DR. MC NEIL: I am in pretty good touch with the UK group because I know the people there. The Canadians I get reports from.

DR. MARK: Thank you.

DR. SHEWMON: Tell me briefly what the difference is between basalt and granite. They are both igneous; one is crystalline?

DR. MC NEIL: I have a feeling you ought to ask a geologist. We have got a geologist from our branch behind us. A geologist can tell us the difference.

DR. HACKBARTH: My name is Claudia Hackbarth, with the staff.

The difference between basalt and granite. There is a compositional difference in that basalt is much more mafic. That stands for more magnesium and more iron. It

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crystallizes at a very high temperature.

Granite is more sialic. It has more silicon and more aluminum and less magnesium and iron and would tend to crystallize at a lower temperature.

There is also a textual difference in that basalt is a rock that has solidified from magma very quickly, so that there are very few crystals in it and quite a lot of glass; whereas, in granite it is cooled slowly and crystals were able to nucleate and grow and it has no glass in it.

DR. SHEWMON: Does that mean that the granite tends to fracture more because of its coarseness than the basalt or not?

DR. HACKBARTH: No, I don't think you could say that. They both fracture pretty well.

DR. SHEWMON: Thank you.

Nobody is talking seriously about granite repositories?

MR. COSTANZI: No, that is not true. The prime candidate for the second repository right now is in granite. DOE does have an active program looking at granite, crystalline rock, and we will be turning our attention to granite in 1987, FY '87.

DR. SHEWMON: Okay.

MR. COSTANZI: The next one is -- again, we want to talk about the long-term performance project at Battelle

Columbus.

(Slide.)

Now, we want to turn our attention to the waste form itself.

DR. STEINDLER: I am sorry, before you leave that long-term performance of the metallic components, that program has been going for some years.

Have you arrived at a statable conclusion in terms of the original goal, the definition of rates and extrapolated all the figures in, let's say, carbon steel?

DR. MC NEIL: I believe that we shall have a usable model for the growth of large pits in carbon steel. We have drawn several preliminary conclusions with regard to other issues.

If I state a research conclusion, our present experimental basis indicates that unless radiolysis introduces some new complication, which they may well do, we have got to deal with that, also. If DOE uses their present designs, does good centrifugal casting, careful welding, stress corrosion cracking will probably not be an issue.

We have already told you my conclusions. The thing is there is no one conclusion. You are getting a number of different conclusions.

But I must point out that these are preliminary research conclusions, and we made the condition that

radiolysis not introduce any new complexities into the reaction.

It is a very, very big "if."

MR. COSTANZI: Let me state that conclusion in a bit different way.

We do believe that we will be able to evaluate and conclude whether or not we agree with DOE's demonstration of overpack performance in a basalt repository, especially if they make an argument such that stress corrosion cracking would be a problem and the waste package is likely to lose its integrity. I think we will be able to make a reasoned judgment as to whether or not we think they have supported their case.

We think we are in a good position or will be in a good position to do that.

DR. STEINDLER: Would that conclusion also hold for the enclosure area?

DR. MC NEIL: Assuming good welding practice, I believe so. And again, remember there is another big "if" here.

WGT, a part of the Division of Waste Management, and we have had to form our views of the groundwater chemistry based upon what BWIP has given us. That is assuming again that we are not seriously in error on that.

DR. SHEWMON: Might we postulate that pitting

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will be the controlling process here?

DR. MC NEIL: We have had no meeting on waste packaging for several years, but insofar as we are aware, they seem to focus mostly on general corrosion.

MR. COSTANZI: Dr. Shewmon, was your question on the rate and what we are doing relative to extrapolation of short-term tests and long-term performance -- was that sufficiently answered?

I feel uncomfortable.

DR. SHEWMON: The question was answered as well as it can be answered.

MR. COSTANZI: I still feel uncomfortable. I may not have understood your question.

DR. SHEWMON: I feel uncomfortable, too, but I think neither one of us is going to feel any more comfortable.

You do not have models which, with any assurance, can predict behavior of kinetics for 300 years by pitting.

MR. COSTANZI: Today that is true, but that is not -- you know, we are proceeding to see if that can be done.

DR. SHEWMON: But they have a comfortable feeling or at least feel that they are making progress with the model?

MR. COSTANZI: Yes, on a number of fronts.

A number of our programs are doing that, yes.

DR. SHEWMON: I am a little confused on what you just got done saying about BWIP, although that is not the main purpose of what we are doing.

I had thought you were going to be judging it, you and DOE would assume or would agree that the canister would probably hold out for an adequate period of time and that they would improve it or assert it on the basis of general corrosion and you would agree with them on the basis of having evaluated the pitting corrosion.

MR. COSTANZI: I hope not. If DOE, indeed, makes the argument that pitting corrosion is not going to be a problem, I believe that we will be able to judge whether or not they have proven their case.

That is what we are talking about. If we suspect -- and it is a suspicion -- that there is not likely to be general corrosion or pitting corrosion, then they will have to make that argument. That is presumably the argument they will make.

But whatever argument they make, we feel that we will be able to say you have not proven your case or you have proven your case.

DR. KASSNER: Don't you think that beyond that that what you, NRC, should be looking at are the consequences of, let's say, a premature failure of the

canister under the two different modes?

MR. COSTANZI: Absolutely.

DR. KASSNER: After you decide that, you might say, hell, it is not important. The egress of radionuclides through a pit or group of pits is really not going to change the picture at all. In other words, that is still going to be the slow buildup and the release of these things to the environment, depending on the model.

MR. COSTANZI: There are two aspects of consequence. There is a requirement which DOE must meet on containment integrity from 300 to 1000 years, and they must demonstrate that the radiation will be substantially contained within the waste package for that period of time.

DR. SHEWMON: Do you think that would be enough, for substantial containment?

MR. COSTANZI: Substantial containment. This is the question which the Division of Waste Management is addressing, its technical assistance program. But the question of whether containment is substantial is of two points:

One, the premature release, which ends up being a premature release of radionuclides to the environment and subsequently is a violation of the standard promulgated by EPA as well as the 10 CFR, Part 60, there is another consequence which relates to the subsequent performance of

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the repository itself in terms of the controlled release requirements and the containment on the downstream waste packages.

So in terms of whether or not premature failure of one package is significant with respect to meeting the containment requirement -- is a threat to the public health and safety is one aspect of the question.

The other question is: does premature failure of one package grow like a cancer throughout the repository and then the premature failure of everything.

Those are two different kinds of questions.

We need to examine both of them.

DR. SHEWMON: Do you want to answer your question again?

MR. COSTANZI: We in Research are not doing that.

MR. KASSNER: I think I'll pass. But you know in the reactor you run with essentially failed fuel elements. You know, you can tolerate a certain number of minute leaks in fuel elements and people have learned to live with that.

I am just trying to say the situation might occur here.

MR. COSTANZI: If you're asking the question do we know whether or not we can live with that, the answer right now is no, we don't. And the reason that we don't know is because we haven't looked at the question. And the reason we haven't looked at the question is because the set of questions which we have to answer is much larger than the dollars we have available to answer those questions.

MR. KASSNER: In addition to the modeling that you're doing on these atomic processes here, I think someone should be looking at this so that they can put some of these in perspective. And that's the only comment I was making.

MR. COSTANZI: It's just that we're not doing it now.

DR. SHEWMON: Bring it up this afternoon.

Any other questions on this?

(No response.)

DR. SHEWMON: Let's go on.

MR. COSTANZI: I would like to then talk about the waste form. And Dr. Stohl of Battelle Columbus is the principal investigator on this project. The staff member, Dr. Peter Kihm, who is the project manager of this project, is not available today. He is on travel.

The objective of this part of the program is to assess the range of expected performance of high level waste glass and spent fuel in its high level waste form. What is being done is that laboratory experiments are being conducted on the leach properties of high level waste glass under a variety of conditions of the spent fuel.

Again, it is to provide NRC with a basis for evaluating DOE's demonstration of performance of the waste form, that performance demonstration being part of DOE's argument that it will comply with the controlled release requirements of 10 CFR Part 60.

In terms of the glass experiments that have been conducted, experiments have been done to test the predictions of glass in solution model and the effect of crystal size on the leachability of the glass has been investigated. The effect of organic acids which might be natural organic acids that might be found in the repository

on leachability of the glass has been investigated.

The spent fuel program is in its infancy at Battelle Columbus, and they are now developing -- have developed a test plan for looking at the properties of spent fuel as a waste product. So Dr. Stohl is here to answer questions on that program.

I would also like to introduce Dr. Means, also of Battelle Columbus.

DR. SHEWMON: Do you feel that the spent fuel program is going to be one more of the stability of Zircaloy in general or the statistics of a few defects?

MR. STOHL: Well, we are looking at the entire system; that is, the Zircaloy irradiated UO2 system. And the primary emphasis right now is on developing techniques that we can use with spent fuel, beginning with unirradiated UO2. That's the focus in the near term.

Later on we will be looking at what we call separate effects or combined effects with Zircaloy. At the same time--

DR. SHEWMON: I don't know what you said. Did you say you are going to restudy the corrosion rate of Zircaloy in the first stages of the project?

MR. STOHL: (Shaking head negatively.)

DR. SHEWMON: What data will go into your model? You can get anything out of your model that you put in, I'm

sure, or you can get any answer you want if you put bad enough data in.

MR. STOHL: Well, I'm not sure of the thrust of your question.

DR. SHEWMON: The thrust of my question was:

Do things that in the real fuel that you're going to -- somebody will put underground that the main problem will be general corrosion or defects at a few points? When you said "We're making a model --"

MR. STOHL: I didn't say we're making a model.

DR. SHEWMON: A systems approach?

MR. STOHL: A systems approach. In the near term we are looking at the spent fuel assuming that we have defects in the cladding.

Later on we will be looking at the cladding itself, but we don't feel that, again, general corrosion of Zircaloy in these groundwaters is a problem, but that localized corrosion may be a problem. And we have done some literature review to indicate that under some conditions, under our problems, even with Zircaloy you get stress corrosion cracking and pitting can be a problem as well as hydrogen embrittlement.

So those are things that we will be investigating probably in Year Five of our five-year plan of our program.

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DR. SHEWMON: To what extent have you learned anything from fuel that has been in pools for ten years, or is that temperature too low?

MR. STOHL: That's a very controlled chemistry which is high in boric acid. The temperature is lower but there are things that certainly you can learn from that, but not much, I don't believe, that can be extrapolated to localized attack.

DR. SHEWMON: And you think that the defects that will come are from localized attack, not manufacturing defects?

MR. STOHL: That's correct. In our integral test program which we will get onto perhaps a little bit later on, we are studying, as I mentioned in a comment earlier, intentionally-defected Zircaloy fuel rods along with in-service defects to try to understand or to try to determine whether there are things happening there under those conditions that we were not able to consider or predict on the basis of separate or combined effects testing.

DR. SHEWMON: In general, what is known about the leachability of this fuel?

MR. STOHL: Well, UO2 is a fairly resistant material. The leachability is related very strongly to the oxygen that's available in the groundwater.

Again as was mentioned earlier by Dr. Costanzi and others, we need to have a very good handle on what that groundwater chemistry is, and its oxygen content. Now most of the tests that have been done so far, for example at PNL, have not been very strongly controlled as far as oxygen level, and those tests are now being repeated.

DR. SHEWMON: Is there much redistribution of fission products after the clad/fuel interface in commercial power elements?

MR. STOHL: Well, it certainly is there because you have a very high temperature gradient. The situation that we have in a repository, you have a fairly flat gradient across the fuel and it operates at lower temperature.

DR. SHEWMON: I'm aware of all that. My question was-- My background is much more in fast reactor fuels which are run at both higher temperatures and higher temperature gradients.

MR. STOHL: Yes.

DR. SHEWMON: And there you do get redistribution of fission products after this interface.

MR. STOHL: That's correct.

DR. SHEWMON: Is there as much, or almost no transport of fission products after this interface, or what ones transport?

MR. STOHL: I don't have any hard data but I think that from what we know right now, we would expect very little transport due to thermal processes. What you may have is dissolution-enhanced diffusion after your tube breaks up.

DR. SHEWMON: When this thing comes out of a reactor is there an enrichment of fission products at the fuel/clad interface?

MR. STOHL: Certainly.

DR. SHEWMON: Okay. What is known about that then? Which ones enrich? And is it appreciably less than in fast reactor fuels, or do you happen to know?

MR. STOHL: You're talking about looking at fast versus light water reactor fuels?

DR. SHEWMON: I'm talking about light water reactor fuels before anybody even thinks of putting it underground.

MR. STOHL: Oh, certainly you do have your more volatile materials throughout the interface, your cesium, strontium, iodine. That is going to be at or near the surface. We do know from microprobe examinations what the distributions of those materials are.

And certainly in many of the tests that we will do and that PNL has done for DOE, you will remove those materials early on in the leaching process.

DR. KASSNER: Dave, the number of fuel elements that seem to be coming out of reactors that have defects is apparently going down to almost negligible in recent years.

MR. STOHL: That's correct.

DR. KASSNER: So the point is that I'm just curious as to how much emphasis should be placed on leaching of fission products on a very mildly defective cladding. In other words, we're not splitting these rods open. The defects are so small and tight usually that they can't even be found, and the only way you can detect them is by fission gas coming out during operation.

MR. STOHL: Yes.

DR. KASSNER: So I was wondering how much emphasis we should even be looking at. In other words, the failure rates are down to a couple hundredths of a percent. As a consequence of that, NRC and other people aren't even funding much in the fuel performance area any more.

MR. STOHL: Let me respond to that in two ways:

One way is that we don't know what credit DOE is going to take for the cladding.

The second point is that the early fuel is more likely to be put into the repository sooner than the fresh fuel, which means that that material will have a greater defect rate and therefore, be more likely to give concern.

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DR. KASSNER: The other comment is that the stuff that has been laying around in ponds for 20 years apparently is performing very well. In other words, there is no degradation by virtue of storing it the way they are now. And in fact, I hear of cases where you can take the stuff out and stick it back in the reactor and reirradiate it and it performs well.

So apparently the cladding with the Zircaloy or oxidized coating on there is pretty impervious to insults from laying around in the spent fuel pond for a long time.

So the only thing I'm saying is I don't know how much relative to other things you might be doing that this work should involve. It might be a difficult problem to study and then, after you do it, the consequences might not be so large. And maybe you could anticipate that, based on what is already known.

MR. COSTANZI: This is a similar situation to one of many we have, that DOE -- we do not know whether or to what extent they are going to take credit for the cladding as a containment barrier or as a barrier to the leaching of the fuel after containment is lost.

We need to study the effect of the degree to which the cladding integrity has been violated on the leach rates of spent fuel because we are going to need to address that question in licensing hearings.

DR. SHEWMON: A matter of clarification:

You say you need to study the effect of the integrity or deterioration on leachability, but what he was saying is that indeed, there is a substantial amount of evidence that says there is no deterioration, that these things are going to come out at a maximum one in a thousand having a very tight leak.

MR. COSTANZI: There is no guarantee today that after the spent fuel is pulled from the pool, stored in an MRS and the assemblies have been disassembled and the pins consolidated that the integrity of the cladding which you're observing today in spent fuel pools will be the same as the cladding which goes in the repository.

DR. SHEWMON: That's quite possible. On the other hand, they do take these things apart and reassemble them, so unless they get pretty ham-handed in the repository, the technology is available for doing these things. So it's possible that they may run trucks into them, but....

MR. COSTANZI: I don't think the insult has to be that large.

DR. SHEWMON: It probably doesn't, but I don't think the insult probability is too likely if they indeed use reasonable care, is my point.

MR. COSTANZI: I have no basis for agreeing or

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disagreeing with your statement.

What we are acting on in terms of doing this research is the presumption that we will be asked if 10 percent of the pins have cladding defects, what effect does that have on the leach rate of the fuel. We are going to have to address that question.

DR. SHEWMON: You also ought to address and if it is one percent and one-tenth of one percent, because one-tenth of one percent is an upper limit as to what is coming out of the reactor.

I don't remember ever hearing of more than-- One in ten thousand is what they're talking about now. My impressio is that if they get an order of magnitude above that, they have to take it out just to meet operating limits. So I doubt if you are going to have very much stuff that is any place near ten percent or even one percent, simply from what they were allowed to operate the reactors with.

MR. STOHL: I think what you're doing is quoting current practice.

DR. SHEWMON: No, I'm not. Current practice is well down towards one in ten thousand. You can check it out.

MR. STOHL: Yes, I agree with you.

DR. SHEWMON: And that is not what I said. I

said one in a thousand, which is ten times as big. And I don't know what--

MR. STOHL: I'm saying that in the distant past the failure rate was larger than that. When we were learning about fuel and fuel/pellet interaction -- fuel/cladding interactions, -- excuse me -- fuel densification, there were a lot more failures in LWRs than we have currently.

DR. SHEWMON: But I doubt if you ever got up to ten percent. If you got to one percent and operated with it, I didn't hear about it.

MR. STOHL: I am not familiar with the numbers. I can't quote them. There were many failures.

MR. STEINDLER: Is the thrust of your effort then going to be to answer the question that you just posed? Namely, if you have some fraction of failed elements in a repository, what is the expected release rate of whatever fission products you are interested in?

If that is in fact the thrust of the program, do you have some kind of a sufficiently generic model of what the geometry of the system looks like in order to be able to obtain an answer that is extrapolatable to the real world?

MR. STOHL: Our objective is not to create a model; our objective is to try and get an understanding of

what 'mportant phenomena are occurring. And we try certainly in our test program to be as prototypic as possible.

MR. STEINDLER: It isn't at all clear that a simple understanding -- and I use the word advisedly, "simple" -- a simple understanding of the phenomena will allow the regulators to quantify the output which is really their job.

I guess I have to go back to the relationship between whatever data your are planning to get, which apparently does not include models, and the application of those data in an assessment to determine whether or not the Department of Energy can meet regulatory criteria. How is that translation going to be made?

I mean supposing you have an infinitely detailed understanding of the reaction of water with spent fuel of some particular heat generation rate, burnup, and isotope distribution. It isn't at all clear to me how that relates to the real problem of a leaky fuel element in a repository sitting in basalts.

Who is going to make that translation, and how specifically, or even generally, are you going to get from your data to that bit of information that is obviously going to be needed by somebody?

MR. COSTANZI: The overall performance of the

waste package, which would include the performance of the spent fuel in the repository environment, is a question which again is being addressed by the Technical Systems Division of Waste Management.

What we are trying to do in this program is provide input to that effort. We are not really trying to directly make the connection of if the behavior or the leachability of spent fuel is so much, then what does that mean?

In this program what we are trying to do is determine what determines what environmental conditions or groundwater conditions of the cladding in the integrated experiments that we are doing, the condition of the overpack, have on the ultimate leach rate or leachability of the waste water, the rate at which radionuclides are leached from the waste form into the underground facility.

Putting that together in a coherent model by which you can take raw data and design data and measurement data on waste forms from the Department of Energy and crank it through several calculations to end up with a specific release over time by release rate is not the objective of our program at this time.

DR. SHEWMON: You are setting up a straw man to a certain extent. This afternoon, as you well know, we will hear from the Aerospace people who are trying to set up a

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model which is being funded by the NRC which somebody apparently feels will be useful to the NRC.

Have you ever had any requests from them, or ever asked them what sorts of data in this area they would need for their model?

MR. COSTANZI: We have interacted with both the Division of Waste Management project manager and the Aerospace researchers very frequently in this program. Dr. Stohl has attended a number of the program reviews of that Aerospace program at the request of the Division of Waste Management, working very closely with them.

DR. SHEWMON: I think that's an answer of Yes to my question, but I didn't get a Yes.

MR. COSTANZI: Yes, that is an answer to your question.

MR. STEINDLER: That leaves open then, and perhaps the time would be this afternoon, to see whether or not it is obvious how the data output of Dave's program is going to be used by the folks who eventually are going to try and convert this into a model.

I must say right now it isn't very obvious how that is going to be done. It strikes me that release rates are strongly geometric-dependent, and I can't conceive of a program that is going to last a few more years that can go through the broad basis of geometry of failure, cladding

failure versus leach rate.

Clearly if you punch a hole in the top of the plenum, for example, and you blow the gas out that was inside the container, a single hole, even if the rest of it is saturated with water, isn't going to leach with anything other than diffu- nal movement of the fission products that are currently at that cladding/fuel interface.

That will give you a burst and you end up with a spike, and the rest of it now is strictly diffusion.

It is a sufficiently complex system so that the kind of experiments that I think I've been trying to track in this program, -- you know, they do publish fairly frequently -- it just seems to me it isn't very obvious how that translation is going to be made from the Battelle data to the modeling folks. Contacts between them may be strong but the relationship and the utility of the output isn't clear.

Perhaps this afternoon it will be made clear.

MR. COSTANZI: I don't know that we can say in all cases that we have an isomorphism between the results of -- either in terms of the understandings and conclusions which we draw or the data which we generate in this program to models being developed by the Division of Waste Management.

MR. STEINDLER: If that's not the case then I

think one could challenge you and ask why you are doing the work.

MR. COSTANZI: In some cases we're trying to learn to walk before we run. How the information is to be used in terms of very specific ways, saying well, we have this set of data done, these experiments, we have this set of data and we're going to use these as an input to this model over here to predict this kind of an outcome, in some cases we are at that stage and in many cases we are not.

We don't know yet. We don't know what the model needs to consider. You know, there is no point in loading a model with irrelevant data. We are having to consider processes which don't really have much of an effect.

MR. STEINDLER: I think you put your finger on part of it. There is certainly no point in loading the model with irrelevant data. There is equally less point in generating your own data and it is that issue that I-- I am not sure that this is the place or the time to consider that.

DR. SHEWMON: This is as close as we are ever going to get, at least in the past several years, so I think it is worth pursuing.

MR. STEINDLER: Perhaps this afternoon we will clarify that position, but I think it is critical.

This is a significant expenditure of resources at a time that, as Mike says, you folks are not exactly flush with resources. Your schedule, as we have mentioned yesterday, is extremely tight. So it seems to me a strong focus of research related to the needs of the regulators in an obvious sort of way is mandatory. And my problem is right now I can't see it. Perhaps I will see it better this afternoon. I hope so.

Other comments?

MR. ETHERINGTON: A quick clarification.

You mentioned experiments to determine the effect of crystal size on leach rate. Was "particle size" intended rather than "crystal size?"

MR. STOHL: This is the glass work you're referring to?

