UNITED STATES
NUCLEAR WASTE TECHNICAL REVIEW BOARD

WASTE PACKAGE WORKSHOP

May 18, 1998

Doubletree Hotel at Tysons Corner
Falls Church, Virginia

WORKSHOP PANELISTS

Daniel Bullen, Member, NWTRB, Chair
Paul Craig, Member, NWTRB
Priscilla Nelson, Member, NWTRB
Richard Parizek, Member, NWTRB
Alberto Sagüés, Member, NWTRB
William Barnard, Executive Director, NWTRB
Carl Di Bella, Senior Professional Staff
Lake Barrett, OCRWM
Kevin Coppersmith, Workshop Facilitator, GEOMATRIX, Inc.
David Shoesmith, Atomic Energy of Canada Limited
Digby Macdonald, SRI International
Joe Payer, Case Western Reserve University
John Kessler, Electric Power Research Institute
Michael Streicher, Consultant

ATTENDEES

Jeffrey Wong, Member, NWTRB
Daniel Fehringer, Senior Professional Staff, NWTRB
Daniel Metlay, Senior Professional Staff, NWTRB
Russell McFarland, Senior Professional Staff, NWTRB

NWTRB STAFF

Karyn Severson, Congressional Liaison
Linda Hiatt, Management Analyst
Davonya Barnes, Staff Assistant

I N D E X

PROCEEDINGS

8:30 a.m.

BULLEN: Good morning. My name is Daniel Bullen.

Welcome to the Waste Package Workshop. This workshop has been organized by the Nuclear Waste Technical Review Board, specifically the Board's panel on the repository. I'm a member of the Board and of the panel on the repository. Priscilla Nelson chairs the panel and would ordinarily be making these introductory remarks. But since Alberto Sagüéz and I are the principal instigators of this workshop, she has graciously ceded to us the privilege of making the opening and closing remarks. You will hear the closing remarks tomorrow from Dr. Sagüés.

Just a reminder on what the Board is. In 1982, Congress enacted the Nuclear Waste Policy Act. That law created the Official of Civilian Radioactive Waste Management, or OCRWM, within the U. S. Department of Energy, and charged OCRWM with developing repositories for the final disposal of the nation's spent nuclear fuel and high level waste from reprocessing.

Five years later, Congress amended the Act to limit OCRWM to characterizing a single candidate permanent disposal site, a site about 300 meters under the crest of Yucca Mountain on the western edge of the Nevada Test Site in the

State of Nevada.

In the same amendment, Congress created the Nuclear Waste Technical Review Board as an independent federal agency to review the validity of OCRWM's scientific and technical program and to periodically furnish the Board's findings, conclusions and recommendations to the Secretary of Energy and to Congress.

The President appoints our Board members from a list of nominees submitted by the National Academy of Sciences. Terms are for four years, and reappointments are possible. A member whose term has expired continues to serve until his or her successor is appointed.

The typical format for a Board meeting is to have speakers from DOE, the M&O or other organizations. Board members listen to each speaker's presentations, then ask questions and make comments. We will follow the typical format until the mid-morning break. Then those at the front U-shaped table will participate for the balance of the workshop in a facilitated group brainstorming session about alternative waste package designs and about research needs to support the alternative designs.

There are five groups at the front table: Board members, Board staffers, independent experts, DOE and M&O speakers, and a facilitator. I will introduce each person starting over here and going around the table clockwise.

Carl Di Bella is a chemical engineer on the Board's staff, where he specializes in materials, waste form, and waste package issues.

Now, going around the table, Mike Streicher. He's a materials engineer and corrosion consultant. Mike retired from Du Pont in Wilmington after 30 years service there dealing with materials issues. He then became a research professor at the University of Delaware and organized a corrosion research laboratory. Mike is one of a group I called "independent experts" a moment ago. There are five of them at the table. These are experts in the field who are employees of some entity other than DOE, the M&O, or the Board.

Next is Paul Craig. He's been a Board member since January, 1997, and is Professor of Engineering Emeritus at UC-Davis with expertise in energy policy issues associated with global environmental change.

John Kessler manages EPRI's Spent Fuel and High Level Waste Disposal Program. He has degrees in Nuclear Engineering and Hydrogeology.

We have a space reserved for Priscilla Nelson, as I mentioned. She is a program director of the Directorate for Engineering at the National Science Foundation. Her expertise is in rock engineering and underground construction.

Joe Payer is Professor of Materials Science and Engineering at Case Western Reserve University. And I'm going to add an anecdote here. Joe also serves on the Performance Assessment Peer Review Panel as a materials expert, a panel that is reviewing DOE's PA efforts for viability assessment.

Alberto Sagüéz. Alberto is also a Board member, or has been a Board member since January of 1997. He is Professor of Materials Engineering within the Civil Engineering Department at the University of South Florida. His expertise lies in corrosion and materials engineering and physical metallurgy.

Next to Alberto is Digby Macdonald, who very recently joined SRI International as Vice-President for Physical Sciences. Just before that, he was professor in the Materials Science Department at Penn State.

The vacant seat next to Digby's is mine. I'm a nuclear engineer and a faculty member directing the nuclear reactor laboratory within the Department of Mechanical Engineering at Iowa State University. My areas of expertise are nuclear waste management, performance assessment modeling, and materials science.

David Shoesmith is a physical scientist and the head of the Corrosion and Electrochemistry Section at AECL's Whiteshell Laboratory in Pinawa, Manitoba. Even though he is here as an AECL employee, he will be moving on to the Materials Science Department at the University of Western Ontario in a matter of a few days or so. Or is a few months? Two weeks. Close enough.

Richard Parizek is not here. He's another member of the Board class of 1997. He's a Professor of Geology and Geo-environmental Engineering at Penn State, and specializes in hydrogeology and environmental geology.

Bill Barnard is the Board's Executive Director. He's an oceanographer. If sea-bed disposal is ever considered seriously again, we are ready.

Next we have our three speakers for this morning. Lake Barrett is a nuclear engineering and the Acting Director of OCRWM. In just a little while, he will be updating us on the status of the program. He will also tell us about some of the important goals for the program after the viability assessment is delivered this fall.

Dave Haught--we've got him in the wrong order here. Okay, Dave Haught, raise your hand because you're not Dave Stahl--is an employee of DOE with the Yucca Mountain Project in Las Vegas. He's an electrical engineer with responsibilities for oversight of the waste package design, materials testing and modeling, and waste form testing and modeling areas.

David Stahl is an engineer and materials scientist.

He manages the waste package materials program. He is employed by Framatome Cogema Fuels, as a unit of the M&O.

And we are pleased to have Kevin Coppersmith as our workshop facilitator. He is Principal Geologist and Vice-President of Geomatrix Consultants, Incorporated, and specializes in performance assessment and hazard analyses. Over the past 18 months, Kevin has facilitated both the Waste Package Degradation Expert Elicitation and the Waste Form Expert Elicitation, in addition to three other expert elicitations, as I understand it, which makes him especially qualified to serve as facilitator here. I will turn the meeting over to Kevin immediately after the mid-morning break.

I want to point out that we have one other Board member in attendance, Dr. Jeff Wong. Stand up. He is the Chief of Human and Ecological Risk Division of the Department of Toxic Substances Control at the California EPA, and a toxicologist with expertise in risk assessment and scientific team management. He chairs the Board's Panel on Environment, Regulation, and Quality Assurance.

Also present are other members of the Board's professional staff, and I would like to acknowledge another specific staff members, Linda Hiatt, who was in the back of the room. She was instrumental in putting together all the logistical arrangements for this meeting, and although we didn't write it into the speech, I want to also acknowledge Carl Di Bella, who is sitting at the front table, for putting together the technical portion of this meeting and bringing together the panel of experts that we have at the table today. I really appreciate his efforts and it makes our job as Board members that much easier.

Bill Barnard will describe the workshop and its ground rules in just a moment. But first i want to mention something that is very important for us, and that is public participation. To be sure that we don't miss questions and comments from the public, we have set aside two 30-minute periods, one at the end of the day today, and one just before lunch tomorrow. We will give first preference to those who sign up with Linda Hiatt at the back of the room. Please try to limit your comments to a few minutes. If you wish, you may give Linda written comments of any length to be incorporated into the transcripts.

An now I'd like to turn the meeting over to our most able Executive Director, Bill Barnard, who will give us some more details about the meeting. I will act as chair during the four presentations before the break. After the break, the workshop will become the responsibility of our most able facilitator, Kevin Coppersmith.

Bill?

BARNARD: Thank you, Dan. And I want to thank everyone for coming today. I appreciate your participation.

Since the announcement of this meeting about a month ago, many people have asked why is the Board holding a workshop on waste package design. As Dan Bullen just explained, the Board's congressional mandate is to review the technical and scientific validity of activities undertaken by the DOE as part of its program to manage and dispose of commercial spent fuel and high level radioactive waste.

So the Board is sponsoring this workshop to better understand current expert thinking on waste package designs and the studies needed to support those designs. This information will strengthen the Board's technical basis for evaluating the scientific and technical merits of any and all waste package designs that may arise from the DOE program.

The idea to have this workshop was first discussed by Drs. Bullen and Sagüés on February 2nd of this year following the special session on the Waste Package Degradation Expert Elicitation that was held in Vienna, Virginia. Dan and Alberto met in our Arlington office late that afternoon and talked about different waste package designs and the R&D needed to support those designs for licensing.

Among other things, Dan and Alberto discussed whether it made more sense to put the C-22 on the outside of the package and carbon steel on the inside, rather than vice versa, as in the current design. They also asked themselves if one corrosion resistant material like C-22 is good, why not use two, or even three or more such materials.

After about a half hour of discussion, it became obvious to all three of us that expanding the discussion to include a greater number of experts could provide the Board with some valuable insights that would be useful to us in evaluating the DOE efforts to design a waste package for Yucca Mountain. Based on information we had received on the DOE's development of a TSPA, it also seemed quite clear to us that long-lived waste package could become an increasingly important component of the repository design.

Now a few words about the meeting format. As the agenda indicates, the workshop is divided into three parts. During the first session, the Department of Energy will make three presentations: Lake Barrett, the acting director of OCRWM, will brief us on the status of the overall program. We will then hear presentations from Dave Haught on the waste package design and Dave Stahl on waste materials research program. After a short break, we will then have two brainstorming sessions involving all the experts around the table.

The first brainstorming session will address different waste package designs. The second brainstorming session will look at the research needed to establish confidence in those designs.

During these sessions, we hope to explore as thoroughly as possible any alternatives for waste package design. Consequently, we encourage all of you at the table to feel free to share your thoughts and ideas. The more innovative the ideas, the better. As I mentioned earlier, the information that comes out of the workshop will be used by the Board simply to expand its technical basis for evaluating DOE's waste package designs.

As with all public meetings sponsored by the Board, the proceedings of this workshop will be recorded and a transcript will be available from the Board upon request. As Dan mentioned, Kevin Coppersmith will be facilitating the two brainstorming sessions.

Now, for a few ground rules. After each presentation this morning, questions or comments of a technical nature will be solicited, first from those at the tables, and then, time permitting, from the floor. For those panelists at the table, it will not be necessary for you to state your names. However, if any comments are made from the floor, I request that you please state your name and affiliation for the record.

I'll now turn the meeting back to Dan Bullen.

BULLEN: Thank you, Bill.

Our first presentation is on the spent fuel disposal program status and post-viability assessment milestones and will be made by Lake Barrett. Lake?

BARRETT: Thank you, Dan.

First, I have to compliment Bill. This is twice I've heard Dan Bullen speak in the last week, and both times, he stuck to the script. Very well done. Bill has become as master as controlling Board members. Well trained, and you acknowledge it, too. That's the good one.

Thank you, folks. There will be copies of my remarks I think Allen will hand out as well. Thank you for the opportunity to appear here today to provide our perspective on the status of the program for this year and the coming years.

The two Davids will give you many more details concerning the engineering aspects of the waste package, and they'll be supported by numerous staff here as well, and I thought I'd use my time to talk a little bit about the policy settings, but try also to focus in on the importance that the engineering and the design aspects are in the national policy setting.

This is a particularly important year for the program as we complete the viability assessment for Yucca Mountain. As you know, the viability assessment is a management tool for the program and provides important informational input to the policy process. Its completion will culminate a three year effort by the program to assemble the information collected by the site characterization into a workable repository concept for Yucca Mountain, and to focus the program on the key remaining issues.

One of the key VA products is the design of the repository based in part on our current understanding of the site. The VA design is not intended to be the final design, and we recognize that this product and other VA products will not be sufficient for site recommendation nor licensing. Their completion, however, will help integrate the ongoing activities and guide the completion of the site characterization and design by identifying those areas where additional scientific and technical design work is required to evaluate the site and prepare a complete, defensible license application, if the site is found suitable.

One of the foremost challenges in a complex first-of-a-kind endeavor is to develop a working concept and define the work required to implement that concept. The viability assessment will provide a working repository concept that will continue to evolve with the collection of new information and the evaluation of design options and alternatives. We must carefully balance the need for progress in developing a coherent working concept with the recognition that such a concept will invariably change over time.

In reality, development of a repository will preserve the options available to future generations for the disposition of nuclear materials while meeting the commitment that this generation has made to safely manage nuclear materials it produces. Development of a geologic repository at Yucca Mountain is not a pursuit of an irreversible or irretrievable disposition. Future generations will make the ultimate decision on whether it is appropriate to continue to maintain the repository in an open monitored condition, or to close and seal the repository based on development of their own criteria and level of certainty regarding repository performance at that time.

To ensure this flexibility for these future decision makers, the repository is being designed with the capability to be closed and sealed as early as approximately 50 years from the initiation of waste emplacement. This would allow closure 10 to 15 years after the last waste package is emplaced. Our designs would also allow the repository to be kept open for at least 100 years from initiation of waste emplacement, and have a reasonable expectation that it could be kept open, with appropriate maintenance, for approximately 300 years.

Our plans call for a substantial effort after the viability assessment to complete site characterization, to continue our design activities, and to develop and document the technical bases for the site recommendation. This plan, supported by adequate funding, should provide the sound basis for a national decision on geologic disposal early next decade. Our plan includes publishing a draft environmental impact statement in '99. This document will describe the environmental impacts of a Yucca Mountain repository under a bounding range of implementing alternatives.

After public hearings and consideration of comments as required by NEPA, we will publish a final environmental impact statement in the year 2000. Should the technical information assembled by the program indicate that geologic disposal at Yucca Mountain is an environmentally sound approach for the management of radioactive wastes, we will complete the evaluation of the site and prepare the technical documentation necessary for a site recommendation in 2001. Should the site be designated under the law, we will submit a license application to construct a reposition in 2002.

Our basic approach has been to focus first on developing site information required to design a site-specific repository system and assess its performance. The Board's recent report emphasizes the importance of both natural and engineered barriers to repository performance. We agree, and our analyses demonstrate, that the performance of the engineered and natural barriers are highly interrelated and cannot be evaluated in isolation of one another.

Our efforts also indicate that advanced design work, including meaningful evaluation of alternatives, requires an increased understanding of the site and the development of detailed process models that were previously unavailable. These advanced design activities are an essential part of the technical work planned for after the viability assessment.

I recognize from the last few Board letters that you would like more alternative design work to have been completed sooner. As you know, we have had to cope with budget reductions and still maintain a proper balance between the scientific investigations, engineering design, and construction, for example, the east-west cross drift which was strongly recommended by this Board. We must do this while maintaining strict Nuclear Regulatory Commission, nuclear quality assurance requirements and good financial and management controls across the program. I, too, wish that more resources had been available to further accelerate work on engineering design alternatives. However, I believe we have balanced the program overall in the best manner that we could do under the conditions.

This meeting focuses on the waste package, an important element of the overall design. The Board has long advocated the use of a robust waste package to provide enhanced containment and to compensate for uncertainties in the performance of the natural system. Several years ago, the program developed a robust waste package concept that has been refined in our current reference design. This package includes both corrosion allowance and corrosion resistant material and is designed for long-term performance in the unique repository environment.

The current waste package design represents a substantial improvement over the one centimeter thin-wall, stainless steel waste package described in our site characterization plan, and is responsive to many earlier recommendations from this Board. Therefore, we believe the reference design is a robust design for a 10,000 year standard. We acknowledge that we may have to evolve an even more robust design or alternative design as recommended by the Board in its most recent report.

This Board has recommended that we develop viable alternatives to the current reference repository and waste package design, and that these alternatives evolve over time as our understanding of the site and the interactions between the natural and engineered system further evolves. We agree that the repository and waste package designs should not be prematurely fixed and that other potential design options should not be foreclosed. At the same time, however, a workable reference design is essential for the repository viability assessment and the completion of site characterization.

The Chairman of the Nuclear Regulatory Commission noted several years back that the lack of a coherent design concept had been a source of discomfort for the Commission. We recognized this concern and have developed a reference design concept for the repository system. This concept is an assessment of the performance provided for the frame of reference required to evaluate the sufficiency of site characterization data and analyses, as well as the potential benefits and costs of design alternatives and options.

The Board has recommended that alternatives beyond design add-on options should be addressed on a cost-versus-performance choice in the viability assessment. Addressing design alternatives, different from design add-on options, will continue to be an important part of the overall design process.

For the viability assessment, however, we believe that the feasibility of geologic disposal at Yucca Mountain is best addressed by focusing on a reference design concept. This ensures that the concepts of the viability assessment rely on consistent information, and the results are not biased by the selection or omission of particular alternatives, thereby obscuring the intent of the assessment.

Even though emphasis on the VA is on a reference design and its options, we will qualitatively address various design alternatives which will be more thoroughly evaluated in later program stages. The Viability Assessment License Application Plan will describe our approach to these evaluations. We further expect that design alternatives will continue to be evaluated throughout repository licensing, construction and operation.

Our design strategy recognizes the need for a workable reference design to support the development and review of a license application, as well as the reality that technological advances can be expected over the decades of repository operation. We are preserving the flexibility to ensure that design features identified now, as well as those that emerge with advancements in technology, can be accommodated in the repository development process.

To efficiently manage the program, however, minor modifications, as well as major design changes or paradigm shifts, must be implemented through a formal design control process. Not only is formal design control a good management tool required by our quality assurance program, it is an absolute requirement under Nuclear Regulatory Commission regulations.

In conclusion, it is clear that the geologic disposal program faces a number of challenges this year and the succeeding years. The program is focused on completing the viability assessment as required by Congress and the President. The viability assessment will provide a sound basis for completing the additional work needed to make a site recommendation in 2001 and license application in 2002, if the site is found suitable.

This milestone is important to the Nation's geologic disposal program and will represent the culmination of a significant effort by all the program participants. We intend that this assessment will provide an unbiased, technically sound, state-of-the-art analysis of a potential repository at Yucca Mountain.

Thank you for the opportunity to address the Board today, and I'd be pleased to try to field any questions that the Board would like.

BULLEN: Thank you, Lake. Questions from the table? Carl?

DI BELLA: This is Carl Di Bella. The decision to have a reasonable expectation of a 300 year opening for the repository I think is a relatively new one. It is to me anyway. I began to hear about it perhaps two months ago.

Does this mean, if it is indeed a relatively new one, that it is too late, in effect, the decision, to give it the full treatment in the VA design, the implications of the 300 year opening, or will it be fully treated in the VA?

BARRETT: First of all, let me give you a little background on the concept of a monitored geologic repository. This is nothing technologically new at all, so there is no design changes, there is nothing different in the repository design than it was a year ago.

In conversations I've had with various people, mostly outside the government, one of the concerns people seem to have was an issue of what I would view as technological arrogance, that we were going to dump this dangerous stuff down a hole, put a couple of plugs and a couple of markers in place and walk away and leave it, and they said who do you think you are that you know for 200 years in the future of what the situation will be, and this didn't sound like a good idea.

People didn't really understand how retrievable or irreversible this was. Also remarks from Nye County regarding what they referred to as an open repository system, which is--the jargon is at issue, but obviously that's what they call it--that they were concerned about this, as I would refer to as sort of the technological arrogance, who are you engineers and scientists who think you know it all for the next 200 years, and we don't. We know quite a bit, but we don't know everything, and we know we don't.

So we talk to the engineers about, well, how robust was the design, how forgiving was the design, as we say that we'd like it to be, and the design criteria for the repository for some years has been basically 100 years. This will encompass the 50 year retrievability requirement of the NRC. When you ask the engineers, well, how stable are those tunnels and what are our reasonable expectations of how long they will maintain, you know, stability, basically the information we got back from the engineers was up to about 300 years. Now, you'd have to do some maintenance at that point.

So we decided to basically portray it a little more accurately by saying that we won't be the ones who will decide to seal that repository or not. None of us will probably be alive at that time--some will. So what we decided to do was more fully articulate the design and the robustness and the flexibility of the design. So we're saying that the design and the licensing basis would be to close and seal the repository shortly after operation, 10 to 15 years, just like it always was. That's what the licensing case would be based upon. But basically the decision to seal it or to maintain it in a monitored condition for as long as the future generations want to do that, they can do that.

So it's basically just a different articulation of the same design, same system as it was before. So I don't expect that we have to--there's no engineering rework to do. It's just a matter of explaining I believe a little more fully and comprehensively what a Yucca Mountain repository is, and that's really the concept of what it was.

PAYER: Joe Payer, just to follow up on that. Does that mean--and I'm just trying to get the concept clear in my own mind--does that mean in perception or reality that the drifts will remain, the liners will remain intact so you could drive a vehicle down in there for 300 years, the same way you could in year one? Or does it just mean that the packages are intact and could be retrievable, even if there were liner collapse or--what does it mean, retrievable, like day one? And then the issue is are we really asking the liner then to last 500 years or 300 years.

BARRETT: It's closer to the former, but this is not a black and white, you know, demonstrated in the adjudicatory process that this is so. The design criteria for the structural tunnels is 100 years. So we have very high confidence that from year one to year 100, you can go in and you basically have geometric stability. Yes, you might have some flaking, yes, you might have some pieces come out type of thing, but basically you can go in there.

Now, the reference design, as you know, is a high radiation field and we're going to do studies on ventilation temperatures later, so it may be hot, it may be cool, but if it was hot, you can cool it with air, but you basically run in the remote gantry--I think you've all seen the pictures, and the two Davids will go through that following me--you could go in there without any geometric problems and stability problems for the first 100 years. Probably you could all the way to 300 years. Now, maybe a piece of concrete might fall out in the year 250, fall down on the rail and could potentially interfere with the robotic gantry, which is the present thought, which means you could--we have the ability in the design with a monorail on the top to send in monitors, T.V. cameras, and you could have some robotic type of thing to deal with. And the highest extent, you'd have to take the packages out, which you can do, and put them in another drift and then go in and do maintenance. But you would not send people into those drifts.

So I would say that we have a very high degree of confidence for the first 100 years, it would be very minor maintenance. From 100 to 300 years, there's various degrees of maintenance. After 300 years, the engineers say, well, I don't know, maybe big sections of that thing are falling down at that point. Then it would be refurbishment. If you had major collapses, you could dig those packages out. I think that would be, the engineers would tell me, well into the post-500 year point that we would have that situation. But I don't want to leave the impression that we can demonstrate from an engineering point of view that there will be no flaws in the tunnel for 500 years. That is not a design requirement, and I don't wish to change the design and shift it around to build a higher engineered liner system in the tunnel. And we're going to evaluate, as we will say in the VA, other design alternatives which could include smaller tunnels, and maybe not having the concrete liners, and rock walls will be sufficient.

BULLEN: Lake, along those lines, let's come back in time a little bit. You mentioned that the LA is going to come by the year 2002, and so the design changes between now and LA probably won't be significant. But it looks like you have the flexibility to change waste package design before emplacement actually starts. Do you think the design is going to change from LA to 2010 when you're emplacing packages based on what you've learned by the construction of the repository, or based on what you know from further tests that have been run? Is the likelihood that that flexibility exists?

BARRETT: The flexibility is there to do that. I would say that when we go into--the license application design will be a design that is sufficient to substantiate the licensing case. Now, to say in the year 2002, is that same design going to be the design for the last waste package in the year 2035? The answer probably is no.

Now, what about the design in the year 2035? How does that stack up against the license application design in 2002? I would say that it's going to be as good as or better than. Okay? So the design evolution would be as good as or better than your licensing case. And a lot of scenarios would be just in reactors. I mean, you started off with most of the reactor designs today are in the 1960s and early Seventies, you know, but yet computers have come along, so you have integrated control panels, whereas before you had the old analog.

So an awful lot of things have added, but basically it's as good as or better than. I mean, the welding and possibly materials change, possibly sizes. I would say clearly the concept of some of the robotics, which there's so many rapid advances in robotics that the design for the gantry in 2002 I doubt will be the same one in 2035, but it will be as good as or better than, and that would be the licensing process, the equivalence of the 10 CFR 5059 for reactors would be applied for the repository if we go forward. So all of that, all of the engineering, design control criteria that would be used in regulated society would spin down on this. So it would be as good as or better than.

BULLEN: Okay. Other questions from the table?

MACDONALD: Yeah, Macdonald. Why is it that we have ended up with essentially two repository scenarios, a rock sale scenario and a tuff scenario? What were the driving forces? Why not end up with a single scenario? Why not fill the civilian waste in the caverns in New Mexico in rock salt?

BARRETT: It's a very complicated societal question. Basically in the 1970s, this nation debated about the proper path forward for nuclear waste, and at one time, the WIPP facility site, which was evaluated, as well as there were nine high level waste sites that were evaluated about that time--actually 17 other sites, there was a big debate about how to go forward and do this.