MR. ETHERINGTON: No; the glass has no crystals.

MR. STOHL: Yes, it does have crystals. You always have some crystals in high-level waste glass, and the concern is that if you do have additional crystalization due to other processes -- thermal or mechnical or what-have-you -- is that going to lead to greater leach rates.

MR. ETHERINGTON: Wouldn't you expect continuing crystalization from the radiation over a period of time?

MR. STOHL: To a small degree, but not very large. I think the greater problem is the thermal effect.

MR. ETHERINGTON: To what percentage is crystal in the glass, about?

MR. STOHL: In the range of 5 percent.

MR. ETHERINGTON: 5 percent. Thank you.

DR. SHEWMON: Is there an upper limit specified by DOE yet on what the center temperature of the glass will be and, thus, by calculation what the outer surface temperature will be?

MR. STOHL: There have been calculations based on the design concepts for the waste package repository system of the thermal gradient across the waste form. I can't quite those number.

DR. SHEWMON: My question was whether there was an upper limit on the centerline temperature which you knew of, and then it would follow by a fairly straightforward calculation what the outer limit --

MR. STOHL: I think Michael mentioned a 260 degree C. temperature level. Is that at the surface?

MR. MC NEIL: The centerline limit I do not remember in detail. There is a centerline limit temperature but it was adopted by one of the DOE sites, and that is of the order of 500 degrees Centigrade.

DR. SHEWMON: Is it known? Do you remember what that could correspond to, then, to a surface temperature?

MR. MC NEIL: As I recall, through the container for the overpack--

DR. SHEWMON: I mean the glass surface.

MR. MC NEIL: No.

DR. MARK: Several times hydrogen embrittlement has been mentioned as a matter of either interest or concern; I thought concern, mainly. I understand circumstances where it is a concern, but I don't see why it is a concern here, although there may be obvious answers.

Obviously, ductility is not an important characteristic of something quietly in its grave like these things.

MR. STOHL: I'm not sure I understand the thrust of your question.

DR. MARK: Why are you interested or concerned with hydrogen embrittlement?

MR. STOHL: Because it is certainly going to, or could lead to failure of the overpack.

DR. SHEWMON: How could it if there's--

DR. MARK: There's no pressure.

MR. STOHL: There's residual stress which is present in the overpack itself due to the welding, which could, because of its geometry, be high enough in the hydrogen embrittlement mode to cause failure. That's something that is a possibility.

DR. SHEWMON: With no overt plastic strain?

MR. STOHL: Correct.

DR. SHEWMON: And low carbon steel.

MR. CARTER: Can I ask a couple of questions?

One, you mentioned UO-2. Is that the only type of fuel that will be disposed of in the waste form?

MR. STOHL: The bulk of the waste form will be spent fuel, as indicated earlier by Dr. Costanzi, and most of that would be UO-2, irradiated UO-2.

MR. CARTER: What other types would you consider,

or have you considered, other than UO-2?

MR. STOHL: There may be other types of fuel that will eventually find its way into a repository. But many of those odd materials have been either stored or reprocessed.

MR. CARTER: A couple of things on the glass.

One, what degree of homogeneity do you get in the glass form itself?

MR. STOHL: It depends on where you're doing it. If you're doing it in a laboratory, certainly you get a very homogeneous product. When you're dealing with these large canisters you find that you do have inhomogeneities.

MR. CARTER: What about the brittleness of the glass?

MR. STOHL: There was some work done-- At Iowa, was it? --at the University of Iowa on half-size canisters which looked at cracking using cooling rates that DOE was contemplating. And certainly there seems to be not too much of a strong relationship between the cooling rate and the crystal size of the resultant product. You get, I think, 1-centimeter size crystals, or something in that order during the cooling.

MR. CARTER: The other thing I was interested it, How, actually, do you do the leach test on the glass?

MR. STOHL: The preliminary work was done in small bombs using standard MCC techniques. I can give you

more detail on that if you'd like.

DR. SHEWMON: Any other questions on this project?

(No response.)

DR. SHEWMON: Let's move on.

(Slide.)

DR. COSTANZI: The next project is the glass analog study at Argonne National Lab.

This program has ended in FY85, and the idea of this was to understand the aging process of glass; that is to say, is glass going to age in the repository such that when glass as a straight waste form starts leaching after containment, it is going to look -- or how different will it look from the glass that is originally prepared and on which laboratory tests are performed.

The way this was done is that the materials characterization center protocol tests were conducted on boro-silicate glass representing a high level waste form and on basaltic glass of fairly recent origin, and it was -- those tests were compared with old natural glasses.

This program, as I say, has now ended, mainly due to the fact that we don't believe that glass now is going to be a major constituent of a waste repository, and we feel that for the time being our study of glass is sufficient that we can fairly confidently evaluate anything that DOE

claims about the leachability of the glass.

DR. SHEWMON: Does crystalization change the leachability rate by an order of magnitude?

DR. COSTANZI: It does change the rate. I'm not sure about how much. Perhaps Dr. Stohl can--

DR. STOHL: Larry Hench from Florida did some work with Savanna River glasses, and he found that indeed if you just had a few percent -- 10, 20 percent crystallinity, that you're only talking about a leach rate of maybe twice what you had normally. But if you did go to fully crystalline material, then the leach rate could go up by as much as an order of magnitude.

DR. SHEWMON: Is that because of accompanying loss of integrity, or ease of penetration, or the chemistry of the rated dissolution of the surface, or the enhanced rate of dissolution--

DR. STOHL: I think it's the surface area.

DR. SHEWMON: Let me say it a different w.y.

Is it an increase in surface area, or an increase in reaction rate per unit area?

DR. STOHL: I think it's just...

Jeff?

DR. MEANS: I'm sorry; I missed the question.

DR. SHEWMON: Does crystalization mean that you also get more surface area available, or does the rate of

dissolution per given unit of surface area go up?

DR. MEANS: I think it's just strictly an increase of surface area. However, solubility is a limiting factor in the dissolution rate.

MR. ETHERINGTON: Is there a volumetric change during crystalization; is that a factor?

DR. SHEWMON: Is there a volume increase during crystalization? That was Mr. Etherington's question.

DR. MEANS: I suspect a rather small volume change; I'm not sure what the direction is.

MR. ETHERINGTON: It can be big in some materials, like silica?

DR. MEANS: Yes.

DR. SHEWMON: Let's go on.

(Slide.)

DR. COSTANZI: Let's now turn our attention to the question of the waste package environment, and again talk briefly about what we're doing in that area.

We'll begin with talking about what we're doing in that area at Battelle Columbus Laboratories, and that is looking at the interaction between the overpack material and the groundwater environment, particularly in basalt and in tuff.

A groundwater chemistry model has been developed by Battelle Columbus, and we are now doing a number of

experiments -- or weighing experiments, I should say, to validate that model. They have demonstrated that there is indeed a significant change in the groundwater chemistry due to the corrosion of the overpack. That's what we expected. The question was how is that change made, and how do you mode. it.

DR. SHEWMON: When you use the word "corrosion of the overpack," could this also be termed ion exchange, or is it actually erosion away, dissolution and transport of the overpack away?

DR. COSTANZI: In order to have sustained corrosion there has to be some movement of corrosion products away. But the tests that I believe were being done have been static testing and have not been flow-through tests.

DR. SHEWMON: Do you know the process of ion exchange? Have you have heard of it?

DR. COSTANZI: Yes.

DR. SHEWMON: Okay. My question was, Is it a matter of ion exchange or is it dissolution and transport away? And I guess your answer is that since the stuff wasn't moving it wasn't transported away. But what was the model of the changes that were occurring when you termed this "corrosion of overpack?"

DR. COSTANZI: I do not know the details as to

the chemical model. Perhaps Dr. Means would be able to answer that question.

DR. MEANS: Frankly, I'm not involved in this modeling work, and I don't know the specific answer to your question. I suspect that the alteration products are assumed to remain in place.

DR. SHEWMON: So there is a dissolution and transport away postulated?

DR. MEANS: Yes; by diffusion.

DR. SHEWMON: Is that right?

DR. MEANS: Yes.

MR. ETHERINGTON: Generally we have a very clear description of the difference between granite and basalt. Do we have a description of tuff?

DR. SHEWMON: And then you might also talk about bentonite.

MS. HACKBARTH: Claudia Hackbarth, NRC Staff.

Tuff is also an igneous rock, it's also a volcanic rock analogous to basalt, but it has the composition of granite; okay? So that means that the tuff sites, the rocks are rich in aluminum and silicon and sodium and are relatively oxidized as far as rocks go; whereas basalt is a more basic reduced composition. Tuff is a volcanic rock, as I say, so therefore it's partially glass and partially crystals.

At the tuff site some of the units are welded; they fell while they were very hot and were compacted by the weight of the overlying rock, and therefore are very dense, with little pore space, whereas some of them are fairly porous.

DR. SHEWMON: Sometimes overpack is used to describe the steel container, and sometimes that's called the package. I'm sorry; we're still on steel, we're not on rock; is that right?

DR. COSTANZI: I don't understand your--

DR. SHEWMON: You're talking about the dissolution of overpack, and I had thought we had gone to rock. But as I look back on your handout, carbon steel is overpack.

DR. COSTANZI: Carbon steel is overpack; that's correct.

DR. SHEWMON: Pardon me. Packing and backfill. So overpack is packing, and packing is bentonite?

DR. COSTANZI: That's correct.

DR. SHEWMON: Okay; pardon. Then I don't want to know about bentonite.

DR. COSTANZI: Shall we move on to the next?

DR. SHEWMON: Which one have we just left?

DR. COSTANZI: The one we were discussing is the--

DR. SHEWMON: This was the modeling of overpack corrosion?

DR. COSTANZI: Yes; the modeling of the interaction between the groundwater and the overpack.

DR. SHEWMON: Sorry; my questions all had to do with what would happen with rock, and that's why I was talking about ion exchange, and you were confused because you thought I was ion exchanging the steel. I'm sorry.

DR. COSTANZI: Okay; if you wish to move on to that, we'll just skip the next one and then come back to it.

DR. SHEWMON: I don't know what the next one is because it's your list. And if I have your list I don't recognize it.

DR. COSTANZI: The next project is the coupled processes interaction at Lawrence Berkeley Laboratory.

This program is a program to identify what are, and what is the significance of the coupled processes, coupled thermal, hydrological, mechanical and chemical processes which affect repository behavior.

What they are doing is examining hydro-thermal systems as analogs essentially to the thermal-mechanical perturbations of in-place waste in a repository. The idea is to provide essentially an initial integration of the research we're doing.

As you've heard today, we're doing research on

overpacks, we're doing research on waste forms. In other areas of research we're doing volcanic chemistry, geology, hydrology.

The idea of this project is to try and get some feeling for how these various disciplines are going to interact, what are the interactions among them in the repository environment.

They have been focussing on the explanations, or the models, if you will, which have been developed to understand hydro-thermal systems as natural analogs to a repository.

DR. STEINDLER: Are you doing this for all three sites, or potential sites?

DR. COSTANZI: Yes, for all three.

DR. SHEWMON: Basically, this can greatly enhance the transport because you've got now an active circulation mode, or the potential for it; is that right?

DR. COSTANZI: Whether or not that convection because of the heat is a significant method of tranport of radionuclides out of the respository is one of the questions being examined.

I didn't answer your question "yes" because I don't know whether it's significant or not.

DR. SHEWMON: But the potential is there?

DR. COSTANZI: The potential is there, and this

is one of the things we're examining.

DR. SHEWMON: It's a possibility.

Any questions on this?

(No response.)

DR. SHEWMON: Go ahead.

(Slide.)

DR. COSTANZI: The next project, also at Lawrence Berkeley Laboratories, is site geo-chemistry, and we're only going to talk here about the part of that program which deals directly with the waste package environment. Part of this program also deals with the transport of radionuclides in the area which is thermally undisturbed by the emplacement of the waste.

In particular we are looking in this program at diffusion of radionuclides through packing material, we are looking at the interactions of the basalt and groundwater and tuff groundwater systems, and how elevating those systems to repository temperatures changes the geo-chemistry of the system.

We have also taken a look at the effect that leachates from the spent fuel as a waste form -- from the uranium oxide in the spent fuel might have on the overpack material.

Again, this is to make sure that we understand what's going on in the waste package environment; to what

extent the assumptions that are being made both by the NRC and by DOE in their tests on waste package performance are consistent with what we really expect to go on inside the repository.

MR. STEINDLER: The problem I had on this one is, I couldn't quite figure out what it is that you are doing in the tuff area. Tuff has no packing material, they don't have any bulk -- at least they think they won't have any bulk water flow because they are in the unsaturated area. The mechanism for transport through the pores strikes me as being still up for grabs.

What's the activity in the area of tuff?

DR. COSTANZI: The activity that we have done in tuff areas has been a small part of the effort, for those reasons. But we have just looked at what the interaction of the tuff and any water which would seep into the repository might be under the elevated temperatures, essentially to look at the chemistry.

MR. STEINDLER: This is tuff-water interaction, not the canister?

DR. COSTANZI: Not the canister.

MR. STEINDLER: Are you obtaining kinetics for those reactions?

DR. COSTANZI: I believe so. I think George Birchard can answer that. He's the project manager.

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MR. BIRCHARD: I'm George Birchard, research staff.

The answer to your question is yes. The work on tuff, by the way, is really just beginning in this project. We're trying to finish up the basalt work.

But the answer to what will be done is, the steam, which will be at about one atmosphere, will be reacted with the tuff at elevated temperature, both to predict what it does to the tuff and what the groundwater chemistry will be that reacts with the tuff.

DR. COSTANZI: Even though the tuff repository is in the unsaturated zone, there is still water present.

We understand from DOE that they intend to place -- to have very high temperature waste packages in the tuff repositories essentially to drive the water out and keep it away from the waste package.

There will be alterations of that water in the tuff rock during that process.

Eventually, of course, packages will cool and water will come back. Now, the degree to which that is going to contact the waste package and provide a mechanism for movement of radionuclides is another question.

MR. STEINDLER: When you say there is water present, you're not suggesting there is condensed water in bulk form?

DR. COSTANZI: No; it's in the unsaturated zone.

MR. BIRCHARD: Well, the unsaturated zone does have water in the pore space.

MR. STEINDLER: I very carefully said "bulk form."

DR. SHEWMON: While you've got the microphone, would you tell me briefly what they learned from the basalt part which is apparently over?

MR. BIRCHARD: The basalt part is not yet over, it is being completed. And the basalt part has shown that at elevated temperatures, at least--

DR. SHEWMON: How elevated?

MR. BIRCHARD: Up to 250 degrees C.

(Continuing) --that the chemistries seem to be predictable based on models using lower temperature data. There seemed to be a good degree of similarity between the results of higher and lower temperatures; which is very reassuring to us, in that it gives us some hope that DOE will be able to make predictions about the groundwater chemistry at elevated temperatures--

DR. SHEWMON: Let me ask you--

MR. BIRCHARD: --with a limited amount of data base development.

DR. SHEWMON: When I first heard about high level waste, maybe a decade ago, maybe more, people were talking

about packing bentonite in and around it. It was supposed to be, I get the impression, fine enough to pack in fairly tightly and had the nice property that, when it hydrated or came in contact with water, it expanded and really choked things off.

To what extent can one model and take credit for that? I haven't heard about it yet.

MR. BIRCHARD: Let me tell you what bentonite is. I have a very simple answer. It is Kitty Litter.

DR. SHEWMON: Okay; so it's rather porous?

MR. BIRCHARD: What they do is, in the repository they won't give it to you in the same loose form, they will presumably pack it in. It is actually clay, it is volcanic ash that is weathered into expansive clay. If you put it in a repository it will swell up and fill pore spaces and cracks.

There is a model in one of the tasks at Berkeley that has been developed to predict transport through compacted bentonite, and it involves diffusion -- both bulk diffusion and surface diffusion mechanisms. And they've been able to do this at lower temperature. Now we're planning to do some work over the coming year at higher temperatures with compacted bentonite.

DR. SHEWMON: What's low and what's high?

MR. BIRCHARD: Lower temperature would be at room

or ambient temperature, the higher temperature will be just below 100 C., so about 90.

DR. SHEWMON: Now, given the results there, how many hundreds of years can we get out of that? Or is it a no; never-mind geologic time?

MR. BIRCHARD: The answer is that there are some questions that we're not addressing about bentonite. Particularly, in this project we have assumed that the bentonite maintains its mineralogical and chemical stability. Now, that may or may not be a correct assumption, depending on reactions that would occur at different sites and at different waste packages.

But assuming that we maintain the stability, one can then predict the rate of diffusion of mass out from the waste packages; and that is something that DOE, certainly BWIP, has been working on and taking credit for.

So we are developing the ability to assess what they will be doing.

DR. SHEWMON: And it is a significant barrier?

MR. BIRCHARD: It can be extremely significant. It can be very effective at limiting mass transport. It can also be very significant with respect to maintaining the canister integrity because it also controls the flux of water in and out from the canister. So if you have very little movement of water into the canister or overpack,

naturally you will be greatly adding to the stability of that waste pack.

DR. SHEWMON: So if we just wait until after lunch, and rush back early, we'll hear how all of this is integrated into the aerospaces model.

DR. MARK: Two questions. I was not supposing you would have any water migrating into the waste package when the temperature on the outside was much above 100 degrees C.

MR. BIRCHARD: That's something I'm not going to presume one way or another. I guess that's part of the--

DR. SHEWMON: That's not necessarily what happens here. It depends on--

MR. BIRCHARD: For the tuff, though, it would be -- that would be true. For an unsaturated zone, yes, you'd only have vapor. But, say, for the basalt site it would be different.

DR. MARK: Now, how long does the temperature outside the package itself stay above 100 degrees?

MR. BIRCHARD: Claudia, your project, I think, addresses that more directly than mine; or maybe Nick wants to answer.

MS. HACKBARTH: That's a good question, how long and how hot the waste package will stay and will be.

I think that depends, it depends on how much

groundwater circulates around and takes away the heat from the waste packages; it depends on what goes into the waste packages, and it depends on how closely the waste packages are placed together.

So I don't think we really know.

DR. MARK: Well, I'm sure there isn't a unique answer, but is it like 100 years, or 1000 years?

DR. COSTANZI: I think that the current designs call for the peak temperatures in the repository to be reached at like 100 to 150 years after closure. The package itself will remain above -- current designs call for the initial surface temperatures to be above 100 degrees C., and they will remain that way for at least that long, perhaps even longer.

DR. MARK: Well, my simple thought here was that the period of high temperature is also -- is limited perhaps to one or a few hundred years, and your waste pack is going to be guaranteed to be intact for three hundred years, so that some interest in the high temperature chemistry might be relieved a little bit by saying you don't -- anyway, should be mitigated by the fact that the phases don't totally overlap.

DR. SHEWMON: Battelle Columbus should be studying steam corrosion not water corrosion, do you think?

DR. COSTANZI: I think you're right. We have not

addressed steam corrosion. There's a lot of things we aren't doing yet; and that's one of them.

I'd like like to make a further remark about bentonite as a packing material.

DR. SHEWMON: As soon as you quit, we'll go to lunch, so don't make it too long.

DR. COSTANZI: Okay.

A program at Argonne looked at the effect of temperature on the packing material, at bentonite. What they found was that bentonite subjected to high temperatures in an autoclave, essentially water above 100 degress C., showed an appreciable loss of its mechanical properties, its swelling properties, and the like, and its chemical properties as a clay. However, hydro-thermally altered bentonite, that is to say bentonite exposed to steam, changed both its physical properties and its chemical properties, and as a packing material became essentially useless.

Bentonite heated in a dry atmosphere--

DR. SHEWMON: "Useless" means that then it allowed circulation?

DR. COSTANZI: It means it doesn't swell any more and it doesn't stop anything.

Dry bentonite -- that is to say, heated dry, above 100 degrees C. -- also did not seem to appreciably

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lose any of its chemical or mechanical properties, the ability to swell and essentially inhibit the flow of water.

But in an environment where you have high temperatures, temperatures above 100 degrees C., and have 100 percent saturation, that is, you have a steam-air environment, bentonite will very likely deteriorate and will not be a serviceable packing material.

DR. SHEWMON: I rule that we will not talk about sealing rock masses at the University of Arizona unless somebody asks a question about it after lunch.

We'd like to start again at twenty minutes to two.

(Whereupon, 12:45 p.m., the subcommittee was recessed, to reconvene at 1:40 p.m. the same day.)

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AFTERNOON SESSION

(1:45 p.m.)

DR. SHEWMON: Why don't we go ahead? We will pick up a number of members soon.

This afternoon we start with the Division of Waste Management's presentation.

Who leads off?

MR. JOHNSON: John Greeves.

DR. SHEWMON: Okay.

Please begin.

MR. GREEVES: I just wanted to make a couple of introductory remarks, and really just show a couple of points that were made by Hub Miller yesterday, and try to put a couple of things into perspective.

(Slide.)

Hub went through this particular slide with you yesterday, and I think it is important to remember what the resources available to the Staff are to evaluate these problems, and I just wanted to remind you of what our funding level is, what the level of effort that we have in looking at the various high-level waste problems are, and what the Department of Energy is focusing on.

You are aware of the number of FTE within the division, and this is all of the program areas. You are only looking at the one, the waste package materials

program today, and their funding comes out as a fraction of this 6.7 million dollars in technical assistance funding as compared to what is obviously a much larger volume of dollars that the Department of Energy is putting into their programs.

DR. SHEWMON: So you have to try harder.

MR. GREEVES: We have to try hard, and we have to be astute as to what it is that we look at.

Jack fully agrees with that.

DR. SHEWMON: I'm sure that is what that outburst meant.

MR. STEINDLER: I have heard this comparison now for a couple of years running between the big steam roller in DOE and the poor folks in NRC that have so much fewer resources. I'm not sure I know what that comparison is meant to highlight.

The job that you folks have is drastically different, and the mere comparison of either FTEs or dollars in the context of your mission is wholly illegitimate. You are not digging holes ten feet in diameter, six hundred feet deep, or at least you are not supposed to be as far as I know, et cetera, et cetera.

So tell me a little bit about why you bring that comparison up almost continuously.

MR. GREEVES: There is no question the reason

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that is brought up is that at times when we are asked about our program, we get the impression that folks are wondering why we aren't out forging ahead, getting out the answers and developing the answers to some of these questions when in fact, and rightfully so, we are at a much reduced FTE level and technical assistance dollar volume level that allows us to effectively go out and audit the DOE program, not go out and do their job.

Our job is to-- And I have another slide that I was going to show

(Slide.)

Effectively what our job is -- and this is addressed to the technical assistance part because that is what you are reviewing today -- is to utilize Staff and technical assistance to go out and critique the reviews of the various DOE documents.

And it obviously takes a great deal more FTE and resources to get out and as you say dig the hole and do things like that, so we critique that and prepare guidance at a level but not get out and map out how they are supposed to come up with the answers to the problems.

We keep ourselves in the role of being independent to be able to critique that, but at the same time to provide some guidance at a level that helps them perform that.

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The other thing that we are doing with technical assistance help is to acquire modeling and assessment capabilities, some of which you will be hearing about that the Aerospace group is helping us with, and then, on a very selective basis, on occasion run some sort of a confirmatory measurement test.

So I just wanted to make sure that as Hub was mentioning yesterday, the whole distinction between the Department of Energy and the Staff is in mind as we go through these discussions.

DR. SHEWMON: Go on.

MR. GREEVES: At this point I will turn the mike over to Andrew Johnson and have him introduce himself.

MR. JOHNSON: Thank you very much.

The people I have here: Ken Chang on my right, who is a project manager for the Aerospace contract as well as one with Oak Ridge National Laboratory, and behind me I have Ken Stevens from Aerospace, and Peter Hsu from Brookhaven. Both of these are the lead people for their respective contracts.

Basically our responsibility here is set out in 10 CFR 60, and as you recall from the discussion yesterday, 10 CFR 60 provides some quantitative criteria on the waste package and the engineering barrier system, and these are the containment criteria and the release criteria in

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Section 60.113.

It is our job to review DOE's programs and their designs to make sure that when they submit a license to us that we have the capability to evaluate their designs to meet the Part 60 criteria.

Now we believe that the engineered barrier features in the waste package are going to be extremely important. As you know, there are a lot of inherent uncertainties in geological systems and we feel that because you can control and you can design the engineered features of the repository, there will be an awful lot of emphasis placed on that during the licensing.

Our programmatic efforts at this time have been primarily oriented to keeping abreast of DOE activities, reviewing their work, and also to identification of problems that we wish to present to DOE. And the identification of problems are done through literature searches, through some limited experimental testing, and through the research programs that we heard about this morning.

We are also involved in direct feedback to the Department of Energy, and that is primarily through out Waste Package Workshops. The purpose of these workshops is to obtain from DOE an up-to-date status of what they are doing and also to provide them technical feedback on areas that we think that they need to emphasize.

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We had one this past July, the Nevada program, and as was mentioned earlier, meetings with the SALP and BWIP were originally scheduled for the month of October but have slipped, and we are in the process of trying to reschedule them.

Now because each of the three DOE programs have unique waste package designs and have to deal with unique environmental conditions, NRC has to do a great deal of work in order to get ready for licensing as well as review the interim documents such as the site characterization review. And it is absolutely essential that we have high quality technical assistance work to help us in supporting this licensing effort.

Now before I get any further here, I think I should try to address some of the questions that were brought up earlier today.

The first thing I would like to talk about-- Maybe this would be a useful time to go over the individual waste package designs, and I believe you have passed out diagrams of each of the waste packages. And Dr. Shewmon, if you would like, I will go through each of these so that we know what we're talking about for each of these designs.

DR. SHEWMON: Fine.

MR. JOHNSON: The first page is the basalt design, and what it is is a cannister made of carbon steel,

and in it is placed either the glass, the borosilica glass, or the consolidated fuel rods.

The cannister is then emplaced into a carbon steel overpack which is approximately three inches thick. That overpack with the cannister then goes into a hole on which packing, a bentonite-basalt mixture of packing is placed. And the particular design that is shown here is the reference design that was presented in the BWIP environmental assessment. And they would emplace the packing in segments and it would completely surround the overpack.

Now more recently I think that BWIP has recognized some difficulties in emplacing the segmented packing pieces and what they are suggesting now is perhaps putting the packing into another carbon steel container that would contain everything. It would contain the packing plus the overpack and the cannister. And this whole big piece would then fit right into the hole.

DR. SHEWMON: Now this packing material that you would put inside sheet steel would be the bentonite?

MR. CHANG: It has 75 percent bentonite and 25 percent basalt, crushed basalt.

DR. SHEWMON: Now if you put it or they put it in cans, isn't it going to remove most of the virtues it has for being there?

MR. JOHNSON: No. I believe that what they assume is that if water corrodes through the outer steel carbon container, it will be absorbed by the bentonite -- the packing. The packing will then swell and it will further seal off the introduction of any additional water.

DR. SHEWMON: The outside cannister will corrode first and you don't need it until -- before that.

MR. JOHNSON: Right.

On the next page, which gives the reference design for the salt repository, and this again is their proposal as stated in the environmental assessments, what this is is again a carbon steel cannister into which borosilica gas or spent fuel rods are emplaced. This cannister then is put into a large carbon steel overpack. These overpacks would be approximately six-inch thick carbon steel.

And the overpack with the cannister would then be emplaced in a hole and backfilled with crushed salt. So in this particular design there really isn't a packing as there is with the basalt design, but the void between the overpack and the hole would be filled with crushed salt. And we would expect that that crushed salt would eventually become a homogeneous piece of the wall over time because of the lithostatic pressures which would be applied in the hole.

DR. SHEWMON: What do you assume about water actually flowing through the salt?

MR. JOHNSON: I'm not quite sure as to how much water is going to get into the salt bed itself. There is brine, though, that is a part of most of these salt formations and the brine has the tendency of migrating toward a heat source.

DR. SHEWMON: I'm familiar with that, and the reason I asked about it is that from a materials viewpoint, this seems to be a pretty miserable environment to get involved in. But it is one of the old favorites that many people thought had many virtues, and if they are indeed correct I suspect it has to be because the long-range migration through this stuff was highly inhibited by the continuity and self-healing virtues.

That's why I asked about the long-range migration of water.

MR. JOHNSON: Well, I'm not prepared to talk in detail about the hydrologic questions that are involved with the salt repository. But there apparently are questions as to, you know, now that you have drilled a hole from the surface, how do you keep water out, how do you seal that hole to prevent water from perhaps some of the above levels from affecting the repository engineered features.

There are many questions regarding hydrology

that I believe need to be addressed in this case, and unfortunately I don't think I'm prepared to discuss those in detail.

DR. CARTER: I wonder if I could have you put, say, approximate dimensions on some of these parts?

MR. JOHNSON: If you look on the last page of the handout there are some dimensions. Basically the size of these containers vary with whether or not you have spent fuel in them or borosilica glass, but they range-- For example, the cannister for spent fuel is, you know, approximately 20 inches in diameter. It ranges to almost 20 feet high for some of the designs.

The dimensions for some of the glass waste forms are thinner in terms of its diameter and also range about 12 feet high.

DR. SHEWMON: Where did you get the 20 feet?

MR. JOHNSON: I believe that 20 feet is the-- I think that's the salt waste package. It's about 20 feet.

DR. SHEWMON: The highest thing I see here is 4.6 meters, which doesn't.... Go ahead.

MR. JOHNSON: That's 18 to 20 feet high.

DR. SHEWMON: I would say.... That's secondary. I can do that on my own time.

MR. JOHNSON: The last design is the one from TUFF, and this is a single stainless steel cannister for

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spent fuel, about a centimeter thick, half an inch thick. The reference material was Type 304L stainless steel.

For the glass case, their current design is a carbon steel process vessel into which you would pour the glass and that process vessel would then be emplaced into a stainless steel container.

That's not completely shown on this diagram, but if you look at the glass can, the present TUFF design is to put that cannister with the glass in it into another container that is the Type 304L stainless steel, again about a centimeter thick.

DR. SHEWMON: So this area that is labeled "gap" here would be filled with reinforced stainless steel?

MR. JOHNSON: No. See where it says "cannister"? That's the 304 stainless steel. The gap is just air.

DR. SHEWMON: And that thickness is likely to be what?

MR. JOHNSON: For the gap or--

DR. SHEWMON: The cannnister.

MR. JOHNSON: The cannister is one centimeter.

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MR. ETHERINGTON: How much did you say?

MR. JOHNSON: One centimeter.

DR. SHEWMON: And the gap i there why?

MR. JOHNSON: The gap is there because you have to build a bigger hole than what you are putting into it. I am not exactly sure of the dimensions of what that gap is. I would assume it would probably be a couple of inches.

DR. SHEWMON: They didn't show a gap in the first one, I guess, yet there has to be a collar clearance.

MR. JOHNSON: On the first two, the salt, the gap is filled with crushed salt and on the BWIP design, the current design with the carbon steel container which everything fits in there, would again be you know a small gap in there.

DR. SHEWMON: Fine.

MR. JOHNSON: Are there any questions on what the designs are or what we are talking about?

(No response.)

MR. JOHNSON: The next thing I would like to talk about is a question that came up earlier in the meeting and it had to do with failure criteria. I believe Professor Kassner asked what would be the failure criteria and the failure criteria for the containment. If you remember from yesterday, we have a performance objective which says that for a period of 300 to 1000 years a containment should be

substantially complete. And we have published a paper on what we mean by substantially complete, and let me give you some background.

As you recall, there is another performance objective and that was for a release rate. And that release rate was -- we would allow releases on an annual basis of one part in ten to the fifth of the inventory at 1000 years. In other words, we would allow a certain number of curies to be released following a thousand year period. And our failure criteria then is to say that from zero to 1000 years we would also allow the same number of curies to be released.

Now at an earlier time, because you have decay taking place, the total fraction that we would allow to be released over the first thousand years would be a great deal less than at 1000 years.

In other words, your inventory is going to be much higher say at 100 years than at 1000 years. We would still allow the same number of curies though to be released irrespective of the time period. Now earlier on of course the release fraction is going to be quite a bit lower than at 1000 years.

Now what this means is that say you do have a pit or a crack in the containment and some material does get out, if DOE can demonstrate that that total amount of

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material is less than the total number of curies that we would allow at the time period of 1000 years, we would say that that meets the criteria of substantially complete containment.

DR. SHEWMON: Is that in a Staff technical position some place where people can see it?

MR. JOHNSON: It is in a paper that was published at I think it was a meeting in Albuquerque about a year and a half ago. And we are going to put that into a technical position. DOE has had some input on that and basically they are pleased that we are giving them that flexibility.

The other question that came up during this morning's meeting had to do with information exchange. And I believe the problems that were identified with research primarily revolve around the availability of published documents from the DOE program.

DOE does have a very complex, perhaps Byzantine, review structure which makes it difficult to get timely documents that are completely published and have gone through their QA system and so forth.

So what we have done -- we have recognized it as a problem, too, and what we have done is we've set up a mechanism to allow our technical people to talk directly to their technical people and we have an agreement and a series of contacts between each of the projects.

And this exchange agreement has worked out pretty well and I think that the technical people at DOE have been more than happy to discuss technical areas with us and have been very open, so I don't think it's as bad as what was portrayed earlier this morning.

There is also another program that we have underway --

DR. SHEWMON: What level was that signed off on?

MR. JOHNSON: Pardon?

DR. SHEWMON: This agreement, at what level was it signed off on? Did you negotiate it or your boss or did it go on up at the...

MR. JOHNSON: I think it was agreed between the division director level and the Ben Rushe level.

And there is also another --

DR. STEINDLER: Excuse me, before you leave that, can I interpret that to mean that anybody in research or NMSS can call up anybody else in the various projects?

MR. JOHNSON: No, there are specific contacts in specific program areas. For example, in waste package, we have a contact name and a contact name for the salt project, for example. And we have project leads in each of the three repository areas for waste package. There is one contact that can call a counterpart for the DOE program.

And so say somebody from research had a question,

he could go to the lead in that particular area and make a conference call or something to get whatever information was needed.

DR. STEINDLER: So what you are really telling me is that there is an enormous barrier of communication between NMSS and research?

MR. JOHNSON: I wouldn't say that. I would say that perhaps not all of the questions that are arising are using this mechanism to the full advantage.

DR. SHEWMON: What I heard was that there is a great reluctance to pass out documents before they have been vented at several levels and properly aged in DOE. They can get verbal contact set up if they go through the right channel. I'm not sure whether the channel was negotiated only for their division or whether research comes under it or not.

MR. JOHNSON: Research is not particularly a part of this agreement but if they had questions they could go through the contacts that have been established to get their problems solved.

DR. STEINDLER: I may have overstated my case slightly but maybe not all that much.

DR. SHEWMON: Research has to come up and say mother may I before they....

MR. JOHNSON: There is also another area that

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we have tried to get better information exchange into and that is also part of this agreement between the Department of Energy and our division and this involves -- as you recall yesterday, Hub Miller talked about their being on-site representatives for each of the repository programs.

And what we had established is an agreement that we can send our Staff out to the on-site representative and our Staff can go through technical documents, drafts, data logs or whatever that is desired under the guidance of the on-site representative. So there is a mechanism for us to get information on a more timely basis than having to wait for the reports to be formally approved.

DR. STEINDLER: Would somebody from research be able to latch onto the site rep that you have and get his or her questions answered that way?

MR. JOHNSON: I believe that if a research person wanted to do this, I believe it could be arranged. That question hasn't come up yet. This program with an on-site rep was signed, what, about a month and a half ago and we have had a couple of meetings under that program that have been very worthwhile. But there is no reason why research couldn't ask us to go forward on that.

MR. ETHERINGTON: DOE is concerned with safety and retrievability. Is NRC concerned only with safety?

MR. GREEVES: Retrievability is one of the performance objectives and NRC has a responsibility to review all of the performance objectives.

MR. ETHERINGTON: Even though it isn't a safety consideration?

DR. SHEWMON: You can't say it's not a safety consideration, that may be why it's there.

MR. JOHNSON: It would be a safety consideration in terms of the operations and maintaining occupational exposures at a certain level and so forth. In terms of the preclosure safety aspects it would be important.

MR. GREEVES: The hearing board is going to have to make a finding on all of the performance objectives, and one of which is retrievability. The Staff will be expected to come to the hearing and state its position on the retrievability at that site, the Department would state its position and we would go from there. But the board would have to make a finding on retrievability.

MR. JOHNSON: There was another area that had to do with how do we use research information. And I think the basic kind of information that is coming from the research program revolves around identifying problems that should be presented to DOE for them to address.

And we expect that the Department of Energy, in their application will provide us a fairly complex

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performance assessment analysis and that performance assessment analysis will have included with it perhaps computer codes and models and so forth.

And our objective would be to review those computer codes in that assessment to make sure that all of the right mechanisms for failure are discussed completely and thoroughly. And in order for us to do that, we use our research programs as a mechanism for obtaining information as to what are the appropriate failure modes that DOE should be looking at, is this a problem, is this not a problem. And the use of that data would be used to do that.

DR. SHEWMON: So would you have a list of what you think are the most important failure modes?

MR. JOHNSON: I think the most important failure modes in terms of corrosion are the localized failure modes. That would include pitting, stress corrosion cracking, hydrogen embrittlement and so forth, crevice corrosion cracking. These are items that we identified in our comments on the environmental assessments.

The environmental assessments that were done had a short discussion on how they would handle waste package. And in it, their evaluations only considered uniform corrosion.

DR. SHEWMON: When was that?

MR. JOHNSON: This was published December 20th of

last year. And our comments were generated and submitted to DOE at the end of March 1985.

DR. MARK: You mentioned hydrogen embrittlement and that's of that three-inch or six-inch steel welded, covered.

MR. JOHNSON: Yes.

DR. MARK: If that's the most serious failure mode, there can't be any very serious failure modes.

MR. JOHNSON: I don't know if that's the most serious failure mode. What I said is that we thought that localized corrosion phenomena like hydrogen embrittlement may be the principal failure modes and these failure modes are going to be something that DOE has to address. I don't believe I have the information to say that, yes, this is the principal one or this isn't. But I think that there is enough information to say that these are areas that DOE needs to consider.

DR. MARK: You spoke of occupational exposure. Are the standards there the same as for reactors?

MR. JOHNSON: Yes, the Part 20.

DR. MARK: Now there has also been reference there to their mining techniques. Why should NRC take that on at all? Why isn't that given to the Bureau of Mines to answer that?

MR. JOHNSON: I think John should answer that.

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MR. GREEVES: The Bureau of Mines is one of our principal consultants.

Again, remember that the Staff is going to have to, when they come to the hearing process, indicate what their view is on those findings, hopefully with someone like the Bureau of Mines as a consultant supporting us.

And as far as mining techniques, principally that will come up in cases where the Department of Energy backfills around a particular canister and claims that they can retrieve it. Well the Staff better have a position on can they mine that out and can they make a finding that retrievability is an option available to this particular site so that the Commission will have an option when it comes time to decide whether to terminate this particular site or not. And if they decide hey, we can't leave it here, then that option to retrieve still is available to them. So it can involve mining techniques depending on the design that the Department comes up with.

MR. ETHERINGTON: What does backfill mean, just blocking the opening or filling in all of the cavities around the casks?

MR. GREEVES: It is design-dependent. To date we have been seeing things at the Nevada project where there is no backfill. The Department is saying we are going to put the canisters in and we think it is good enough to leave

it in there and we can meet the performance objectives without a backfill environment. At the BWIP project, we see them talking about packing materials. And essentially we have seen designs ranging the full spectrum.

And it is in large part going to depend upon cost factors. It is very expensive to leave all these rooms open and ventilated until the very end and then get your license and march forward and backfill it. If there is any way that they can provide a backfill to essentially put it in its final environment with assurance that if things went wrong they could go back and remine it, it is far cheaper to do it that way. So they are still playing with the dollars and cents aspect.

DR. SHEWMON: So it is really a question of whether this stuff is cementatious or whatever the word is that you backfill with whether it's -- if it's sand you can get it out a decade later and if it's sale you can't or something.

MR. GREEVES: Well the mining folks can get just about anything out if you pay them enough money. They have proven that in adverse environments. That is for a resource, an ore.

In this particular case we are talking about hot radioactive wastes, so how you go back in there and excavate such an item is a ticklish question and I think we are on

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the record with the environmental assessment comments that Tim mentioned earlier as saying we in some cases see retrievability as being a fairly straightforward operation but in environments like salt where the rock naturally creeps even under ambient conditions, that is accelerated under thermal loads, we need to have some pretty serious discussion with those folks if they have designs that don't sleeve the waste and provide access to them.

So again DOE has got to come forward, tell us what their design is and allow us the opportunity to review that and engage them in some dialogue on it.

DR. STEINDLER: You have just indicated, and I think correctly, that the mining folks can retrieve anything given enough money. Why doesn't that apply also to retrieving something that happens to be radioactive? There I would ask then why fuss over this business of retrievability since it is probably commonly agreed that if you are willing to spend enough money you can retrieve anything.

MR. GREEVES: Well retrieving hot radioactive waste is not something the mining industry has done in the past, and they also have a habit of --

DR. SHEWMON: We never had enough money.

MR. GREEVES: They also have a habit of claiming they can do things in advance, and what's your guarantee

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that they are going to be able to do it?

So it's a ticklish situation. Some of these projects they identify they are going to use overcoring techniques. Well can you imagine using an overcoring technique which is just boring a larger hole around it and if your measurements are off you could cut right through the waste package if you found yourself in a situation where the QA or the original records on where the darn thing is placed were wrong. So it's not a trivial situation.

Again, we have been asking the Department to come forward with a serious design concept and then we would take it up. We would involve folks like the Bureau of Mines who we are going to look to on issues like this.

DR. MARK: You mentioned the business of maintaining ventilation if you didn't stuff the cavities full. I g he impression that wherever you put waste the crust consists almost entirely of water, groundwater. It's like power plants attract earthquakes.

(Laughter.)

Are you also going to keep this darn thing pumped out of water so that there is guaranteed water flow?

MR. GREEVES: The Department is going to have to come forward with a design that proves that they can get it back out. I spoke about ventilation for an example. What they have to assure is that they can get back in there and ventilate. They may not maintain ventilation, for example, on a particular design during the operational phase. They may shut a particular drift down and just not ventilate it.

But they have got to have a design option available that they can get back in there and ventilate that drift. It doesn't necessarily even include having this ventilation equipment onsite. They have to have a design in place, reviewed by us, that shows that if necessary, they can come back in and reventilate that area.

As far as the hydrologic aspects, some of these places will have water coming into it. They are going to be concerned about two situations. One is the safety of the workers involved here, and two, any scenarios as far as being able to get back in if the decision is made to retrieve the waste. And they need to have a design option available to assure that.

It doesn't necessarily have to be in place at all times because it's expensive.

DR. SHEWMON: Does that answer your question?

DR. MARK: Yes.

MR. STEINDLER: Let me ask one more.

You talk about retrievability as though it were in a class more tightly controlled than even the reasonable assurance aspect of containment. Are you also planning to define a "reasonable assurance" aspect of retrievability designs?

MR. GREEVES: It, to my way of thinking, is not in any different class. We are going to have to make a finding that there is reasonable assurance at the license application stage that there is an option to retrieve the waste. We are going to have to call on folks like the Bureau of Mines, other technical consultants, DOE's counterparts, and they are going to come forward and state that there is a reasonable technology available, designs are available to meet the retrievable option.

MR. ETHERINGTON: Does the transportation cask stay with the cannister until it is inserted into its final hole, or does it stay up top?

MR. JOHNSON: I think that the whole area of when things get put together hasn't quite been decided by the Department of Energy. They are looking at doing some work in terms of packaging the waste at what they call a monitored retrievable storage facility.

What exactly that will encompass hasn't been decided. They are in the process of doing a series of optimization studies to look at which is the best way of

packaging waste, should part of it be done at the waste generator, should part of it be done at the MRS, should part of it be done at the repository itself?

Basically we would expect that there would be some container -- I don't know whether it would be the overpack that would be put on at the MRS or not, but in some place there will be a central facility for consolidating rods, putting it into some container, taking those containers on a unit train to the repository.

That transport cask would again be reused and the containers themselves would be brought to the repository. If an overpack or whatever was needed at that point, it would be placed in there and then it would be put into the mine. But the transport cask would be reused.

Are there any other questions on those?

(No response.)

MR. JOHNSON: Well, let's go into the TA program. Ken Chang will make the presentation. He will describe the five technical assistance projects that we have underway.

(Slide.)

MR. CHANG: This is the title of my presentation. Basically it is an overview of all the high level materials technical assistance projects that we have to answer the question of whether DOE's waste package can meet the requirement described in 10 CFR 60.