It was decided after a debate in Congress, which is the proper place in a democracy to do that, it was chosen that the WIPP would be a non-NRC licensed facility. Originally, it was going to be NRC licensed back in the early to mid Seventies, and it would be for defense waste only and for defense material. It was paid for by the defense people out of the defense budget, and that was the path forward, and the 100 and some page Nuclear Waste Policy Act was passed in '82, which said here would be the process to determine the site for the high level waste, and that started off as commercial high level waste. And they debated in Congress about should the defense high level waste go into that material--into that site, and that was left to a president to decide later, and President Reagan decided that we would commingle the defense high level waste.

It came down to a policy call by the folks at the time how big a bite to take and how much can you chew and swallow. It was decided that this site would be basically just a transuranic TRU material, which doesn't have heat generation. It's easier to deal with than dealing with the high level waste as well. I think as most of you know, the EPA Administrator Browner granted the WIPP certification last week, and the WIPP is going forward. Now, it took a long time to get this done and meet the requirements.

There were again policy debates in Congress on land withdrawal and all the other issues. But it was basically a policy call by the Congress and the President as to how much of a bite to take at once and how broad do you do it. If you try to do it all at once, I would suggest that we'd probably still be debating if we try to do the whole thing at once. So that was a call that they made.

BULLEN: Other questions from the table? John Kessler?

KESSLER: Lake, you said it will stay open for up to 300 years with appropriate maintenance. For the purposes of this panel, should we assume then that we don't need to worry about things like rock fall for up to 300 years?

BARRETT: I would not preclude this panel from looking at anything as a given. I mean, I think you are looking at the waste package and its engineered barrier and its interrelationship to the natural setting. I would not put a constraint on you one way or the other. When I asked our engineers, you know, to tell my bosses, and this is an issue that our Undersecretary Muniz is interested in, reversibility and technological arrogance aspects of things, you know, that's what was our best estimate. Now, I guess I would be interested to see what the Board might do. So I would definitely not put any bounds on what you look at. I'd like to hear what you all might have to say about that.

BULLEN: Further questions from the table? Questions from the audience?

(No response.)

BULLEN: Seeing none, thank you very much, Lake. Appreciate it, and have a good afternoon.

BARRETT: Thank you.

BULLEN: Our next presentation will be on the current waste package design efforts, and it will be made by David Haught from DOE.

HAUGHT: My name is David Haught. I work at the Yucca Mountain Project Office, actually the Yucca Mountain Site Characterization Project Office. I work for the Department of Energy, and I oversee the development of the waste package.

My purpose, and Dr. Stahl's purpose here today, is to make certain that the panel members here are aware of what we have done, what we are doing, and what we intend to do in the future, and to answer any questions that you might have. To that end, I do have a supporting cast with me, and if Dr.

Stahl and I cannot answer a question, we'll call on them.

I do have some backup slides that I am not going to go over that are after a slide called backup that have some additional information. And also I would like to thank Claudia Newberry for keeping us all fenced in and getting these presentations together.

Okay, the first thing I want to talk about is where is Yucca Mountain? In case some of you do not know, Yucca Mountain is about right here. It's about 100 miles northwest of Las Vegas. It is in a fairly remote area, which is one of the reasons why this particular site was selected.

Subsurface facility: this is somewhat cartoonish, but it gives you an idea. There is a backup slide that goes into a little more detail about what the subsurface facility is. The important thing is it is within a block of rock called Topopah Welded Tuff, and it is above the water table.

Here's an overview of the surface facility. Material is delivered by rail, and all of the waste will come into the waste handling building where the waste packages are loaded. They're put on a shielded transporter and sent down into the mountain.

I'm going to put this one up and I'm going to talk a bit about it. Our reference VA design, we use a thermal load of--and it's actually an areal mass load--of 85 metric tons of uranium per acre. The drifts are spaced about 28 meters apart, and we do enter-leave defense high level waste and commercial spent fuel. The drift wall temperatures are as shown. We have a design goal of keeping the drift wall below 200 degrees C. Our current design, we can achieve 160 to 180, and the air flow through the drifts are as shown.

The difference in the two is this is an emplacement drift that has been fully loaded, and the drift has been closed, but the repository has not. And I believe the air flow there is for monitors.

Okay, now this is sort of a schematic of what goes on down into the emplacement drifts. This is the main drift here where the waste is delivered down, and this is an emplacement drift. What happens is is we have a manned locomotive that brings a shielded transporter down into the drift. It is on a cart, and then a remotely controlled gantry picks it up off of its cart after it's been pushed out of the transporter, and it takes it to its spot in the emplacement drift. Our current concept, we do not have a lift-over of a package over a package, so we don't really have operationally intended to be pulling waste packages out and pulling them out over, but we still have the flexibility to do that.

Okay, our major design goals are as shown. I want to highlight a couple of them. We have worked on--10 CFR 60 requires substantially complete containment for 300 to 1000 years. We have a program goal to maintain containment for at least 3000 years. We also have a goal of protecting the waste from seeping water for approximately 10,000 years. We want to maintain the fuel rod peak cladding temperature down below 350 degrees C. to avoid crypt rupture.

And then also my guess is the focus of this meeting is going to be on containment, but we do have another significant goal that we have to meet, and that is criticality control.

The waste forms that will be covered by the waste package are there's a commercial spent fuel, pressurized water reactor, boiling water reactor. We have vitrified waste currently in our baseline from three different sites: Savannah River, West Valley and Hanford. There is also in our baseline other DOE spent nuclear fuel, and there are approximately 250 different types that we're going to try to license by categorizing them.

We also have Navy spent fuel and plutonium, excess plutonium, weapons grade plutonium that will come to us in two different forms, either commercial MOX or as an immobilized form, a can in canister ceramic and immobilized in vitrified high level waste.

The design basis waste package environment is as shown. We do have the high thermal load. In times past, we had had both a high and a low thermal load. Some of the containers we shown as they will see dripping water. Now, that is after the temperatures have gotten down to a point to where the water starts coming back and the humidity starts going up. There is water in the vicinity of the waste packages, and here's the pH range, and this is a pH range over time and actually over spaces as well. So it's not that, you know, at a given time, a waste package may see this and at another given time and place, you know, the pH will be somewhere in that range.

STREICHER: Is a question permitted here?

HAUGHT: Yes, sir.

STREICHER: Radiolysis will affect that pH?

HAUGHT: Yes.

STREICHER: I'm seen statements that nitric acid and hydrogen peroxide could be produced?

HAUGHT: Yes. In fact, one of the--I think I've already shown that slide--but we have designed our waste package such that we have sufficient shielding to, at least during the preclosure period, to preclude radiolytically enhanced corrosion. But it is something we are going to have to pay a little closer attention to. And, Dave, are you going to be speaking to that later?

STAHL: No really, but I could mention that indeed we will be doing some confirmatory testing and evaluation next year on this issue. But many people who have reviewed this work, including the Waste Package Degradation Expert Elicitation Panel, have agreed that radiolytic enhanced corrosion is not going to be a problem.

HAUGHT: Okay. I'll put this slide back up briefly. And I've got a scatter plot here of the commercial spent fuel. What I want you to take away from this is that we have a great variety of fuels out there, and what we have attempted to do with our design is to come up with a fairly generic design that will capture the majority of the fuels. Our baseline goal is to try to capture 90 per cent of this scatter plot as you see here, and we do that by looking at the thresholds first of the criticality potential here, and then there's the thermal, which tends to be largely a function of burnup.

Okay. And in doing this, what we have done is we have come up with a design basis thermal and criticality fuel, and we've designed our waste package using that fuel as an analysis tool.

SAGÜÉS: Excuse me. Could you put up that graph again?

HAUGHT: That one?

SAGÜÉS: Yes. And, again, could you please go over the units in both axis?

HAUGHT: Okay. Well, first off, this is a scatter plot. These are the fuel assemblies that are out there. Okay? And the color code addresses the quantities. Well, in this axis, you have the initial enrichment, and on this axis, is the burnup or service history of the fuels.

SAGÜÉS: You mean that is the past burnup, the burnup to day, you mean, or the projected?

HAUGHT: The projected as received at the repository.

SAGÜÉS: And the units are again?

HAUGHT: This is just a strict percentage, and this is gigawatt days per metric ton of uranium.

KESSLER: Does this assume burnup credit?

HAUGHT: Well, this chart, yes. Well, burnup credit is not the way to look at this. This is a representation of the characteristics of the fuel that is out there as far as enrichment and burnup.

KESSLER: But you've got 70 and 98 per cent. I assume that's your K effective, or something like that?

HAUGHT: No, no, no. That's strictly the--

KESSLER: That is the number?

HAUGHT: Yes, this bit right here is the 100th percentile. All these lines here are percentiles.

BULLEN: Percent of the inventory?

HAUGHT: Percent of the inventory.

KESSLER: Right. But what does the 98 per cent criticality curve mean? 98 per cent of the fuel is less than what; K effective less than what? Isn't that .95?

HAUGHT: .95 And that's just the K infinity. Yeah, these are K infinity lines.

KESSLER: Okay, thank you.

HAUGHT: Okay?

SAGÜÉS: Then, for example, let's go to one of the red dots there near the center. That means that, say, like about 500 to 600 assemblies, it will have had so many gigawatts? And also, I don't understand normalization per MTU.

BULLEN: That's just a standard normalization for how much energy you get out of an assembly.

SAGÜÉS: Yeah, but you've got or you are going to get out of it?

BULLEN: This is gotten. This is all that's gotten in the reactor. So this is in reactor energy release. And so they've got some that have already been burned to 40,000 megawatt days per metric ton, 40 gigawatt days per metric ton, and they expect to top out at around 70 for some future fuels, although there are some close to 70 right now, aren't there, a few assemblies?

HAUGHT: Tom, are you aware of any over 870?

BULLEN: There aren't any over 70.

SAGÜÉS: Okay. So the lower the dot is, that means that that particular assembly has a lot of energy to give out yet?

HAUGHT: Well, yeah. As you get out this way, you have a very reactive fuel, and as you go up this way, the fuel is less reactive. Okay?

Now, one of the things that Lake mentioned this morning at breakfast is--and let's remind ourselves we do have to license whatever it is that we come up with, and it's a little bit of a different realm than just putting together an intellectual exercise. We actually have to go in front of lawyers and then a regulator and convince them that the things that we say we can do, can in fact be done, and that the waste packages that we come up with will be specific to what it is that we put in. Now, that's not to say that each individual waste package will have an individual license, but the license that we will be granted will say you can use this design and put this fuel or this fuel or this fuel in it, and you can put it in that mountain. And in this case, we are talking about a potential repository at Yucca Mountain.

Another key consideration is the fabrication and inspection specifications that we will use. To the extent that we can use the industry consensus codes, we have some sort of precedence.

Now, this is a chart, and there are some backup charts on this one to describe what these various options were. This is a progression of how we have gotten to the design. Key date is 1992. We did do an evaluation of seven different design options, and pretty much ever since 1992, we have had the robust/multi-barrier design.

The multi-purpose canister was added in 1993, which is really just you could consider it another waste form. It's a canisterized spent fuel.

1996, the advanced conceptual design, the corrosion resistant material was Alloy 825, and we had a carbon steel on the outside. We still carried a large and a small waste package at that time, and a high and a low thermal load waste package at that time. The low thermal load waste package had an outer barrier of Monel 400.

1998, our viability assessment design, the corrosion resistant barrier is C-22. The corrosion allowance material is A-516, a carbon steel. We generally have only the large waste package design. There is one case of a small waste package design for thermal considerations, and it is strictly the high thermal load. We do not have a special low thermal load design at this point in time. And, of course, the license application design is still to come.

Now, at long last, I'm actually ready to talk about the waste package design itself, and let me point out some of the features. As I said, the inner barrier, the corrosion allowance barrier is alloy. The inner barrier is C-22, not Alloy 625. That was the previous incarnation. So we'd make a correction there. The outer barrier is A-516. It's a carbon steel. The inner barrier is about two centimeters thick. The outer barrier is about ten centimeters thick. The diameter is roughly two meters, and the length is roughly five meters. The basket is is a tube design. The tubes contain the spent fuel assemblies. There are interlocking plates of a stainless steel boron material that is tack welded together. That is the neutron absorber material.

And then basically what--the way it is built is that the inner and outer barriers are fabricated using a shrink fit technique. The outer barrier is heated up, the inner barrier slid inside, and then it's allowed to compress in. And actually there is a demonstration of this that's going to be done tomorrow at Ranor in Massachusetts.

What we will get at the repository is we will get the can, and then we will get the two lids. So we'll get the transportation cask, we'll unload the transportation cask with the fuel in it, and then do the closure welds on the two lids.

PAYER: Excuse me. What's the weight of the steel and the C-22 respectively?

HAUGHT: The weight of the steel and the C-22 respectively? Tom, do you have that?

DOERING: We'll get that to you.

PAYER: Okay.

HAUGHT: We can look that one up. Okay, our TSPA-VA calculations to date showed this type of performance. Let me go through this. This is one of the realizations for our TSPA runs. This refers to the northeast area. The PA of the repository is carved up into blocks. This is the northeast area of that block, which is one of the more conservatively modelled areas. This is using the spent fuel waste package. The high level waste, glass waste package performance is similar. We do not have backfill. There is water always dripping once the temperature and humidity profiles are permitted and there is a switch for that. And then a waste package is 100 per cent wetted, and what that means is that the drips are sufficiently distributed as they come down to wet the entire waste package surface.

SHOESMITH: Excuse me. When you say that northeast area is a conservative area, I presume that means that that area has dripping and some other areas don't; is that what the conservatism is?

HAUGHT: Yes. A couple of things I'd like to point out is that we begin--

STREICHER: Excuse me.

HAUGHT: Yes. Is there another question?

STREICHER: This is carbon steel?

HAUGHT: The way this chart is is here is the carbon steel, here's the performance of the carbon steel, and then these lines are the performance of the C-22. Okay, so what we have happening is that the carbon steel starts to go away, and C-22 begins to corrode.

MACDONALD: And exactly how were these calculations done?

HAUGHT: The calculations were done--okay, it's a stochastic process for how the environmental parameters are modelled, including the switches for the temperature and humidity and when it drips and so forth and so on. But the actual models of the performance of the barriers are based on abstractions of process level models that were produced at Lawrence Livermore.

Okay, a couple of things I'd like to point out as we begin to see first failures of waste packages at around 2600 years.

KESSLER: Can you define a failure?

HAUGHT: A failure is a breach of a waste package, so now we have a hole that goes from the outside to the inside. We no longer have complete containment. Okay?

Our 50th percentile is here. It's around 200,000 years. And you'll see that these lines are almost coincident with one another. The failures are dominated by patch failures as opposed to pit failures. So we believe that at least our model, our PA models are showing that general corrosion is the dominant failure mechanism.

STREICHER: What kind of environment are you assuming here for the failure of C-22?

HAUGHT: Dr. Stahl, can you address that?

STAHL: Yes. In these cases, we're assuming that you have very aggressive environment, but there is a spectrum of conditions, and I can talk about that in a little bit more detail in my presentation.

SAGÜÉS: Excuse me. Does that include the so-called juvenile failures?

HAUGHT: No, this does not include juvenile failures.

KESSLER: Remind me again, David, these patches are what, the 310 square centimeter patches?

HAUGHT: That's correct.

PAYER: David, you said that these are always dripping and 100 per cent of the canister patches are hit by drips. Are you sure of that? I thought the whole fact is wetted, meaning it's in 100 per cent relative humidity, but the drips only hit a certain percentage of patches.

HAUGHT: Okay, what this means is--well, first off, what it doesn't mean is it doesn't mean that we basically have the drip just raining on a waste package. You know, randomly water is dripping down on waste packages. So in a given time, you're only going to have just whatever drip is happening. But the drips are spatially varied such that they cover the entire waste package. So it's not that we have, you know, a recondensation of water on the waste package due to humidity.

STAHL: I'm just a little confused still about the difference between a pit and a patch penetration is. A pit is a localized corrosion failure?

HAUGHT: That's correct.

STAHL: A patch is a general corrosion failure. So these are independent failure processes. A pit doesn't grow into a patch?

HAUGHT: Right.

BULLEN: One quick question back to the corrosion allowance breach. You don't have your first package failure until 1000 years, even with dripping water, or is there a dry-out time that you're assuming in this calculation?

PAYER: The dry-out period is built in.

BULLEN: Is there a dry-out period built in?

HAUGHT: Yes, there is a dry-out period.

BULLEN: Okay. Does the dry-out period end in 1000 years?

HAUGHT: Roughly.

BULLEN: Okay, thank you.

HAUGHT: Yes.

BULLEN: Getting back to radiolysis, you assume that there isn't any here?

STREICHER: I don't believe we have addressed radiolysis as yet.

STAHL: Well, we've done some calculations which indicate that because the waste is decaying, the dose on the surface of the degraded package is always below the threshold that radiolytic corrosion is a problem. But we intend to do some further evaluation on that subject.

BULLEN: I have to ask, Dave, that threshold being what? What was the threshold?

STAHL: Somewhere in the 100 r per hour range.

BULLEN: And you're going to verify that?

STAHL: Yes.

STREICHER: And that means no nitric acid, no hydrogen peroxide being generated?

STAHL: Correct.

KESSLER: Have you analyzed for any remaining water inside the package?

HAUGHT: Yes, we have.

KESSLER: Because I thought in some of the early dry storage designs, they had up to, what, a liter of water, or something like that?

HAUGHT: Our analysis is showing more like the most water that you could get in a waste package with the drying specs that would be applied is somewhere in the order of 80 grams.

KESSLER: And that's on drying specs that are currently used for current dry storage containers?

HAUGHT: Tom, help me here.

DOERING: Tom Doering with Waste Package Development.

What we're doing out at the repository is actually dry loading, so the fuel actually goes into pools. Before it gets put into the waste package, we're actually going through a drying system, therefore, sort of making sure it's dry. Unless there is a failed fuel rod that has water logged for some reason, that's the only way we at this time anticipate any kind of moisture getting into the waste package itself. So the waste package, to make sure that we don't have internal radiolysis, is dry from start to finish. That is one of the reasons why we'd like to hold to dry loading.

In some of the radiolysis evaluations we've done, we've looked at a lot of radiolysis, a lot of shielding calculations that we've done on that one, again, through the decay, the majority of the radiation that we're dealing with is a gamma dose, an alpha dose for radiolysis inside the gamma dose on the surface, and those gammas do go away with time, and the time we're really interested in is around the 3000 year plus time frame, most of the gammas really have gone away and have left the neutron and the secondary gammas there, which are very low dose on the outside, therefore, making the radiolysis sort of a second order or third order effect.

STREICHER: There have been reports about chlorides in the water. Are you assuming the presence of chlorides and the fact that if they are present, they will be concentrated at the hot surface?

HAUGHT: Thus far, I don't believe we have seen that, have we?

STAHL: I'll talk about that when I give my presentation. So please hold the question.

HAUGHT: He will cover that.

MACDONALD: One question. The calculation assumes that you've got to have a liquid phase, a liquid water phase, in order to have corrosion; is that correct?

HAUGHT: That's correct.

MACDONALD: But, in fact, from a corrosion point of view, what you require is a proton conductor on the outer surface. And that can be a hydroxide. And wouldn't it be wise to calculate the temperature as being the temperature at which you went from a hydroxide to an oxide rather than the temperature at which you get rid of a bulk liquid phase?

HAUGHT: Dave, can you address that?

MACDONALD: This is something that, when I did some modelling work for Livermore many years ago, bothered me.

STAHL: Yes, certainly you're going to get ion oxyhydroxide if that's what you're concerned about, but you'd still need to have some relative humidity to have that form. Otherwise, you'd just have perhaps an oxide phase. We've examined that and we don't believe that is a problem for the first 1000 years. But certainly that can be further evaluated. That's based on the work of Greg Gdowski at

Livermore, and perhaps you could follow up with him on that subject. Dan, do you want to add anything more on that?

STREICHER: The bottom line is what is the possibility of ferric chloride being formed? Because that would be aggressive to the C-22.

STAHL: I'll address that when I make my presentation.

KESSLER: Now is probably a good time to ask these questions since you've got that figure up. Back on Figure 9, you said something about you have a goal of containment of waste for at least 3000 years, and then you've got another goal of 10,000 year waste protection from dripping water. What I wanted to know is, I know that Dave Stahl has mentioned this in the past, but I forgot what the numbers are, quantitatively what do those word goals mean in terms of containment?

HAUGHT: Okay, word goal containment, substantially complete containment means less than 1 per cent failures. Okay? So a single failure does not concern us. So if you take this--actually, we don't have a 1 per cent line here, but if you were to extend this line out to about where the 1 per cent would be, you start seeing failures on the order of about 20,000 years is what our projections are telling us now.

KESSLER: Okay. But the point is you've got a containment of waste for 3000 years, and then this waste protection for 10,000 years?

HAUGHT: Right.

KESSLER: So the containment of waste is just like a first pit through or anything like that in 3000?

HAUGHT: Right.

KESSLER: And that's the less than 1 per cent?

HAUGHT: That's the less than 1 per cent.

KESSLER: Less than 1 per cent of the packages will have even just one pit or one hole in 3000 years?

HAUGHT: That's correct.

KESSLER: And then at 10,000 years, you've got less than 1 per cent of the packages that have a hole big enough to allow water into it?

HAUGHT: Dripping water onto the waste form.

KESSLER: Okay. And that's defined as what, a single patch failure or two patch failures, one at the top and bottom, so you get flow in? I mean, what--

STAHL: Just one at the top.

KESSLER: One at the top. One patch failure at the top. Okay, thank you.

HAUGHT: Dr. Craig?

BARNARD: For those of us who aren't familiar with the jargon, can you explain what a juvenile failure is?

HAUGHT: Okay. A juvenile failure would be a waste package that would have some latent failure as a result of just the material or the fabrication process that was not caught during inspection.

Okay, I'm going to leave the existing design up. We are also looking at some alternative designs or design options. I'm basically talking about the license application time frame here rather than the VA.

One of the options that we are looking at is a two corrosion resistant material design without identifying what those two materials might be. We are also looking at alternatives that would involve a shielded waste package which would allow a person with proper ventilation in the drift to actually go into an emplacement drift. We're looking at applying ceramic coatings to the waste package. And these two options, design options, are features really that we could add regardless of our waste package design in general.

Another thing we're looking at is using this same VA design, but thickening up the inner barrier. Our calculations today are showing you can actually buy time in doing so. And there is a backup chart in there that gives some indication of as you add more C-22, the additional containment time that you might get out of that.

Now, that is all I had prepared to discuss. If there are any further questions that we have? Yes.

KESSLER: Maybe you're the wrong person to ask, and we'll get this later on, but on this patch size of 310 square centimeters, now, you've got these goals that are based on single patch failure. What is you--I mean, where is this 310 square centimeters coming from? Is it based on something having to do with the performance? In other words, 310 square centimeters is a critical size in terms of allowing a certain amount of flow-through? What if you had chosen 31 square centimeters that had the same criteria, where would the terms be?

STAHL: The patches were basically selected just for statistical manipulation. There is no--

KESSLER: Right. But you've got arbitrary criteria built around these statistical numbers you happen to have chosen, and it also I presume will affect your failure distributions based on the 310 square centimeter patch size. If you'd chosen a different patch size, you might have gotten a different failure distribution. I mean, I'm asking the question.

STAHL: I'm not sure that it would be a major influence on the patch size.

KESSLER: Okay.

STAHL: I think you'd have to ask Joon Lee, who's done the detailed analyses.

KESSLER: Okay.

HAUGHT: Any other questions?

AHN: Tae Ahn, NRC. In your major design goals, you addressed substantially complete containment, NRSC Part 60, and that rule is currently under consideration of amendment, and my question is whether your design is based on TPA result, calculation result which is based on both criteria. That's my first question.

My second question is water, groundwater, you said bicarbonate solutions. Is that your severe environment or by carbonate, do you mean the various other constituents such as silica or fluoride or chloride?

HAUGHT: The first question, we are aware that Part 60 is under some consideration of revision to go to more of a total system performance standard as opposed to the sub-system standards. We are not going to get out ahead of that. We do--you know, Part 60 is still on the books and we still have to show compliance with that.

Now, what we have done in the meantime is we have put together a repository safety strategy where we have identified certain factors that are important to maintaining the total dose or reducing the total dose to the public, and one of those factors is containment of the waste until such time that the waste cools down to where you don't suffer the rapid oxidation and the increase in surface area of the fuel itself.

Another thing has been the dripping water. So what we have done is we're trying to meet the current regulation, knowing full well it may go away, but in the meantime, we are also being compliant with our repository safety strategy, which actually is a little more in line with what we anticipate the new rule may be, more of a total system performance standard, in which case, we will still need to come up with some allocation of performance to the engineered barrier.

Your second question is we are doing our testing under a variety of groundwater chemistries. We've taken the standard J-13 water and we are running it at ten times concentration of the species in the water, a thousand times concentration, and then we have varied the pH actually from two to eleven, is it, Dave? Two to eleven. And I believe, Dave, can you speak more to that later?

STAHL: Yes.

HAUGHT: And you can get some more details when Dave starts to talk about the testing and modelling.

BULLEN: Chairman's prerogative here. Carl Di Bella has the last question, and then we're going to move on to the next speaker.

HAUGHT: Make it good.

DI BELLA: How are you handling the radiolysis issue on your dual CRM design? In other words, are you trying to design it so that the radiation field on the outside of the waste package would be approximately the same as the existing design, or are you saying we don't need as much shielding because the two CRMs are going to be more resistant to radiolysis, corrosion?

HAUGHT: Okay, a couple things. Where I believe we are right now and, Dave, interrupt me if I'm wrong, is that we're beginning to think that we may have overshot the amount of shielding necessary to preclude the radiolytically enhanced corrosion, at least during the preclosure period of time. And so we believe we actually can skinny down the thickness of some of the materials, which actually it applies to the corrosion resistant materials as well.

As far as any in depth analysis of how much shielding would be required for a given material, I don't believe we have done anything on that as yet.

STAHL: We're in the process of looking at that. But I think the principal driver is not so much the radiolysis, but the mechanical strength of the package. And if you have further questions, I'm sure Tom Doering could answer those.

STREICHER: A short one. As the carbon steel is corroded away, will radiolysis increase?

STAHL: No. I'm saying it will decrease because the dose is roughly being decayed away at about a half life of 35 years, on average.

BULLEN: The gamma dose?

STAHL: Yeah, the gamma dose.

STREICHER: So you're counting that the two factors will sort of--

STAHL: Yes, basically aqueous corrosion is not starting for about 1000 years, but in that time, you've got 30 half lives, and the dose is negligible.

BULLEN: Thank you, David. Appreciate it.

Our next speaker will tell us all about the materials research program associated with the waste package design, and it's David Stahl from Framatome Cogema Fuels. David?

STAHL: Thank you.

I'm going to give you an overview this morning of our materials testing program, and in passing, also talk a little bit about our long-term plans.

Most of the work in the metallic container area is the work of Lawrence Livermore Laboratory and Dr. R. Daniel McCright is here. If you have any further questions, followup, whatever, please talk to him.

This is basically the outline of my presentation. I'm going to talk very briefly about the objectives and our strategy. I'll walk you through how we are developing materials data and models, talk about some of the inputs to materials testing that David Haught briefed you on to some degree, mention a little bit about material selection in regard to Alloy C-22, then go into a little bit more detail on the status of the materials testing program, and then summarize.

Now, as I indicated, our objective is to provide the scientific basis for materials selection and performance for both waste package design and EBS system and performance assessment. There is an existing ASTM procedure, 1174, which lays out in detail a comparison of modelling and testing, such that you can look at the--evaluate, rather, the long-term behavior of the material.

As David Haught has already talked about, we have controlled design assumptions regarding waste package lifetime, 3000 years, and keeping the water from contacting the waste form for 10,000 years.

Now, here's our testing strategy. It hasn't changed much in the last few years. We've identified the candidate materials. We've identified the active degradation modes, and I'll talk about those briefly. We've performed preliminary tests. We've conducted degradation mode surveys, and these have been reported and I think the Board has copies of all of these. As a result of those, we've developed long-term plans. We've received input from technical experts, including this Board. We're beginning to identify analogs and we'll be collecting shortly in situ data from the large block tests and the single heater test, and we'll be collecting as well information from the drift scale heater test as that proceeds.

As I mentioned, we have a parallel effort of development of models and performance of tests, and that information is provided to performance assessment and design. There are a variety of different tests that are performed; characterization tests, you can understand the degradation processes themselves, service condition tests, which are basically run at temperatures and other environmental conditions that we would expect at Yucca Mountain. We have accelerated tests that are performed under more aggressive conditions. We will be doing confirmation testing as part of our performance confirmation period. And as I mentioned, we will be including some analog evaluations.

Dave Haught has already talked to you about some of the assumptions in regard to the environment and what those design basis environments are. We have test conditions indicated here, basically just what David Haught indicated. We have a range of pHs and a range of temperatures which we feel bound the expected conditions.

One of the things that we've added recently is to test under concrete-modified conditions where we have the addition of I believe calcium sulfate to the system. We're also looking at whether we need to test under saturated conditions. We believe that 1000X J-13 is fairly close to saturation, but we are looking at whether we need to go to full saturation for some of these tests.

MACDONALD: How confident are you that J-13 in composition is going to remain relatively constant?

STAHL: Well, the J-13 composition is taken from a well which is in the southeast portion of the Yucca Mountain site. There have been a variety of different wells that have been examined. There's also been many attempts at extracting pore water from the rocks. Those have given similar compositions. Basically, it's a sodium bicarbonate groundwater with something around 10 ppm of chloride. So we think it's representative.

MACDONALD: Are there any metals such as copper in that groundwater? Are there trace amounts of copper?

STAHL: Very low copper, as I remember. It's not measurable, I believe.

STREICHER: Does this include the recent findings about chlorides from isotopes, chloride isotopes that were found in the water at 800 feet?

STAHL: Well, they're certainly at different portions of the repository, such as perched water zones where you have slightly different chloride levels. But we believe at the repository horizon, that these are the chloride levels that are appropriate. But we do have concentration. I don't have the table, but certainly when you get up to the 1000X J-13, you've multiplied that chloride concentration, which was about ten parts per million, to 1000 times that, and this is the basis for our test.

We're also doing, as I'll talk about in a minute, a lot of more aggressive electrochemical tests at much higher chlorides.

STREICHER: So we're talking about a relatively concentrated chloride solution. Also, I note the pH is two now rather than--

STAHL: 2.7 is the lowest actually that we've tested. We had hoped to get to two, but it's on the ragged edge of the ability of the tanks, and I'll show them in a minute, to withstand an acid, so we kind of had to compromise and went to 2.7.

STREICHER: Is there an iron content involved here? What would be the concentration?

STAHL: The concentration in the iron in J-13 again is pretty low. There is some residual iron, but it's very low. I'm sorry i don't have a chart of composition of J-13.

STREICHER: Getting back to the possibility of getting a ferric chloride solution--

STAHL: I'll talk about that in a minute.

As far as the container materials under test, I think most of these have been covered by Dave Haught, but this is the detail of the various materials. We also have some intermediate corrosion-resistant materials, copper-nickel and nickel-copper. We have a host of corrosion-resident materials.

One of the things that I wanted to mention is that as far as the materials testing program, we're hopefully more conservative than what we would anticipate in the design range that was indicated by David Haught. So we have, as I indicated, a variety of different materials in the corrosion-resistant area. You have nickel-rich alloys, G-3, G-30 and 825. We have nickel-base alloys, 625, C-4 and C-22, and then titanium alloys, Grade 7, 12 and 16.

There are other materials that we're testing, 304-L, 316, low carbon in the case of without boron, and 304 and 316 with boron. These are for criticality control. We also have some Zircaloy specimens there to support the Navy reactor program, and I'll talk about this a little bit later. We have some ceramic oxide coatings, various oxides and mixtures of oxides that we might utilize as a spray coating on the carbon steel.

KESSLER: Is the Zircaloy virgin Zircaloy, or is it--

STAHL: No, it's a Zircaloy--what is it, Zirc-4?

MC CRIGHT: It's material that Westinghouse data supplied us, and they really didn't give us all the details of it because I think it may be verging on classification issues, but it's what they use for their program. We're also testing some hafnium that they supplied also.

KESSLER: Dan Bullen answered the question I really was wanting to ask but didn't ask very well, which is it's unirradiated.

MC CRIGHT: It's unirradiated, yes. It's stock material.

STAHL: This is a list of the degradation modes that we considered. We have models for many of these for TSPA-VA, and certainly we will have models for all of these for LA. Some of these are preliminary; others are more advanced. And I'll talk a little bit about these a little bit later on.

BULLEN: David, before you leave that, we saw that there was a threshold for failure of the corrosion allowance barrier of about 1000 years, which was dictated by the dry-out time. Does your corrosion allowance humid air corrosion, general aqueous corrosion model have spalling in it, or is the oxide remaining intact during your modelling?

STAHL: No, we assume that you're going to have spalling because we have basically a linear rate of corrosion. So we assume that there's no barrier. You don't have diffusional resistance there.

BULLEN: And so the distribution that you get in your corrosion allowance barrier that Dave showed us has to do with the fact that it's not dripping all the time in the same place on the same package; is that why you get such a distribution of failure? If you're dripping and it spalls, it would seem to me it would fail pretty quickly.

STAHL: What Dave had shown was a conservative assumption in the northeast quadrant. You have to look at the totality of packages in all of the different quadrants to come up with a total.

Just a word about selection of Alloy C-22. I could back up to say something in regard to the concept of the A-516 and the corrosion-resistant barrier. This was basically developed by myself and Tom Doering in 1992, and we've evolved a little bit in regard to the corrosion-resistant barrier. We've moved from A-25, which was our original basis. That was as a result of tests that were performed by Lawrence Livermore Lab on the thin walled package. And we've gone through 625, and now looking at C-22 as the reference material. Certainly C-22 is more resistant to crevice and pitting corrosion than 625, and certainly more than 825. It has excellent phase stability as opposed to 625. It's more resistant to stress corrosion cracking, and what we found more recently is the weldability of Alloy C-22 is not significantly different than 625.

BULLEN: David, before you leave that one, in your phase stability study, did you do a survey of the phase stability of the as welded material, or is it phase stability of base metal?

STAHL: Yes, as welded material. In fact, there was a mini-workshop a couple of weeks ago that the Nickel Development Institute has sponsored, and they were looking at just that issue, and Dave Moeller is--Ralph Moeller, excuse me, is here to answer any questions you might have about that particular workshop.

Okay, I'd like to move on to some of the results that we have from our testing program. Many of you have seen this picture before. This is our corrosion test facility. We have 24 of these tanks which contain a variety of different solutions. We do not test all materials under all conditions, however. We don't have carbon steel under pH 2.7, for example, because we recognize that that's not an appropriate test condition, nor do we expect the carbon steel to see that pH. Carbon steel would be exposed to more neutral conditions, or possibly the high pH conditions as a result of water modified by concrete.

So we have 18 vessels that look at standard corrosion with weight loss, crevice and U-bend specimens, and we have six additional vessels that are evaluating galvanic couples. These couples are mostly A-516 with the corrosion resistant materials, but there are also some Monel samples in there as well. I think I've covered that point.

We have one year test results for carbon steel, and they basically confirm that the rates are what we had predicted at the beginning, basically tens of microns per year for the one year tests. For the corrosion-resistent materials--oh, let me just show here the racks of specimens.

We have in inventory now something like over 16,000 specimens. This is the water line roughly here. So these specimens in the upper region are exposed to humid air, and below this, are those specimens that are immersed in water. These are the U-bend specimens, and these are the crevice specimens which are torqued to the standard pressure levels.

Just very briefly, I could show some of these. These are just some tests taken out for carbon steel after six months, and you can see, as we know, that carbon steel rusts. We want to measure that of course as a function of time, and as I indicated, the rates are about what we'd expect. We have tested the C-22 and all of the corrosion-resistant materials. As you can see, basically no attack for those materials. The only specimens that we've seen some attack is on 825 in acidified water. This is the pH of 2.7.

BULLEN: Is the gasket on that material teflon?

STAHL: Yes, it is.

BULLEN: Did you have any degradation of the teflon that you noted? Is there any Hf that you're--

STAHL: Yes, there is some Hf that does come out, and we see some of this perhaps staining in some of the titanium specimens.

BULLEN: But it was the 825 that was the only one that showed significant pitting?

STAHL: Correct.

BULLEN: Okay.

STAHL: I don't have the titanium specimen, but it did show some staining, which we believe was a result of the teflon degradation.

STREICHER: 825 is out of the picture now; right?

STAHL: It is out of the picture, but we're keeping it in the test program for comparative purposes. For example, and I'll talk about a little bit of the electrochemical tests, it's a good measure of pitting rate. We can readily measure it.

STREICHER: Yeah, it has copper in it and it's extremely vulnerable in sea water, for example.

STAHL: Yes. Well, as you know, it was not the best selection for aggressive conditions.

One of the things that we got from recent presentation by Ralph Moeller was one of the samples that was taken from Kure Beach, this is a sea alloy, and you can that it's just as shiny as the day it was put out there in 1941. It experienced a range of temperature, humidity, precipitation, salt spray and occasional--well, that should be inundation. You can see that the original mirror finish is still intact.

C-22 of course is a newer alloy. It's more resistant, and we expect that its performance would be much better than the C alloy.

One of the interesting things that we've started is some crevice corrosion testing utilizing a fiber optic probe. And this addresses the issue that you raised, Michael, having to do with what is the ferric ion concentration in that crevice. We think that it's low, and that we don't have, even though we have a high chloride concentration, we don't believe that we have a chloride dominated corrosion process because we do have inhibitors that are present in the J-13 water, and that's what we believe is the reason why we do not see any localized corrosion of these materials, as you do have nitrates, sulfates, bicarbonates in that water.

At any rate, the objective of these tests is to actually follow the corrosion chemistry as a function of time, and we have some of these probes developed which are sensitive to each of these particular ionic species, and we will be setting those tests up to examine the corrosion, as I said, as a function of time.

SAGÜÉS: Excuse me. Of course the concern may not be just the iron in the J-13 water, but the degradation of the corrosion allowance metal and the fact that those corrosion products may be resting on top of the C-22 for an extreme amount of time. And that could, of course, greatly overshadow whatever iron existed in the J-13.

STAHL: Yes. This is somewhat tied to the next bullet. We're doing some drift testing onto heated surfaces, and basically we're looking at the electrolyte chemistry has to do with the concentration of those ions as a function of time, but also we plan to do some tests where we have corroded most of the way through the carbon steel, and then begin to look at the corrosion of that crevice as it proceeds, so we'll have hopefully a better understanding of just the point that you're making, what impact that iron and all of the other electrolytes in that water have on the corrosion of the corrosion-resistant material.

STREICHER: Is J-13 really the only source of water? And if nitrate is acting as an inhibitor, where is that nitrate coming from?

STAHL: There is nitrate in the J-13 water about the same level as chloride. So as you concentrate the chloride, you're also concentrating the nitrate and to some degree the bicarbonate, although that depends on the pH.

STREICHER: I'm wondering could it happen that the nitrate over the years might disappear? I don't know, is that agricultural in origin or where is that coming from? And is that the only source of water, or is there some other water such as has been detected with chlorine isotope recently?

STAHL: Well, there are recent results from the single heater and the large scale heating test, and what they found there, and perhaps there are others in the room that could give more specific information, but what they found is that water basically evaporates through the fractures, leaving behind some of those salts. It will condense in the cooler regions, and after the thermal pulse passes, that water will then begin to come back. At that point, that water is basically deionized water. It's pretty benign water. But as it potentially comes back and could drip onto the waste package, it will pass through these fractures again--could pass through these fractures again, and then pick up some of those salts and reprecipitate them onto the heated waste package.

There's some discussion also that some of these fractures can in fact be sealed as a result of these evaporation processes, but we don't consider that as part of our analysis. We assume that the water will come back, and you do have the potential of concentrating it in salts.

Greg Gdowski has addressed some of those issues in regard to the chemistry of the water and as a result of concentration and he's actually doing long-term relative humidity tests with carbon steel as the emphasis there, but he's also looking at other materials, and he's looking at both salted and unsalted surfaces. Basically what he's done is taken concentrated J-13, spraying it onto the heated surface so that he gets a concentrated salt film, and then uses that specimen in his relative humidity chamber tests. And he's found the weight of corrosion basically as a function of time with the long-term relative humidity tests, and also some thermographometric analysis testing that he's been doing. And I can give you a little more details on those tests a little bit later, if you'd like.

We've done a great deal of short-term and long-term electrochemical tests and crack growth tests with C-22 and other corrosion-resistant materials. And as I mentioned it's a good comparison with some of the corrosion-resistant test data that we've gotten in the long-term. It shows some material exposed in acidic brines at 90 degrees C. with 10 weight per cent sodium chloride, and you can see, as predicted, we get a lot of pitting of 825, a little bit of pitting of G-3 and G-30, not many indications of anything happening to the titanium Grade 12, the C-22 or the C-4. As I said, that's consistent with basically what we expected for those materials.

We also have MIC, microbiologically influenced corrosion tests underway. We're looking at nutrient requirements. This is one of the things that Dr. Brenda Little had pointed out as part of the original waste package degradation expert elicitation that we need to look at the mass balance in the system, and we're doing that. We're looking at biofilm generation because we think that's an important precursor to microbiologically influenced corrosion, and we're looking both at carbon steel and the corrosion-resistant materials, including C-22.

As far as the carbon steel, in many of the experiments that we've performed so far, we've got about a four or five fold increase in corrosion rate of the carbon steel. We have not seen any increase, in fact, measured any corrosion in the Alloy C-22 in these tests.

One of the options that we're considering is ceramic coating on carbon steel. We believe that if we have an impermeable coating, that's going to resist corrosion. There was concern of the Board and others that carbon steel--excuse me--ceramic coatings on carbon steel would not survive. We have some results here, somewhat qualitative at this point, in regard to some plasma spray and high velocity oxy fuel tests. I'm not sure what the order is here. But here is a plasma sprayed coating on carbon steel substrate, and you can see in this case, it's not a very dense coating, so you do get some corrosion at the interface.

Over here, with high velocity oxy fuel spray, we have a much denser coating, and you can see some cracks in the system. There's no corrosion at the interface.

What we further had done is to actually put some notches in the specimens. This happens to be with and without a bond coat here. This is a nickel bond coat. And in this case, we did see some corrosion, and here's another one without the coating, and there's no corrosion at this interface. These have been exposed at three months concentrated J-13 at 90 degrees C. pH is about 9 1/2 I believe in these tests.

Here's another one again comparing the plasma sprayed low density coating. You can see that there's been some corrosion there, and some corrosion here at the notch. And, again, in comparison to the dense coating, no corrosion.

So we think that ceramic coating is a viable option. Tom Doering and his people have looked at the mechanical stability of that system using an element analysis, and they believe that those coatings will withstand the handling loads.

We're also doing some tests on concrete materials looking at the changes of structure as a function of pH and temperature. We're doing some testing, aging these in high relative humidity, and we're looking at some samples that we've intentionally carbonated to convert the structure to calcite.

Certainly if you do have the carbonation, it means that any water dripping down through that system is going to be more in the calcite dominated region, which will probably be in the range of about 8 1/2. Without that aging treatment, the pH of that water might be in the range of 11 or 12.

The last bullet here, we have developed models that describe the performance of these materials, and we've provided those models to Performance Assessment for TSPA-VA. In the back of my packet, there is a listing of those models. I won't go into them at this particular time, but I certainly can answer questions.

We have some work that we would like to get going at the University of Virginia, particularly looking at some of the technical basis for pitting corrosion in these corrosion-resistant materials. We're looking at model parameters such as pit birth and pit death, what those stabilization criteria might be for those pits, and the pitting rate, and we'll be comparing the corrosion-resistant materials, the A-25, the 625 and the C-22.

Next fiscal year, we'll focus on determination of the stifling parameters. We feel that as a function of time, the pits will stifle after modest penetrations, and those pits will not be reinitiated. And as I mentioned, the studies will be performed as a function of temperature, pH, chloride concentration and electrochemical potential.

The Board asked me to talk briefly about the performance confirmation testing program, and this is basically our plan. There are four different bullets. We certainly will continue our off-site laboratory tests at Lawrence Livermore Lab. Both the long-term corrosion test facility and the relative humidity chamber tests, we hope to run those for very long periods of time. We will be conducting an in-situ waste package monitoring program that involves remote sensing and visual examination, and Lake Barrett talked a little bit about that earlier.

We will have witness specimens. We have, as I mentioned, some of those in the large block, single heater and the drift scale heater tests, and we will have some in the repository as well.

The last bullet has to do with examining of dummy waste packages which do not contain radioactive material, would be heated by heaters, and we may do destructive exams if needed, based on what we observe for those particular dummy waste packages.

SAGÜÉS: Excuse me. The witness specimens on corrosion, do you have any right now in any of the alcoves in the ESF?

STAHL: As I said, we have them in the drift scale heater test, and we had some in the single heater tests, which we will be pulling out shortly, as well as the large block test.

SAGÜÉS: I see. Not in any of the other alcoves at this time?

STAHL: No.

SAGÜÉS: Okay. And they haven't pulled out any of those specimens yet?

STAHL: No. They are just beginning to disassemble, I believe, the large block test. Bill, is that correct?

CLARKE: In about another two weeks.

STAHL: So we will be pulling those samples out shortly, as well as from the single heater test.

SAGÜÉS: And also like, what, carbon steel, I presume and--

STAHL: Carbon steel and the corrosion-resistant materials. They do have microbiological materials that are coupled with those samples, so we'll be looking at some of those MIC effects as well.

SAGÜÉS: And how were those placed? Like in holes drilled into the--

STAHL: Depending on the particular experiment; for the large block tests, there are holes that are drilled orthoganally in the system, and we have put in different samples with packers, the various positions. For the single heater test, most of those are outside of the heated area, I think loosely hung basically. Is that correct, Bill?

SAGÜÉS: And is there anything being done from the point of view of testing with possible backfill materials by any chance?

STAHL: Not to my knowledge, but we do have in the drift scale heater test, we do have a section of the test that has different concretes, so we will be looking at the behavior of those materials as a function of time. But I don't believe at this point we have any backfill materials being evaluated.

I was asked also to cover, and I'll do so very briefly, the waste form testing and modelling program. Most of this work is being done by Argonne National Lab and Pacific Northwest National Laboratory. We have several kinds of tests going on. We have oxidation tests. We're looking at our existing spent fuel approved test materials. We're evaluating oxidation rate.

We have some new approved test materials that will be added to those tests shortly. These are high burnup, boiling water reactor fuel materials, and we will hope to receive, we hoped at the end of this year, probably early next year, some high burnup, 60,000 plus megawatt day per ton pressurized water reactor fuel, which we'll add to that test as well.

We have flow through, alteration and unsaturated drip testing that's being done at Argonne both on glass and spent fuel, and we will be adding new ATMs to that as well. For the glass testing, that includes both the Westinghouse, Savannah River glass and the West Valley glass.

An important item there is the colloid tests and the cladding integrity tests with rod segments just starting up. That's going to provide some input on radionuclide release.

BULLEN: Dave, are you doing any characterization of the cladding with respect to oxide thickness, hydrides, hydride orientation, reorientation under stress?

STAHL: Absolutely. We have characterized all of the approved test materials for just those parameters that you mentioned, and we will be characterizing the new spent fuel ATMs as we receive them. Certainly those are critical parameters.

BULLEN: Have those characterizations been incorporated into your modelling of long-term clad performance, or is your clad performance still based on--

STAHL: Hold that question and let me get the next slide. I talk about cladding here.

BULLEN: Okay. I need to read ahead.

STAHL: We also have glass parameter tests underway at Livermore looking at the effects of iron and magnesium. One of the things that we were concerned about early on was whether the presence of iron or magnesium would either add or subtract to the glass dissolution rate, so Bill Borcier is doing some parameter tests there.

Lastly, we have degradation and release models that we've provided to Performance Assessment for TSPA-VA. Models requiring degradation have been provided to permit cladding credit to be assumed for the base case. This is a model that's based mainly on mechanical performance. We're just beginning to look at the chemical performance of Zircaloy in that regard. We just initiated a literature survey on Zircaloy performance. We've had discussions with the Navy as well to try to get as much information as we can from them in the unclassified literature, and we have some long-term localized corrosion tests that we've proposed and hopefully we'll start next year.

Now, let me try to address your question. Do you want to ask it again, please?

BULLEN: Well, actually along the lines of the long-term corrosion tests that have been proposed, I guess the concern that we have is a couple week ago, we saw that there was an extrapolation from C-22 data that said that, you know, the clad was going to be about 100 times better, and so you're using 100 times those types of numbers. I guess the more important data would be taking a look at what's going to actually go into the repository with respect to irradiated or non-virgin fuel, as Dr. Kessler noted, and how that might perform long-term, and how those models might be incorporated.

STAHL: Absolutely. The plan for these particular tests would be to start with unirradiated material, and what we were concerned about, and this came out of the Waste Form Expert Elicitation, was potential localized degradation, and what we would plan to do here would be to have an array of unirradiated clad and then surround it with iron oxide and other corrosion products to evaluate crevice and potential pitting corrosion, and we would hope that we'd also get some information from the previously mentioned fiber optic tests. So there would be hopefully some information that we could add to from those experiments.

The next phase would be then to do similar tests on irradiated material. The former tests would be done at Lawrence Livermore. The latter tests would likely be done at Argonne National Lab where they have already the Zircaloy specimens available.

BULLEN: Any clue as to the timing of those tests and when results might be available for LA, I guess is the lead question?

STAHL: We would hope to have some information for LA.

STREICHER; Do you have any data yet on attack on the glass?

STAHL: Yes, we do. Certainly we have models and glass degradation information that we have provided. We've compared the results that we have generated with those of other international programs, people around the world that have been looking at glass. All the answers certainly are not in. One of the concerns for glass is the fact that glass is a metastable phase and when you do have water attack, you do get different clay phases and silicates formed, which incorporate to some extent some of the radionuclides. And what we're looking at there is basically the potential for a plutonium colloid formation and the stability of those colloids over time. That's part of the tests that I mentioned earlier.

STREICHER: Glass is attacked by just boiling water, borosilicate laboratory glass.

STAHL: Absolutely. Sure.

STREICHER: And at 200 C., it disintegrates in a matter of weeks, so that glass is not very stable when it's in contact with water.