(Slide.)

Here are the our topics that I am going to discuss. Basically you go over, you know what the overall objective for the program is, the five TA contracts in force, and some of the related contracts including research contracts and also other technical assistance contracts that other branches and sections of our division handle.

Also other than that we will go through some of the specifics of the technical assistance contracts. All the contracts that I am going to talk about basically will be the TA contracts in force or sponsored by the Materials Section.

Our section has the responsibility to make sure to review whether DOE's high level waste package can meet the 10 CFR 60 requirements or not. And I am going to go through some of these requirements with you.

(Slide.)

I believe most of you have a few pages of the 10 CFR 60 and it contains all of the requirements that the waste package must meet. In 60.11(a)(6), the title is "Site Characterization Report," and basically on (6) it says that DOE must provide a site characterization report to include a description of it, and a plan to do it, and a conceptual design. So presumably it will include a conceptual design of the waste package.

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It will also include a description for all issues related to the selection of the site. So that takes care of 60.11.

60.111 addresses the overall performance objectives.

In 60.111(a) it addresses protection against radiation exposures, releases of radioactive material.

In section (b), 60.111(b), it addresses the retrievability of the waste that we discussed just now.

60.112 is the overall system performance objectives of the geologic repository after permanent closure.

Most of the waste package requirements however are addressed in 60.113. In 60.113, specifically in paragraph (A), it says that containment of high level waste within the waste package shall not be less than 300 years nor more than 1,000 after permanent closure of the geologic repository.

And in 60.113(B) it addresses the 10 to the minus 5 control release requirement.

The next part of the requirement is 60.135. Over there it addresses the criteria for the waste package and its components, and it describes in detail that, you know, the waste package design must be designed so that the in situ chemical, physical and nuclear properties of the waste

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package and its interactions with the emplacement environment do not compromise the function of the waste package.

In 60.135(b)-- In (a) it also includes some of the factors it must consider, and these are listed as follows:

It must consider solubility, oxidation/reduction reactions, corrosion, hydriding, gas generation, thermal effects, mechanical strain, and so on.

In the other section, (b), it talks about specific criteria, things like it must not contain explosives, pyrophoric, and chemically reactive materials.

It should not contain free liquids, and how, you know, it should be designed for handling. It must be specifically identified so that you know, you know, what you put in.

And then there are more very specific requirements for waste form, including solidification, consolidation, combustibles, and so on.

And I think in the research project that we mentioned, that we discussed this morning, most of these criteria have been addressed.

In 60.137 and 140 specifically it talks about performance confirmation, and over there it talks about designing for anticipated events.

But you will find when you go through 10 CFR 60, even though, you know, the waste package was designed for anticipated events, we must also analyze for unanticipated events because that is required by the EPA standard.

In 60.142 it outlines the design testing required for the waste package, and over there it mentions about it must test for thermal interaction effects of the waste package, backfill, rock and groundwater.

And in 143 it talks about monitoring and testing of the waste package.

Now all these requirements must also conform with a quality assurance program. And what it says in 150 and 151 is basically DOE must have a quality assurance program and also it must conform to 10 CFR 60 Part 50.

DR. SHEWMON: What does that mean, 10 CFR Part 50?

MR. CHANG: 10 CFR Part 50? It has a list. It has quality assurance items--

MR. JOHNSON: 10 CFR Part 50 is the reactor regulation, and Appendix B is a series of quality assurance guidelines that nuclear power plant facilities must comply with.

DR. SHEWMON: I hope you don't generate as many tons of paper per ton of product as they do, and get more improvement in quality out of it for what you do spend.

MR. JOHNSON: I think our objective here is a high quality assessment by the Department of Energy and our objective isn't just to make paper, as you indicated.

DR. SHEWMON: Stated another way, it would be nice if you got a high quality of product. Whether a high quality of assessment would do that I don't know.

MR. CHANG: With all these requirements, you know, I guess if you want to reduce it to two questions, basically I guess as far as our project is concerned, --

(Slide.)

-- it's basically how is DOE expected to approach the waste packaging licensing issue and, as far as NRC is concerned, now is NRC going to use the technical assistance program that I am going to discuss described here to assess the DOE data or analyses?

(Slide.)

We expect the DOE to more or less follow these steps here. Basically we expect the DOE first of all to define what the waste package environment is first in terms of temperature history, groundwater flow, groundwater chemistry, and so on.

And then we expect DOE to perform a series of experiments and tests to develop a data base so that they could use whatever models that they develop to verify that indeed it satisfies the performance requirements.

And so basically you know it would probably reduce to the situation of trying to verify whether indeed their models reflect the real-life situation. And our program, the technical assistance program, basically will help NRC to put together methodology which we feel that we have confidence to use to evaluate whether DOE's claim for their waste package performance is adequate or not.

MR. STEINDLER: You indicated that you expect the Department of Energy to complete those four items you have up there. Is that your judgment of what DOE is supposed to do? Is that the NRC's judgment, or is that a statement that is written down someplace so that all the parties can understand that very clearly?

MR. JOHNSON: I think that those kinds of things are what DOE is doing right now, and that's what we expect to see as part of the license application.

DR. SHEWMON: Will that data base be available to use soon enough so that you can use it for your evaluation, or is it just available to tnem?

MR. JOHNSON: In order for us to do an evaluation, all of that information that goes into that license application would have to be available to us at the time they submit the license application. And we are hoping that we can get access to the information as it is generated so that we don't have to wait for the license application

before we first see a lot of this information.

DR. SHEWMON: How much are you getting currently that would be in the data base area which would....

MR. JOHNSON: I think that the data that we're getting is limited because the programs that DOE has in the waste package area have been pretty limited.

DR. SHEWMON: How long will you have to review this package after it is submitted?

MR. JOHNSON: The plan is for a three-year review period of the license application, although their current mission plan gives us 27 months.

DR. SHEWMON: Now this three-year review is before the hearing begins?

MR. JOHNSON: I think that review includes--

MR. GREEVES: The three years I believe Tim is referring to is the hearing process. The license application hits the door and then three years later the hearing process is supposed to be over.

DR. SHEWMON: My question was how long this data base was going to be in your hands before you were able to start making decisions.

MR. GREEVES: In theory we have access to data essentially as it is produced. We have an agreement with the Department of Energy that states that Staff has access to the data as it is produced.

Now I think you are fully aware that the Department probably produces enough data to inundate virtually everybody. And these Appendix 7 -- or the visits that Tim mentioned earlier are the mechanisms whereby some of the Staff would be able to, on a selective basis, go out to the site and take a look at some of the very recent data that the Department has been generating.

So we are not going to wait until the license application hits the door before we have visibility of data. We are going to be looking at that data essentially quickly after it is produced.

DR. SHEWMON: It's one thing to go across the country to look at it. It's another to have it in printed form so somebody who wants to indeed use the data base has it. Now that has to be in written form. You don't trust somebody's memory or trip notes.

MR. GREEVES: It is not a trivial problem to achieve what I'm sure your goals would be, but mechanisms are in place. Congress set up--

DR. SHEWMON: Are or are not?

MR. GREEVES: Are in place.

Congress set up the site characterization process in the Nuclear Waste Policy Act. It allow for consultation between DOE and NRC as DOE was gathering this data. And we cannot-- None of us can afford to wait until that license

application hits the door before we have access to and begin reviewing and looking at data and even asking questions.

DR. SHEWMON: And when is that first one supposed to hit the door?

MR. GREEVES: I was referring to the license application.

DR. SHEWMON: So am I.

MR. GREEVES: That's the document 19--

MR. JOHNSON: -- 91.

DR. SHEWMON: We've got a few years then.

MR. JOHNSON: I think that's a real good question because my feeling at this point is that DOE really needs to put a stronger emphasis on the waste package program in order to be in a position to submit the data by 1991.

MR. CHANG: 1991 may be the wrong time, but I think to address some of the long-term data, you know, I'm not too sure we really have that much time.

MR. ETHERINGTON: Is there a mechanism for expressing any reservations that you might have before the application is submitted by DOE?

MR. JOHNSON: Yes, there is. One mechanism was our review of the DOE's environmental assessments.

We also, as part of this workshop program,-- That's another mechanism for us to provide them feedback.

The next major one is the site characterization

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plans, the documents that are supposed to be available during 1986. And these documents will contain the test programs and plans that DOE will undertake in order to qualify the waste package.

And I think that will be the first time that we see in detail exactly what they want to do in order to demonstrate compliance with Part 60.

(Slide.)

Here is our program objective for our technical assistance program. Basically there are two objectives.

One is the program is supposed to put together methodology to assess whether the DOE's waste package design will meet the performance objectives of 10 CFR 60.

And the second objective is to identify information needed for NRC, you know, to do this job.

And I want to emphasize Number Two because basically on Number One, we are not expected to do DOE's job, and we do not have all the resources. So basically our emphasis is really on Number Two.

DR. SHEWMON: Where are you on that now?

DR. CHANGE: I think as I go through the Vu-graphs you will probably have some kind of judgment regarding where we are.

(Slide.)

To put it in engineering terms, what does it mean by this? That means that at the end of our TA project, TA program, rather, we want to be able to say according to NRC's methodology what is the predicted number of waste package failures as a function of time. That means, you know, we plot it in terms of number of failures versus time up to 10,000 years.

DR. SHEWMON: What is a failure? Is that a pinhole leak or busted in half and scattered around? Someplace in between, presumably.

DR. JOHNSON: A failure could be a pinhole crack if it results in the release of radionuclide materials.

Again, the failure criteria is, as I discussed earlier, it's a substantially complete containment. So the waste package has a number of different components, and because one of those items fails doesn't mean the entire waste package has failed.

DR. MARK: This is not hung on the leach rate at 1000 years?

DR. JOHNSON: There are two criteria: one is containment and the other is release rate. The failure

analysis that Dr. Shewmon was referring to I think refers to the containment performance objective. The release rate -- you would have a release rate after you've had failure of your containment features, or components in your waste package, and you have a mechanism for radionuclides to get beyond the waste package boundary.

DR. SHEWMON: But you're getting all tied up shoveling smoke here.

DR. CHANG: Probably No. 2 is more definitive in regard to answering your question, because failure may not mean very much.

DR. SHEWMON: It doesn't tell me from what you've said so far, but it should; so try again before you get rid of the slide: let's try to get an answer.

DR. GREEVES: Tim explained to you the significance issue earlier, and that is, essentially, to my way of thinking, an answer to your question about failure.

DR. SHEWMON: It's not to mine. Maybe I'm just slow.

It seems to me there has to be a question of how many curies per minute, or how many grams per hour, or something we are talking about which leaves the waste package.

DR. GREEVES: We're talking about rate at that point, and I think Tim explained earlier the analc y of

10 to the minus-5 as far as the significant failure that he essentially said was our position as far as the containment failure. Containment is either there or it isn't there, and...

DR. SHEWMON: Sorry; 10 to the minus-5 is neither here nor there, it is a finite rate.

What I'm trying to get is, if you have to talk about what a waste package failure is you have to know one when you see one, you have to have decided how big a hole it is, and what mechanism is carrying it through the hole. Or at least I don't know how you calculate a rate without that.

DR. JOHNSON: I agree that all of those pieces are part of determining what failure is and determining what the rate of release is; and all of those pieces are going to be part of our evaluation.

Now, DOE-- It's not quite as simple as saying, well you have a crack, you have a failure, because-- A crack in what? The design is a multi-component design, there are certain features that are going to have different rates at which they allow access to water and contact of nuclides with water, and transport of the radionuclides.

DR. SHEWMON: I agree all those things come in. But your goal is to predict the number. And if you can't identify one when you see one, then I have great difficulty knowing how you're going to predict the number.

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MR. STEVENS: Let me take a crack at this question of failure.

If, for example-- Now, when we're talking about the waste package here, we have to remember that we're talking not only about the waste form and the cans, but the packing material as well. NRC might some time in the future define a failure to mean any release of radionuclides of an observable quantity outside the package. If that were true--

DR. SHEWMON: It depends on how good your eyes are when you get to "observable," then.

MR. STEVENS: Or you could define an amount which you could model, have some sort of threshold, a calculated amount. Then if you have that criteria you could, with your modeling, tell how many failures, package failures, you had had, That could be defined to be very restrictive or very loose.

The point right now is, that does not have to be defined, and it probably should be deferred until you see the whole picture. Because the question of reasonable assurance and the degree of protection afforded by the geology and all the things that enter into the general determination of "reasonable assurance" will end up as part of that definition.

What we are doing is developing a methodology so

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that NRC can tell how much comes out, when. And then the failure can be defined arbitrarily or by judgment.

DR. SHEWMON: Well, could we rewrite that slide up there to say: The product of NRC's analysis of DOE's waste package performance will tell how much is coming out, when?

DR. CHANG: Basically the source term, then.

MR. STEVENS: The reason this occurs is that 10 CFR 60 is written to have two requirements: the substantially complete containment independent of how much comes out and the idea that whatever comes out should be smal' not pre-defined.

DR. SHEWMON: But Tim said earlier that they had defined some sort of a rate as to what could come out, and that wasn't zero. And so there is a finite leak rate that is possible. And it seems to me that before that first sentence can have any meaning that at least I can grasp, there has to be some way for getting between number of whatever is called failures in the words that he's using up there, and whatever these words are Tim was talking about in terms of how many curies come out. And that is what I'm trying to get a handle on.

MR. STEVENS: When Tim was talking about that policy that ties it to the amount that is allowable after the 1000-year period,--

DR. SHEWMON: Yes, but he also said that ties it to what's allowable before the 1000 years.

MR. STEVENS: Precisely. Now, inherent in that is a certain amount; which means that you could have a large number of packages failing, with failure being a very small release in that case, or a few packages perhaps with total release.

And so the number of failures that occurs depends on what you define as your failure criteria.

DR. SHEWMON: So that there is yet another dimension up here, and this predicted numbers of failure is a multi-dimensional space where there is predicted numbers of different sizes in addition to predicted numbers?

MR. STEVENS: It is at the moment. As time progresses it will become more clear.

DR. SHEWMON: You're an inveterate optimist.

MR. STEVENS: Always.

MR. CHANG: Okay. No. 2 is, basically, you can calculate the source term. Because in order to answer the second requirement, which is 10 to the minus-5, no more than 10 to the minus-5 on the radionuclides to that environment, you need to know what the source term is.

I'd like to remind you that 10 to the minus-5 is designed for an engineered barrier, it is not specif cally a part of the waste package. That means that they could

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design a system in which case you can end up with more than 10 to the minus-5 getting out of the waste package, but it would not get out of the engineered barriers.

DR. MARK: That 10 to the minus-5 doesn't apply to each package, it applies to the whole content of the repository?

DR. CHANG: That's right.

DR. MARK: So you could have one out of a thousand things fail completely, or one of 10 to the 5th fail completely and you're still okay?

DR. CHANG: That's right.

DR. MARK: The others are tight.

DR. CHANG: Of course if you can satisfy that requirement with the use of a waste package, basically you've got the job done.

DR. SHEWMON: If bentonite keeps everything from moving, you don't care about the waste package.

MR. STEVENS: And you haven't got a failure.

DR. MARK: I think I don't care about it already, but...

(Laughter.)

DR. MARK: The bentonite and the rock will look after everything, and that's the one comfort we have.

DR. STEINDLER: Is this Item 2, Source Term, anything other than an origin code?

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Your description sounded as though it was simply an enumeration of nuclide contents of the waste.

DR. CHANG: Okay; by "source term," we mean what gets out of the waste package.

DR. SHEWMON: Out of the engineered barrier.

DR. CHANG: That way you can keep the calculation.

DR. STEINDLER: So it's a source for a subsequent geologic migration into the near field.

DR. CHANG: That's right.

(Slide.)

We have five projects in the technical assistance program. The first one is 3164, which basically is an review of the overall DOE waste package program. This program started in 1981 and is going to end in 1985. It is being done by Brookhaven National Lab.

DR. SHEWMON: What division? Who, up there?

DR. HSU: I'm Peter Hsu, Brookhaven Lab.

It's in the Nuclear Waste Management Division. In that division we just work for the NRC in high and low level.

DR. CHANG: The second project is a review of waste package verification test program, and basically is done by the same group at Brookhaven National Lab.

The third project is basically a continuation of

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the Brookhaven project, because the Brookhaven project is going to phase out within a year or so.

The NBS has started--

DR. SHEWMON: What was your statement, that BNL is going to do what over the coming year?

DR. CHANG: BNL -- the work at BNL is phasing down. Basically the project will not continue any more after '85. The kind of work that will continue in this program basically will be done by the National Bureau of Standards. And the emphasis will be more on the test data.

DR. MOELLER: Will the Brookhaven group then just close shop?

MR. JOHNSON: Maybe I should go into a bit more detail as to what has transpired here.

The program that we had at Brookhave was run by Dr. Sweitzer, and Dr. Sweitzer decided that he would be more effective -- have more access to DOE information and be more effective to the repository program by doing consulting work directly for the Department of Energy. That obviously put us into a difficult situation, and we had decided to move our resources to do the same work but with the National Bureau of Standards rather than Brookhaven, because of the programmatic decisions made at BNL.

DR. CHANG: The fourth project that we have is at Oak Ridge. This project is also going to be phased out.

The reason is basically that this project basically defines what parameters should be considered, and what are the ranges of parameters, definition of some of the failure modes, and to more or less qualitatively prioritize all the failure modes that should be considered in NRC's evaluation.

The work has been more or less, 80 percent, completed.

Finally, you know, all of the work done here we feel like that we are at least able to do a first trial how to analyze the waste package.

So basically the 4165, the Aerospace work, is our first attempt to analyze DOE's waste package performance.

DR. SHEWMON: Are there yearly reports put out by the BNL program?

DR. CHANG: Yes. They're all on the table there. When I go through the individual projects I will also mention a few things about the reports.

Now, naturally, I think with all of the discussion yesterday and today I don't suppose you expect these projects by themselves to be able to do anything in regard to solving the problem.

DR. SHEWMON: Can we get on the distribution list for Aerospace's annual reports? Apparently we weren't on the ones at BNL and ORNL.

MR. JOHNSON: Yes.

DR. SHEWMON: Thank you.

DR. STEINDLER: Are the Aerospace reports issued under NUREG CR documents?

DR. CHANG: A draft has been done, it has not been published.

MR. STEVENS: It will be under NUREG.

DR. STEINDLER: The will be under NUREG.

MR. STEVENS: They will be NUREG documents. There's a pretty broad distribution of those.

DR. CHANG: All these five projects more or less draw upon the findings that you have discussed in your research project. And also, in addition to that, there are also other technical assistance projects that are sponsored by other branches of our division that we draw on for information.

DR. SHEWMON: Do you ever put out a wish list, or how do you make your particular prioritized list of needs known?

MR. JOHNSON: I think it is done through an interaction with Research as well as with the other groups within our division.

DR. SHEWMON: It is handled by word of mouth?

MR. GREEVES: Not totally. We do periodically write formal requests to the Division of Research

identifying what the areas, as perceived by us, are in most need of research.

DR. CHANG: What I'll do, I will flip over the list of research projects and technical assistance project.

(Slide.)

Practically all these have been discussed in this morning's discussion. A couple of them may not, and I have to find out which ones they are.

For instance, I don't think this one was discussed. (Indicating)

MR. STEVENS: Yes, it was.

DR. CHANG: So basically these are the reseach projects that we draw information from. I'm talking about NRC projects now.

DR. SHEWMON: Do you find that it meets all your needs?

DR. CHANG: I think our attitude is, you know, basically we try to use all information that is available from the DOE projects first, and in many cases we try to identify the information needs that are not adequate from the DOE project and then we reflect it to the Research people and they try their best to answer it.

In a lot of cases I'm afraid that, you know, we don't get all the information that we ask for. Most of them trying to get information on how to develop a predictive

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trying to get information on how to develop a predictive model. It's a very difficult problem.

DR. SHEWMON: By the time things get submerged someplace behind a computer code it becomes totally inscrutable. If garbage went in, it's a lot harder to tell by that time whether it did or not.

DR. CHANG: That's right.

(Slide.)

The other technical assistance contracts that we draw information from, these are the technical assistance projects. All of these projects are sponsored by our division; okay; these are not sponsored by our branch, but they are sponsored by our division.

You might notice that basically they are done by Sandia National Lab and Oak Ridge. And, incidentally, Virginia Hunter, who talked yesterday, also came from Sandia National Lab. I think they are basically the same group of people.

Okay; with that, I am going to go through very briefly each individual project in regard to what the past is and, finally, you know, what has been done.

(Slide.)

In 3164 there are two tests. Basically test one is to review the waste package data, and that means DOE waste package data, in regard to accuracy, reliability and applicability in regard to licensing needs.

And we also reviewed the DOE waste package in regard to DOE's technical approach and what are the limitations of those data, whether indeed they apply to the real world situation, also whether they can be modeled or not, because some are information -- for instance, they have a lot of leaching data measured at room temperature which we don't find in a repository, you don't have room temperature until maybe several hundred years, so it doesn't even apply for the first 3- or 400 years. Your waste package basically passes the test anyway.

DR. SHEWMON: Does this tie into some other, more global code that talks about how it gets from the waste pack breach to the engineering barrier or whatever -- yes, barrier was the word you used -- and how it gets from there into the near field or whatever Marty's term was?

DR. CHANG: Okay. I think the majority of the work is basically review on DOE's waste package data. There is some work on reviewing the model but not too much.

DR. SHEWMON: You aren't answering my question.

MR. JOHNSON: Let me try to. DOE is generating a lot of waste package data --

DR. SHEWMON: I want to know what NRC is generating.

MR. JOHNSON: That data is going to be used by the Department of Energy to develop models and a performance assessment analysis approach which will be submitted to us for our review. It will be our objective to understand those models, the approaches that were taken, the data that was used in order for us to make an evaluation that, yes, this data in the model does reflect reality that -- or it is conservative and it will meet the performance objectives. But our work is not oriented to developing those models but it was to develop --

DR. SHEWMON: I know this, I have been told it six times in two days and what your responsibility isn't. My question was it is halfway obvious to everybody in the room that the waste package is not the be-all and end-all. We have to worry about how it gets out to the environment. DOE will have a program, will you accept their computer program for modeling this or do you have your own for checking?

MR. JOHNSON: We probably will not develop an independent set of models in order to bounce DOE's results off of... There may be some cases where there are particularly pertinent areas which we do wish to model but we just don't have the resources to do a total independent

confirmation of everything they do.