STAHL: Absolutely. But some of the silicate and clay phases that form are fairly resistant. The question is where the radionuclides go. Do they leave the waste package or leave the engineered barrier system? And that's the direction that these tests are taking, is to look at the mobility of those radionuclides once the glass dissolves and reprecipitates.

STREICHER: You're sort of counting on their being retained in the environment?

STAHL: That's basically what we find, and certainly if you'd like more detail, you should talk to John Bates at

Argonne National Labs who does those tests. I can talk to you after the meeting.

In summary and in closing, we've done long-term service condition type tests, short-term aggressive condition tests, and model development. These have provided inputs to design and TSPA-VA and will certainly follow our testing strategy, as I indicated in the diagram early on, to meet TSPA-LA needs.

One of the things I did want to mention is how we document that information. We have developed the models and abstracted models, as David Haught had talked about. We provide those as they are generated, and then we have a formal transmittal process and we have some documentation that that information is published in. We have the Waste Form Characteristics Report for the waste form work, and the Engineered Barrier Characteristics Report for the engineered barrier system work, and the Board should have copies of these documents.

BULLEN: Thank you, David. We'll take a couple of quick questions. Richard Parizek, by the way, I'd like to acknowledge. Dr. Parizek joins us from the far away state of Pennsylvania, and he is here with us at the table now with his first question.

PARIZEK: I was looking at the ceramic coating experiments you're doing. I was wondering whether you're doing anything at all with rock fall type conditions. You talk about handling loads, which I assume is--and it's true about all of these questions, what to do with rock fall is in all testing.

STAHL: Well, as Lake Barrett had mentioned, early on before closure, you have the precast concrete liner that protects the package. Prior to closure, if we had a ceramic coating, and perhaps even if we don't have a ceramic coating, we may backfill, and that backfill would protect the ceramic coating against rockfall. It was not in our design that the ceramic would be thick enough to withstand a rock fall.

BULLEN: Any other questions from the table?

COPPERSMITH: I have a quick question. It relates to the issue of testing versus modelling, long-term behavior. A lot of the testing obviously--and it's too bad Paul Craig left, he'll be back soon--but the issue of taking relatively short-term test data and extrapolating out obviously the long time periods that are needed for performance assessment, many of those models that we saw in the waste package degradation expert elicitation are based on electrochemical theory. They're based on things other than purely observed empirical data and the long-term corrosion test. You didn't talk about that. Is that part of your materials testing program to deal with the nature of penetration laws, pit growth laws?

STAHL: Yes. Let me amplify it. We have several objectives, certainly one in VA is to provide models that we have reasonable confidence in based on semi-empirical models. The objective of course is to have mechanistic models where applicable. So we're doing some of that basic research to look at some of the parameters that would be input into mechanistic models.

The ASTM procedure that I mentioned, 1174, kind of documents and walks you through that process where you can develop mechanistic models where appropriate. If you can't, you have to fall back on semi-empirical ones. But also coupled with that is the use of natural analogs. We will have a summer student starting in the next couple of weeks actually looking at some of the data that's out there on natural analogs, both for the metallic barriers and for the waste forms, to give us a better handle on where we need to go to compare the performance of some of those analogs to the projected performance of our container materials and waste forms.

So we hope that it's an integrated approach based on short-term tests and the natural analogs, and as I mentioned, the performance confirmation program that will continue well past the license application to continue to collect data. So hopefully, we'll have 100 years of data before we're ready to close, which we can compare with the data that we've generated to this point, and the models and the model predictions. So hopefully, we'll have a handle, and I think we have to present that information to the regulatory commission on the basis of the application to close the repository, so that we have a handle on the long-term performance.

MACDONALD: Dave, can I just make a comment?

STAHL: Of course.

MACDONALD: The question is not whether a model is semi-empirical or mechanistic. The question is whether the model is deterministic. A deterministic model is one whose output is constrained by the natural laws, and it seems to me that the only thing we can rely on for sure every 10,000 years is perhaps that the natural laws remain the same.

STAHL: Well, there's no question that we have--

MACDONALD: The question I have is to what extent are you making sure that these models are deterministic, that is, they obey the conservation of charge, energy and so forth?

STAHL: Yes, that is--certainly from my perspective we look at deterministic models. But we have to, as I indicated in the chart, abstract those models for use in total system performance assessment, and as indicated by Joon Lee, not here at this meeting, but in other meetings, that information has to be sampled, various thousands of realizations are done for a variety of different configurations, so there is some probabilistic aspects to determining the dose to the public based on the deterministic performance of any particular barrier.

BULLEN: Any other questions from the table?

(No response.)

BULLEN: Okay, if not, we will adjourn. We'll take a 15 minute break. Oh, was there--Teon, do you want to ask a question?

CRAGNOLINO: I've got a question, please.

BULLEN: Gustavo?

CRAGNOLINO: Gustavo Cragnolino, Center for Nuclear Waste Regulatory Analyses.

Only a few months ago, there was a decision to change a corrosion-resistant material from 625 to C-22. I'm asking now are you going to keep for the viability assessment the wrought carbonate steel, A-516, or if you have in mind possible use of the other alternative material that has been considered here?

STAHL: Currently, the reference design is the A-516 and the Alloy C-22.

BULLEN: Okay, we will adjourn for 15 minutes. We'll reconvene at 11 o'clock. I would remind you to fill out your luncheon order forms before the end of the break.

Thank you.

(Whereupon, a brief recess was taken.)

BULLEN: As you're getting settled, I will take the

Chairman's prerogative and I'm no longer the Chairman any more. I'm going to turn this over to Kevin Coppersmith. An, in fact, tomorrow I don't have to be Chairman, because closing remarks will be by Dr. Alberto Sagüés. So I will visit with you as Chairman and just become a panel participant, which I'm looking forward to.

Thank you. Kevin, it's all yours.

COPPERSMITH: Thank you. There may be still a few people waiting in the lunch line, but hopefully they'll be along with us shortly.

I have a couple of view graphs to just set the stage. I'm supposed to talk about ground rules for the remainder of the workshop, and I will do that. But I want to relate a little story first.

I think one of the things that I've learned over the last 18 months, we've had, for those who don't know, and there are a few in here who aren't grizzled, experience competitors from the Expert Elicitations, five expert elicitations were conducted over the last 18 months. Each of those elicitations was designed to get an uncertainties associated with certain process models feeding into the total system performance assessment.

Two of them were engineered, looked at the engineered system, the waste package degradation and the waste form degradation, radionuclide mobilization elicitation, and three of them dealt with natural systems issues, unsaturated zone flow, near field altered zone coupled effects, the influence of the thermal perturbation on the rock system, the hydrologic system, mechanical and chemical interactions, and finally the saturated zone flow and transport elicitation.

Those elicitations are done to help characterize uncertainty, and part of that process we found in dealing with experts, each of those had about five or six experts on each panel, is that it's very useful to have interaction among the experts. In fact, it's vital that interaction occurs. It's an opportunity to share data. It's an opportunity to challenge each other's ideas, and so on.

What we did in those then is had three workshops in each one of those elicitations. So basically over the last 18 months, there have been 16 workshops that have been conducted for the expert elicitation process, and I got the wonderful advantage of being the facilitator for all of those workshops.

It's kind of I view the process then of being asked to help facilitate this workshop is kind of like frequent flyer mileage. As you accumulate hundreds of thousands of frequent flyer miles, your advantage at the end of it all is you get free trips, you get on a plane and do more travelling. So after 16 workshops over 18 months, I've been awarded the advantage of being able to facilitate another workshop. I look forward to it. I understand that I won't get frequent flyer mileage for doing so, but that's okay. We'll proceed.

I should also make a comment that this is a very unique type of format for the Nuclear Waste Technical Review Board. The first Board meeting that I went to a few years ago was the type that Dan Bullen wouldn't fit into very easily. It was one of very formalized interchange, presentations, questions and so on. We're now at a point we're actually having a brainstorming type of session, and I really look forward to that process.

We won't have the lights down long, because I just want to go through a couple of ground rules, and then we'll have some opportunity for discussion.

I want to go back to the workshop purpose. I think this helps set the stage for what will happen in the remainder of the discussions. The purpose here is to develop a basis for the Nuclear Waste Technical Review Board to review and evaluate the scientific and technical merits of waste package designs and materials research activities.

So as we go through the remainder of the agenda, we'll deal with, first of all, the issue of the environmental scenarios. As all of you know, the environmental conditions that the waste package will need to face over its lifetime are critical to understanding of the corrosion modes, the rates and processes that will occur. That is what will occur first. We'll try to break those into a series of environmental scenarios, and Carl will begin with that discussion, and I'm sure there will be additional discussion on further trying to define those scenarios.

We'll then try to link those to potential designs, waste package designs that could meet or withstand or somehow deal with the environmental scenarios. And obviously there's more than one design that can do that. The present reference design for the viability assessment is one that can, to a certain extent, deal with that, and maybe to a large extent deal with those scenarios, but there may be other designs and design concepts that we'd want to consider. This is an opportunity to brainstorm some of those to, as I say, go outside the box and consider alternative scenarios to deal with the environmental cases.

And then finally, a goal, since we're not going to be able in the next several hours to come up with full design characterization, is to develop research needs, to develop and perhaps relating those to alternative designs, what are some of the research needs, how can that research be conducted. I think it is important to get some idea of the time frame of the research, thousand year corrosion tests may be difficult to carry out, even though Dan McCright says you can do that type of thing. We should look at I think, and we alluded to a little bit earlier, not only just purely empirical types of research needs, but those things that deal with modelling as well. Some of the key, I think, challenges of dealing with long-term waste package behavior is being able to model over long time periods behavior over thousands of years.

And so that I think when we deal with the research needs, it should incorporate not only what could be done in a laboratory or testing environment, but what might be done in modelling as well. So that's the direction we're going to go.

What they've told me in this is what we'd like to get out of the process is the interchange, the pure fact of being able to talk about alternative conceptual designs and research itself, the process is important, and that's part of what we're trying to get at. But also, a list of research needs, what types of things that could be done to evaluate alternative designs, waste package designs. And when we do that, we'll be able to leave tomorrow on time and catch our flights.

The approach we're going to take--by the way, this particular logo is just one that comes up on Power Point. I don't know where it is. It looks like a crevice attacking some poor alloy, and I won't say what that alloy is. I'll let the panel decide.

Basically, this is a facilitated brainstorming approach to the workshop. This is what I was told would be the case. So I've looked at those words. Facilitated to me means that there's going to be some focus and direction that will be defined as we go through various components. We're now dealing with environmental scenarios, for example, and let's try to keep the discussion on the environmental scenarios, and then we'll get into designs that can handle the scenarios, and so on. So there's some focus and some direction.

We do need to make progress. We need to keep moving along, and that again is part of a facilitated process. We try to keep the verbal assaults to a minimum; physical assaults to even a lower level.

I'm going to ask, these are five elicitations, but two other very large elicitations were carried out for volcanism and for seismic hazard for purposes of design and licensing, and in the volcanism, I think the volcanism takes the cake for the most contentious group of people I've ever seen. We had to almost physically restrain individuals from hitting each other. Everything since, corrosion scientists are verbal but not necessarily physical in their assault.

Brainstorming to me means a free flowing discussion which is, again, an opportunity for individuals of the table to ask questions of each other, to put forth ideas that in fact are half baked, maybe half backed by data as well.

I noticed this at about 30,000 feet last night they decided I would lead it. There's also a certain amount of trial and error that people should feel is okay, that they can put some ideas out. You know, let's go with a 20 centimeter thick ceramic coating with a half twist, and then someone else will say but how are you going to carry that, how will that--what sort of dynamic loads with that handle, and so on. I think it's possible to lay out some ideas here that may be very preliminary, without fear that in fact this will later on be claimed to be an idea that isn't so great.

Another one that I think that's important that I mentioned earlier is that in fact some of the testing ideas and some of the research needs that are laid out in this discussion I think again can be areas that in fact the Department of Energy or other groups are already conducting. I don't think we need to do a detailed mapping.

When someone says, well, let's throw this alloy into a solution of pH 2 1/2 with this type of mix and solution, and then Dave or Dan or someone else says we're already doing that, or we're doing it at 90 degrees, and so on, I think right now, and in fact it may be later on after someone looks through in detail the types of suggestions that are made, that in fact much of what's suggested is going on one way or another. I think we don't need to go through that sort of accounting at this time. This is not a report card on DOE or any other group. This is an opportunity to brainstorm ideas that relate to a potential design, potential research.

If that research is going on, great. If it isn't, someone needs to evaluate whether or not it makes sense to go ahead and carry that research out.

Some of the ground rules: anyone who's gone to the elicitation workshops knows that we have a table like this and we limit the discussion to the people that are around that table, and that's the focus that we're going to have at this meeting. There may be opportunities at certain--there will be opportunities at certain points for the public to comment, and those are in the agenda and we'll make sure that we honor those. That's both today and tomorrow.

There may be some other cases where we need some information on particular issues, and we may call on someone in the audience like Dave or Dan or others to help us with that if there are certain parameters or certain information that would help us in the discussions.

There's a thing that we call a "Parking Lot" that Bill Barnard is going to handle, which is an opportunity--one of the things that happens in waste package degradation is that in fact there are a number of issues that are someone tangential to our central issue of waste package performance. They usually relate to environmental conditions. People like to get into a lot of the what ifs about different thermal loads, the interaction with the chemical environment that the waste package might see, and a number of other issues that may become tangential. Those we'd like to take and put into a "Parking Lot" and defer those. If we have time, we can discuss them in more detail later. But it's an opportunity to maintain some focus.

I think also in terms of the waste form and some of the issues related to the degradation process, radionuclide transport in and out of the package, those areas may be ones that end up more in the "Parking Lot" than the focus of the discussion here. We really are trying to deal with the waste package itself.

Obviously, there are a number of issues that influence waste package design, including its internal and external environment. But if we get to where some of those issues, we're bogging down, I'm going to take the prerogative and have Bill put those in the "Parking Lot."

Anyway, when I think of a parking lot, for anyone who's done QA, when your QA guy comes in to audit you, he says, well, all of your records survived the parking lot test, which means you can take all of your records, drop them in a box in the parking lot, and you could put them all back together, everything is in its proper order.

We had a small fire in our office the other day. The sprinkler system went off. 20 offices got water in them. And about ten of those offices, after the fire fighters had been through, had an opportunity to exercise the "Parking Lot" test on all of their records.

As we go through talking about the environmental scenarios and then developing alternative design concepts, they will be developing kind of an alternative list, a waste package alternative list. Carl will be keeping that list as we go through. He's an excellent scribe, I understand, speaks the language well, and we'll ultimately be using that list to link to research needs and research activities. So we'll try at certain points to get maybe a clarification from Carl, what are you hearing, what does your list look like, as we go through.

It is brainstorming, so that means that it's a relative free flow, but you need to talk one at a time. One of the major advantages of this meeting over earlier TRB meetings I think is that we don't have to identify ourselves beforehand. That's amazing. I don't know how many times I've heard "Bullen, Board, Bullen, Board." So we don't have to do that here. But if two people talk at once, it's impossible for the recorders to be able to tell what's going on, and if five people talk at once, I have a button over here I understand that will shut off everyone's mikes. I don't want to use it, but I will if I have to. I had a number of teachers that used to say that.

We will adjourn promptly at 3:30 tomorrow. There's a potential in fact that we may try to adjourn a little earlier than that, depending on people--right now, I'm hearing an estimated--or probability distribution around the estimated time of arrival at Dulles about the 92nd percentile that has people missing 5 o'clock flights. So we'll have to see how probabilistic we want to be on that issue. Deterministically, we'll all make it. John and I were on a three hour delayed flight last night, so we'll definitely make it.

A few things. I went through some of the materials that the Board gave to me to sort of talk about this workshop, trying to define the scope of it, and there are some things that are clearly in the scope, some things that are sort of outside of the scope, and I wanted to just sort of share those. This may change somewhat as we go through. It's a little bit fluid. But just to give you a feel for the items that we say are sort of they're in the scope, you just sort of frequently ask questions about this workshop, things like drip shields and waste package fillers, and so on, that may seem to be pretty tangential might be important in fact in characterizing the environment, and in fact important to the waste package design. So I think we will include those. I don't think we want to spend a lot of time dealing with the details of a Richard's barrier, or some of the dynamics of how a drip shield might work, but some of those ideas I think can be incorporated, other than metallic materials is definitely in the scope of work, various types of, you know, thousand year plastics, and so on, I think would be in there.

Any package size or capacity, as long as it basically can handle a PWR assembly I think would--sort of a minimum size, but I think we need to think about the fact that perhaps some of the dimensions can be different from the current dimensions for the reference case. The possibility of some mechanical processing before putting it into the waste package, I don't think we want to get into all the big issues of chemical processing that might go on, but potentially some of those issues might be important in a certain design if it requires a good deal of processing that's different than anticipated, that might be something that needs to be considered.

We're looking at, when it comes down to research suggestions, we certainly can deal with the reference design; that's a possible area where, as an alternative, we could be talking about research needs, whether or not those--what those research needs would be.

And finally, when it comes to waste package closure, the issue of welding and non-destructive examination, and so on, are issues that should be considered when evaluating the potential design.

What's not in the scope or what's sort of oblique to the scope, I think getting into a lot of the issues that deal with the near field environment, the issues of pre and post-closure ventilation, with the exception of what Carl will talk about, sort of dictates the temperature regime. I think we should just assume that if we have a ventilated system, the sort of temperature profiles that Carl will show are the profiles that would exist. Let's not get into alternative ventilation schemes.

One of the things that we really see in this is that because many of the corrosion scientists deal very specifically with a set of conditions, or a set of pH and temperature and oxygen, partial pressures, et cetera, et cetera, that we want to spend a lot of time defining those, and once those environmental scenarios are set, then you can go forward with the other parts of the--dealing with the waste package, and we will go through that. But I think we don't want to bog down on that aspect where we spend hours and hours trying to further define the environmental conditions. And the waste package degradation expert elicitation in fact, I have the world's longest set of e-mails, sequence of e-mails spanning about two or three weeks when that panel dealt with the issue of trying to define discrete environmental scenarios that they could use. And they finally arrived at that, but it's an area that is potentially very difficult to get your arms around. So we're going to try to do it in a more general way as we go through here.

Some of the other issues that deal with normal loading, let's give--Carl will give a set of conditions and we'll assume that those exist as opposed to developing line-loading configurations, distances between packages, and so on. So that's one of the oblique things that are out of scope. When we get into some of the other types of waste forms and other areas like that, I think it simply does not--it's not the majority of what's in the inventory, and there are hundreds of other types of waste forms that we could spend time on. So we really want to deal primarily with commercial spent fuel, high level waste glass.

Okay, what we're going to do now is Carl Di Bella is going to go through the environmental scenarios that should be considered first, and then we'll talk about some of the alternative conceptual designs that might meet those. So, Carl, you're next.

DI BELLA: Thanks a lot, Kevin. This is going to be a very short talk on what I'm calling Tunnel Environment Scenarios, and it's only necessary so that we all agree that we're working on the same problems here at the table. And if we don't agree with what I say, it doesn't make any difference, let's agree on something so that we can get on, and we'll have the service and services that we need to specify for the packages defined.

I'm going to talk about these scenarios, first talking about temperature versus time, then the composition of the water and other environmental chemistry aspects, and then talk a little bit about criteria that we should be looking at to admit an alternative into the set of alternatives that we're going to come out of from this session, and then I'm going to give some of the specific criteria that the program is using right now. And very fortunately Dave Haught's talk particularly, and Dave Stahl's also, have already given some of this information, so I'll be able to go through it pretty quickly.

I'm going to leave this slide up. I have here three different temperature versus time scenarios, and I want to point out this is a logarithmic scale on the bottom, so that's a little bit misleading, and I'll be explaining each of these scenarios. What I'm plotting here is the highest temperature on a waste package surface which happens to be in the current configuration underneath the waste package, as it reflects the heat and exchanges it back and forth with the bottom of the invert.

Let's look at the red line here just for a moment. This is derived from the current design for a high burnup, young fuel, 21 PWR waste package. You'll notice the temperature peaks at about 50 years at close to 300 degrees Centigrade, and then slowly declines, passing this line, which is the boiling point of pure water, way out here at the 5000 or so time frame. This is about the hottest package that you could get under this current thermal loading scenario, and it's located in the center of the repository.

The actual curves are going to be unique for each package, and they're going to fall here or below it, depending on the thermal power of that package, the thermal power of that package's neighbors, and whether the package is located in the center of the repository or out close to the edge of the repository. The numbers themselves aren't all that important. What is important to look at is the basic pattern that is followed.

Now, two other scenarios, and I've called this Scenario 1. This is the current design; Scenario 1. Scenario 2 is a much lower thermal loading kind of scenario. You'll notice the peak is much lower, occurs earlier, and the waste package crosses the boiling point line well before 1000 years in this particular case. And Scenario 3 is a ventilated scenario, where the waste package temperature is just slightly above boiling for not long at all, and then it falls below boiling at 100 years and we turn off the ventilation. We could just as well, particularly have hearing Lake's talk, say we'll turn off the ventilation at 300 years, and you would expect this would extrapolate a little bit lower here, and the peak would be even lower.

But notice nevertheless that still after the ventilation is turned off, this thermal curve still crosses boiling at relatively near to the point where the unventilated design is.

These are the three different scenarios that we will use. We'll assume in the period say for the read line while the temperatures are above boiling, that we won't have any refluxing in that period of time, that we will have an environment that is relatively benign from a corrosion point of view, and this will be true for the second thermal loading. There's relatively little corrosion that takes effect in this period, relatively little in this period, too.

However, after we cross this line, or somewhat before it, we begin having dripping on waste packages, and we'll assume that the waste package that we're going to talk about is one of those that gets dripped on. Some will be dripped on, some won't be dripped on, and the percentages will probably vary, but we'll assume in all cases that we're talking about a waste package that is dripped on. I'll leave that up.

PAYER: Carl, what's the basis for these calculations?

DI BELLA: These calculations were derived from--these two scenarios were derived from a recent EPRI report on the effects of thermal loading and ventilation. This was derived from a Yucca Mountain Systems Engineering Thermal Study published in 1996 about the effect of high and low thermal loading.

BULLEN: And these are all for 21 PWR waste packages, large packages?

DI BELLA: Yes, they are.

KESSLER: EPRI modelling is a little on the conservative side, so I'd say we over estimated temperatures a bit certainly for the red curve.

DI BELLA: Yes, for the red curve, that's true. And that's why I say all waste packages are going to fall here or below this curve, depending on their location, and so forth.

PAYER: How much below that red curve would the top of that package be? Does the whole thing shift to the left?

DI BELLA: Not much, 10, 20 degrees for the current waste package. But remember, we may not be talking about the current waste package.

PAYER: Well, the issue I think when we say conservative or non-conservative, it's conservative by determining a lower temperature perhaps. It's non-conservative by saying it's going to get wet after a longer period of time. Is that correct?

KESSLER: Yes.

DI BELLA: Yes, all packages, or almost all packages are going to get wet sooner than this, not longer.

KESSLER; Right. The other thing, Carl, that may be relevant and may belong in the "Parking Lot," I'm not sure which, is that this is basically you're assuming a package in the center of an infinitely wide repository, so there's no edge effects here at all, and we didn't really assume any effect of infiltration to lower temperatures or anything like that. So there may be a distribution of temperatures, all of which would be lower, implying that you're dropping below boiling could occur much earlier than that red curve, yes.

MACDONALD: The assumption that corrosion is not an issue until you drop below the horizontal line, is that based upon experimental evidence?

DI BELLA: No, it's not a good assumption. I shouldn't have stated it that way. Just assume that the environment doesn't have any--that there is no liquid water present under these conditions right here. The temperatures are what they are. And then you all can decide whether corrosion is significant in that period.

PAYER: Just to reiterate the ground rules, the game we are here to play today is at the end of your session, we're going to have these scenarios defined and those scenarios will be what we discuss. Those scenarios are not necessarily what--that we haven't reached a consensus that those are in fact the relevant scenarios. We hope there's a linkage between them, but--

DI BELLA: Exactly. So if we were to spend the time to come up with a consensus, I think at 3:30 tomorrow, we would still be defining scenarios.

PAYER: I understand. I just want to make sure it's really emphasized. Because this will drive an awful lot of the conclusions that come out of it.

DI BELLA: That's correct. And this is important that by the time I sit down, that we agree with these, add to them, modify them, but that we then are able to move forward.

Okay, some other assumptions that go with this then on these scenarios, first of all, assume that the pressure in the drift is atmospheric. Most people don't say this, but you do have to make that assumption. Also assume that the groundwater in the Yucca Mountain area has this kind of concentration. We talked about this earlier. It's essentially a near neutral sodium bicarbonate kind of water. There is some chloride in it. There's a little bit of iron in it. Now, that's the groundwater in the vicinity of Yucca Mountain.

I'm also asking you to make the assumption that this groundwater isn't very different than the water that's in the pores at Yucca Mountain. However, that's going to be drastically affected by the temperatures. The thermal, the heat from the waste package is going to drive initially the water out of the pores. It's going to evaporate it all. It's going to go higher to the edge of the boiling front where it will condense, and eventually, when temperatures get below boiling, that water is going to come back and it may come back through fractures so fast that it hasn't come to equilibrium with the salts that are there, and be essentially pure condensate. Or it may come back and be highly concentrated.

In any case, that's where I'm going to stop. But the next step is, of course, it can concentrate when it hits the hot waste package. But we're going to start with this water.