DR. SHEWMON: Will you set somebody else to run their program?

MR. JOHNSON: Yes. We will have access to their codes, we will have people who are knowledgeable in the way those codes work and what is put into them.

DR. SHEWMON: Thank you.

DR. MARK: This particular thing on the board is essentially finished now --

DR. SHEWMON: It hasn't started.

MR. JOHNSON: This is the Brookhaven contract. The Brookhaven contract itself is winding down but the same type of evaluations of DOE data will continue under the National Bureau of Standards program.

DR. MARK: That's a different fin, though.

MR. JOHNSON: Yes, it is a different one.

DR. MARK: And the BNL contract goes to '85, I guess that's fiscal so it's over....

MR. JOHNSON: There is some additional money still left in that contract that will continue through a good part of fiscal year '86.

DR. MARK: What do you call that in a generator, it's the coastdown.

MR. JOHNSON: That's correct, right.

DR. STEINDLER: Let me ask a question:

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The Brookhaven group at one time was a fairly large and, I guess viewed by some folks, as a fairly competent group of people. You have told us about the disappearance of one guy and the decision made to move the program to the Bureau.

Are you buying the salvage, to some extent, the institutional memory and the rest of the useful aspects of the Brookhaven people that are not moving to DOE?

MR. JOHNSON: The Brookhaven people that aren't moving to DOE is primarily Peter Hsu and half of Terry Sullivan. The rest of that program that we had been working on in our study is all now with the DOE program.

Now in terms of the institutional memory, we have tried to phase out National Bureau of Standards beginning last year and we are still keeping on BNL through a good part of this fiscal year to try to smooth the transition and allow NBS the time to gear up and understand what the problems are.

(Slide.)

DR. CHANG: I think there was a question on verifying computer codes and basically we.... In A-1757, some verification work was done by Sandia, we checked their code and are relying on that.

(Slide.)

4171, as I mentioned, is a continuation of

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Brookhaven National Labs' project. So you will find that is very similar excepting we have added a couple of experimental tasks; that means we find some discrepancy in DOE's task, we like to verify, you know, spot-check it. But we do not expect that to be a large amount of work there.

(Slide.)

3167 is the second project we have at Brookhaven. Basically there are also two tasks. The first task is to identify the types of tests needed by NRC for DOE to do to demonstrate that the waste package can indeed meet the performance objectives. The second part of task one is to determine what are the test parameters and what ranges -- what is the lowest level and highest they can take in the waste package environment in all three types of repositories. And after doing that if there are any gaps then the project will identify them.

DR. SHEWMON: Is that largely completed?

DR. CHANG: That's largely completed, yes.

And the second task is more or less just a short-term technical assistance program; more or less like, you know, when we have SCR review, then we would request Brookhaven to do that or we have a special DOE report that we need to have reviewed, we would ask them to do that.

DR. SHEWMON: What do you feel is the most worthwhile idea that you got out of that?

DR. CHANG: All these tests here?

I would say number one and number three, because number one, more or less --

DR. SHEWMON: What types of tests did they say could be used or would be most useful? I ask you for an idea...

DR. HSU: One of the main functions in this 3167 program is to look at all the standard types of tests that MCC, for example, was generating for the DOE side. For example, we look at say the MCC test for solubility --

DR. SHEWMON: What's MCC?

DR. HSU: Materials Characterization Center out at PNL. They are the outfit that develops standardized tests for the DOE community.

So we look at these various leaching and solubility tests and, as experimentalists, we try and pick holes in them, suggest improvements. And when the NRC representative -- who is part of MCC as an observer at all the MCC meetings, he will transmit our comments to the MCC group to try and suggest these improvements in these test methodologies. That's one of the principal functions that we had in this verification test program.

We would also look at some of the DOE reports that might describe leaching and corrosion and we would evaluate whether are the actual test methodologies that the

various DOE groups were using were good, bad or indifferent. In some cases DOE, for example, would carry out leaching tests that lasted for 28 days in the ionized water. And a lot of this type of information isn't really applicable to the real repository environment. And we would come out with critiques of these reports and submit them back to the NRC as well. We would also write them up in our monthly reports and some of our bi-annual reports that you see here. And all of these, I think, are read by the DOE.

So on behalf of the NRC, we tend to give DOE feedback on what the NRC side feels is good about the program and what --

DR. SHEWMON: How many of these corrosion tests were there from MCC?

DR. HSU: There's only one that really sticks out in my mind and that was a standardized test for test corrosion cracking, and that one we critiqued --

DR. SHEWMON: Are we including leaching tests under corrosion?

DR. HSU: No, the leaching tests are something separate.

DR. SHEWMON: There's one of those?

DR. HSU: Five, low temperature, high temperature, solubility, they cover the whole gamut.

DR. SHEWMON: Is there a uniform corrosion test that has come out of there yet?

DR. HSU: I don't think I've seen one but in many cases MCC will take standard ASTM tests and modify them to their needs. But in many cases ASTM and MCC cases are quite similar.

DR. SHEWMON: Fine. Okay. Thank you.

(Slide.)

DR. CHANG: Okay. The next part is from -- this is the part done by Oak Ridge. Basically in all the work done, completed at Brookhaven, we gather all information on environmental parameters in the repository and also failure modes. Basically this report more or less consolidates all this information and tries to formulate a specification regarding what kind of model DOE can develop and how the model can be evaluated by NRC. And so this work is being completed and it will be done in a few months. And as a matter of fact, a draft has already been -- the first wording has been finished.

DR. SHEWMON: "Predict repository environmental parameters," is that chemical parameters or what?

DR. CHANG: Chemical, thermal and physical also in terms of the hydrostatic pressure and so forth.

DR. STEINDLER: How did you manage to complete this when, in fact, as far as I know, the Department of

Energy doesn't know what the parameters are? For example, the basalt program.

MR. JOHNSON: I don't think the project is completed in that we have a code that does everything. What is being done here is there are a series of smaller codes, say, that model thermal considerations, thermal parameters, mechanical parameters, transport parameters, corrosion parameters and DOE is in the process of developing a whole series of small codes. Now the question is how do you take all those small codes and put them together into one computer model that will appropriately interact all of those different codes at the appropriate time.

The code that Aerospace is currently working on is a mechanism for trying to integrate all -- a number of small codes, none of which are completely developed right now but we are using something in there and the code is more of a data manager, it allows you to take PDF's from individual codes, combine them together and they result in a final performance assessment, PDF, that will present reliability data, overall reliability data from all of your input codes.

DR. CHANG: The report for this project is not the code itself --

(Slide.)

-- rather, it points out a different aspect. For

instance, it identifies the pertinent repository parameters that cannot be neglected if you want the analysis to be of any meaningful help; and secondary, would be to identify and prioritize qualitatively those pertinent failure modes that should be considered like stress corrosion cracking, hydrogen embrittlement and whatnot, what are the parameters that should be included.

DR. STEINDLER: Are you saying that this program has identified and prioritized the pertinent failure modes?

DR. CHANG: Yes, qualitatively.

DR. STEINDLER: Well you are certainly well ahead of DOE, aren't you?

DR. CHANG: I think this work --

DR. STEINDLER: -- materials of construction and you are already prioritizing failure modes, that strikes me as being a little ahead of the game.

DR. CHANG: It is our first try. We don't claim that we have identified all failure modes, but we have done it to an extent that we can.

MR. JOHNSON: I think the approach here is to develop a method by which you can evaluate all the failure modes. It doesn't matter if we have chosen the correct ones at this particular time or not, but it presents a method by which these interactions can be related.

DR. STEINDLER: You can go through the exercise,

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I guess, but I guess my problem is if you can't evaluate whether or not what you have done is correct the whole exercise may be somewhat....

MR. JOHNSON: Well I think the answer that comes out may not be the same answer that DOE will get ultimately with their project but the objective here isn't to get an answer, it is to develop an approach to handling this kind of information and the data.

DR. STEINDLER: Yes, but what I'm saying is having gotten the approach you won't know whether that's right or wrong unless you get an answer and check the answer against the real world, and if you can't do that then you won't be able to tell whether that methodology makes any sense. Do I have this all backwards?

DR. HSU: Well in some cases I think that most people who have worked in the corrosion field have a very good idea of what metals like this are failed by or mechanisms. For example, carbon steel, most people might think that pitting is very important, niform corrosion is also something that has to be considered, hydrogen embrittlement. There's a suite of different corrosion mechanisms that people have to consider. And you may find a lot of information in the open literature that will tell you which ones of these are very important and which ones are trivial.

In terms of, say, uniform corrosion for a canister maybe this thick (indicating), nobody really thinks that this is going to fail by that mechanism so that might be right at the bottom of the pile. Well in respect to pitting, for example, that might be somewhere very high up. And if there is disagreement between NRC and DOE then that comes up for negotiation.

But what the NRC tries to do is to make sure that no potential failure modes slip through the cracks. We want to make sure that DOE addresses all possibilities.

And in some cases the NRC does sponsor research to try and identify failure modes. I know in the early days when we started to work on titanium-based materials in a salt repository environment, the people at Sandia were saying that crevice corrosion was not a problem. Within about six months of starting our problem in the research office, we decided that it was and I think that DOE is now actively considering this as a substantial failure mode in a salt repository program.

DR. SHEWMON: Okay.

DR. CHANG: One of the main reasons we go through this project is basically we have to go through the same steps that more or less DOE has to go through in order to put together our methodology also.

(Slide.)

So before the project goes quite so far, the final step is really the Aerospace project of trying to evaluate all of the methodologies that DOE has presented so far for long-term performance of waste packages.

And in doing task one, we have also tried to use task two. Basically task one and task two are completed. The bulk of task two is more or less a survey of qualitatively outlining all the failure modes which should be considered. And we have tried to crank out a few evaluations but we found out the job is really very tangled.

And the third task here is basically to perform a performance assessment of DOE's waste package design.

DR. STEINDLER: You say this program started in '83?

DR. CHANG: Yes.

DR. STEINDLER: And you have not yet issued the first annual report?

DR. CHANG: We have a draft methodology report and that is the next item I am going to cover.

(Slide.)

Okay. These are reports that we have completed. Under 3164 we have an annual report as well as semi-annual report.

Now what we did in Brookhaven is after completing

a semi-annual report we would compile and update information we did in previous years so that, you know, by looking at the last report you had some idea of what the previous report is. We have a total number of seven volumes and there is an eighth volume going to be completed within six months or so, and that will conclude on this project here (indicating).

The second project, the second group of reports, comes from 3167, also done by Brookhaven, and we have completed six volumes and I believe there is a seventh volume coming out.

Okay. The other reports basically are also done in Brookhaven's work under 3164 and -67. These are actually -- Do we have a GTP for this?

And this here, waste package reliabilty, basically generates that information of the GTP that was discussed yesterday.

DR. SHEWMON: Where does that one come from?

DR. CHANG: This one here?

DR. SHEWMON: Who wrote that one?

DR. CHANG: Brookhaven.

DR. SHEWMON: Which part of '85 is that coming out in? If it has a NUREG number, does it mean it's on the street?

MR. WICK: The report has not been issued as a

NUREG. I hope to get that issued in November as a NUREG. I handed out the report as I got it from Brookhaven; it was one of the handouts yesterday.

DR. CHANG: And this is the report that I discussed with you just now. This was completed in '83 and in regard to the Aerospace report we have a draft done and that was completed, the draft was done in May 1985 and I have copies of all of these reports right here, so if you want to take a look at it...

DR. SHEWMON: The question was on seven and you have told us about six other reports now. When you point at the table, do you have a draft of seven on the table?

DR. CHANG: I have a draft of this one, number seven.

DR. SHEWMON: When you point at what's on the table --

MR. STEVENS: That's a combination of Brookhaven, Aerospace and Oak Ridge.

MR. GREEVES: This entire package is a combination --

DR. SHEWMON: I know, but five minutes ago he asked about number seven and...

MR. STEVENS: Let me address that. The Aerospace work -- it started late in calendar 1983. The Aerospace work has been documented in a number of small reports and

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there he has listed the methodology report, the major one. That's the one he's talking about in answer to your question.

DR. SHEWMON: That didn't answer my question.

DR. STEINDLER: The reason for the concern is obviously that should be a very important report to have DOE read to understand what you're driving at, that is, what NRC is driving at when they talk about assessing methodology. The sooner it gets out on the street the sooner the DOE program can react in some way.

MR. STEVENS: This particular report has already gone through two drafts and has been distributed to all of the DOE repositories, DOE headquarters, Dr. Pickford and his people, they have all got inputs which are partially reflected in this version and will be totally reflected in the version coming out as a NUREG approximately in November.

DR. CHANG: Basically we are in the process of making changes in those reports to reflect the comments.

DR. SHEWMON: I was just looking for the report that Everett said he handed out yesterday and I guess I found it. It is something with a table of contents on it.

Go ahead.

(Slide.)

DR. CHANG: So basically what does this mean, this report? So far what has it done for NRC other than --

because we are not at licensing time yet. So I have summarized four areas where these reports have helped us, the materials section, in our work so far.

Basically in March 1983 we completed NUREG 0960 which addresses the BWIP draft, the site characterization report, and we used this report here to support a large argument that we made.

The second area is the recent draft EA that we completed a few months ago and we also used these reports extensively. The work that is being done on this project is also used to support the waste package workshop that we have, for instance, in NTS our waste package workshop in July. And other than that we have also developed short-term testing capability at Brookhave. We are able to do short-term testing and corrosion, leaching and so on.

DR. SHEWMON: It seems to me a fundamental question which, if we have heard an answer to it, I have missed it, which had to do with given these various areas which you have what kinds of pour-through it is going to constitute, what kind of a leak rate over time. The fond hope was that as we covered the Aerospace -- my fond hope was we might learn something. I haven't learned anything about it yet except it is a hard problem.

DR. CHANG: The Aerospace project, we only addressed the waste package.

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DR. SHEWMON: There are still three barriers in that waste package and there is a fundamental question about how big a hole does it take before indeed the waste package leaks significantly. And you guys define significantly, nobody else. Now when do we get an answer to that or who is working on it?

DR. CHANG: We have done some scoping calculations and, Ken, you may want to address who is responsible.

MR. STEVENS: There are several barriers in the waste package, the outermost being the package material which to a degree may inhibit water coming in and radionuclides going out. Next in you have the overpack and there is a separate canister inside and finally the waste form inside.

What we are doing is looking at the process going in and coming out, that is, how much time is required for the water to get through the package, how much time is required for the water to corrode the overpack and go in succeedingly, then leach the nuclides from the waste and then transgress out.

Yesterday, when we talked about these probability distribution functions there are certain delays provided by each of these barriers so you can use that to find the time of egress of radionuclides from the whole system. Along that process you can also calculate the amount of radionuclides coming out.

DR. SHEWMON: And the amount is also proportional to the cross-sectional area at each point very often.

MR. STEVENS: Yes.

Now DOE has not decided whether they in their

own analysis will take credit for particular barriers or the way in which they will take credit.

For example, one of the DOE projects has said that until they develop a good mechanism for relating this cross-sectional area to release, if they get any appreciable hole in the container they are going to consider it failed just by definition and then they don't have to worry about that.

DR. SHEWMON: I don't know what "failed" means. Does it mean that everything there stands stark naked or does it mean there is no resistance from a square millimeter someplace in the can?

MR. STEVENS: If they take that approach and they say we have defined this package to have failed, one can assume conservatively obviously that the package is no longer there. That's very conservative.

What we want to do is to develop a capability to be able to tell realistically how much protection is or is not afforded, irrespective of what DOE says now or later it is going to claim because they might get caught with their...

DR. SHEWMON: Their can down.

MR. STEVENS: -- with a need to come back for more credit. We would like to be able to have the capability to analyze this.

DR. SHEWMON: If we come back next year maybe I will get an answer to my question, is that what you're telling me?

MR. JOHNSON: A lot of the information that goes to answering the question you asked is going to be generated by the Department of Energy. In other words, if you have a canister, you have two pits in there, how fast does water get through, how fast do the radionuclides come out; we are depending upon DOE to provide that information if they choose to go in that direction of taking credit for that kind of scenario.

DR. STEINDLER: What you have just heard is that you are not going to do that, what you have just heard is you are going to do a more realistic calculation in order to be able to evaluate the situation.

MR. JOHNSON: We are going to try to have the mechanism to do that but we are going to try to be depending a lot for the details on the Department of Energy.

MR. GREEVES: I think what we need to do is revisit one of the slides that Hub Miller put up yesterday and it is this busy one in Seth Copeland's handout and it essentially is intended to portray a picture of where we need to be in license applications and we're not there yet. The calculations have not been made, these folks are struggling trying to get in a position to be at the point

shown on this chart. We're in the waste package environment anyhow. We will be utilizing codes and models not developed independently by NRC but once it exists and we have had a chance to revalidate it at a level. We need to be at that point at the license application stage and these folks are in the process of getting there.

This particular approach is laid out in what we call our licensing assessment methodology document, the strategy document, and we would be happy to provide that to you. It's just I think a little bit too much to absorb at this meeting. I would ask you to revisit --

DR. SHEWMON: Yes, and what I am afraid I would get would be something that shows 64 boxes connected by 132 arrows pointing in all directions and it still wouldn't really comfort me a particular amount. I guess what I am looking for is some idea that somebody has sat down with a certain number of models and said if it goes by only diffusion that is a failure, if it doesn't go by diffusion and you've got hydrothermal reaction, if there too vection, it takes out this step and goes this fast with a given cross-sectional area.

But so far all we are doing is generating diagrams that talk about things we are going to plan to do some day.

MR. STEVENS: No, the work we are doing is now in

process and we are doing just what we want to do. It is not completed yet and it won't be completed for some time. But we share your same concerns.

DR. HSU: There is a small amount of work at Livermore which might be leading the way experimentally and this comes as the leaching of spent fuel with cladding around it. What they've done is to put end caps in small sections of clad U02 fuel from the reactor and they have actually drilled small pinholes and made small cracks in it using a laser and they have placed these in water and measured the radionuclide increase in the surrounding water. And in some cases, they are looking at the periphery of the cracks and the pinholes and they are finding that there is a small amount of deposition there of some of the fuel that is being leached out, so there may be a plugging mechanism.

DR. SHEWMON: Well you're an experimentalist apparently and so am I, but it seems to me in this case you have to have an analytical basis for taking experiments and expanding them to areas where you don't have --

DR. HSU: That's right, yes.

DR. SHEWMON: And that's where I have not seen anything other than very big generalizations.

DR. HSU: I think that maybe we are in a situation where the modelers are trying to be the

experimentalists, but some of this data coming out of Livermore -- which I think will also be expanded to try to look at real failed pins with real natural defects like go into the reactor which might give us a leg up and help the modelists.

DR. SHEWMON: Thank you.

Are there other questions?

(No response.)

DR. CHANG: That's about all I have. I think if you want to discuss a little bit more on the Aerospace project Ken would be prepared to discuss for you on several programs exactly how much we have done because that might give you a little bit of an idea....

MR. STEVENS: One thing I might mention is that this preliminary analysis of the BWIP waste package we hope to complete towards the end of this fiscal year, around June.

DR. SHEWMON: Now is that the draft that came out in May or is this --

MR. STEVENS: The draft was just a report discussing the methodologies that we are using, we have gone far beyond that draft now in terms of what we are actually doing and we hope to complete this preliminary BWIP analysis around May or June of fiscal '86.

DR. SHEWMON: Now when you do that do you take

best estimates or do you do some sensitivity analysis with regard to range of variables to see what the effect is --

MR. STEVENS: Each of the barriers will have models relating to the time required to fail that barrier. Corrosion, for example. And we certainly hope to have not only uniform corrosion but a pitting corrosion model of some kind in there that will tell you when you project that barrier will fail. It won't be a one-number estimate but rather a distribution of failures which will affect the fuzziness.

Now once this is done, you can run the model repeatedly changing the inputs to do the sensitivity analysis. Of course, there are efficient ways of optimizing that sensitivity analysis.

MR. GREEVES: I think that's what you've been looking for and we're just not quite there.

DR. SHEWMON: What I am looking for partly is some evidence of what kind of physical thinking you have going into the model, I also haven't seen any evidence of that. It's partly a strategic question with regard to what you choose to present and partly a package for us as to who we ask to come in and talk about it.

MR. STEVENS: Our role is one of an integrator. We have helped NRC pick a methodology for putting the pieces together. We heard this morning from the research people

talking about understanding the phenomena such as corrosion and then such work gets translated into a model and then the models get ultimately turned into probability distributions for things such as radionuclide releases or barrier failure time. We are putting those pieces together to try and insure that the railroad tracks, while although parallel, do not go like this (indicating). That's our goal.

Now of course it is an iterative process and when we see holes in the available information that's fed back to NRC and DOE and the appropriate research is sought; in many cases it already exists, it's a question of digging it out in some cases.

DR. SHEWMON: When you talked about failure as in corrosion failure, is there any cross-sectional area of penetration at that point in time or does the canister disappear as an effective barrier from that time on?

MR. STEVENS: The methodology is set up so that you can choose whatever failure criterion you want. You can either have a pinhole, a tiny breach, or you can have the whole thing fail or anything in between, you choose.

DR. SHEWMON: Fine. But when you have a pinhole then does that mean that you communicate with the outside only through a pinhole and you have solved the problem of the flux through that pinhole or does that mean that the canister has disappeared -- the pack, I guess, is your word

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-- that the pack has now disappeared and that the package, that hunk of steel, is no longer a barrier for communication with any part of what is inside?

MR. STEVENS: We ultimately hope to be able to have the capability of relating the degree of fa'lure of the package to the releases from the package. We don't have that yet. DOE doesn't have that yet.

In their past program at BNL the performance assessment system study or whatever they are trying to do that, they are confronted with the same challenges. It is difficult to relate the protection afforded by the partially failed barrier before we and they have that capability. We just have to either bound the problem when we are talking about releases from a failed container, make conservative estimates or whatever.

DR. SHEWMON: So it's a bunch of things in series in which one is rate control as the way some of us are brought up to think about things and that's part of what's frustrated....

DR. STEINDLER: This morning in the Battelle presentation, the question was asked concerning the relationship between the results to be obtained on spent fuel and the models or the application in some fashion or another was number one vague or number two postponed until this afternoon when we were going to get the answer on how

those two things fit together. And I must say, we have gotten to the end of the viewgraphs and I haven't seen how they fit together. I don't see yet how the result out of BCL's work is going to fit into any model that I've heard about.

Can you enlighten me?