Concrete may increase the pH of the dripping water, but I would prefer for the sake of this group that we just ignore the possible effect of pH of concrete. Assume that it can be overcome. This is simply to save time.

Other parts of the environment during the high thermal loading--during the high temperature period, the above boiling period, the Mol fraction of oxygen will range from zero to 20 per cent. The Mol fraction of water will range from near zero relative humidity to one.

Assume that at least for this waste package, that the tunnel, when we get below boiling and dripping has happened, that the tunnel has collapsed and that there is some rubble on the waste package.

MACDONALD: How good is the assumption of atmospheric pressure for local areas on the canister surface?

DI BELLA: That's just why I gave that last thing about the rubble. The bulk pressure in the drift with rubble in it is atmospheric. What the local pressure might be under a piece of rubble that may have had some salts dripping in there and so forth, I don't know.

MACDONALD: Has anyone tried to estimate what that might be?

DI BELLA: David, do you know, David Stahl?

STAHL: No.

DI BELLA: All right. Each of you have a paper copy of this so you can refer to the water composition if it's necessary later on.

Now, what should we be looking for in alternatives? Well, they've got to be better in some aspect or another than what we have now, and we don't have to justify why we think they're better in a quantitative sense, but here's some possible reasons. It could give better performance. That is, it could fail later, thereby reducing the eventual dose to the public, or it could, after it fails, release radionuclides more slowly, or maybe there's even another way of contributing to performance.

It might be lower cost. It could be that it's easier to determine what's going on during the preclosure period, it's more inspectable, or after welding, it can be inspected, so we're more confident that what we're putting in there is correct. It could just be a more flexible design, able to handle different kind of environments, for example, or it could be something that's easier to explain. Any of these are possibilities, and maybe you can think of some more.

And here are some other criteria. These are actually criteria from the project's control design assumption document that I have paraphrased. One criteria is no significant radiation-induced corrosion on the outside of the waste package. Another criteria is that there are multiple barriers. Another one is fuel cladding temperature is limited to 350 degrees Centigrade, and this is based on preventing creep rupture damage to the fuel cladding, which is actually a time/temperature kind of function, but the 350 catches it with those kind of profiles. And another one is to limit emplacement drift wall temperature to 200 degrees Centigrade.

These are all criteria just to be aware of. There's no reason that this group has to honor these criteria particularly, but if a waste package alternative comes up that doesn't meet them, at least we should be prepared to discuss that.

SHOESMITH: Carl, the last one of those, the limit on the drift wall, looks like an absolute criteria that would be difficult to breach. That's the performance the mountain relies on. All the others look negotiable. But is that one really a negotiable one, that last one, that drift wall limitation?

DI BELLA: I believe that criteria originally was set by the project based on thermal mechanical kind of considerations as to the stability of the tunnel.

SHOESMITH: Is it an absolute criteria?

DI BELLA: It is their criteria right now. It is not an absolute criteria.

KESSLER: I think that origins words is stand in for a temperature gradient in the rock, which is really what causes your rock stresses. And it was just a convenient measuring point, but it's really the gradient that causes the rock stress. So if you can come up with a design, I would say that perhaps might slightly exceed the 200, but has a very gentle thermal gradient, perhaps that would be something that would be worthy of more exploration.

PAYER: Wouldn't it also be the case if you consider a design that was backfilled early on, that that would not be a necessity?

DI BELLA: Maybe the cladding temperature wouldn't be one, but the backfill would probably protect the emplacement drift wall temperature.

BULLEN: But the comment is that these are not hard and fast with respect to discussion at this table?

DI BELLA: No, I want you to be aware of them. I don't want them to be limiting, but when we come up with a new idea, I want you to keep these in mind. They're not hard and fast, not a one of them.

Does anyone want to add environmental criteria or adjust them from what I've said, our scenarios?

PAYER: Well, two of the properties of the environment maybe we'll get into soon, but you didn't mention anything about pH of the environment or the oxidizing potential of the environment, and those are tantamount.

DI BELLA: Yes. That's correct. The expert elicitation panel--no, that's not right. The environment is essentially always oxidizing, not strongly oxidizing. The pH is near neutral. I'll let you decide what that is.

Now, that's the environment that the waste package sees. That's not the result of the interaction of the waste package with the environment. The interaction of the waste package with the environment is something that will depend on what the materials are that the waste package is made out of, or at least partially depend on that. I think that this group needs to discuss that.

MACDONALD: If I could just follow up on this thing that concerns me? A number of years ago, many years ago, Potter and Mann, and then later on, we looked at the same problem, just known as the fast growth of magnetite on carbon steel, and actually it's manifest by attack in the crevices and pressurized water reactive steam generators, but essentially the scenario goes like this; that if you have a sufficiently concentrated chloride solution--getting back to your comment, Mike, of concentration of the electrolytes--and sufficiently low pH, which you can get from the hydrolysis of cations from the concentrated solutions, and a sufficiently oxidizing environment, which you can get from the presence of oxygen, then you can obtain corrosion rates of magnetite--I'm sorry--of ion for magnetite that are in the centimeters to meters per year rate, extremely fast. And that's always been an issue that I've had with the use of carbon steel in these systems, that people in my opinion have not looked at the way out scenario, and it's not so way out after you've studied it for a while.

So I guess a question I've got for the people who carry out this work, principally Lawrence Livermore, is whether they have actually looked at the fast growth of magnetite on carbon steel. And I guess I should be talking to Dan.

MC CRIGHT: Well, I guess the answer to that is no, we haven't looked at the fast growth per se. And, again, I think partly is to take account of all the ionic species that are in the J-13, because I think we have some inhibiting pH buffering, corrosion inhibiting, all different, we have a mix of ions and some aggressive ones, and it's almost a balance of how that ionic soup plays out that really matters. But, again, our experiments to date have not suggested that there's--the only way I can see that really where we've had a high, exceedingly high amount of corrosion on carbon steel is where we've had a lot of carbon dioxide in the vapor phase above because of bicarbonate decomposition to form CO2. But in the water itself, because of all the salts that are there, we get such a nice buffering scaling effect on the carbon steel, plus we've reduced our oxygen solubility substantially, so we've normally seen that most mechanisms seem to drive toward lower corrosion rates than higher.

MACDONALD: This will be looked at--

MC CRIGHT: That's right. That's right. But like I said, these are just the results we have to date, and we certainly haven't reviewed the full spectrum of possible chemistries.

STREICHER: You have eliminated radiation-induced corroding, in other words, formation of hydrogen peroxide or nitric acid, but you have stated that nevertheless, it will be an oxidizing environment. That means iron, corrosion products from iron will go to the ferric state, and you will have loaded up the environment with evaporated J-13 water, which has chloride in it. So there will be chloride salts there, and there will be ferric ions there, and the natural pH of ferric chloride is 2. In other words, you've going to drive this in the acid direction, and the one thing that this wonderful C-22 can't take is ferric chloride at 100 or higher Centigrade because there will be pitting.

DI BELLA: That's correct.

SHOESMITH: Excuse me. Could I just comment on that, Carl?

DI BELLA: Yes.

SHOESMITH: That scenario is in the present model, the possibility of pulling the pH down to less than 3 by pushing the ferric ion concentration up, that came out of the expert elicitation process. So it is in the model scenario.

DI BELLA: That's exactly the kind of reasoning that we are supposed to be using here to come up with waste package alternatives, precisely. I hope I haven't eliminated it.

STREICHER: Well, the next step then is to put the steel inside and the C-22 on the outside.

DI BELLA: Well, you're not supposed to do it during my talk.

COPPERSMITH: We can do it, but we'll do it in a minute.

One of the things that is very important here is we're following exactly the same scheme as the expert elicitation. We're dealing first with bulk environmental conditions, and these are the types that we're talking about here. Some of these are generalized, some of them represent the center of the repository, and so on. Certainly dripping J-13 is a generalized condition.

But then we rapidly go down to localized conditions, and not localized in the sense that the pH within--that's highly localized in very aggressive conditions. But the potential for more localized types of conditions that might, for example, lead to the presence of ferric ion, the development of ferric chloride and some of the low pH that goes along with that, that type of scenario was one that involves drips, the types of assumptions that we're talking about here, drips, evaporation, all of them, maybe occluded sites within corrosion products that would allow for that type of condition to exist in a localized state. And so I think that it is important that those be included.

They may not fall out well in this, but I think in the discussion we should have here, and I'll let you get down to it in a minute, we should go on I think and try to talk about potential for localized environments, like the development of the lower pH type system, that could happen maybe under specialized conditions of dripping and heat and evaporation and so on, but nevertheless should be contended with in thinking of alternative designs.

The overall bulk environmental condition may be relative--but we do need to think of some of the more localized environments as well.

PAYER: Just thinking out loud here, I'm not sure that the three scenarios are going to be in any significant way from a technical standpoint corrosion different. What's different, it seems to me, is the time at which you reach the wet period. And so if we eliminate how long it will be hot and dry, I think we're talking about three of the same scenarios. The same processes are going to kick in. There might be a difference in the level of radiation, the radiolysis when those kick in, but I'm wondering if those are the right three scenarios, or if the three scenarios are in fact going to be different.

DI BELLA: That's a good question. Let me point out one thing that's very difficult to see on this diagram because of the fact that it's exponential in the X axis. The amount of time that the lower thermal loading scenario stays above 50 degree Centigrade, or above 60, between 60 and 100, is lower, and if you assume that that's going to be your time of most aggressive corrosion, you have a shorter period of aggressive corrosion. So that's a difference, but it's a second order kind of effect, I agree with you. And this is the time if we wanted to find another kind of scenario to do so.

PAYER: Maybe that's the point. If the red curve, rather than the loading on it, but if the red curve was a hot package and the green curve was a cooler package and the blue curve was a yet even cooler, then it doesn't necessarily have to be how we start out with them, but I mean the blue one could be on the edge isolated, and the red one in the center and the hottest, maybe that's the more important twist to put on the scenarios.

BULLEN: But I have a question for you, Joe. How would you change the design of the family of packages if you had that sort of distribution? Wouldn't you still design for the worst case?

PAYER: Would I? Yes.

BULLEN: I mean, all material scientists would do exactly that; right?

PAYER: Well, I'm not sure that two or three scenarios here is going to get us any different. Maybe we'll work with scenario one and we'll just say, well, that's the same as two and that's the same as three; it's just going to happen quicker. If that's done, maybe we're done tomorrow morning at 10 o'clock.

DI BELLA: Well, I doubt if that's going to happen.

SHOESMITH: There are specific differences on those three scenarios. For instance, just on the issue of the top bullet on the left, the radiation-induces corrosion, this is not a factor for the red and the green, but sufficient humidity around for the blue dashed line that you would potentially have 100 years of radiolytically induced corrosion for that particular scenario. So it would be a factor. So I think there is a discrimination based on those scenarios.

BULLEN: Unless you self-shielded the packages, which drives--

SHOESMITH: Oh, you've got some trick up your sleeve?

BULLEN: That's the whole purpose of this workshop, is if you had that kind of scenario, then you probably wouldn't want radiolysis, so you'd have to worry about other issues.

SHOESMITH: Correct, yes.

MACDONALD: This is a question. How important is cost in this whole scenario?

DI BELLA: We are technical people sitting around the table, not policy people. So I think you just have to consider it from a relative point of view. I don't know the answer to the question. It's obviously important, but how important, I can't tell you.

BULLEN: No gold.

SHOESMITH: But any respectable engineering process has to involve the cost as a significant factor.

KESSLER: I just want to caveat once again the curves that you borrowed from the EPRI report. That report and those calculations were done for a very different purpose than you're now trying to apply them for. We model this as conduction only when we know that there's hydrothermal that's going on. I do know that we've never pushed any of our analyses past 1000 years, which was already pushing the envelope, given that they were conduction only. And then I would say that the red curve is an over estimate of reality. We did it to say are there--we were trying to identify what some set of packages might get us into trouble from a loading standpoint. Now you're applying that to--using it for a base case for container corrosion, and I'm very nervous.

BULLEN: Would you concede that it would never get any worse than that?

KESSLER: Yes.

BULLEN: That's all we wanted to bound.

COPPERSMITH: And we didn't have the Department of Energy discuss all the calculation they've done. But, you know, basically there's a whole family of calculations.

KESSLER: Yeah. I think it's not that I'm--

COPPERSMITH: This is just sort of the upper end of all of it.

KESSLER: Right. I guess I'm not so concerned about the 300 C. sitting out there, but I think it's this idea that if we're worried about when they come down and what the radiation is when you go back to getting oxygen in the system and when you start getting a significant amount of humidity in the system versus time, that may govern our thinking.

COPPERSMITH: There are some issues I think we need to talk about, because a lot of this comes out of the thermal hydrologic modelling that's going on. I mean, this is a waste package temperature profile on the surface, but obviously it comes from modelling that's done in the rock, and then assumptions about the types of conductive radiation, or other types of heating, thermal conduction that goes on within the drift.

But basically, I think that picture is one that's just representative of the overall types of temperature/time histories, an inverse relationship with relative humidity profile that's basically a very low relatively humidity that climbs, just you know a compliment to this. And, again, those are generalized types of profiles as well, but I think it's important for people to realize that in fact the rock and its matrix has got a lot of water in it, and once we do go below boiling, the relative humidity of the air in the drift will be very high, in the 90, 95 per cent range. So that's basically the general trends.

I guess the issue would be whether or not there are significant differences from that type of heating scenario, and something that in fact just hovers around the boiling point. I think that's the difference that Carl is trying to make here. Now, whether or not those are different from a corrosion standpoint, we really should--I want to have environmental scenarios that are distinct enough that you can actually say okay, here's one scenario, a package of temperature, relative humidity, pH, what have you, that is distinct enough from Environment B that you could deal with a different design concept. And it might be that in fact if all of those environments are possible, you roll them all up and say okay, let's have a design that handles them all, but for this exercise, I think we're better off if we can try to define what's really distinct.

Do you agree with the fact that in fact Scenario 3 is different from 1 and 2? Those can be kept separate?

Okay, one other point I wanted to make on oxygen partial pressures and that whole issue of whether or not there's oxygen sparging early on and so on, I've been following that from the near field coupled effects standpoint a little bit, and in fact that's still an area of very large uncertainty. There's adequate evidence that the mountain is--air permeability is very high. The mountain breathes very well, and it's not a closed system. So I think it's probably the general thinking at this point is that there will be plenty of available oxygen throughout the entire temperature/time history.

BARNARD: Carl, on Scenario 3, what's the relative humidity? You're actually ventilating those tunnels, right, with outside air?

DI BELLA: Yeah, but the temperature of the air probably--actually, John probably knows this better than I do, but it probably stays below 50 degrees Centigrade, so it's just slightly lower than whatever the ambient relative humidity is there at Yucca Mountain, which is probably on the order of 30 or 40 per cent.

COPPERSMITH: Well, should we assume then for Scenario 3 that it would have those types of relative humidities during the period that it's ventilated?

DI BELLA: Yes, but for radiolysis, I think it would depend on the concentration of the water.

COPPERSMITH: Right.

PAYER: But that would be consistent with the scenario, wouldn't it, that you'd have a lower relative humidity and those temperatures for 100 years, and then when you turn the ventilation off, would you just assume the relative humidity comes back? People have talked about how much are you going to dry out the mountain, but I don't think we can deal with that. Let's just say when they turn off the ventilation, it then goes up to saturated relative humidity and temperature goes where it does. I think if we do that, we've got three different scenarios. I guess I've gone full circle now.

DI BELLA: I was going to say does anybody want to add a fourth? If not, I'd like to sit down.

COPPERSMITH: Well, we may get to that. You can go ahead and sit down.

I want to, at the risk of imposing too much of what DOE has already done, I do want to show a little figure that's a logic diagram, logic sequence for the waste package model that DOE has, and I want to show that primarily from the standpoint of the environments, because the issue came up of concentrated solutions, of high chloride concentration, where that would occur and how that would occur, and I don't want to dwell on this.

BULLEN: Kevin, can you put a mike on for transcribing, please? Thank you. Bullen, Board.

COPPERSMITH: This diagram basically shows a combination of a bunch of things, but the thing I want to focus on are some of the environments that are considered, and a key issue here is the waste package temperature, which we're talking about, and alternative waste package temperature/time histories that could be considered. And there's near field models that bring in pH and chloride and other things, and oxygen partial pressures, those are all considered, the thermal perturbation, and they feed into this overall model as well as the other modelling that's done. But a key consideration is whether or not there are drips.

As Carl talked about here, we should be assuming that in fact drips are occurring in these scenarios. You can see as you trace down the no drip, then you have to deal with issues of relative humidity, alternative corrosion mechanisms for the corrosion allowance material, and on down, but you don't develop some of the concentrated solutions and more aggressive conditions in the no dripping scenario.

Giving the dripping scenario, we have a potential for elevated pH, as we talked about. Here, I think we're not going to deal with that issue, but it is important potentially of having elevated pH, alkaline conditions as water goes through the concrete. Obviously then the concrete at some point, debatably hundreds of years, will be gone, and those elevated conditions presumably will also be done. But that can have an effect with elevated pHs, of change in the corrosion mode, high pitting may occur within the carbon steel, and so on. But there are different--this is more of a localized corrosion process, the general corrosion process otherwise.

So these modes are driven by these environments up here, and these are uncertain environments, and we're talking about what we should be assuming for this analysis. When we go through the drip scenario and take it down into where we actually penetrate the carbon steel, this is where we have the opportunity for more aggressive conditions, pHs that go down, the potential presence of ferric ion and so on. That's kind of the logic sequence that gets us to that, and I think that what we're talking about here is including that scenario, forgetting its probability, which may be very low, but including this scenario in our discussions, and allowing for the potential for evaporation concentration to occur, that would then lead to either general corrosion of the CRM or localized corrosion model for the CRM. Again, that's highly dependent, but it's a type of condition that can exist.

BULLEN: It also assumes that we have a CRM on the inside and a CAM on the outside.

COPPERSMITH: Exactly.

BULLEN: We can change on that in a pen stroke by saying well, this is the same condition, and so you get to the diamonds right above there, drips, no drips, RH and pH, and then we have to say how does that change with respect to the design.

COPPERSMITH: That's right. Absolutely.

BULLEN: Now, before we dive into what you were going to do, my staff and the wonderful people in the restaurant tell me that everybody put noon for their lunches, and it is now ten after, and so the lunches are ready. So if you want to break now and do it exactly one hour and then get back into the scenarios, I'm not concerned any more--

COPPERSMITH: It seems like a wonderful idea.

BULLEN: I'm just telling you that the lunches are ready, so if you want to go pick them up, we can go.

COPPERSMITH: So moved.

BULLEN: One hour.

(Whereupon, the lunch recess was taken.)

AFTERNOON SESSION

COPPERSMITH: Are we ready to reconvene? We've been going through the environmental scenarios, and if I can sort of hazard a guess at a consensus, it might be the one that Joe Payer voiced, we will consider those three temperature and time histories at the present time to be separate scenarios. I think there may be some linkages between the scenario, the temperature scenarios, temperature time/histories, if you will, and other environmental conditions.

We talked about the fact that the low temperature scenario may be one that we have other considerations that we need to think about. But I think right now, there's enough independence between let's say the chemical conditions that we would assume and these temperature/time histories that we can push forward.

For example, we might consider in all cases the potential for drips to evaporate on the waste package surface. All of those are sufficiently warm, particularly the waste package relative to the drift wall that evaporation may be considered to be a possibility. Maybe it's more likely in one scenario than any other, but that might be a consideration in the case of all three.

You might also consider in the case where any of those temperature profiles go below essentially the boiling point, that we have, as we move out in time, high relative humidities. And, again, their timing may be different, but the effect on things like chemistry, relative humidity are essentially the same. So we may not need to deal with, and maybe we can proceed with talking about some alternative designs, we may not need to deal with a strong set of specific scenarios for each one of those temperature/time histories. If we do, we'll return to those. We can always go back to that. That's the advantage of brainstorming. We can come back. Okay?

Are there any other questions on the environmental scenarios that should be considered in this process? You can keep that tentatively there's a possibility of revisiting those if we need to, but I just want to make it at least clear enough right now that we can think about types of designs that might be consistent with those scenarios.

KESSLER: Kevin, I can't remember what we decided about rock fall, whether we're just going to consider it occurring at any time?

COPPERSMITH: Well, I think we heard arguments this morning and we heard them before that in the first few hundred years, it looks like the--well, certainly in the first hundred years, there's a period of retrievability and the concrete liner will be the mechanism for basically stopping any rock falls. I mean, that's the system that should work. Whether or not it will last, I always hear the same thing, on the order of a few hundred years is the period where that type of liner system should essentially mitigate against instability. Beyond that period, then I think we should think about potential for rock fall. So I think here that we should, we should incorporate that concept.

Obviously, there's a whole series of issues on the size and distribution of rocks that would fall, and so on, but let's say that, yes, we should be thinking about mechanical integrity over a time period.

Are there other questions related to that, to the environment? I was talking with some people at lunch. It's an area of a lot of very active research right now, trying to look at the potential for refluxing, when the refluxing and the condensate, whether or not it would pass around the drifts, whether or not it will go back into the drifts, that whole area is an area of a lot of modelling and research right at the present time.

Okay, so let's talk about the second step, which is potential alternative designs that we might want to think about, and then see how they might work relative to the environment, and then relative to some of the other criteria that we talked about. Does anyone have any ideas? Come on, there's got to be a--Joe, what's your design? What do you think might work, and then we'll let everyone else tell you why it won't.

PAYER: Titanium on C-22.

BULLEN: Do you want to take notes at the board there, Carl? Can you put that on the flip chart?

COPPERSMITH: If you can write on the flip chart, so after I forget what Joe says, in five minutes, I won't say it again.

DI BELLA: I already have a number one here, because Mike gave it during my talk.

BULLEN: Make that number two.

DI BELLA: Okay.

BULLEN: Joe can be first.

COPPERSMITH: Any ideas? Let's talk about some of the specifications of that. I mean, any ideas about thicknesses or other aspects of a conceptual design? Maybe I'll start by asking why you like it.

PAYER: Primarily because I'm not a fan of steel. So that eliminates that.

COPPERSMITH: Okay. But there's other things other than titanium.

PAYER: So I think either C-24 on titanium or C-22 on C-22 or flipping the titanium and C-22. The real concept is two CR, two corrosion resistant metal layers.

KESSLER: What do two give you that one corrosion resistant layer doesn't give you?

PAYER: Just the redundancy in fabrication. I would think there would be some fabrication benefits.

BULLEN: Different susceptibilities to failure mechanisms? I mean, they're both--are they both initiated in the same way?

PAYER: No. If you mix the titanium and C-22, there are some overlap in environmental conditions. The mechanical aspect is something that has to be dealt with. If you keep the current project criteria of being able to drop from a height onto the edge, then you have to go to very thick--I would hope there would be ways of dealing with--

BULLEN: Can I comment on that? I mean, part of the problem with respect to dropping it is that you're worried about juvenile failures, but I would contend that if we ever dropped it, we'd never emplace it. I mean, we're pretty smart engineers. I'm not putting the big dented soup can in the mountain. I'm probably going to take it back and refit it. So that's one of the concerns that I always heard that, well, juvenile failures could be because we dropped it, and I always felt bad about that because I don't think we're ever that silly. If we drop it, we're going to fix it. I guess I have a question about your structural integrity is all going to be the C-22 then?

PAYER: Apparently.

BULLEN: So it's going to be beefy? Meaning like four or five centimeters of C-22 to lift 80 tons or 60 tons or something like that?

PAYER: I've not dealt with the mechanical aspects.

BULLEN: Well, that's just a ballpark number. You'd end up with like four or five centimeters of C-22. And then what kind of titanium would you stick on it; a half centimeter, a centimeter, a millimeter?

PAYER: A half a centimeter.

SHOESMITH: I think two barriers of this kind with a much more resistant material on the outside is a good idea, whether it be C-22 on the outside or titanium is something that could be debated. What I would suggest here is if you're going to use a real high grade material on the outside, if we were willing originally to think of titanium on carbon steel, why would we not think of titanium or C-22 on something a lot cheaper on the inside, on the understanding that the outer barrier is now going to give you such great protection that the criteria required for the inner barrier are nowhere near as stringent and, therefore, you can go with a cheaper material. That combination looks to me like something somebody eventually will say we will never fund this. So I would suggest that, yeah, some corrosion allowance material on the outside, but some cheaper material on the inside, and I would myself prefer to stay away from carbon steel.

BULLEN: What's your outside barrier?

SHOESMITH: Okay, well, you could go with a low grade nickel alloy possibly on the inside. On the outside, I would say either titanium or C-22. I don't think that that choice is a major criteria. The C-22 will have some potential corrosion problems. I think the titanium will have none, or very minor ones.

SAGÜÉS: May I bring something up that may simplify the discussion perhaps a little bit? How about addressing the question of the inside of the package, just plain inside of the package, with the C-22, two centimeters of C-22 on the outside and 10 centimeters of carbon steel on the inside, before perhaps going into some variations of it. I'm saying this because in several packages they relatively compare with other things--would anyone be willing to consider discussing that before going to more complicated variations?

BULLEN: Well, that's an--it's inside container, C-22 over carbon steel?

COPPERSMITH: You know, one of the things that we didn't talk about was the internal waste package environment, which of course right now, the most likely penetrations--well, it looks like from TSPA results, in fact, the general corrosion is the primary mechanism which can occur in humid air conditions anywhere on the package. But for dripping conditions, they most likely occur on the top, evaporation would occur and localized penetrations would occur on the top, and we'd be dealing with potential filling of the package with water, either bath tub or just some invective flow, and then a basic process to get breaches throughout the rest of the package. So it is right now--provide some time period for that inside out process to work as well.

BULLEN: Being biased by our recent trip to Europe, I'd like to add a fourth one below Alberto's and have C-22 over just a cast iron inner barrier, make it cheap, and if the C-22 is a primary barrier, then the cast iron is basically there for structural strength, and then we can talk about excluding moderator materials by other means later.

DI BELLA: Any cast iron, or do you want nodular?

BULLEN: Oh, I like nodular because it upsets the NRC most.

PAYER: We are going to discuss the current--

BULLEN: I'd like to, sure. Do you want to put it up here?

COPPERSMITH: I assume that, you know, the first step is to get these down, and then we'll have to go back through the pros and cons and look at sort of evaluation--

PAYER: Zero we put down, or baseline, or whatever. That is what's being proposed today.

MACDONALD: We should probably also think of nonmetallics. What are--I've often wondered what about graphite? Now, graphite is found in nature and producing mildly reducing conditions. It's thermodynamically stable. So we'll take the cast iron a little bit--

BULLEN: Sure, why not. Do you want to put it inside of a corrosion resistant barrier?

STREICHER: To do what?

BULLEN: I'm sorry?

STREICHER: What do you want this to do? Mechanical--

MACDONALD: No, no, not mechanical. Chemical stability. Dick and I were on the KBS-2, National Academy of Sciences Evaluation, KBS-2 plan back in, when was it, '79, or sometime like that, and what the Swedes had proposed was to electrochemically engineer the system so that the copper was in a thermodynamically stable state, and then the question of chemical kinetics presumably would never arise. I agreed with the general concept. I disagreed with some of the particulars, mainly that they had sulfate in the bentonite, which could be acid--sulfate producing bacteria. But this is the same sort of proposal. Instead of just dealing with kinetics, why don't we see if we can engineer a system so that under the expected repository conditions, it's thermodynamically stable, and then the issue is moot.

SHOESMITH: How easy would it be to close a graphite package to guarantee that it was--

MACDONALD: That's an engineering detail. I don't know. But if we were to take that philosophy, let's appeal to nature to help us out on this, you know, if we tried titanium, for example, you know, we're dealing with a highly active metal with a very high energy density, and we're relying upon a passive film to protect it. The same is true with chromium, nickel and iron. Copper, we're not. You can engineer the system so that copper is stable and, you know, witness the fact that you find lots of copper, native copper, that's been around for geological times, and the same is true for graphite. That was an appealing aspect of the KBS-2 plan.

PAYER: You don't find copper in the oxidizing environment.

PARIZEK: Does graphite care about oxidizing and reducing environment? Does graphite care whether it's oxidizing or reducing?

MACDONALD: Graphite, as I understand it, is found in the native state in relatively oxidizing environments compared with copper.

COPPERSMITH: Is that true? Is there a geologist in the house? I pictured it to be more in reducing conditions.

MACDONALD: If you get it too reducing, you end up producing methane.

STREICHER: What about the radiolysis?

MACDONALD: Well, that I don't know. It's a very good point.rgue is having a corrosion resistant metal alloy on the outside, and maybe a graphite on the inside so that by the time the corrosion resistant metal had corroded away, the activity would have--the dose rate would have decayed away to have been negligible. And I think the point that was made was about 30 half lives. I mean, it becomes negligible.

COPPERSMITH: Any other concepts?

PAYER: Those are all the materials related issues.

KESSLER: Before we leave the materials one, again, I don't have a good feel for how the container expert elicitation finally wound up, but one of my concerns is that the way that expert panel and DOE has currently modelled things, it suggests absolutely abysmal behavior from the concept that was just abandoned, which was mild steel outside, 825 or 625. I always get the numbers confused. And I'm just wondering whether it's--why that's been abandoned is because there is pretty much universal agreement that that was a bad choice of materials, or whether it was just modelled so incredibly conservatively to try in this attempt that that was what was needed to get through the licensing process, that that material was abandoned. And it's not clear to me what the basis for abandoning the old material was, to move from the 25 to C-22.

COPPERSMITH: Dave Stahl, do you want to give a brief explanation of why C-22 was chosen? You heard the word brief.

STAHL: David Stahl, M&O. Basically, the requirements for the waste package have been enlarged over time. The 825 combination with carbon steel we thought gave adequate life for the 300 to 1000 years, which was the original goal. But the goal has been widened, and as such, the 825 could not longer meet that requirement. So we moved to 625, and then later to C-22.

KESSLER: But it can no longer meet the requirement based on a certain approach, conservative versus best estimate approach. That's what I'm trying to get at. Was it abandoned because there was universal agreement that in reality, we really felt that 625 was going to behave that badly, or it's an understanding of what we felt was necessary to model things conservatively?

STAHL: Well, I'm just suggesting it's a match of performance with need. Now, the 825 met the need. When the need expanded to longer waste package lifetime, you needed the more corrosion resistant material.

Can also reminds me that we had additional water flux in the mountain, and that gave additional distress to the 825 and to the 625 because of the potential for concentrating those waters.

KESSLER: All that in my mind the increased water flux has done is maybe increased the proportion of packages that might get wet, but there was always the potential before that a few packages would get wet.

PAYER: The issue I think is that the environment has not to date been--the extremes of the environment--the middle of the road environment and the water coming into the drifts is arriving at some consensus, and it's not far from what Carl put on there earlier. The changes to that environment and how it will be modulated when it goes through corrosion products, deposits in crevices has not been well defined. And my bias is that the models are not, while they're reasonable models, they're not reliable enough that you could really say they define the extremes and the experimental measurements of trying out where those environmental extremes are yet to be done.

So if we look at the realistic kinds of environments that you could foresee, and I think we could write 20 per cent ferric chloride off the table, it's not going to get that oxidizing, but 1 per cent ferric chloride, I don't know. And so if you look at the realistic kinds of extremely, C-22 gives you a broader range of suitable material than 625 does. That's the rationale. Now, if you could get a better agreement on where those extremes are, then 625 may have enough.

COPPERSMITH: I think that agreement is very difficult to reach from the environmental condition standpoint, to narrow it down significantly more from where it is now.

PAYER: I think there's some reasonable work that's been underway, and some of us heard Greg Gdowski has been doing some work, you know, that shows some real promise of coming up with a more realistic set of conditions, and the work that David Andreson and the folks out at Waste Package, they made great progress in that. But it hasn't been closed yet, so maybe we're jumping over into research that still needs to be done. But it's not as hopeless a case as we'd thought.

SHOESMITH: I think the issue here is having chosen carbon steel for the outside, I'm not suggesting now that it should be totally abandoned, because it's such a good cheap material in terms of mechanical stability and everything else, so this is not meant to say that the present one should be eliminated. But having chosen carbon steel, the whole corrosion issues arise from that choice, and having made that choice, it's very hard to justify any inner barrier material except a very expensive one. So by going with something cheap on the outside, you push up the criteria required on the inside, and I don't think 825, given what we have here on most of the models, would be able to withstand the conditions that might arise, needn't necessarily arise. That's what drove it to C-22.

But if the outside material is way better, and even quite thin, then what you put on the inside will not have to be the same stringent requirements. So I think the decision to get to C-22 is purely and simply because there's so many corrosion issues with the carbon steel that you cannot be certain of. It's already designed to corrode away at a certain rate, so you automatically push the requirement for the inner barrier to the best material you can find. So you get away from C-22 if you stick with something cheap on the outside.

BULLEN: Are we wedded to C-22 as an outer barrier if we decide to flip them, though? Do you want to go back to 625? Do you want to go back to 825?

SHOESMITH: I think if you're going to go with an outer barrier of a better quality, then you wouldn't put in a material which is going to have trouble with highly saline hot conditions. So I think you're still going to go--it would still be smart to go with something like C-22. I don't know about five centimeters. Why would you go with something so thick for a corrosion barrier. If it serves some other purpose, then it should be specified.

BULLEN: Two centimeters of C-22 and stick some Monel on the inside, or something like that?

SHOESMITH: I'm not saying titanium on the outside. I'd go with what they pump out in Henderson, which is .65 millimeters. That's all the corrosion barrier you should require for that kind of material.

BULLEN: Millimeter or centimeters?

SHOESMITH: Millimeters.

BULLEN: Millimeters?

SHOESMITH: Well, if you multiply it by three and you're getting ten to five years out of it, so you get ten to five for--three times ten to the five, it's minuscule.

SAGÜÉS: I would like maybe to go back a little bit and see if we can address the matter of whether this group may feel that the tenths of the meters of carbon steel on the outside are actually doing a negative calculation to the overall durability. I mean, do we feel that that is actually creating a problem, that rather than being a barrier that will retard the corrosion process, it may actually have such a--or can actually have such a chance of making the conditions worse, that indeed, the balance is a negative effect, and the chance being presumably the development of a ferric chloride type of environment on the outside. Is that a real threat, or is it maybe are we sort of over reacting?

COPPERSMITH: That's a very good question.

MACDONALD: I think certainly the danger that pertain to the fast growth of magnetite--and by the way, the one compound that doesn't do such a fast growth of magnetite is ferric chloride--should be looked at very carefully. It's been reported with Potter and Mann back 25, 30 years ago, and there are circumstances where magnetite grows in a linear or autocatalytic manner on carbon steel. And to me, it's easy to envision that you could get these conditions existing on the canister surface. So I would again caution on the use of carbon steel. I like the idea of having something like C-22 on the outside.

KESSLER: But for the purposes of this panel, isn't it just a matter of saying go look at certain--can we recommend certain tests or surveys of certain data? We're saying well, gee, we don't know, or maybe it's an issue. Well, I thought the point of this panel was to suggest ways to resolve the issue one way or the other.

BULLEN: That's right.

COPPERSMITH: That's the next step.

MACDONALD: As I said, there is danger in the literature. I'm not saying that those data necessarily would answer this question, because they were obtained for a different system, mainly to do with nuclear steam generators.

PAYER: There's some major concern of carbon steel in the system in any case, even if it doesn't grow by this fast growth magnetite, which you've mentioned, or is in centimeters--meters per year. We know it will corrode in millimeters per year, or certainly fractions or tenths of millimeters under the environmental conditions.

We also know that it's going to double its size.

It will be a times two volume expansion when it does grow, and one of the very real concerns, not in the fast growth magnetite, it causes the denting problem, but there's also a problem called pack-out in carbon steel, in weathering steel, where steel to steel joints have been--rivets have been popped and the steel beams have been distorted, bent by the growth of corrosion products in the crevices between these joints.

So I think there's a real concern, a real possibility that the outer can, once it's penetrated if it's steel, that steel bracelet then can start to corrode in the crevice between the steel and the C-22 and start to crush the inner can. I think that's a show stopper. I really do.

COPPERSMITH: I want to return to that, because I think for some of these dealing with the pros and cons that volume expansion issue can be potentially very important. But one thing I want to do is to get--initially get a master list here, and then I want to go back systematically through those.

To do that, some of the things that I was--again, we're not setting out a rigid set of evaluation criteria like somebody would be doing in a real design review or design evaluation. But some of the things that we have been thinking about or told to think about deal with the strength, the structural integrity issues, corrosion resistance obviously, weldability, the size of geometry requirements, maybe inspectability, radiation shielding. I think criticality may be a lesser issue of MIC, the microbiologically influenced corrosion.

Some of those, in thinking about those, do they provide any additional design options that you might want to think about? Or I hate to get into options 2-A, 2-B, 2-C, but there are what has been called design enhancements that might be considered, like ceramic coating or other types of features that might, in fact, interact with and give better performance to the set that's up here.

BULLEN: Along those lines, almost all of these designs lend themselves to enhancements, I guess is the word, which would include the use of a potential for self-shielding the package. And if you looked at the back of Dave Haught's presentation, he had information on self-shielding, which was self-shielding by adding extra layers on the outside. The use of fillers was mentioned previously. Some type of consolidation of the rods and fillers in association with that to allow us to have maybe a smaller package, smaller package with a self-shielding component so we don't have to have the robotic emplacement, but also so that we don't have to worry about the radiolysis effects ever, I mean, because there won't be an issue, depending on even what the outer barrier is. If you've got a self-shielded package that's in the 100 mr/hr range, pretty much everyone would look at that and say that's not going to be a radiolysis or radiolytically induced corrosion product problem.

PAYER: How do you get the self-shielding?

BULLEN: Oh, I think you get the self-shielding by basically--I'd take the C-22 outer and carbon steel inner and actually cast it. I'd cast it with graphite in it so that you can get the neutrons down, because that's one of the biological hazards that you'd worry about. I'm looking at a Pollux container that the Germans have designed. I have no grandiose plans myself. I'd basically just borrow from everyone else. So put C-22 on the outside of a Pollux container and tell me why it will or won't work. And since I'm going to spend this money, I might as well consolidate my fuel rods now so that I can get a lot of waste into one package.

Pollux container that they've built that's the biggest is 12 PWRs, so I'd want to consolidate and put 24 PWRs in the 12 space, and obviously then what I've done is I've mechanically changed the cladding, and I also have to worry about whether or not the clad temperature is exceeded. Well, there's a recent article in <u>Nuclear Technology</u>, February of this year, by the Germans that said that during interim storage, they worry about the 350 degrees C. clad limit, but after that, they let it go to 450 or higher for the cladding, because they're not going to take credit for cladding as a barrier, which could be a contentious issue until the data that Dr. Stahl mentioned this morning are available on the testing that they've done.

So if we abandon cladding, that gives us a different option. The other thing you do when you consolidate is you get some self-shielding. The inner rods are shielded by the outer rods, obviously, because you've got some mass in there, or some density, if you will, that will do it.

SHOESMITH: What does consolidation involve, Dan?

BULLEN: Consolidation actually is a disassembly of the fuel assemblies. You've got to push the rods out of the spacers. Now, the argument is you're not going to get a lot of reduction in volume, and they're right, because you've still got to get rid of the spacers. But the spacers aren't going to--I'm putting all the fuel in one can and worrying about greater than Class C wastes somewhere else.

Now, this is a contentious issue. People don't want to do it.

SHOESMITH: Well, it sound like a major handling facility on the surface.

BULLEN: Is that a problem?

SHOESMITH: It doesn't sound like a particularly safe procedure.

BULLEN: Well, why is it not safe? I can understand how consolidation would be a problem if I asked you to do it in your spent fuel pool at the reactor. But if we're dedicated to doing this--

SHOESMITH: I'm just asking you the question.

BULLEN: Oh, no, I understand and I would hope that they would be getting very good at it.

DI BELLA: Dan, for the purposes of this list, can I put number six down as number five, plus rod consolidation?

BULLEN: You've got to put it a little further--you've got to have some beefier--well, not graphite. The graphite inner is basically--number four with some neutron shielding in there. And it would probably have to be bigger, too. So basically, number four is not going to be a 21 PWR assembly; it's going to be a 12 consolidated.

SHOESMITH: So if I understand this consolidation correctly, you'd have to strip all the cladding off all the fuel?

BULLEN: No, no, no. I'm just taking the assemblies apart. All I'm doing is putting the cladding together. I'm taking the spacers out, and the theory would be two to one, but that's probably not good. You could probably get 1.6 to one, 1.7 to one. So ultimately what I'm looking at is a waste package that's going to cost me over a million dollars apiece. If you looked at Dave Haught's numbers in the back, you could go to $2 million apiece, depending on how you self-shielded it. So if I have 15,000 packages, if I cut that down to ten, then I'm saving, you know, somewhere between $5 and $8 billion. Now, we don't have to worry about cost, but as a taxpayer, I think $5 and $8 billion is a lot of money, and if I can do it cheaper and--I mean, if the packages are going to be high real estate value, I might as well put as much waste as I can in each one.

MACDONALD: It is to me.

BULLEN: I was going to say, just give me the interest on that for a while and I'd be a happy man. But it's just consolidating the rods.

Now, obviously the rods are going to get hot. Some of them may actually creep up to--if you don't have oxygen in there, I don't think they're going to unzip, and so I'm interested in putting as much into a waste can as I can. I'm not concerned about the 350 temperature limit, which is why I asked Carl originally if any of those were hard and fast, and obviously it wouldn't be because the thermal limits are going to be exceeded if you consolidate, even in a small package.

COPPERSMITH: But those are issues I think to remember the pros and cons. It might be that in fact the cladding issue and guaranteeing unzipping or guaranteeing a much higher likelihood of unzipping may be an issue.

BULLEN: There might be a higher likelihood of unzipping, because that's probably something that I would acquiesce to. But I think if you don't have a 350 degree temperature limit, and you allow me to do a post-weld heat treat on a C-22 weld, then I'll say that I'm not worried too much about metallic formation long-term any more. I think that issue goes away. So then I don't worry about a failure mechanism, but I know what the base metal looks like. The base metal looks like something that's probably going to dissolve and, you know, take ten to the fifth or ten to the sixth years to do it. From a materials point of view, I'm being very simplistic. I'll stop talking for a while now, Kevin.

COPPERSMITH: Well, I like it. I like it.

SHOESMITH: Well, I'm just wondering what this does to-- I mean, you can make it sound like an easy surface handling process, but I don't think it is an easy surface handling process. Would this be something done dry, or would this be something done wet?

BULLEN: Actually, you'd have done it both ways, as I understand it. You can do it in pool, or you can do it dry. And actually, the question is are you going to compromise the clad in some cases? Sure, some of them are going to break. Do I care? If I'm breaking 90 per cent of them, sure, I probably care. But if I'm breaking a few per cent, and a few per cent are already breached to begin with, do I care? Particularly in light of the fact that I don't know from a QA point of view if I'm ever going to know enough to take credit for the cladding.

SHOESMITH: Are you now inventing a compatible design with other designs which would have to accommodate glass waste forms or Navy fuel? Are we now going to two diverse handling procedures for this waste?

BULLEN: How so?

SHOESMITH: If you consolidate--you're going to consolidate the fuel bundles.

BULLEN: Right.

SHOESMITH: But I'm presuming that some of the Navy stuff comes sealed with hands off, and I presume the glass form stuff is not going to be treated this way, therefore, are you now introducing two streams of container design?

BULLEN: No, I'm going to use the same container design, aren't I? If it's good enough for consolidated fuel, I think it's probably good enough for Navy fuel and it's good enough for glass. That's--

KESSLER: You're just putting something inside the same kind of container that's different.

BULLEN: I have a different thermal constraint. My packages might be hot for a while, which may mean I want to ventilate for 300 years, but that's an option that we can talk about later.

PAYER: Well, I don't know, you threw in about 17 different options, I think. You said it was simple, but it didn't sound very simple to me. I'm having trouble.

BULLEN: Well, the people who have ever pulled rods will say it's never simple because they really don't want to break them.

PAYER: Well, beyond that, though, you said a little hotter. How much hotter? Because that's going to have an effect on the loading. You're going to keep it 200 C at the drift wall? You said, well, we might have to ventilate for 300 years. You know, that's--

BULLEN: All of those are costly. All of them are changes in options. But if I'm looking at waste package materials performance--

PAYER: I'm not trying to argue your concept. I was trying to keep notes, but I didn't get nearly enough footnotes in.

Is that really your option up there? I don't think that option describes it. All the other ones were pretty much material substitutions, and I was--

BULLEN: Sure.

PAYER: Material substitution, ventilation--

BULLEN: Change the paradigm completely, you're right, which is what I thought we could do in this workshop.

COPPERSMITH: Just jump a little farther outside the box. That's all right.

BULLEN: I never saw a box. Nobody said there was a box I had to be in. So I'm way out--

COPPERSMITH: You're back in the parking lot.

BULLEN: Well, did I get to the parking lot yet? I'm getting close. Okay, Bill.

COPPERSMITH: Carl?

DI BELLA: Should I put something down here?

BULLEN: Flip it over, or say seven, yeah. You've got number four, consolidated, more shielding/ventilation. You've got to add all this stuff to make it within the parameters. I didn't want to exceed the 200 degree C. wall limit. I don't think that that's something that's within the scope of this area.

PAYER: I think your options--I don't think it's described there in number six yet.

BULLEN: Okay. Consolidation, more shielding, ventilation. What did I miss? Anything else that's necessary?

COPPERSMITH: This is for any materials?

BULLEN: Well, this is going to be C-22 over some kind of cast iron. That's fine.

PAYER: Well, you had a significantly different surface facility.

BULLEN: Well, yeah, it's going to have surface--but consolidation means that to me.

COPPERSMITH: Okay.

BULLEN: And if consolidation is done at the mountain site, not done at the reactors, I mean--

COPPERSMITH: And the same sort of geometry, overall package size?

BULLEN: No, not necessarily. Obviously, we're going to end up with something that's going to look more like a 12 PWR than a 21 PWR, primarily because--I mean, if I make it extremely large, then the guys that do the ground support are going to yell at me that they can't take a 475 ton cask in there. You know, I can design them one and it would be shielded and safe, but I think we've got to look at something that's reasonable. And so reasonable is on the order of a 12 PWR size. It may have a bunch more fuel stuffed in it. I don't think you want to do this with a 21 or a 44 BWR because I think that's probably getting pretty big, and it's probably getting pretty hot in the middle, too.

Have I done the thermal analysis? No. Do I know that I'm going to stay criticality safe? Well, if I don't have any other empty spaces in there, yeah, I'm probably doing pretty good. Then I can get rid of that borated stainless steel, because that always poses me a problem. I don't have to worry about moderators--or absorbers hanging around. But I've got carbon steel in there that's going to swell and pop my can open, according to Joe here. That might be a problem. So I'd listen to Monel if you want to stick it in there instead of carbon steel. I'm not wedded to carbon steel. I just need something that I'm going to cast in place or is going to take up most of the space. Then I'll fill it up the rest of it with fuel.

SAGÜÉS: I would say maybe it would be a good idea to take these things one at a time. And the first one, I'm still fixating on the first question in here. Do we have anyone here who would like to make a point that indeed if one were to start from the beginning, carbon steel on the outside is indeed a bad idea? I'm just trying to seek--I mean, I can see a whole bunch of minuses.

COPPERSMITH: Yeah, let's deal with option zero and talk about the pros and cons of that design.

STREICHER: Iron oxide will take up a lot more volume than the steel. If that concrete exterior is anywhere close to that carbon steel, that will be cracked by the corrosion process.

BULLEN: A couple meters I think, or about a meter. A couple meters is the distance.

COPPERSMITH: There's quite a bit of space.

BULLEN: I guess the key is if it were a dry repository, which was originally proposed, if we had a tenth of a millimeter per year and it was an oxidizing air environment, carbon steel would behave as they expected it to behave. But since we've got the potential to drip water on it, even though Dave told me--Dave Stahl said it was spalling this morning, I didn't see that at the thousand year delay time. Dripping water is going to make carbon steel spall, so I didn't think that that was a good idea. I mean, in a wet repository, you don't put carbon steel on the outside, is my gut response, in answer to your question. But that's just my opinion, and I could be wrong.

COPPERSMITH: Yeah, but what are the cons? Why not?

BULLEN: Why not? Because it's going to rust.

COPPERSMITH: Is it a rate of--

BULLEN: It's going to go up pretty quick. I mean, we've been to Livermore and seen--you know, they put it in the J-13 water, and it rusts.

COPPERSMITH: That's exactly what you'd expect; right?

BULLEN: In an air environment, particularly a dry air environment, it would be fine. I mean, we have corrosion allowance barriers that are used as, you know, oxidizing steels all over the place.

SAGÜÉS: So it rusts. So what? It takes a couple hundred years, or a few hundred years--

BULLEN: Well, I think that's the problem. It rusts too quickly.

KESSLER: You're saying it all falls off, too.

BULLEN: I think it falls off.

KESSLER: It's one of the things that we've explored and EPRI's PA model is the standard fits of potential corrosion products, and so we're delaying things and we're finding that that's what's going to happen. If the corrosion products are tenacious, it's conceivable that they can do you some good once it's corroded through.

PAYER: The experience is that as the steel corrodes there is almost a two to one volume expansion. And so the oxide layer grows to maybe a couple millimeters thick and it spalls off in layers and they grow another, and so that would still remain. That's also going to be very non-uniform, even though we call it general corrosion, it's not exactly the same everywhere. So there's going to be areas where the growth is deeper.

The down side of the volume expansion, if you're going to use any sort of--we haven't talked about this--but in a backfill, if you try to use some sort of capillary action, or whatever, the growing corrosion products inside would tend to disturb that mightily.

It corrodes not only just in drips, but in wet, when the relative humidity is down. The dry oxidation is not much, but in a moist atmosphere, 90 per cent, 95 per cent relative humidity, it corrodes at a fairly rapid rate, particularly if there's some chlorides around. So the down side is the corrosion, and I think it's more realistic to think that many of the packages would corrode, and once they get wet, you get tens of years service out of them, and then they're going to start to be penetrated. Make that a hundred years, but certainly not thousands or tens of thousands.

The attributes of it is it's got tremendous mechanical integrity. You can in fact take rock falls and bounce it down the drifts. You can't really do that, but it has some significant mechanical integrity to it for the thickness. It's inexpensive material. It's ductal. You can fabricate it at those thicknesses. It's possible to weld it. So it's got some positive things to it, but I think the corrosion of the canister itself, and then the subsequent putting all that ferric ion in the location gives you--significantly broadens out the environments that can attack the underlying corrosion resistant metal. And when I look at all those types of things, then the steel becomes a detriment.