MR. STEVENS: Yes. In order to get these failure distributions for individual barriers you need things such as leach rates from the materials so things such as the Battelle Columbus work would feed directly into that.

We maintain the close contact with them and the other projects to have access to their information and work it into whatever we are doing.

Another example is the work that DOE is doing at Westinghouse Hanford specifically for the TUFF program and the leaching of spent fuel; Peter alluded to that earlier. And so what they're trying to do is get rate functions for leaching from spent fuel as it comes out of reactors, not assuming any further degredation but just simply the degree of the failed fuel that comes from the reactor, whatever that is. And they are doing actual experiments that are also synthesizing holes in not failed fuel and things like that. So what we do is take that information and use it to turn into distributions.

DR. STEINDLER: The comment I made this morning

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indicated -- or at least I thought I was trying to indicate, the fact that you get a result out of experiments, at least as I understand them, is very much geometry-dependent. If the experimental program is going to work its way through all conceivable geometries of failures of various sizes of a single fuel rod, neither you or anybody else is going to meet the schedules that are required.

Further, you indicated that the model is going to be ready fairly soon --

MR. STEVENS: The first pass.

DR. STEINDLER: -- the model will allow you to, at least on a computer, pick any failure criterion you would like. That to me sounds like a disconnect between the rather slow and perhaps guided, but not obviously so, experimental program and the model development.

Do I have a wrong picture?

I'm worried precisely about the kind of track that you just held up, namely, the left hand rail of railroad A is going to meet the right-hand rail of railroad B somewhere even though they are both six feet two inches apart or whatever the gauge is.

MR. STEVENS: One of the challenges in this business is the realization that simplifications have to be made in order to get the job done. And, as an example, our first pass will not include the degree of protection

afforded by a partially-failed barrier except in a sort of gross ratio sense or something like that. We certainly hope that as better information comes in from DOE and NRC research programs and our own work we will be able to whittle away at these simplifications and get better and better at it. It may be that it will be in NRC's best interest to simply rely very heavily on the models that DOE is using and convince itself -- NRC, that is -- that they're good models and then run it using NRC's inputs. There is no reason it has to be an either/or, that's balanced against what we're doing --

DR. SHEWMON: Finish your sentence and then I have one more.

MR. STEVENS: I'm finished.

DR. SHEWMON: Tell me a little bit about the hydrothermal aspects and how you would model that. It seems to me the fact that you've got a heat source here and a heat engine has to influence an awful lot of the flow. Is that part of what you are modeling or does that come next year?

MR. STEVENS: We are going to rely very heavily on the model that has been done by other people in this regard. DOE has done some hydrothermal modeling using one, two, and in some cases three dimensional codes which look at the hydrostatic flow as well as the temperatures.

So what we are doing is taking the results from that work and hypothesizing the environment that the waste packages in the respository are subjected to and looking at the effect on individual waste packages of things such as temperatures and pressures.

DOE has indicated that they will probably make some relatively homogeneous assumptions about the temperatures that packages are subjected to, at least in their initial work, and not do a lot of very sophisticated hydrothermal modeling. At least they haven't so far. They may ultimately. We certainly are not in a position to do it more extensively than they do.

What we have discovered is that there are ways to get response surface functions and run models a few times and see basically what the environment is there, and then plug that into the analysis without having to try to simulate everything at once with hundreds of variables. That would be an untenable problem.

DR. SHEWMON: The problem, though, that is fundamental but may not be untenable is is the rate

controlling the process usually a static fluid, is it thermal hydraulics in terms of its heat generation that pumps it around, or is it the rate at which the local river is flowing past this thing?

How would you go from that-- Which one of these is the model to try to use it seems to me is fundamental.

MR. STEVENS: Yes. Now you may be familiar with DOE's model and one that BWIP is using now, CHAIN-T-MC, which is a Monte Carlo radionuclide release and transport model. It is a rather sophisticated model that tries to couple as many of these things as possible in there.

And even though it has not been validated in the formal sense, it is rapidly proceeding toward that state. And we have access to that model and we hope to be using it as well as looking at where its weaknesses might exist.

DR. SHEWMON: That's all I have.

MR. KASSNER: Getting back to this corrosion thing, in these charts there was some information on the thickness of the containers. If we go back there simply, were those things -- were those thicknesses of the container based on any measure of expected corrosion rates, or were they just picked on available sizes?

MR. STEVENS: During the design process, DOE has worked a number of assumptions into the container design. Many of the container designs, as I understand it, were

based essentially on uniform corrosion. In some cases there was some sort of fudge factor thrown on for the non-uniform corrosion.

My understanding is that even though DOE has not yet very extensively looked at pitting corrosion because it is very difficult to quantify, they recognize -- and NRC articulated it rather firmly in their comments on the environmental report -- that it is necessary to look at non-uniform corrosion.

The design process that DOE has set up has an iterative process and if another failure mode is hypothesized or if you can get a handle on it, then appropriate design changes have to be made.

The policy apparently is that there are certain things that we want to assure. We want to assure that the can is going to be there for the required time, considering mechanical failures and corrosion and other things. So if it turns out that we are too close to the margin we'll make it thicker or we'll use another material.

MR. STEINDLER: Are you suggesting that radiation reduction or shielding was not an issue in the design criteria that DOE used?

MR. STEVENS: Oh, no, it is. In fact the self-shielding design such as the very thick, say ten inches or so, or even more for the salt program had a great deal of

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effect of radiolysis which, in turn, has an effect on the chemistry which, in turn, affects the corrosion.

So the salt program has put a lot of emphasis on comparing the self-shielded and the not so thick designs the other repositories have but to a much lesser degree.

DR. SHEWMON: Thank you very much.

I guess that concludes our session for today. The Committee will now go into executive session to consider our report. I think that means that you are free to leave or abuse yourself in any way you want to. Thank you for coming.

(Whereupon, at 3:48 p.m., the meeting of the committee was adjourned.)

CERTIFICATE OF OFFICIAL REPORTER

This is to certify that the attached proceedings before the UNITED STATES NUCLEAR REGULATORY COMMISSION in the matter of:

NAME OF PROCEEDING: ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

SUBCOMMITTEES ON
WASTE MANAGEMENT AND
METAL COMPONENTS

DOCKET NO.:

PLACE: WASHINGTON, D. C.

DATE: FRIDAY, OCTOBER 25, 1985

were held as herein appears, and that this is the original transcript thereof for the file of the United States Nuclear Regulatory Commission.

(sigt) Anne G. Bloom

(TYPED)
ANNE G. BLOOM

Official Reporter
ACE-FEDERAL REPORTERS, INC.
Reporter's Affiliation

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(sigt) David L. Hoffman
(TYPED)
DAVID L. HOFFMAN

Official Reporter
ACE-FEDERAL REPORTERS, INC.
Reporter's Affiliation

OVERVIEW OF HLW MATERIALS TA PROGRAM

TOPICS TO BE DISCUSSED

1. PROGRAM OBJECTIVES

2. TA CONTRACTS IN FORCE

3. RELATED CONTRACTS

4. DISCUSSION OF SPECIFICS OF TA CONTRACTS

REGULATORY ISSUES OR LICENSING FINDINGS TO BE SUPPORTED BY THIS PROGRAM

60.11A(6), 60.11A(7), 60.11A(8)	CONTENT OF THE SCR
60.111, 60.112, 60.113	OVERALL PERFORMANCE OBJECTIVES
60.135(A)	CRITERIA FOR THE WASTE PACKAGE AND ITS COMPONENTS
60.137, 60.140	PERFORMANCE CONFIRMATION
60.142	DESIGN TESTING
60.143	MONITORING AND TESTING WASTE PACKAGES
60.150, 60.151	QUALITY ASSURANCE

(A) HOW IS DOE EXPECTED TO APPROACH THE WASTE PACKAGE LICENSING ISSUES?

(B) HOW WILL THE PROGRAM HELP ASSESS THE DOE DATA OR ANALYSIS?

(A) DOE EXPECTED TO:

1. DEFINE WASTE PACKAGE ENVIRONMENT

2. PERFORM EXPERIMENTS AND TESTS TO DEVELOP DATA BASE FOR MODEL

3. MODEL PERFORMANCE OF WASTE PACKAGE IN THAT ENVIRONMENT

4. PERFORM TESTS TO VERIFY MODEL PREDICTIONS

(B) PROGRAM WILL HELP NRC BY ASSEMBLING THE METHODOLOGY WHICH NRC CAN USE TO EVALUATE WASTE PACKAGE PERFORMANCE

PROGRAM OBJECTIVES

1. ASSEMBLE METHODOLOGY TO ASSESS WHETHER DOE WASTE PACKAGE DESIGNS WILL MEET PERFORMANCE OBJECTIVES OF 10 CFR PART 60

2. IDENTIFY INFORMATION NEEDED TO ASSESS WHETHER DOE WASTE PACKAGE DESIGNS WILL MEET PERFORMANCE OBJECTIVES

PRODUCTS OF NRC ANALYSIS OF DOE WASTE PACKAGE PERFORMANCE

1. PREDICTED NUMBER OF WASTE PACKAGE FAILURES AS A FUNCTION OF TIME.

2. SOURCE TERMS

   A. TOTAL

   B. INDIVIDUAL RADIONUCLIDES

HLW MATERIALS TA PROGRAMS

FIN	CONTRACTOR	TITLE	CONTRACT PERIOD
A-3164	BNL	REVIEW OF DOE WASTE PACKAGE PROGRAM	1981-1985
A-3167	BNL	REVIEW OF WASTE PACKAGE VERIFICATION PROGRAM	1981-1985
A4171	NBS	EVALUATION AND COMPILATION OF DOE HLW PACKAGE TEST DATA	1985-1987
B-0288	ORNL	EFFECT OF REPOSITORY ENVIRONMENT ON PERFORMANCE OF WASTE PACKAGE/ ENGINEERED SYSTEM COMPONENTS	1983-1986
A-4165	AEROSPACE	PREPARATION OF ENGINEERING ANALYSIS FOR HLW PACKAGE IN GEOLOGIC REPOSITORIES	1983-1986

SOME RELATED RES CONTRACTS

FIN	CONTRACTOR	TITLE	CONTRACT PERIOD
A-6764	BCL	LONG-TERM PERFORMANCE OF HLW PACKAGING MATERIALS	FY82-86
A-3237	BNL	CONTAINER ASSESSMENT	FY83-85
A-3269	BNL	PITTING CORROSION	FY84-86
A-3040	LBL	GEOCHEMICAL ASSESSMENT OF NUCLEAR WASTE ISOLATION	FY82-84
A-2230	ANL	LAB ANALOG OF LEACHING	FY82-86
A-2239	ANL	MODIFICATION OF BACKFILL MATERIALS	FY82-84
B-7278	MSC	EFFECT OF MANUFACTURING PROCESSES ON MATERIAL PROPERTIES AFFECTING FAILURE MECHANISMS IN HLW CONTAINERS	FY84-87
A-1266	SNL	DEVELOPMENT OF A METHODOLOGY FOR RISK ASSESSMENT OF NUCLEAR WASTE ISOLATION IN ALTERNATIVE GEOLOGIC MEDIA	FY83-85
D-1146	NBS	STATISTICS OF WASTE PACKAGE FAILURE BY PITTING	FY84-86

SOME RELATED TA CONTRACTS

FIN	CONTRACTOR	TITLE	CONTRACT PERIOD
A-1756	SNL	GEOCHEMICAL SENSITIVITY ANALYSIS	FY84-87
A-1158	SNL	REPOSITORY SITE DEFINITION AND TECHNOLOGY TRANSFER	FY79-87
A-1165	SNL	TA FOR PERFORMANCE ASSESSMENT	FY81-87
A-1757	SNL	TA IN NUMERICAL MODELING ASSESSMENT OF HLW REPOSITORIES	FY84-86
B-0287	ORNL	TECHNICAL ASSISTANCE IN GEOCHEMISTRY	FY82-84
B-0290	ORNL	LAB. EVALUATION OF DOE RADIONUCLIDE SOLUBILITY DATA, RETARDATION PARAMETERS, LAB. TECHNIQUES	FY82-87

FIN A-3164 REVIEW OF DOE WASTE PACKAGE PROGRAM

TASK I: REVIEW OF WASTE PACKAGE DATA BASE FOR:

A. ACCURACY, RELIABILITY AND APPLICABILITY

B. TECHNICAL APPROACH

C. LIMITATIONS OF THE TESTING OR DATA COLLECTION TECHNIQUES

D. SIGNIFICANCE OF THE DATA REGARDING RESOLUTION OF WASTE PACKAGE PERFORMANCE ISSUES

TASK II: GENERAL TECHNICAL ASSISTANCE

FIN A-4171 EVALUATION AND COMPILATION OF DOE WASTE PACKAGE TESTS DATA

TASK I: REVIEW OF WASTE PACKAGE DATA BASE FOR:

A. ACCURACY, RELIABILITY AND APPLICABILITY

B. TECHNICAL APPROACH

C. CONCLUSIONS DRAWN

D. SIGNIFICANCE OF THE DATA REGARDING RESOLUTION OF WASTE PACKAGE ISSUES

TASK II: IDENTIFICATION OF ADDITIONAL DATA REQUIRED AND IDENTIFICATION OF TESTS TO GENERATE THE DATA

TASK III: EXPERIMENTAL TESTS

TASK IV: GENERAL TECHNICAL ASSISTANCE

FIN A-3167 WASTE PACKAGE VERIFICATION TESTS (BNL)

TASK I. EVALUATION AND IDENTIFICATION OF PERFORMANCE VERIFICATION TESTING

1. IDENTIFY TYPES OF TESTS NEEDED TO DEMONSTRATE THAT WASTE PACKAGE CAN MEET PERFORMANCE OBJECTIVES OF 10 CFR PART 60

2. DETERMINE TEST PARAMETERS (AND THEIR RANGES) THAT WILL REPRESENT THE WASTE PACKAGE ENVIRONMENT IN BASALT, SALT AND TUFF REPOSITORIES

3. IDENTIFY INFORMATION NEEDS NOT PRESENTLY IN TEST PLANS THAT MUST BE SUPPLIED AS PART OF SITE CHARACTERIZATION

TASK 2. SHORT-TERM TECHNICAL ASSISTANCE AS REQUESTED IN WRITING BY THE NMSS PROJECT MANAGER

FIN A-0288 EFFECT OF REPOSITORY ENVIRONMENT ON PERFORMANCE OF WASTE PACKAGE/ENGINEERED SYSTEM COMPONENTS. (ORNL)

TASK I: SPECIFICATION OF A MODEL/METHODOLOGY TO PREDICT REPOSITORY ENVIRONMENTAL PARAMETERS

TASK II: EVALUATION OF MODELS

TASK III: GENERAL TECHNICAL ASSISTANCE

CONTENT OF NUREG/CR-4134 - MAY 1985

1. IDENTITY OF THE RELEVANT REPOSITORY PARAMETERS

2. IDENTIFY AND PRIORITIZE PERTINENT FAILURE MODES

3. GUIDANCE ON FEATURES OF THE MODELS/METHODOLOGIES CONSIDERED ACCEPTABLE FOR WASTE PACKAGE PERFORMANCE EVALUATION

4. A SUMMARY REVIEW OF EXISTING MODELS/METHODOLOGIES CURRENTLY EMPLOYED IN DETERMINING ENVIRONMENTAL PARAMETERS RELEVANT TO WASTE PACKAGE PERFORMANCE

FIN A-4165	PREPARATION OF ENGINEERING ANALYSISF FOR HLW PACKAGES IN GEOLOGIC REPOSITORIES (AEROSPACE)
TASK I.	EVALUATION OF METHODOLOGY TO ASSESS LONG-TERM PERFORMANCE OF WASTE PACKAGE
TASK II.	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
TASK III.	ASSESSMENT OF PERFORMANCE OF DOE WASTE PACKAGE DESIGN

REPORTS COMPLETED TO ADDRESS VARIOUS ASPECTS OF WASTE PACKAGE EVALUATION

1.	REVIEW OF DOE WASTE PACKAGE PROGRAM	(NUREG/CR-2482, 7 VOLUMES FEB. 1982 - SEPT. 1984)
2.	REVIEW OF WASTE PACKAGE VERIFICATION TESTS	(NUREG/CR-3091, 6 VOLUMES APR. 1983 - JULY 1985)
3.	WASTE PACKAGE PERFORMANCE AFTER REPOSITORY CLOSURE	(NUREG/CR-3219, VOL. 1, AUGUST 1983)
4.	WASTE PACKAGE RELIABILITY	(NUREG-0997 - 1985)
5.	REPOSITORY ENVIRONMENTAL PARAMETERS RELEVANT TO ASSESSING THE PERFORMANCE OF HLW PACKAGES	(NUREG/CR-4134 - MAY 1985)
6.	POST EMPLACEMENT MONITORING	(NUREG/CR-3219, VOL. 2, MAY 1983)
7.	DRAFT, METHODOLOGIES FOR ASSESSING LONG-TERM PERFORMANCE OF HLR WASTE PACKAGES	(AEROSPACE)

UTILITY OF PRODUCTS TO DATE IN NRC'S WORK

1. NUREG-0960, DRAFT BWIP SCA, MARCH 1983

2. NRC COMMENTS ON DOE'S DRAFT EA'S FOR NINE POTENTIALLY ACCEPTABLE SITES FOR THE FIRST REPOSITORY, MARCH 1985

3. SUPPORT WASTE PACKAGE WORKSHOPS

4. SHORT-TERM TESTING CAPABILITY AT CONTRACTORS

PROJECTED SCHEDULES FOR WASTE PACKAGE PERFORMANCE EVALUATION

1.	USER'S MANUAL FOR ASSESSMENT OF BASALT WASTE PACKAGE	SEPT. 1986
2.	BASALT WASTE PACKAGE PERFORMANCE ASSESSMENT REPORT*	SEPT. 1988
3.	ENGINEERED BARRIER SYSTEM PERFORMANCE ASSESSMENT REPORT*	JAN. 1989
4.	SALT WASTE PACKAGE PERFORMANCE ASSESSMENT REPORT*	MAR. 1989
5.	TUFF WASTE PACKAGE PERFORMANCE ASSESSMENT REPORT*	SEPT. 1989
6.	GRANITE WASTE PACKAGE PERFORMANCE ASSESSMENT REPORT*	MAR. 1990

*INTERIM PRODUCTS WILL BE READY IN TIME FOR NRC REVIEW OF DOE's SCP.

(b) For use during site characterization or construction, as components of radiographic, radiation monitoring, or similar equipment or instrumentation.

§ 60.8 Reporting, recordkeeping, and application requirements: OMB approval not required.

The information collection requirements contained in this part affect fewer than ten persons. Therefore, under section 3506(c)(5) of the Paperwork Reduction Act of 1980 (Pub. L. 96-511), OMB clearance is not required for these information collection requirements.

[47 FR 13774, Apr. 1, 1982]

§ 60.9 Employee protection.

(a) Discrimination by a Commission licensee, an applicant for a Commission license, or a contractor or subcontractor of a Commission licensee or applicant against an employee for engaging in certain protected activities is prohibited. Discrimination includes discharge and other actions that relate to compensation, terms, conditions, and privileges of employment. The protected activities are established in section 210 of the Energy Reorganization Act of 1974, as amended, and in general are related to the administration or enforcement of a requirement imposed under the Atomic Energy Act or the Energy Reorganization Act.

(1) The protected activities include but are not limited to:

(i) Providing the Commission information about possible violations of requirements imposed under either of the above statutes;

(ii) Requesting the Commission to institute action against his or her employer for the administration or enforcement of these requirements; or

(iii) Testifying in any Commission proceeding.

(2) These activities are protected even if no formal proceeding is actually initiated as a result of the employee assistance or participation.

(3) This section has no applicaton to any employee alleging discrimination prohibited by this section who, acting without direction from his or her employer (or the employer's agent), deliberately causes a violation of any requirement of the Energy Reorganization Act of 1974, as amended, or the Atomic Energy Act of 1954, as amended.

(b) Any employee who believes that he or she has been discharged or otherwise discriminated against by any person for engaging in the protected activities specified in paragraph (a)(1) of this section may seek a remedy for the discharge or discrimination through an administrative proceeding in the Department of Labor. The administrative proceeding must be initiated within 30 days after an alleged violation occurs by filing a complaint alleging the violation with the Department of Labor, Employment Standards Administration, Wage and Hour Division. The Department of Labor may order reinstatement, back pay, and compensatory damages.

(c) A violation of paragraph (a) of this section by a Commission licensee, an applicant for a Commission license, or a contractor or subcontractor of a Commission licensee or applicant may be grounds for:

(1) Denial, revocation, or suspension of the license.

(2) Imposition of a civil penalty on the licensee or applicant.

(3) Other enforcement action.

(d) Actions taken by an employer, or others, which adversely affect an employee may be predicated upon nondiscriminatory grounds. The prohibition applies when the adverse action occurs because the employee has engaged in protected activities. An employee's engagement in protected activities does not automatically render him or her immune from discharge or discipline for legitimate reasons or from adverse action dictated by nonprohibited considerations.

(e) Each licensee and each applicant shall post Form NRC-3, "Notice to Employees," on its premises. Posting must be at locations sufficient to permit employees protected by this section to observe a copy on the way to or from their place of work. Premises must be posted not later than 30 days after an application is docketed and remain posted while the application is pending before the Commission, during the term of the license, and for 30 days following license termination.

NOTE: Copies of Form NRC-3 may be obtained by writing to the Regional Administrator of the appropriate U.S. Nuclear Regulatory Commission Regional Office listed in Appendix D, Part 20 of this chapter or the Director, Office of Inspection and Enforcement, U.S. Nuclear Regulatory Commission, Washington, D.C. 20555.

[47 FR 30456, July 14, 1982]

Subpart B—Licenses

Preapplication Review

§ 60.10 Site characterization.

(a) Prior to submittal or an application for a license to be issued under this part DOE shall conduct a program of site characterization with respect to the site to be described in such application.

(b) Unless the Commission determines with respect to the site described in the application that it is not necessary, site characterization shall include a program of in situ exploration and testing at the depths that wastes would be emplaced.

(c) As provided in § 51.40 of this chapter, DOE is also required to conduct a program of site characterization, including in situ testing at depth, with respect to alternative sites.

(d) The program of site characterization shall be conducted in accordance with the following:

(1) Investigations to obtain the required information shall be conducted in such a manner as to limit adverse effects on the long-term performance of the geologic repository to the extent practical.