STREICHER: Would the corrosion products interfere with ventilation, if that's what you're counting on?

PAYER: Well, it would do that, too. You're going to get this thickness. If you had to go back and for whatever reason people talk about retrievability, picking up a can that's got non-uniform corrosion problems on it that are layering and spalling, just the effects of that seem to me to be somewhat problematic.

COPPERSMITH: Now, some of these issues, like rockfall, for example, it's difficult to say how important that is, because it isn't clear right now how common or how likely or how big rockfall will be. For example, if the work that was done on rockfall looking at the thermal load says that in fact 90 per cent of the drifts are going to be collapsed in a thousand years, then rockfall might become a bigger issue than if it says that after a thousand years, there's going to be 10 per cent or 2 per cent of the total drifts. So it's a potential pro in your argument. It's mechanical strength. But right now, it isn't clear, at least to me, how important that issue is.

PAYER: I mean, I don't know how important rockfall is either, so the can had better be designed for it.

KESSLER: That's what I was just going to say. If we're exploring scenarios where basically we are going to blow off cladding in terms of going to a much higher temperature, then you can introduce backfill as an option. And if you're going to consider consolidation, you can consider backfill.

BULLEN: Sure, But I don't want to go much higher because, you know, there are certain limits that you don't want to exceed. I wouldn't, you know, take the repository up to 1000 C. or anything like that, John.

But I guess before we continue, I also want to make an acknowledgement here. We have been joined by Dr. Priscilla Nelson, a Board member who has graciously decided to join us this afternoon, and is going to contribute greatly to the rockfall question.

COPPERSMITH: What is the probability of rockfall?

BULLEN: I wanted to acknowledge that Priscilla is here. So thank you, Priscilla. And go ahead and continue, John.

SHOESMITH: Just on the--I'm sorry--just on the issue of carbon steel, I'm not quite as pessimistic as Joe. I don't necessarily believe that the wedging is a show stopper or necessarily that you will spall the same site for a sufficient length of time that you will destroy this in tens of years as opposed to longer periods. But the issue is still there, which is that the burden of proof that the carbon steel is going to perform is very high. You can't ignore these issues. You have to look at them. And these are things which will take a lot of time.

The wedging stress issue is not something that you can do experimentally. You can only really attempt to calculate what that effect will be. So although I don't necessarily think it stops the show, it's an awkward thing to have to deal with.

COPPERSMITH: This is internal as well as external?

SHOESMITH: Internal, and I think the volume expansion will do exactly the same as it does on the outside, and possibly worse because it's got less places to go.

MACDONALD: And if one was to insist upon using the carbon steel, one ought to use a weathering steel.

PAYER: Well, I think that's the wrong way because it stays wet. If it stays wet, then you never get the benefit of the weathering steel.

STREICHER: What about the loss of radiation shielding as the carbon steel disappears?

MACDONALD: Again, the activity, the dose rate drops off.

STREICHER: Yeah, but that's a race between one and the other.

KESSLER: The assumption is that the dose would fall off faster than the--

COPPERSMITH: Alberto has a viewgraph that's going to answer these questions.

SAGÜÉS: Very quickly, I just want to make a point here. What I think for me is probably the major concern about the carbon steel is the fact that when it corrodes--if it corrodes and spalls and the stuff falls off neatly to the sides and then we end up with a nice shiny clean shell of C-22, I think that that by itself wouldn't be too much of a problem. The problem would be if the carbon steel corrodes, the corrosion products stay in place and all the steel disappears so that there is no longer metallic steel, but only oxides, hydroxides of iron. And then that reacts with the chloride in the water that is dripping into the drift. Well, in that case, we all know this kind of performance for C-22 that shows you what happens at moderate temperatures, 100 degree Fahrenheit, up to 200 degrees Fahrenheit. If you have a high enough concentration of hydrochloric acid, you get into extremely high corrosion rates that could chew through the C-22 two centimeter shell very fast.

And you also know that if you, going back just a moment, if you go ahead and you have here concentrations of ferrous or ferric chlorides, you can go ahead, and if you get closer to a solubility limit, you may end up with low enough pHs in the environment that that could conceivably get you in this kind of regime in which you get the very high corrosion rates.

Now, the question is is the water chemistry, this water would be dripping through the roof of the drift into the material, is that the kind of thing that combined with corrosion products form in the condition that we expect would result in those--in the very low pH that is necessary to cause this kind of corrosion? Is that possible?

PAYER: No.

SHOESMITH: I think the feeling of the expert elicitation process was that the maximum ferric ion concentration you might get would be somewhere around .1 per cent, and that would be enough to suppress the pH to 2.5 at the very most--at the very least, since you're going down. So I don't think it's feasible to get that low.

SAGÜÉS: Okay. So then my question is so what then?

SHOESMITH: Well, okay, for that very reason I don't think it necessarily should be ruled out as a totally inappropriate material. It does still have the potential to protect the C-22 long enough that the C-22 becomes immune to the consequences of this water chemistry. It doesn't have to do that for very long.

BULLEN: A quick question, Alberto, before you put that data completely away. When did you get to a pH of a half? What were those conditions that were necessary for that?

SAGÜÉS: That was a ferrous chloride concentration, ferrous or ferric, for that matter, ferric if you will, of about eight molars.

BULLEN: Would you expect to see that at the pit?

SAGÜÉS: No.

BULLEN: You never see it that low--or that high, I mean.

PAYER: The suggestions are that you have it again, I think it has to be worked out not just with arm waving, but with the real numbers and calculations, but fairly good looks have been taken at it and there's two issues that stop you from getting there, and one is the other anions in solution.

So you don't just have chloride, you have other things that start precipitating out that prevent you from getting there.

MACDONALD: Well, again, at 200 degrees Centigrade, and maybe you won't get to 200 degrees Centigrade, .1 molar ferric chloride solution and 1 mole of sodium chloride induces fast growth of magnetite on carbon steel.

PAYER: But there again, the latest thinking amongst the waste package expert elicitation is the highest boiling point you're going to get is about 105 Centigrade. We're not in an autoclave.

MACDONALD: I'm just thinking of a scenario of dripping onto the canister.

PAYER: Well, you still have atmospheric pressure. You can drip on the canister, certainly, but you can't--

KESSLER: Can we go back and look at this in terms of research needs? I mean, it seems like we're discussing if it's like this, then this could happen. Why don't we just say the research need is to confirm that the ferric chloride content is below X. Can we define it that way?

COPPERSMITH: You can define it any way you want. I think again right now, I want to explore--rather than go to research needs, because we will go to that, and option zero will be one that we explore, I want to make sure we go through the pros and cons of these various designs. And, again, where is the point of where we're done? I don't think we'll get there. But I think we need to weed out the good and the bad for some of these. You know, like where we left the last one was Alberto said if that's the case, then what's wrong with this design. Well, maybe there isn't anything wrong. Maybe we keep it for now and deal with what the research needs are for that particular design. But I do want to weed out, if we can weed out, or modify this list as we go through.

SAGÜÉS: Yes, the question is if things such as, for example, the carbonates in the water would be enough to prevent the development of very low pHs. Now, I understand that the water chemistry is being investigated right now at Lawrence Livermore?

MC CRIGHT: That's right, Alberto. We're looking at especially the electrolyte dripping on the container surface. We're very much interested in that. We want to find out what the saturation values are. For instance, if the ration of the various ions would change because of a decomposition reaction, or what have you, that we are actively investigating the water chemistry right on the container surface.

COPPERSMITH: And that's experimentally?

MC CRIGHT: I mean, then we'll follow that with some modelling, too. I mean, I think most people appreciate here a lot of the modelling tends to fall down when you get to very concentrated solutions because of the different measuring activity coefficients.

SAGÜÉS: As I understand those chemical code calculations that were described last week then, this groundwater is never chloride dominated; it's either sulfate or nitrate dominated?

COPPERSMITH: From those codes at the present time.

SAGÜÉS: So then the question goes back to what's wrong with the carbon steel?

COPPERSMITH: Dave Stahl, do you want to tell us what's wrong with it?

PAYER: You'd still have to deal with the issue of the growth of the iron oxide product, and if that compromises the integrity of--well, it's going to be penetrated someplace. It's not to all corrode from the outside and then just fall off. You're going to get penetration, and now the water can get between the carbon steel and the C-22 and you start growing oxide at that interface, and you still have significant structural integrity of the brace load steel around it. And if that compromises the integrity of it, then you have a chance of splitting the inner liner. That's an issue.

COPPERSMITH: But isn't that an issue for many of these dual wall cases where they form a nice crevice between the two? Is that not the case when you have two CRM materials?

PAYER: That's not the case when you have two CRM materials.

COPPERSMITH: You were able to bond sufficiently that you won't develop that?

PAYER: You don't get the growth of this--

COPPERSMITH: Exactly. But isn't it in fact a potential crevice and--

PAYER: It's a potential crevice, but the types of crevice chemistries you would predict you get in that, the bet is that they will not be so aggressive that the system can maintain that. You don't have the iron that's generating the hydrolysis ions that are driving into these low pHs. It's the corrosion reaction itself that drives that to it. So if both the materials are passive in the crevice, then you don't lose.

COPPERSMITH: Let me ask Dave. David Shoesmith was in the waste package elicitation. Your panel allowed for this potential, very oxidizing potentials, potentially low pH down to 2.5. What were the conditions that would lead to that?

SHOESMITH: Well, you'd have to get dripping. You'd have to have a whole sequence of wetting, drying cycles which allowed you to concentrate at that particular site. And before you could get that kind of ferric ion concentration, you would have to be isolated from the carbon steel, because if you had a crevice between the carbon steel and the C-22, you would always have iron-2 around.

So you're never going to drive the redox potential sufficiently positive to be aggressive on the C-22. So it had to be something which is at a--some silicate deposit or some corrosion product deposit which is predominantly ferric ion, plus this concentration scenario. So it was deemed to be a highly improbable event, but a feasible event, and there didn't seem to be any way whatsoever that you could get this below the pH of 2.5, roughly speaking, which is pretty close to what Joe Farmer calculated was feasible in an active crevice between carbon steel and C-22. And that if you had that pH, the corresponding ferric ion concentration will be somewhere around .1 weight per cent, which seemed fairly benign compared to a lot of the testing data of the C-22 that was out there.

For instance, I think .1 weight per cent ferric chloride that was a crevice corrosion threshold for C-22, which is 100 degrees, so it was a temperature criteria for much higher ferric ion concentration, and we accepted that criteria even though we didn't think you could get within an order of magnitude of that ferric ion level.

COPPERSMITH: But presumably difficult to attain that localized environment?

SHOESMITH: Well, yeah. Also, we ignored the possibility that as you concentrate it, you would see what the chemical codes appear to be demonstrating, which is that the sulfate concentration would always be superior to the chloride one, and that when the sulfate finally precipitated with calcium, the nitrate concentration would still remain as an inhibitor. So that those more benign effects of the complex anion solution that would develop were recorded in that scenario. So things should be a lot better than what came out of the expert elicitation. It's already showing in the TSPA calculations that the localized corrosion failures for C-22 don't look to be causing a significant effect. Now, it's debatable because it's hard to tell in the plots presented.

COPPERSMITH: Yeah, that's what I took from the plots that Dave Haught showed, too. You see the general corrosion is the dominant mechanism.

SHOESMITH: I think Joe has hit the nail on the head. There are two real crunches of carbon steel; that wedging effect plus a wet/dry spalling at the same site every time. Those are the two key issues.

BULLEN: How do those issues change if you flip the barriers? If you put C-22 on the outside and the carbon steel or some steel on the inside--

COPPERSMITH: Wait, before we do that, let me ask if we move from environment one to environment three, how would it change? Let's keep the--

BULLEN: Keep the same material in environment three?

COPPERSMITH: Yeah. Let's move to just a cooler temperature. And presumably, there's a lot more moisture available, either by humidity or more drips.

PAYER: The change starts sooner and goes as fast or faster?

SHOESMITH: Plus, now you introduce the possibility of a radiolytic corrosion, which I don't think will be excessive in the long run, but it's still another factor driving it, could start driving a pit.

COPPERSMITH: Okay. So this design has more problems with the cooler environment?

SHOESMITH: I think the issue is that carbon steel turns up when we start discussing research needs. Whereas, with some of the other materials, there are not as many.

PAYER: One we haven't mentioned is that steel is certainly susceptible to accelerated corrosion by MIC, the microbial attack, and several of the other materials up here are certainly less so, or not at all. And so to some extent, you get steel out, you take the microbial off the table.

COPPERSMITH: Are you writing all these down, Carl, all these pros and cons? Okay.

STREICHER: The idea for the carbon steel was to act as a kind of sacrificial lamb here.

COPPERSMITH: High strength early on, mechanical integrity.

STREICHER: But that the danger is very great that after the lamb has been sacrificed, the conditions will be much worse than they were initially for any number of reasons, which may not be possible to simulate in a laboratory.

COPPERSMITH: Well, are they worse because the lamb was there, or would they be worse just because the environment is getting worse?

STREICHER: No, because the corrosion products will have changed the environment in a way that is hard to predict and that as a result, the C-22 will face an environment that is worse than if you were there with the C-22 by itself without the carbon steel.

MACDONALD: That's a good point, because if the corrosion product was magnetite, magnetite is a relatively good electronic conductor, and if you had reasonable electronic connection between C-22 and the magnetite, you'd end up in a situation having very large cathode connected to your C-22. But you could very well have had--

SHOESMITH: But then of course you wouldn't have the ferric ion concentration if you stopped the magnetite stage. It's a redox buffer also.

MACDONALD: Right. But you'd have oxygen.

SHOESMITH: Yeah, then you won't stop the magnetite. You will go all the way to hematite. Locally, you could get all kinds of behavior, but I think in the long term, you have to go all the way if you have that oxygen around. And if you don't, if you can hold off in the magnetite stage and the redox buffering capacity for magnetite, if it's wet and in contact with C-22, would hold it way below the breakdown potential, so I'd have to go all the way, or you don't effect the C-22.

COPPERSMITH: How about--I hate to even bring it up--how about galvanic protection of the CRM?

SHOESMITH: You would bring that up.

COPPERSMITH: That was a potential benefit of this design, as I recall, and I think everyone agrees there is a question of how long it is, or how short. Does anyone have any ideas about that? Should it be on the pro or the con or indifferent?

SHOESMITH: I think you should forget it.

COPPERSMITH: Tell me what you really think, David.

PAYER: From the concept of getting perhaps another approach on the wall, and that is creating the fuel, and the issue with the passive materials, both titanium and C-22, is there's a critical temperature range in which you're going to see localized corrosion. The upper extreme is where you get an aqueous environment. And the current project thinking I believe is that that's around 105 Centigrade, that because of the concentrated salts, you might get up to 105 Centigrade.

They don't think it's going to go higher than that. Again, that has to be tied up and documented and all, but that's reasonable, I think.

Then there's a lower temperature below which you get it, you cannot sustain crevice corrosion and attack. And so the issue then is with any materials, the titanium or C-22, when do you enter that critical temperature range and when do you get out of it. And if there was a repository design such that when you actually got wet, you were below that temperature range, then you're home free. You know, I mean the reality is if we could put these bundles in C-22 and lay them on the desert floor until they cooled off, then you could slide them down. The worry is what's going to happen to the local environment after we bury them.

COPPERSMITH: Well, there's an alternative design. I think it's a very real possibility.

PAYER: I'm serious, you put them in or you ventilate them for that time period or you do whatever, but the point is that for these materials, for those passive materials, the name of the game is when do they enter that critical temperature range and how is it. And if we can show by the environmental analysis that the critical temperature is greater than 100 degrees for the materials, then we're home free already.

SHOESMITH: Just on the temperature threshold issue, I agree with Joe completely. In fact, to me, this makes carbon steel not look too bad. If you look at these temperature criteria, the maximum temperature which you can support aqueous corrosion, according to these calculations, is about 105. the measured threshold below which you should not crevice corrode C-22 is 102. So there's a three degree range there.

Now, to be conservative, we accept that maybe you can go down to 80 degrees and still initiate localized corrosion of C-22, because 80 degrees is where the C-276 sits on that susceptibility. So this is a very narrow window of aqueous conditions for a very narrow range of temperatures for which the carbon steel has to do its job. So as a corrosion barrier point of view, this is not a massive requirement.

STREICHER; But those temperatures were all determined in relatively short term laboratory tests.

SHOESMITH: Yeah, they were, I agree with you. But they were obtained under conditions which are more conservative than those that we believe are sustainable on the C-22.

STREICHER: But they're not really based on, let's say thermodynamic values.

PAYER: They've been done two ways. One looking at the initiation of the problem. But they've also been done, my understanding is, where you initiate the problem and then come back to these conditions and show that it arrests. And so it's not just waiting--you know, if you're just looking at initiation and you run a test for 40 days, you don't know if it would have started in 41 days. But if you start crevice corrosion in these materials under very aggressive conditions and then drop the temperature back, or drop the environment back and it arrests, I think that does tell you something more important.

I think that the trick in this is going to be to show that the crevice corrosion will not sustain, will not propagate. I don't think--I agree with your comment. I don't think you can persuade people that it will not initiate, because you can't run a test long enough.

BULLEN: But, Joe, were those corrosion conditions where you set the temperatures done under very aggressive conditions, including ferric ions? And so if we preclude the ferric ions, do we care if we're at 104 degrees C. for a while? I mean, without ferric chloride there, can't I have my C-22 sitting at 104 degrees for a long time and not have a big--

PAYER: Oh, yeah, that's the issue. I mean, you have to go to very--

BULLEN: Right. But if I can't get away from being at 100 degrees because I've got hot stuff, then why don't I get rid of the stuff that makes it worse, which is adding ferric chloride to it? So stick to C-22 on the outside is what I'm getting at.

PAYER; Well, if you take the steel out, it gets some of that answered, but it doesn't answer the whole question. You still have to answer what kind of environments could you get at a C-22/C-22 crevice or of rock/C-22. And that work still remains to be done. What I'm saying here today is my best guess and everything, but that certainly, the experiments have to be done to tie that.

COPPERSMITH: And I think you should remember that when we talk about the research needs. I think the other part, though, is that defining the windows of susceptibility, not only when you go into a point where you have a finite probability of initiating localized corrosion, but when do you slide out? What are the stifling criteria? Which of course is very different from initiation. But if you guys don't identify it, I'll identify it. It seems to me that for someone who just knows nothing about the problem other than the fact that there's a beginning, there's three states. You have initiation, you have propagation, and you have stifling. The definition of the stifling, in terms of criteria for, say, C-22 or these other alloys, is critical.

I mean, one of the things that we might have going for us in the repository is cooling with time, and it would be nice to be able to take some credit or know that in fact you might slide out of a window of susceptibility and lead to, like I said, propagation doesn't continue. That right now is not being accounted for other than just the general shape of CRM growth laws.

SAGÜÉS: I have another issue. But even though given if the pH wouldn't get to a value low enough to promote pitting, what could happen to things such as the rate of passive dissolution? Would that be--could, for example, that jack up the potential enough that the passive dissolution rate for the C-22 would become, say, an order of magnitude or two orders of magnitude faster than in a more benign environment?

MACDONALD: The rate of dissolution of oxides is generally--has a kinetic order with respect to hydrogen ions between 1 1/2. Okay? .5 to 1. So at the extreme case, it increases by an order of magnitude for every pH unit you go down. And the rate at which general occurs is governed by the rate at which you dissolve the passive film. In the steady state, the rate of formation of the barrier layer--I shouldn't say passive film, I should say the barrier layer--has to be matched by the rate of dissolution of the oxide. So if you increase the rate of dissolution of the oxide by an order of magnitude, you increase the rate of penetration of the film into the metal by an order of magnitude.

PAYER: What would you peg the passive dissolution rate at, or oxide dissolution of a pH 2.5?

MACDONALD: I've never looked at it for C-22, but for nickel, it turns out to be roughly about an angstrom per 10 seconds.

SHOESMITH: That's a handy unit. At least it wasn't for a fortnight or something.

MACDONALD: It's about an atom per every ten second for the nickel at around about pH 2.1.

PAYER: We've seen some data that suggests that the corrosion rate of C-22 when it's passive is on the order of a micron or a tenth of a micron per year, which gives you thousands of years per centimeter, which gets you into the kinds of lifetimes that we're really trying to get at. And so another given here, I think, is if you use these highly corrosion resistant metals, and they remain passive, then you can look somebody in the eye and say that those types of lives, thousands of years, tens of thousands of years, are possible. The trick is will they remain passive.

MACDONALD: Joe, what's the environment that you quoted?

PAYER: Where it remained passive. And certainly not what Alberto is showing with the--it wasn't a 10 per cent ferric chloride.

MACDONALD: Wouldn't that depend on, for instance, the acid and the temperature can be passive in all of those solutions?

PAYER: To some extent it does, but I still think you're down in a range, even short-term electrochemical tests, if you convert just the passive current densities, and I would guess in a longer term test, that those would continue to drop back, they still give you this micron per year, or tenth of a micron.

COPPERSMITH: Could we go to number three? The CRM, or C-22 put on the outside, put carbon steel on the inside.

SAGÜÉS; Well, do we feel that that's a good idea, I guess.

COPPERSMITH: Which one; the zero is a good idea?

SAGÜÉS: No, I mean say three versus zero, do you think it's--

COPPERSMITH: Well, let's draw that conclusion at the end after we've heard the pros and cons, and then see what--how good it was. Okay, John?

KESSLER: In terms of mechanisms for the inside/out scenario, if we've got these disaster scenarios of the expansion of the corrosion products on the outside crushing the inner container, what about the case of where we've got a hole that gets into the center of the container and it fills up with water and now we've got expansion all into the center?

PAYER: That would be a disaster again. The benefit of that would be--you know, we're doing a material selection here and we're only focusing on corrosion resistance, and that's the danger. You've still got to be able to move these things. You've got to be able to fabricate them. If self-shielding really was a good idea and a necessity, then you can build a thicker carbon steel inside and put the thinner corrosion resistant metal on the outside, and you're betting that that corrosion resistant--you don't have redundancy from the corrosion standpoint particularly. If you penetrate that outer layer, then you're in trouble. But you get the mechanical integrity, the shielding, and you get the toughness type of thing, but you don't have this idea of a double barrier from a corrosion--I don't believe.

KESSLER: I don't like the idea of penetrating the outer layer.

PAYER: I don't either. I mean, that's why I would not prefer that option. But, again, I don't know that we can get the mechanical needs that we have with these very large canisters. It just doesn't seem to me to make sense to make a five centimeter thick C-22 and two centimeter thick titanium. I think that's just not going to happen.

BULLEN: That's way too expensive. But I guess the question that I have for you is if I've got two centimeters of C-22 on the outside of steel, and I actually put a pit through it, how long do I have before I pop the can, I guess is the question?

PAYER: I think a long, long time. But how do you prove that? The things get very uncertain when we start talking about fluid flow.

BULLEN: I've got to get water in the hole--

PAYER: And what's that pit going to look like? Is it going to be a one millimeter straight shot through there, or is it going to go like a lot of crevice corrosion, and I don't know the answers to all that, and I don't know that even bright people can do the experiments.

BULLEN: You're implying that the people just didn't look that bright, so that's--

COPPERSMITH: Well, you can direct it this way. All I give you is the water and the rock. You've got to handle the rest. Where are the benefits? I mean, I guess the idea of we have--one thing we haven't really culled out is sort of this idea of multiple barriers, sort of defense in depth. We want to have this, and once it goes through that, you have another wall of protection, and then that goes through that, and then we now have either cladding or some other way we can resist radionuclide release, and the idea is that in fact we'd like to have multiple barriers. I mean, this is what we're sort of saying, not only from a licensability point of view, the what if case, and you can say, well, if that happens, we've got this. If that happens, we've got this.

PAYER: There has been an argument that thick iron oxide corrosion products would retard radionuclide transport. I've heard that argument. I haven't seen anybody prove it, and I hear people that know those things say it would be very difficult to prove, trying to predict what phases are stable and how they're going to do. If that was true, then that could become the second barrier. It's not a penetration barrier. It's going to slow down corroding before you get it, but if it had a significant retardation then on the radionuclide release, then you do have a philosophy of a multiple barrier. Again, I think it would be very--it's going to be difficult to prove that.

SHOESMITH: The radionuclide transport issue, it's key for one radionuclide, and that's technetium. But all the tests that we did show that effectively, the technetium will reduce on the iron oxide surface, but it very rapidly just passivates the surface. So we could never get a large capacity for reduction of technetium on the iron oxide. It will do it, but it didn't seem to do it for very long. That was a very rudimentary test. It wasn't really a long-term one. But I think it's specific for that one in particular. It would not stop iodine, for instance.

PARIZEK: Do we get all the junk out of it, all the corrosion products that are colloids--I mean, that was the thing that I think, Michael, you were talking about all of this mass, does it stay there?

COPPERSMITH: I don't understand the question.

PARIZEK: Well, are we going to generate colloids that migrate and move the materials into--

COPPERSMITH: From the iron oxides?

PARIZEK: Yes.

COPPERSMITH: I know you're not a colloid expert, but others on the waste form panel--some of the iron oxide hydroxides are good potential colloids themselves, that obviously when you deal with the glass issue, it's an abundant opportunity to--

PARIZEK: I think they would be scavengers for things that we don't want to be moving into groundwater. So that could be a down side.