(2) The number of exploratory boreholes and shafts shall be limited to the extent practical consistent with obtaining the information needed for site characterization.

(3) To the extent practical, exploratory boreholes and shafts in the geologic repository operations area shall be located where shafts are planned for underground facility construction and operation or where large unexcavated pillars are planned.

(4) Subsurface exploratory drilling, excavation, and in situ testing before and during construction shall be planned and coordinated with geologic repository operations area design and construction.

[48 FR 28219, June 21, 1983]

§ 60.11 Site characterization report.

(a) As early as possible after commencement of planning for a particular geologic repository operations area, and prior to site characterization, DOE shall submit to the Director a Site Characterization Report. The report shall include:[1]

(1) A description of the site to be characterized;

(2) The criteria used to arrive at the candidate area;

(3) The method by which the site was selected for site characterization;

(4) Identification and location of alternative media and sites at which DOE intends to conduct site characterization and for which DOE anticipates submitting subsequent Site Characterization Reports;

(5) A description of the decision process by which the site was selected for characterization, including the means used to obtain public, Indian tribal and State views during selection;

(6) A description of the site characterization program including:

(i) The extent of planned excavation and plans for in situ testing,

(ii) A conceptual design of a geologic repository operations area appropriate to the named site in sufficient detail to allow assessment of the site characterization program, with respect to investigation activities which address the ability of the site to host a geologic repository and isolate radioactive waste, or which may affect such ability, and

(iii) Provisions to control any adverse, safety-related effects from site characterization, including appropriate quality programs;

(7) A description of the quality assurance program to be applied to data collection; and

(8) Any issues related to site selection, alternative candidate areas, or other sites, or design of the geologic

[1] To the extent that the information indicated in items 2 through 5 appears in an Environmental Impact Statement prepared by DOE for site characterization at the named site, it may be incorporated into DOE's Site Characterization Report by reference.

repository operations area which the DOE wishes the Commission to review. Also included shall be a description of the research and development activities being conducted by DOE which deal with the waste form and packaging which may be considered appropriate for the site to be characterized, including research planned or underway to evaluate the performance of such waste forms and packaging.

(b) The Director shall cause to be published in the FEDERAL REGISTER a notice that the information submitted under paragraph (a) of this section has been received and that a staff review of that information has begun. The notice shall identify the site selected for site characterization and alternate areas being considered by DOE and shall advise that consultation may be requested by State and local governments and Tribal organizations in accordance with Subpart C of this part.

(c) The Director shall make available a copy of the above information at the Public Document Room. The Director also shall transmit copies and the published notice of receipt thereof to the Governor and legislature of the State and to the chief executive of the municipality in which a site to be characterized is located (or if it is not located within a municipality, then to the chief executive of the county, or to the Tribal organization if it is to be located within an Indian reservation) and to the Governors of any contiguous States.

(d) The Director shall prepare a draft site characterization analysis which shall discuss the items cited in paragraph (a) of this section. The Director shall publish a notice of availability of the draft site characterization analysis and a request for comment in the FEDERAL REGISTER. Copies shall be made available at the Public Document Room. The Director shall also transmit copies to the Governor and legislature of the State and the chief executive of the municipality in which a site to be characterized is located (or if it is not located within a municipality, then to the chief executive of the county, or to the Tribal organization if it is to be located within an Indian reservation) and to the Governors of any contiguous States.

(e) A reasonable period, not less than 90 days, shall be allowed for comment on the draft site characterization analysis. The Director shall then prepare a final site characterization analysis which shall take into account comments received and any additional information acquired during the comment period. Included in the final site characterization analysis shall be either an opinion by the Director that he has no objection to the DOE's site characterization program, if such an opinion is appropriate, or specific objections of the Director to DOE's proceeding with characterization of the named site. In addition, the Director may make specific recommendations to DOE on the matters pertinent to this section. A copy of the final site characterization analysis and the Director's opinion will be transmitted to DOE.

(f) Neither issuance of a final site characterization analysis nor the opinion by the Director shall constitute a commitment to issue any authorization or license or in any way affect the authority of the Commission, the Atomic Safety and Licensing Appeal Board, Atomic Safety and Licensing Boards, other presiding officers, or the Director, in any proceeding under Subpart G of Part 2 of this chapter. If DOE prepares an Environmental Impact Statement with respect to site characterization activities proposed for a particular site, it should consider NRC's site characterization analyses before publishing its final Environmental Impact Statement with respect to site characterization activities proposed for that particular site.

(g) During site characterization, DOE shall inform the Director by semiannual report and by other reports on any topic related to site characterization if requested by the Director, of the progress of the site characterization and waste form and packaging research and development. The semiannual reports should include the results of site characterization studies, the identification of new issues, plans for additional studies to resolve new issues, elimination of planned studies no longer necessary, identification of decision points reached and modification to schedules were appropriate. Also reported should be the DOE's progress in developing the design of a geologic repository operations area appropriate for the site being characterized, noting when key design parameters or features which depend upon the results of site characterization will be established. During this time, NRC staff shall be permitted to visit and inspect the site and observe excavations, borings, and in situ tests as they are done.

(h) The Director may comment at any time in writing to DOE, expressing current views on any aspect of site characterization. Comments received from States in accordance with § 60.61 shall be considered by the Director in formulating his views. All correspondence between DOE and the NRC including the reports cited in paragraph (g) shall be placed in the Public Document Room.

(i) The activities described in paragraphs (a) through (h) above constitute informal conference between a prospective applicant and the staff, as described in § 2.101(a)(1) of this chapter, and are not part of a proceeding under the Atomic Energy Act of 1954, as amended.

[46 FR 13980, Feb. 25, 1981, as amended at 48 FR 28219, June 21, 1983]

LICENSE APPLICATIONS

§ 60.21 Content of application.

(a) An application shall consist of general information and a Safety Analysis Report. An environmental report shall be prepared in accordance with Part 51 of this chapter and shall accompany the application. Any Restricted Data or National Security Information shall be separated from unclassified information.

(b) The general information shall include:

(1) A general description of the proposed geologic repository identifying the location of the geologic repository operations area, the general character of the proposed activities, and the basis for the exercise of licensing authority by the Commission.

(2) Proposed schedules for construction, receipt of waste, and emplacement of wastes at the proposed geologic repository operations area.

(3) A certification that DOE will provide at the geologic repository operations area such safeguards as it requires at comparable surface facilities (of DOE) to promote the common defense and security.

(4) A description of the physical security plan for protection against radiological sabotage. Since the radiation hazards associated with high-level wastes make them inherently unattractive as a target for theft or diversion, no detailed information need be submitted on protection against theft or diversion.

(5) A description of site characterization work actually conducted by DOE at all sites considered in the application and, as appropriate, explanations of why such work differed from the description of the site characterization program described in the Site Characterization Report for each site.

(c) The Safety Analysis Report shall include:

(1) A description and assessment of the site at which the proposed geologic repository operations area is to be located with appropriate attention to those features of the site that might affect geologic repository operations area design and performance. The description of the site shall identify the location of the geologic repository operations area with respect to the boundary of the accessible environment.

(i) The description of the site shall also include the following information regarding subsurface conditions. This description shall, in all cases, include such information with respect to the controlled area. In addition, where subsurface conditions outside the controlled area may affect isolation within the controlled area, the description shall include such information with respect to subsurface conditions outside the controlled area to the extent such information is relevant and material. The detailed information referred to in this paragraph shall include:

(A) The orientation, distribution, aperture in-filling and origin of fractures, discontinuities, and heterogeneities;

PERFORMANCE OBJECTIVES

§ 60.111 Performance of the geologic repository operations area through permanent closure.

(a) *Protection against radiation exposures and releases of radioactive material.* The geologic repository operations area shall be designed so that until permanent closure has been completed, radiation exposures and radiation levels, and releases of radioactive materials to unrestricted areas, will at all times be maintained within the limits specified in Part 20 of this chapter and such generally applicable environmental standards for radioactivity as may have been established by the Environmental Protection Agency.

(b) *Retrievability of waste.* (1) The geologic repository operations area shall be designed to preserve the option of waste retrieval throughout the period during which wastes are being emplaced and, thereafter, until the completion of a preformance confirmation program and Commission review of the information obtained from such a program. To satisfy this objective, the geologic repository operations area shall be designed so that any or all of the emplaced waste could be retrieved on a reasonable schedule starting at any time up to 50 years after waste emplacement operations are initiated, unless a different time period is approved or specified by the Commission. This different time period may be established on a case-by-case basis consistent with the emplacement schedule and the planned performance confirmation program.

(2) This requirement shall not preclude decisions by the Commission to allow backfilling part or all of, or permanent closure of, the geologic repository operations area prior to the end of the period of design for retrievability.

(3) For purposes of this paragraph, a reasonable schedule for retrieval is one that would permit retrieval in about the same time as that devoted to construction of the geologic repository operations area and the emplacement of wastes.

§ 60.112 Overall system performance objective for the geologic repository after permanent closure.

The geologic setting shall be selected and the engineered barrier system and the shafts, boreholes and their seals shall be designed to assure that releases of radioactive materials to the accessible environment following permanent closure conform to such generally applicable environmental standards for radioactivity as may have been established by the Environmental Protection Agency with respect to both anticipated processes and events and unanticipated processes and events.

§ 60.113 Performance of particular barriers after permanent closure.

(a) *General provisions*—(1) *Engineered barrier system.* (i) The engineered barrier system shall be designed so that assuming anticipated processes and events: (A) Containment of HLW will be substantially complete during the period when radiation and thermal conditions in the engineered barrier system are dominated by fission product decay; and (B) any release of radionuclides from the engineered barrier system shall be a gradual process which results in small fractional releases to the geologic setting over long times. For disposal in the saturated zone, both the partial and complete filling with groundwater of available void spaces in the underground facility shall be appropriately considered and analysed among the anticipated processes and events in designing the engineered barrier system.

(ii) In satisfying the preceding requirement, the engineered barrier system shall be designed, assuming anticipated processes and events, so that:

(A) Containment of HLW within the waste packages will be substantially complete for a period to be determined by the Commission taking into account the factors specified in § 60.113(b) provided, that such period shall be not less than 300 years nor more than 1,000 years after permanent closure of the geologic repository; and

(B) The release rate of any radionuclide from the engineered barrier system following the containment period shall not exceed one part in 100,000 per year of the inventory of that radionuclide calculated to be present at 1,000 years following permanent closure, or such other fraction of the inventory as may be approved or specified by the Commission; provided, that this requirement does not apply to any radionuclide which is released at a rate less than 0.1% of the calculated total release rate limit. The calculated total release rate limit shall be taken to be one part in 100,000 per year of the inventory of radioactive waste, originally emplaced in the underground facility, that remains after 1,000 years of radioactive decay.

(2) *Geologic setting.* The geologic repository shall be located so that pre-waste-emplacement groundwater travel time along the fastest path of likely radionuclide travel from the disturbed zone to the accessible environment shall be at least 1,000 years or such other travel time as may be approved or specified by the Commission.

(b) On a case-by-case basis, the Commission may approve or specify some other radionuclide release rate, designed containment period or pre-waste-emplacement groundwater travel time, provided that the overall system performance objective, as it relates to anticipated processes and events, is satisfied. Among the factors that the Commission may take into account are:

(1) Any generally applicable environmental standard for radioactivity established by the Environmental Protection Agency;

(2) The age and nature of the waste, and the design of the underground facility, particularly as these factors bear upon the time during which the thermal pulse is dominated by the decay heat from the fission products;

(3) The geochemical characteristics of the host rock, surrounding strata and groundwater; and

(4) Particular sources of uncertainty in predicting the performance of the geologic repository.

(c) Additional requirements may be found to be necessary to satisfy the overall system performance objective as it relates to unanticipated processes and events.

LAND OWNERSHIP AND CONTROL

§ 60.121 Requirements for ownership and control of interests in land.

(a) *Ownership of land.* (1) Both the geologic repository operations area and the controlled area shall be located in and on lands that are either acquired lands under the jurisdiction and control of DOE, or lands permanently withdrawn and reserved for its use.

(2) These lands shall be held free and clear of all encumbrances, if significant, such as: (i) Rights arising under the general mining laws; (ii) easements for right-of-way; and (iii) all other rights arising under lease, rights of entry, deed, patent, mortgage, appropriation, prescription, or otherwise.

(b) *Additional controls.* Appropriate controls shall be established outside of the controlled area. DOE shall exercise any jurisdiction and control over surface and subsurface estates necessary to prevent adverse human actions that could significantly reduce the geologic repository's ability to achieve isolation. The rights of DOE may take the form of appropriate possessory interests, servitudes, or withdrawals from location or patent under the general mining laws.

(c) *Water rights.* (1) DOE shall also have obtained such water rights as may be needed to accomplish the purpose of the geologic repository operations area.

(2) Water rights are included in the additional controls to be established under paragraph (b) of this section.

SITING CRITERIA

§ 60.122 Siting criteria.

(a)(1) A geologic setting shall exhibit an appropriate combination of the conditions specified in paragraph (b) of this section so that, together with the engineered barriers system, the favorable conditions present are sufficient to provide reasonable assurance that the performance objectives relating to isolation of the waste will be met.

dismantlement to the same extent as would be required, under other parts of this chapter, with respect to equivalent activities licensed thereunder.

§ 60.133 Additional design criteria for the underground facility.

(a) *General criteria for the underground facility.* (1) The orientation, geometry, layout, and depth of the underground facility, and the design of any engineered barriers that are part of the underground facility shall contribute to the containment and isolation of radionuclides.

(2) The underground facility shall be designed so that the effects of credible disruptive events during the period of operations, such as flooding, fires and explosions, will not spread through the facility.

(b) *Flexibility of design.* The underground facility shall be designed with sufficient flexibility to allow adjustments where necessary to accommodate specific site conditions identified through in situ monitoring, testing, or excavation.

(c) *Retrieval of waste.* The underground facility shall be designed to permit retrieval of waste in accordance with the performance objectives of § 60.111.

(d) *Control of water and gas.* The design of the underground facility shall provide for control of water or gas intrusion.

(e) *Underground openings.* (1) Openings in the underground facility shall be designed so that operations can be carried out safely and the retrievability option maintained.

(2) Openings in the underground facility shall be designed to reduce the potential for deleterious rock movement or fracturing of overlying or surrounding rock.

(f) *Rock excavation.* The design of the underground facility shall incorporate excavation methods that will limit the potential for creating a preferential pathway for groundwater or radioactive waste migration to the accessible environment.

(g) *Underground facility ventilation.* The ventilation system shall be designed to:

(1) Control the transport of radioactive particulates and gases within and releases from the underground facility in accordance with the performance objectives of § 60.111(a),

(2) Assure continued function during normal operations and under accident conditions; and

(3) Separate the ventilation of excavation and waste emplacement areas.

(h) *Engineered barriers.* Engineered barriers shall be designed to assist the geologic setting in meeting the performance objectives for the period following permanent closure.

(i) *Thermal loads.* The underground facility shall be designed so that the performance objectives will be met taking into account the predicted thermal and thermomechanical response of the host rock, and surrounding strata, groundwater system.

§ 60.134 Design of seals for shafts and boreholes.

(a) *General design criterion.* Seals for shafts and boreholes shall be designed so that following permanent closure they do not become pathways that compromise the geologic repository's ability to meet the performance objectives or the period following permanent closure.

(b) *Selection of materials and placement methods.* Materials and placement methods for seals shall be selected to reduce, to the extent practicable: (1) The potential for creating a preferential pathway for groundwater; or (2) radioactive waste migration through existing pathways.

DESIGN CRITERIA FOR THE WASTE PACKAGE

§ 60.135 Criteria for the waste package and its components.

(a) *High-level-waste package design in general.* (1) Packages for HLW shall be designed so that the in situ chemical, physical, and nuclear properties of the waste package and its interactions with the emplacement environment do not compromise the function of the waste packages or the performance of the underground facility or the geologic setting.

(2) The design shall include but not be limited to consideration of the following factors: solubility, oxidation/reduction reactions, corrosion, hydriding, gas generation, thermal effects, mechanical strength, mechanical stress, radiolysis, radiation damage, radionuclide retardation, leaching, fire and explosion hazards, thermal loads, and synergistic interactions.

(b) *Specific criteria for HLW package design*—(1) *Explosive, pyrophoric, and chemically reactive materials.* The waste package shall not contain explosive or pyrophoric materials or chemically reactive materials in an amount that could compromise the ability of the underground facility to contribute to waste isolation or the ability of the geologic repository to satisfy the performance objectives.

(2) *Free liquids.* The waste package shall not contain free liquids in an amount that could compromise the ability of the waste packages to achieve the performance objectives relating to containment of HLW (because of chemical interactions or formation of pressurized vapor) or result in spillage and spread of contamination in the event of waste package perforation during the period through permanent closure.

(3) *Handling.* Waste packages shall be designed to maintain waste containment during transportation, emplacement, and retrieval.

(4) *Unique identification.* A label or other means of identification shall be provided for each waste package. The identification shall not impair the integrity of the waste package and shall be applied in such a way that the information shall be legible at least to the end of the period of retrievability. Each waste package identification shall be consistent with the waste package's permanent written records.

(c) Waste form criteria for HLW. High-level radioactive waste that is emplaced in the underground facility shall be designed to meet the following criteria:

(1) *Solidification.* All such radioactive wastes shall be in solid form and placed in sealed containers.

(2) *Consolidation.* Particulate waste forms shall be consolidated (for example, by incorporation into an encapsulating matrix) to limit the availability and generation of particulates.

(3) *Combustibles.* All combustible radioactive wastes shall be reduced to a noncombustible form unless it can be demonstrated that a fire involving the waste packages containing combustibles will not compromise the integrity of other waste packages, adversely affect any structures, systems, or components important to safety, or compromise the ability of the underground facility to contribute to waste isolation.

(d) *Design criteria for other radioactive wastes.* Design criteria for waste types other than HLW will be addressed on an individual basis if and when they are proposed for disposal in a geologic repository.

PERFORMANCE CONFIRMATION REQUIREMENTS

§ 60.137 General requirements for performance confirmation.

The geologic repository operations area shall be designed so as to permit implementation of a performance confirmation program that meets the requirements of Subpart F of this part.

Subpart F—Performance Confirmation Program

SOURCE: 48 FR 28228, June 21, 1983, unless otherwise noted.

§ 60.140 General requirements.

(a) The performance confirmation program shall provide data which indicates, where practicable, whether:

(1) Actual subsurface conditions encountered and changes in those conditions during construction and waste emplacement operations are within the limits assumed in the licensing review; and

(2) Natural and engineered systems and components required for repository operation, or which are designed or assumed to operate as barriers after permanent closure, are functioning as intended and anticipated.

(b) The program shall have been started during site characterization and it will continue until permanent closure.

(c) The program shall include in situ monitoring, laboratory and field testing, and in situ experiments, as may be

tive as stated above.

(d) The program shall be implemented so that:

(1) It does not adversely affect the ability of the natural and engineered elements of the geologic repository to meet the performance objectives.

(2) It provides baseline information and analysis of that information on those parameters and natural processes pertaining to the geologic setting that may be changed by site characterization, construction, and operational activities.

(3) It monitors and analyzes changes from the baseline condition of parameters that could affect the performance of a geologic repository.

(4) It provides an established plan for feedback and analysis of data, and implementation of appropriate action.

§ 60.141 Confirmation of geotechnical and design parameters.

(a) During repository construction and operation, a continuing program of surveillance, measurement, testing, and geologic mapping shall be conducted to ensure that geotechnical and design parameters are confirmed and to ensure that appropriate action is taken to inform the Commission of changes needed in design to accommodate actual field conditions encountered.

(b) Subsurface conditions shall be monitored and evaluated against design assumptions.

(c) As a minimum, measurements shall be made of rock deformations and displacement, changes in rock stress and strain, rate and location of water inflow into subsurface areas, changes in groundwater conditions, rock pore water pressures including those along fractures and joints, and the thermal and thermomechanical response of the rock mass as a result of development and operations of the geologic repository.

(d) These measurements and observations shall be compared with the original design bases and assumptions. If significant differences exist between the measurements and observations and the original design bases and assumptions, the need for modifications to the design or in construction methods shall be determined and these differences and the recommended changes reported to the Commission.

(e) In situ monitoring of the thermomechanical response of the underground facility shall be conducted until permanent closure to ensure that the performance of the natural and engineering features are within design limits.

§ 60.142 Design testing.

(a) During the early or developmental stages of construction, a program for in situ testing of such features as borehole and shaft seals, backfill, and the thermal interaction effects of the waste packages, backfill, rock, and groundwater shall be conducted.

(b) The testing shall be initiated as early as is practicable.

(c) A backfill test section shall be constructed to test the effectiveness of backfill placement and compaction procedures against design requirements before permanent backfill placement is begun.

(d) Test sections shall be established to test the effectiveness of borehole and shaft seals before full-scale operation proceeds to seal boreholes and shafts.

§ 60.143 Monitoring and testing waste packages.

(a) A program shall be established at the geologic repository operations area for monitoring the condition of the waste packages. Waste packages chosen for the program shall be representative of those to be emplaced in the underground facility.

(b) Consistent with safe operation at the geologic repository operations area, the environment of the waste packages selected for the waste package monitoring program shall be representative of the environment in which the wastes are to be emplaced.

(c) The waste package monitoring program shall include laboratory experiments which focus on the internal condition of the waste packages. To the extent practical, the environment experienced by the emplaced waste packages within the underground facility during the waste package monitoring program shall be duplicated in the laboratory experiments.

(d) The waste package monitoring program shall continue as long as practical up to the time of permanent closure.

Subpart G—Quality Assurance

Source: 48 FR 28228, June 21, 1983, unless otherwise noted.

§ 60.150 Scope.

As used in this part, "quality assurance" comprises all those planned and systematic actions necessary to provide adequate confidence that the geologic repository and its subsystems or components will perform satisfactorily in service. Quality assurance includes quality control, which comprises those quality assurance actions related to the physical characteristics of a material, structure, component, or system which provide a means to control the quality of the material, structure, component, or system to predetermined requirements.

§ 60.151 Applicability.

The quality assurance program applies to all systems, structures and components important to safety, to design and characterization of barriers important to waste isolation and to activities related thereto. These activities include: site characterization, facility and equipment construction, facility operation, performance confirmation, permanent closure, and decontamination and dismantling of surface facilities.

§ 60.152 Implementation.

DOE shall implement a quality assurance program based on the criteria of Appendix B of 10 CFR Part 50 as applicable, and appropriately supplemented by additional criteria as required by § 60.151.

Subpart H—Training and Certification of Personnel

Source: 48 FR 28229, June 21, 1983, unless otherwise noted.

§ 60.160 General requirements.

Operations of systems and components that have been identified as important to safety in the Safety Analysis Report and in the license shall be performed only by trained and certified personnel or by personnel under the direct visual supervision of an individual with training and certification in such operation. Supervisory personnel who direct operations that are important to safety must also be certified in such operations.

§ 60.161 Training and certification program.

DOE shall establish a program for training, proficiency testing, certification and requalification of operating and supervisory personnel.

§ 60.162 Physical requirements.

The physical condition and the general health of personnel certified for operations that are important to safety shall not be such as might cause operational errors that could endanger the public health and safety. Any condition which might cause impaired judgment or motor coordination must be considered in the selection of personnel for activities that are important to safety. These conditions need not categorically disqualify a person, so long as appropriate provisions are made to accommodate such conditions.

Subpart I—Emergency Planning Criteria [Reserved]

PART 61—LICENSING REQUIREMENTS FOR LAND DISPOSAL OF RADIOACTIVE WASTE

Subpart A—General Provisions

OBJECTIVES

IDENTIFY LIKELY FAILURE MODES OF HLW WASTE PACKAGE

IDENTIFY ENVIRONMENTAL DETERMINANTS OF WASTE PACKAGE FAILURE

IDENTIFY CRITICAL TESTS AND ASSUMPTIONS NEEDED TO DEMONSTRATE LONG-TERM WASTE PACKAGE PERFORMANCE

PERFORM SELECTIVE EXPERIMENTS TO TEST ASSUMPTIONS AND PERFORMANCE DEMONSTRATION TECHNIQUES

APPROACH

- TRACK DOE DECISIONS ON WASTE FORM AND OVERPACK MATERIALS
- BEGIN WITH EXPECTED REPOSITORY ENVIRONMENTS
- DOE'S RESPONSIBILITY TO DEMONSTRATE ITS CASE
- NRC MUST KNOW WHAT CONSTITUTES A DEMONSTRATION

MATERIALS

WASTE FORM	HLW GLASS SPENT REACTOR FUEL
OVERPACK	LOW CARBON STEEL STAINLESS STEEL HIGH PURITY IRON
PACKING/BACKFILL/SEALS	BENTONITE BENTONITE/BASALT CEMENT
WASTE PACKAGE ENVIRONMENT (GROUNDWATER)	BASALT TUFF SALT GRANITE

PHENOMENA

WASTE FORM	DEVITRIFICATION
ANL, BCL	LEACHING
OVERPACK	PITTING CORROSION
BNL, BCL, NBS, MSC	STRESS CORROSION
	HYDROGEN EMBRITTLEMENT
	MANUFACTURING CONCERNS
PACKING/BACKFILL/SEALS	DIFFUSION
ANL, LBL, U. OF AZ	HYDROTHERMAL ALTERATION
	BOREHOLE, SHAFT AND FRACTURE SEALING
WASTE PACKAGE ENVIRONMENT	GROUND WATER GEOCHEMISTRY
BCL, LBL,	COUPLED PROCESSES

B7278: CONTAINER MANUFACTURING PARAMETERS: MANUFACTURING SCIENCES CORP.

OBJECTIVE: ASSESS THE EFFECT OF VARIABILITIES IN MANUFACTURE ON EXPECTED OVERPACK PERFORMANCE.

SCOPE: PRODUCTION OF SAMPLES OF OVERPACK MATERIAL FOR ANALYSES AT BCL.

PROBLEM: PROVIDE BASIS FOR NRC REVIEW OF DOE QA/QC PROGRAM FOR OVERPACK MANUFACTURE.

A3269: PITTING CORROSION BNL

OBJECTIVE: ASSESS CONFIDENCE IN EXTRAPOLATING SHORT TERM LAB TESTS TO LONG TERM PITTING RATES.

SCOPE: OBSERVE RATE AND CHEMISTRY OF PITTING IN ARTIFICIALLY AGED PITS (LOW CARBON STEEL).

PROBLEM: PROVIDE BASIS FOR ASSESSING DOE'S EXTRAPOLATION OF SHORT-TERM OBSERVED PITTING RATES TO LONG-TERM RATES (300 YEARS) USED IN PERFORMANCE DEMONSTRATION.

D1146: PITTING STATISTICS NBS

OBJECTIVE: ASSESS THE VARIABILITY IN PIT DEPTHS WITH PIT AGE AND ENVIRONMENTAL CONDITIONS.

SCOPE: EXAMINE FLUCTUATIONS IN PASSIVATION CURRENT AS A POSSIBLE PRECURSOR TO THE ONSET OF PITTING. STATISTICALLY ANALYZE DISTRIBUTION OF PIT DEPTH AND SIZE WITH PIT AGE. CORRELATE RESULTS WITH BCL AND BNL (PITTING CORROSION CHEMISTRY).

PROBLEM: PROVIDE AN INDEPENDENT ASSESSMENT OF THE STABILITY OF THE PROCESS OF PITTING (MOST LIKELY FAILURE MODE) IN EXPECTED REPOSITORY ENVIRONMENTS, AND THE EFFECT OF CHANGES IN THAT ENVIRONMENT.

A3237: Container Assessment ~~BNL~~ VNL

OBJECTIVE: EXAMINE THE QUESTIONS OF CREVICE CORROSION AND H EMBRITTLEMENT IN TI ALLOY AND CARBON STEEL OVERPACK MATERIALS.

SCOPE: PERFORM EXPERIMENTS TO ASSESS CREVICE CORROSION IN TI AND H_2 UPTAKE AND ITS CONSEQUENCES IN STEEL OVERPACK MATERIALS IN SALT AND BASALT REPOSITORY ENVIRONMENTS.

PROBLEM: ASSESS WHETHER H EMBRITTLEMENT IS A CONCERN IN BASALT AND SALT REPOSITORIES; ASSESS CREVICE CORROSION BEHAVIOR OF TI CODE 12.

B6764: LONG-TERM PERFORMANCE OF HLW PACKAGING MATERIALS (OVERPACK) BCL

OBJECTIVE: IDENTIFY LIKELY OVERPACK FAILURE MODES UNDER EXPECTED REPOSITORY CONDITIONS (BASALT, TUFF, SALT).

SCOPE: OBSERVE RATES AND CONDITIONS FOR VARIOUS OVERPACK FAILURE MODES: GENERAL CORROSION, PITTING CORROSION, STRESS CORROSION, H EMBRITTLEMENT.

PROBLEM: PROVIDE BASIS FOR ASSESSING CONSISTENCY OF DOE'S DEMONSTRATION OF OVERPACK INTEGRITY FOR 500-1000 YEARS.

B6764: Long Term Performance (Waste Form) BCL

Objective: Assess range of expected performance of HLW glass and spent fuel as HLW waste form.

Scope: Perform laboratory experiments to measure leach rates of HLW glass and spent fuel under a range of expected repository conditions.

Problem: Provide NRC with a basis to evaluate DOE's demonstration of compliance with the controlled release requirement of 10 CFR Part 60.

A2254: Glass Analogue Study ANL

Objective: Understand "aging" process of HLW glass.

Scope: Develop and study natural and artificial glass analogues to HLW glass to coprelate the aging process observed in natural glasses to that expected in HLW glass.

Problem: Assess to what extent short-term leaching experiments of simulated waste glasses are representative of the long-term leaching of 300-10,000 year old HLW glass.

B6764: Long Term Performance (Environment) BCL

OBJECTIVE: ASSESS THE EFFECT OF OVERPACK CORROSION ON THE LOCAL OVERPACK ENVIRONMENT IN BASALT AND TUFF REPOSITORIES.

SCOPE: DEVELOP AND TEST A GROUNDWATER GEOCHEMISTRY MODEL OF PREDICTING THE EFFECT OF OVERPACK CORROSION PRODUCTS ON LOCAL WASTE PACKAGE ENVIRONMENT.

PROBLEM: PROVIDE AN ASSESSMENT OF THE LOCAL STABILITY OF THE OVERPACK - GROUNDWATER SYSTEM.

B3046: Coupled Interactions LBL

Objective: Determine how presence of waste package affects its environment and vice versa.

Scope: Examine hydrothermal system as analogues to thermal perturbations generated by emplaced HLW.

Problem: Provide assessment of interactive changes in the thermal, hydrologic, chemical, and mechanical setting of the pepository - stability of engineered system environment, and evolution of the hydrochemical setting.

B3040: SITE GEOCHEMISTRY (WASTE PACKAGE ENVIRONMENT) LBL

OBJECTIVE: ASSESS GEOCHEMISTRY OF REPOSITORIES IN BASALT AND TUFF.

SCOPE: PERFORM EXPERIMENTS AND ASSESS NUMERICAL MODELS OF THERMAL ALTERATION OF HOST ROCK, GROUND WATER AND PACKING MATERIAL IN BASALT AND TUFF.

PROBLEM: PROVIDE ASSESSMENT OF THERMALLY-INDUCED CHANGES IN THE CHEMISTRY OF THE GROUND WATER/HOST ROCK/WASTE PACKAGE SYSTEM.

B6627: Sealing Rock Masses U. of AZ

Objective:	Performance Assessment of existing rock mass sealing technology for the sealing of shafts, fractures, and boreholes in basalt and granite.
Scope:	Perform laboratory and field tests to evaluate effectiveness of sealing techniques.
Problem:	Provide NRC with a basis for assessing DOE's demonstration that shafts and boreholes will not be a significant pathway for radionuclide migration.

HLW REPOSITORY PROGRAM RESOURCES

	FY '86		FY'87	
	FTE	$ M	FTE	$ M
DWM	110	6.7	107	7.9
RESEARCH	14	3.0	14	6.8
DOE*	342	481	424	698

*(DOE FTE INCLUDES THE MRS AND TRANSPORTATION)

HLW - TECHNICAL ASSISTANCE

1. CRITICAL REVIEWS OF DOE DOCUMENTS
2. PREPARATION OF GUIDANCE
3. ACQUISITION OF MODELING/LICENSING ASSESSMENT CAPABILITIES
4. SELECTED CONFIRMATORY MEASUREMENTS

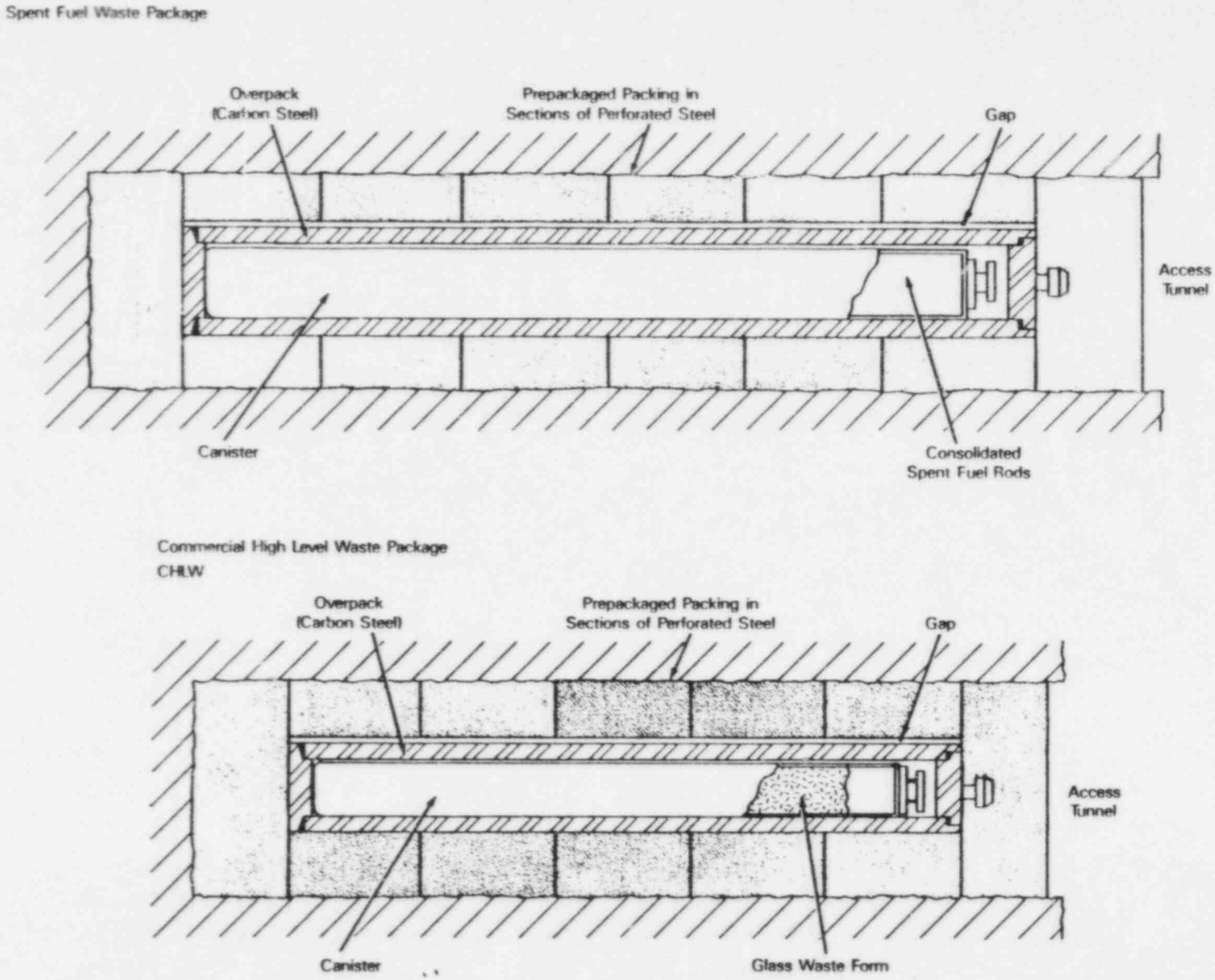

Figure 5.1. Reference Waste Package Conceptual Designs in Basalt (Short Borehole Horizontal Emplacement)

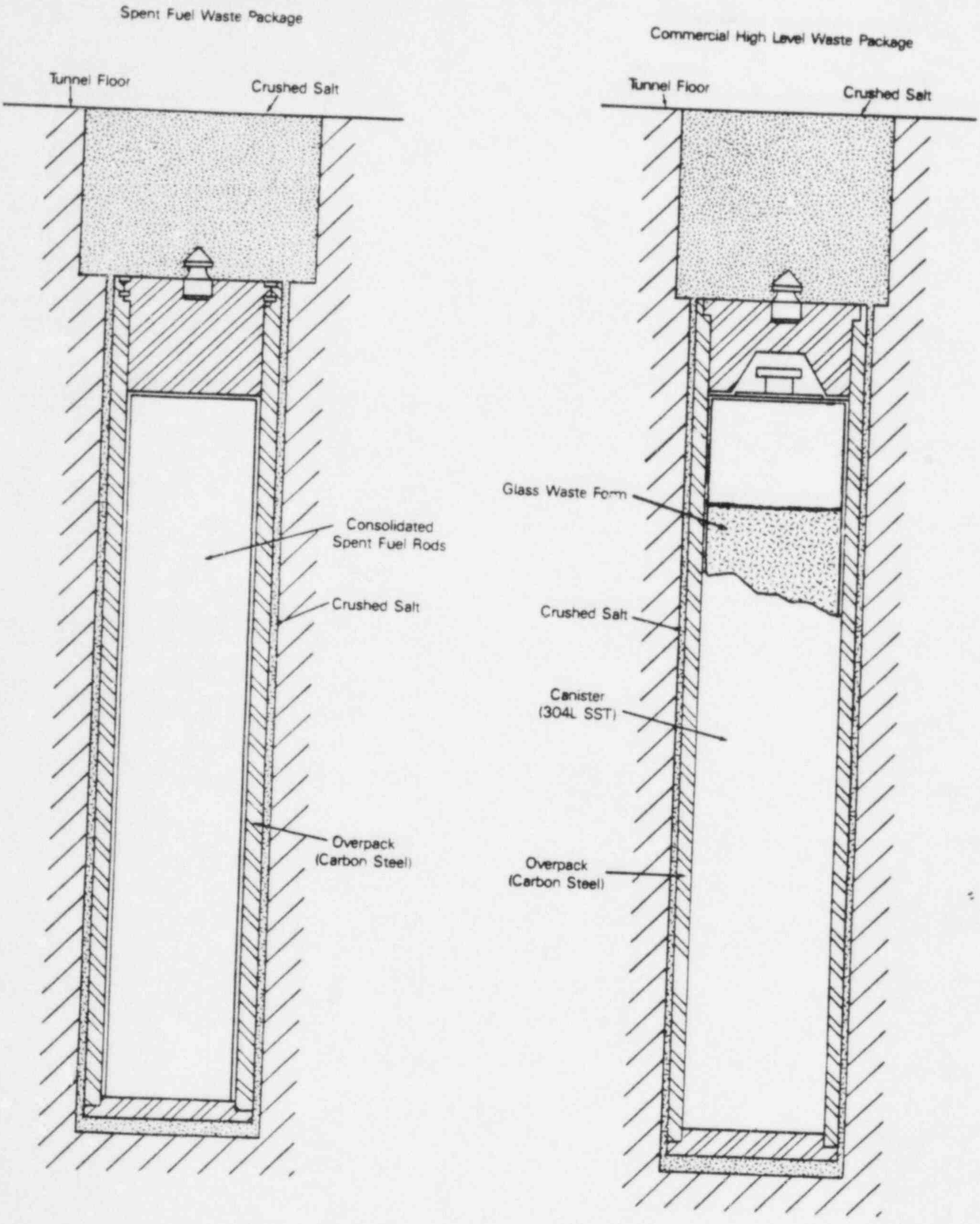

Figure 5.2. Reference Waste Package Conceptual Designs in Salt (Vertical Emplacement)

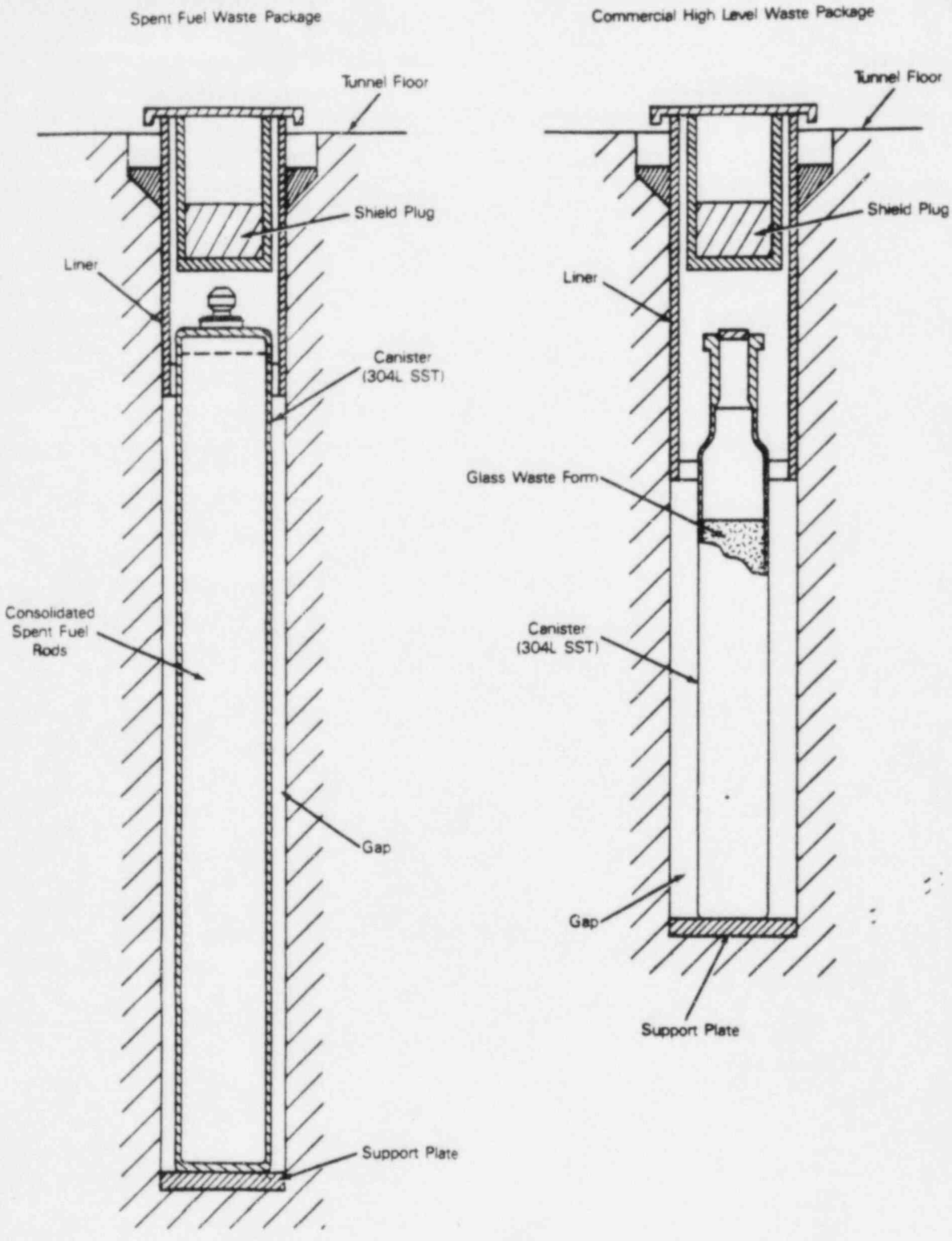

Figure 5.3. Reference Conceptual Designs in Tuff (Vertical Emplacement)

Table 5.1. Comparison of Some Waste Package Parameters for the Reference Conceptual Designs

	BWIP		ONWI		NNWSI	
	SF	CHLW	SF	CHLW	SF	CHLW
SPENT FUEL						
Package Diameter (cm)	50.3	45.6	83.5	89	65.0	32
Package Length (cm)	411	325	448 (PWR)*	457	433 (PWR)	300
Borehole Diameter (cm)	89	84	89 (PWR)	94	71 (PWR)	61
Overpack Wall Thickness (cm)	5.3	8.3	12	15	not used	not used
Packing Thickness (cm) (or air gap)	15	15	2.8	2.5	2.0 (gap)	14.5 (gap)
Loaded Weight (tonnes)	7.6	2.7	20	16.9	4.5	0.82

*PWR = Pressurized Water Reactor