COPPERSMITH: But, again, I haven't heard of any--given the internals, and again we may, by consolidation, get rid of some of the other materials inside, but in general, there seems to be ample opportunity for colloid mobilization within the package. The name of the game then is transport into the near field. So it's, from everything that I've seen, in fact there's just ample opportunity to mobilize. I'm not sure that any of the designs will mitigate that. We might think about it.

KESSLER; The results in terms of colloid generation from the spent fuel and the glass was that, I think this is John Bates' work, was that more than 50 per cent of the glass can be colloidal when it decomposes, and a good chunk of the-

-no, more than 90 per cent of the glass and more than 50 per cent of UO2 can be colloidal. And I know that LANL is carrying along some colloid experiments both on clays and iron oxyhydroxides, and there's talk of actually going to look for the decomposition colloids from the glass and spent fuel as well. So they're looking at all of that, at least they're planning to.

COPPERSMITH: So this design of having carbon steel on the inside could, you know, potentially be a source of colloids. Is that the point?

PARIZEK"; That is what I was wondering. It was just another problem.

COPPERSMITH: Yeah. There's also all the internal support structure, you know, there's a lot of other opportunities.

What are the other pros and cons of that option?

SHOESMITH: Just about everybody else's container or waste package design has carbon steel on the inside. The Canadian one does, the Swedish one does, the Swiss one does, the German one does. But they all avoid this discussion of volume expansion. They don't want to take--they don't want to think about that one. What they really want is--it doesn't apply here--but what they really want is iron-2 to be around to soak up the radiolysis products to stop the fuel reacting. So they want their cake and eat it. This question of volume expansion does not enter the discussion.

COPPERSMITH: Is that because of the conditions are always reducing or--

SHOESMITH: No, it will still react because it's unstable in water. It's a base metal. But it just hasn't received that much discussion. It's just a subject that's been avoided. I'm not quite sure why.

COPPERSMITH: Have any analyses been done on stresses? Has the project done any analyses on stresses?

SHOESMITH: Not on stresses. Some work on chemical reactivity at Harwell.

COPPERSMITH: Any on this type of design? Do you want to report that briefly?

DOERING: Tom Doering with Waste Package Operations.

We have done about a year ago, we looked into what kind of stresses does it take to really collapse the C-22 inward, and I don't know the number specifically, but it was a very, very high number. Again, C-22 is a very, very ductile material, has a high strength on top of that, so it takes a lot of force to get it on to where you start moving, and it essentially has no--there's no critical flaw--so we couldn't make it go with essentially reducing or looking at all the forces that we would anticipate seeing. Some of that information I would need--or have heard here we need to go back and revisit a little bit of that. But also I challenge

I guess the Board, what kind of forces are we looking at so I can put it back onto the waste package? Again, it's a cylinder. A cylinder is very good with stresses also. So it's something that we'd like to take back a little bit. So we saw very high stress resistance on the C-22.

PAYER: I think my suggestion would be to look at, if you were looking at hydrostatic stresses on a cylinder, I think they would be very high. Most likely, these will be more like point loads because the corrosion products aren't going to grow uniformly around, and so you'll have something that's pushing over, you know, like this much of the cylinder at 45,000 psi or so. It will go to the yield stress of either the steel or the--

DOERING: We looked at the hydrostatic plus the point loading. Remember, we do have a basket structure on the inside that can take some load either with and without the basket structure internally also. And so we did it both ways on that. But if we had some different numbers, I'd be glad to take them back and rerun those calculations and see what we have.

BULLEN: Tom, just a point on the C-22. What's the fracture strength? How ductile is it?

DOERING: Ductility, the elongation before it fails is around 44 per cent. And so--

BULLEN: Pretty big load on the inside and not break it?

DOERING: Yeah, we can really move it.

PAYER: But it's not moving against things--again, and I'm not arguing, I think those are the exact kinds of things that have to be looked at, if you can convince yourself, but you're going to be crushing this can in different ways, you're going to move it more--I guess more the strain, I mean, you're going to put two to four inches of strain, and look at what effects that might have on the ends, near the ends, near the welds. If it pushes and there is some internal support, are you going to, you know, cut through the can? Those kinds of things, you know, the good news, bad news kind of thing. And if it can do all this, my experience is on 602, which are much thinner walled, and you know about that as well, they crush, but you're also moving this inch thick support plate around.

DOERING: The steam generator difficulty, I mean, we have received--

PAYER: But the others then that pack out on bridges and construction.

DOERING: And most of the information that we want to take in and re-evaluate, so if we do get some referencable numbers in different areas, I'd be glad to take them back. We have structural engineers waiting to do this.

COPPERSMITH: They should be working on the VA. Actually, I should be working on the VA.

Is there something about the geometry of the carbon steel on the inside that would lead to a greater problem of this volume expansion and compromise of structural integrity?

SHOESMITH: Less space for it all to go. There's not that much inside the waste package. On the outside, it can flake.

COPPERSMITH: It can flake, yeah.

SHOESMITH: There's still the denting issue, but there is space outside. There's not a lot inside.

KESSLER: But I thought that the issue was is that it can develop a pit, and then you've got it on the inside expanding while you've still got the structural integrity to keep the mild steel, there's enough there so that it crushes from the inside.

SHOESMITH: Yes.

STREICHER; That would be different if you're casting.

COPPERSMITH: Any other advantages to this design?

SAGÜÉS: There are a couple of issues that would have to be resolved. One of them would be the question of the supports, on what this thing would lay. What do you do? And right now, the supports are supposed to be carbon steel or cast iron.

COPPERSMITH: I think three pedestals per canister of carbon steel.

SAGÜÉS: Now, the question would be then in terms of C-

22 supports, would there be crevices between the supports on the bottom, or should one use welded legs on the bottom of these containers, and then use the legs as a corrosion allowance? Now, you get into a number of questions that, you know, then the thing may not be rotated around, which now it can be.

BULLEN: If you change that design, wouldn't you just make the supports out of C-22, so you have a C-22/C-22 crevice, which would be pretty passive wouldn't it?

SAGÜÉS: Well, no, actually a C-22 to C-22 crevice would be more of a problem than a mixed metal crevice.

BULLEN: Is it the chromium that gets you--

SAGÜÉS: Because in that case, you're going to have the same theory of chemistry on both sides. The mixed crevice is likely to fail on the side of the less corrosion resistant metal. But a metal to metal crevice could conceivably be-- you could get a very tight crevice that way that you couldn't get otherwise. So that could be a question that would have to be resolved.

STREICHER; Didn't you suggest welding it?

SAGÜÉS: Yeah. For example, I'm just saying, you know, one could use thick welded lids against the outside, and then use the legs as a corrosion allowance, you know, say six inch legs.

STREICHER; Weld the crevice on the container?

SAGÜÉS; Uh-huh. Right. The only crevice would exist between the bottom of the legs and the rock of the concrete underneath.

COPPERSMITH: there's going to be a crevice somewhere.

SAGÜÉS: Yeah, but in that case, it would be at the end of a very long corrosion resistant metal corrosion allowance.

COPPERSMITH: Now, you don't hear much talk about the pedestals. The waste package experts thought about the fact they were all carbon steel and it basically wouldn't be long before the package was laying on the invert, which, you know, again provides for moisture and everything else, an opportunity for the continual wetting along the bottom as well.

MACDONALD: Is the design of this system flexible enough to put the canisters into bore holes in the floor?

COPPERSMITH: Vertically emplaced bore holes, let's see, where have I heard that before? That has been an option.

MACDONALD: The premise of having a dry storage system of course--and so what if we were to drill holes in the floor, put the canisters in, put a layer of bentonite around the canisters and allow the bentonite to swell, and in a sense, we hermetically seal the--

COPPERSMITH: How do you maintain a moisture condition?

MACDONALD: Sir?

COPPERSMITH: How do you maintain a moisture condition to keep the bentonite wet?

MACDONALD: There's enough moisture around.

KESSLER: It would be hardly a problem if the bentonite dried out.

COPPERSMITH: Well, I'm not sure. I mean, you know, certainly during the early stages, it depends on the thermal scenario, but right now, it's pretty hot and dry early on.

PARIZEK: And that's good. Then there's no corrosion then.

COPPERSMITH: Right. But are we at a point now where we say that the entire repository--I mean, flux is high enough now that it keeps it all wet and keeps the bentonite nice and tight, as you would in a saturated type system?

PARIZEK: You've got all those drips. You've got all that roof water coming down.

COPPERSMITH: Well, I guess I'm getting a different picture of the frequency of these waste canisters.

PAYER: It's only wet when it hurts us.

COPPERSMITH: Yeah, when it comes to this case, it will be perfectly dry. That's something to think about, though.

PARIZEK; At high temperature, one thing if the canister temperatures are too high, then the bentonite isn't really bentonite any more.

COPPERSMITH: I'm sorry?

PARIZEK: The bentonite won't be bentonite any more.

MACDONALD: That's true.

PARIZEK: If you don't have a high temperature--

MACDONALD: You'd need something else to seal it.

PARIZEK: Well, just you have to pool the waste first.

COPPERSMITH: Any other pros and cons with this inside/out or outside/in? I guess what started it was the idea of having your first line of defense be the strongest. Basically, envisioning our environmental scenarios, picture number one or whatever the high temperature, ultimately, there's going to be a period where it cools off enough there's going to be dripping down onto the top, onto C-22, let's say now, immediate evaporation, concentration of solutions, maybe intermittent dripping. Are all those types of scenarios okay in this? Are we in better shape with this type of design?

SHOESMITH: It's not clear to me that there's any increase in waste package lifetime by just inverting the two barriers.

SAGÜÉS: I would like to make a little sketch there, if I may that may be addressing this.

So here is time; here is damage, damage measured in terms of maybe the maximum size of the opening of the container or something. And for design number zero, the carbon steel over C-22, we could imagine that the carbon steel deteriorates pretty fast, that it does something like this as a function of time. And then we get to the C-22, and at that moment, for a long time, there is no damage in the form of penetration of the container and the like, and then if there is something--the first penetration happens. It started here when the carbon steel was worn out, and ended here when the C-22 developed the first through pit, for example.

In that case now, the pit gets bigger and maybe other pits originate, maybe like this. And then so this would be for the design zero.

And then for design number three, we will have conceivably something like this, assuming that there was no particular deleterious effect of the corrosion products of the carbon steel on the C-22, we will have in that case something like this. Then at this moment, we have the same distance from there, and at this moment, the deterioration of the inside begins. But if we have a situation such as this swelling of corrosion products inside, we may end up with this kind of a damage. That certainly will be a con against the case number three. So this would be 3-A, for example.

KESSLER: I thought one of the major objections to the current base case was this idea that you've got this ferric chloride, and now suddenly you're assuming that there's no difference in behavior.

SAGÜÉS: That's correct. And this is case three.

However, if there's a difference in behavior because now we don't have these things that caused this thing to be very short, well maybe the behavior will be a case 3-B, which may be something like that. And that would be the 3-B. I think this would be a very simplified view of what I think we're up against. Because of the fast deterioration of the carbon steel, instead of having something that would have lasted a long time, you have something that lasted only a moderate amount of time.

PAYER: What you really lose there is the multiple barrier concept. If you've got the carbon steel on the outside, you get credit for it during the dry period, and then whatever time period you plug in for when it gets wet, and then it goes and you start going on C-22. On this concept, either 3-A or 3-B, you're betting the farm on the C-22, and there is no significant multiple barrier.

KESSLER: I thought we're already betting the farm on the base case design with C-22.

PAYER: We are. In the base case design, you can still take credit for the carbon steel as providing the mechanical, the shielding--

KESSLER: But you can do it--

PAYER: No, you can't.

SAGÜÉS: Maybe of course the swelling is not such a problem.

COPPERSMITH: Take his pens away. Again, the reason to go through this is to see whether or not there are some research needs, in other words, some ways of distinguishing and dealing with that. For example, it seems to me--again, I'm just a geologist--it seems to me there should be some ways of doing stress calculations for this, given the volume expansion and its time history, for the carbon steel on the inside, and look at stresses as they would develop as a function of time.

PAYER: I think they're strain calculations, not stress calculations. Well, the stuff is going to move.

COPPERSMITH: And that's fine if strain is what you're worried about. But, I mean, right now the issue--I get the same feeling, and maybe this is the way the Canadian--I don't know if others have a single wall, single type of alloy, and it essentially is a function that is the--basically, the name of the game is how far out it will be before you finally get penetration and ultimately collapse, I guess in the Canadian case.

STREICHER; But it's the C-22 that's going to move; right? Not the carbon steel.

COPPERSMITH: In terms of this design, design number three?

STREICHER: Yeah. If the C-22 is penetrated and there are iron corrosion products, that steel is going to be so firm because of thickness, that if there's movement, ductility in the C-22, that's the part that's going to move.

SHOESMITH: That's a very good point. There's a potential to avoid the wedging stress on the outside if the carbon steel is on the outside, because you'll always be thinning the carbon steel faster than you're thinning the C-22. So you might get away with it on the outside. But on the inside, you're right, it may be almost impossible to escape that build-up.

COPPERSMITH: Are there any other benefits or cons to that?

SHOESMITH: One specific advantage for having the carbon steel on the outside is that you do define it not only as a shielding, a mechanical barrier, but as a corrosion barrier. If you move it to the inside, it no longer has a corrosion function. Its function on the outside is just its ability to get C-22 through the dangerous temperature zone. So there is a definable corrosion reason for having it there. When you move it to the inside, it is now only shielding, mechanical.

COPPERSMITH: It won't come into play until after that thermal presumably is over.

SHOESMITH: Yeah. That's true.

PAYER: Steel has no lower temperature limit--it continues under the ambient temperature conditions.

COPPERSMITH: But in scenario one that Carl had, presumably it would do well--

STREICHER: Presumably, if the C-22 lasts long enough, the temperature may be low enough so that the carbon steel is not going to corrode that much.

PAYER: Even at ambient temperature, though, the thermodynamic stable form--

COPPERSMITH: And ambient is around 35, 36 degrees, something like that?

PAYER: It would go slower certainly, but it's going to continue to corrode.

SAGÜÉS: How about the matter of the carbon steel, a pinhole penetration of the C-22, how about just simply the corrosion products plugging up the hole?

PAYER: I don't think that works.

SAGÜÉS: Why not?

PAYER: Corrosion in the pack-out condition continues to operate. The corrosion products aren't so dense that they in fact form a plug. I just don't think it's been demonstrated that corrosion products stifle further corrosion, or pits or cracks would stop.

SAGÜÉS: However, this could be tested experimentally in a relatively short time. But still one could very easily check that condition.

PAYER: I think you could run some experiments. I don't think it would be easy, and I'm not sure it would be convincing. But it would be an approach to take, and there has been talk of looking at the effect of corrosion products in pits. I don't think they're easy experiments to run.

COPPERSMITH: One of the things to think about that I have and I'm not aware of for having the corrosion resistant materials on the outside is that it's likely that rocks will come in within a few thousand years and they will be in contact with the waste package, and whether or not that would have an effect on the development of crevices or occluded sites or other things that would affect the corrosion of these types of resistant materials on the outside. That's something to think about.

BULLEN: One of the scenarios that we saw in the original VA, or the VA base case, was that the outer barrier was corroding away, and so the container was not susceptible to rock falls for, you know, 20, 30, 40, 50,000 years, but then all of a sudden, it was. If you've got the corrosion resistant barrier on the outside with rocks falling on it, you don't really care. I mean, it's not going to penetrate--

COPPERSMITH: Well, we were thinking about contact, there will be a lot of contact with the package. Whether or not that matters, I don't know.

BULLEN: So you're just talking about localized corrosion sites?

COPPERSMITH: Exactly.

BULLEN: On the outside. but you could preclude, or at least you could allegedly preclude the fact that rocks falling on it would not harm the container.

COPPERSMITH: That's a good point. Yeah. Mechanical activity actually could be longer having the carbon steel on the inside.

PAYER: The belief is that a rock with a C-22 or a rock titanium crevice could not be tight enough for chemistry that would sustain crevice corrosion. That belief is based on some ancillary data. But, again, it has to be proven out under these conditions with this rock.

BULLEN: You just lead me to a follow on question now, Joe. If I put the C-22 on the outside, how does it fail?

PAYER: Passive fill corrosion, one-tenth of a micron per year penetration.

BULLEN: And so two centimeters is going to last me a couple hundred thousand years?

PAYER: Well, it's not going to be perfectly uniform, but yeah, it will last you a hundred thousand years.

BULLEN: And so how do I do any better than that?

PAYER: You don't need to do any better than that.

BULLEN: Okay.

PAYER: You've got to protect it mechanically and you've got to protect it against earthquakes and you've got to protect it against people drilling through it, human intrusion and all, but that's what I say, if you believe that a passive material can be selected that will remain passive under the conditions in the repository, then it's not ludicrous to think that you can get 10,000 plus year life.

BULLEN: Then a follow on question is that Dave is telling me that I can put in .625 millimeters of titanium?

SAGÜÉS: No, 6.15.

BULLEN: No, .625 is what he said.

SHOESMITH: My point there was that if, let's say, two centimeters is going to get you ten to the five years, five times--let's go two times ten to the five years, then one centimeter is going to get you one times ten to the five, and wall thickness is not a huge criteria for those very slow rates.

PAYER: If you're hauling this around, can you rip through--it becomes a different design criteria now. But from a corrosion allowance, I think that's quite rational. These materials show that there's a real promise to demonstrate that.

NELSON: Can you manufacture something that thin?

BULLEN: The question that I have then is why don't I putt two corrosion resistant barriers on the outside of a structurally robust inner barrier, and use those two mechanisms as my multiple redundant barriers, titanium and C-22 or whatever, and then my beefy shielding and impact resistance, which I'd like to preserve for a long time is whatever I've got, cast iron, steel, whatever is on the inside.

PAYER: Number seven or eight or nine?

BULLEN: Just asking the question as to--

PAYER: I've got to think that the economics would start pushing in that favor as opposed to going to five centimeters of C-22.

BULLEN: Oh, I agree. Five centimeters of C-22 sounds ludicrous if I'm a taxpayer.

PAYER: This guy from Haynes, where is he?

SHOESMITH: He already went out and bought a new--

COPPERSMITH: Okay, we're going to take a break here in a minute when we finish this particular design. We'll take a break. But I want to go back to the question I guess maybe David raised, was what is the difference between zero and three? You know, in both cases, you've got C-22 there. C-22 and passive dissolutions, the expected behavior under the expected conditions, then there's no problem. But it seems to me that we're not dealing with just the expected. I think the issues of the development potentially of ferric chloride and localized aggressive conditions, and so on, that's what we're talking about, and that's what leads to a difference in potentially the attack of option zero. Because otherwise, option zero is driven by passive dissolution, too, and there's no difference, the difference between maybe 250,000 years and 235,000. I mean, it doesn't seem like there's--maybe with the mechanical integrity better with the carbon steel inside. But otherwise, it's the--I think the real difference, and maybe it has to do with how you study them ultimately for research, is the fact does zero cause, like Alberto asked, cause a more--the potential for a more localized aggressive condition that you simply would not get in number three? Because otherwise, they're going to be very similar in terms of the expected condition, expected behavior and expected corrosion rate, or waste package lifetime.

PAYER: There's other issues that get more into the details of fabrication and things. But option zero has the shrink fit between the outer and the inner, which is going to result in significant residual stresses on the inner liner that you wouldn't have perhaps if it was an outer liner or another situation. There's a lot of devil in the details here of just how you put these canisters together and can you heat treat and can you solution anneal. But the issue of shrink fit, which is in the base reference case now, shrink fitting the carbon steel outer layer onto the inner layer, leaves residual stresses that have to be accounted for and dealt with, and they're detrimental.

COPPERSMITH: But couldn't there be a zero that doesn't have that type of fit, a loose fit type of design, same material.

PAYER: No. We could make it one of our options. As I understand it now, that's not--

COPPERSMITH: No, it's not in the base case, but potentially--

KESSLER: So those residual stresses are higher than the weld stresses?

COPPERSMITH: Any other comments on number three?

SHOESMITH: Yeah, I think you are right that you should not get the ferric chloride environment on the outside if the carbon steel is on the inside. But this performance assessment requires that behavior inside be predicted subsequently in some detail. I think the fear even with scenario 3-B is that you would have to start asking questions like if the carbon steel corrodes and expands, which will give first? Will the cladding give first and expose the fuel, or will the remnant C-22 give, and the answer you come up with is most likely to be the cladding, and that you open up the fuel to exposure. So you have an unpredictable scenario inside the container if you put the carbon steel on the inside. And I think that's, just because of an inability to predict what will happen is probably something that should be avoided.

KESSLER: But it's the inability to predict what will happen is why people don't like carbon steel on the outside.

SHOESMITH: Yes.

COPPERSMITH: There's degrees of inability, I guess.

SHOESMITH: Yes. If you're going to have an argument, have it outside the front door, not in the house.

BULLEN: Now, we do design two after the break; right?

COPPERSMITH: Yeah. I want to take a break right now, 15 short minutes. Right now, I've got 3:10. So at 3:25, let's reconvene here. I'll be clapping my hands.

(Whereupon, a brief recess was taken.)

COPPERSMITH: I think this is a very useful, fruitful discussion. Normally, the workshops that I'm involved in we have about 30 speakers, we have an inordinately short period of time to get everyone to finish talking. We have long winded, especially waste package experts. I remember one particular night at 7:10 p.m.--one night John Scully was giving a 20 minute talk and he was on minute 79, I think, 7:10 p.m., there were a few people, scattered people in the audience, and he was sliding in and out of a window of susceptibility, and we finally had to just wrap him up and say, John, I think you've just got to finish it. This is it. And he was almost done with his lecture, you know, five more viewgraphs.

Okay, what we're going to do now, as you went through this, we went through a pros and cons discussion that went very quickly. Some of the people on the panel mentioned that it would be useful for them when we get to research needs for different conceptual designs, if they had remembered some of those pros and cons. Unfortunately, Carl wasn't able, even though he's a very able scribe, wasn't able to get down all the pros and cons, so I thought we would quickly look back at design zero and design three and just quickly write down the pros and cons so we would have those to carry with us, and we'll try to do that for all the designs as we go through.

Does anyone remember any pros to zero? I was going to ask Dave Stahl if he had any, but he's not here.

PAYER: Self-shielding aspect, cost.

COPPERSMITH: I think the idea of multiple barriers.

BULLEN: Multiple barriers, yeah, because you had two barriers, and you could take some credit for the carbon steel.

COPPERSMITH: Right, from a corrosion point of view.

MC CRIGHT: The carbon steel got you through whatever may be the danger temperature range for the C-22?

COPPERSMITH: That's right, during that period, it's actually corrosion resistant, particularly in the dry oxidation period for killer scenario one. Strength, mechanical. Other? Retardation of radionuclides?

Okay, the point was there may be a retardation of radionuclides transporting through corrosion products from the carbon steel. And, again, Joe asked the question what's that based on, and we need to consider some of the data that would relate to that.

PAYER: I would put that in quotes just from what I heard Dave say that it might be used up quicker than what you would think.

COPPERSMITH: Yeah. You know, you asked that question a few times in the waste package elicitation. Did you ever get an answer in terms of whether or not we'd expect or is that something you can take credit for in terms of the transport process through, say, a thick, a nine centimeter thick corrosion product layer, or maybe one that's not as thick?

SHOESMITH: No, I didn't. But it's clear I think from the TSPA calculations that there's not much that they can't be taking credit for if it's technetium, because it's moving through pretty fast. But, no, I never worked out whether--exactly what the thinking was with regard to those corrosion products, except insofar as they acted as a diffusion barrier, not an absorption barrier.

COPPERSMITH: I was going to say some of the--I don't know if anyone here did the modelling of the diffusion gradients through the--nuclides on the way out, but I think in general, it's a high gradient.

KESSLER: That's helps if it's there. But I thought that Joe Payer had just said earlier today that it was going

to--you were only going to build up a couple millimeter layer
and then it was going to slough off.

COPPERSMITH: Well, that's the issue of the spalling.
So that's up in the air. When we get to this pro, this might
be a case where we need to look more, at these temperatures,
what--you know, how tenacious is the corrosion product layer.
Is it prone to spalling and so on.

There was a split level within the waste package.
We asked that given these temperatures, we had a spread from
those who said it's going to be highly tenacious and it's
going to stay there a long time, and those who said it's
going to spall very easily.

PAYER: Another advantage of this is the fabrication
ability. It's readily welded, formed.

COPPERSMITH: Any other benefits? Do you think you got
most of those, Carl?>

DI BELLA: Let me read them back to you. Mechanical
strength, shielding, cost, multiple barriers, get through the
dangerous temperature range, enhanced retardation in quotes.

COPPERSMITH: Alleged galvanic protection. How about
cons to design zero?

SHOESMITH: Ferric chloride. How much that's going to
affect it.

COPPERSMITH: Certainly the source of ferrous ion.
Okay, other cons?

PAYER: The large non-uniform expansion on corrosion, volume expansion.

COPPERSMITH: And is that as it relates to integrity of the inner barrier or just the spalling?

PAYER: Either. Wherever it corrodes, it's going to have underneath the ceramic coating, it's going to have a large volume expansion and crack off the ceramic coating if it starts to corrode.

COPPERSMITH: In fact, I wanted some, you know, version or option of these where we deal with ceramic. I'm not sure if everyone here is comfortable with that. But if I few are, I think it would be a good opportunity to talk about it.

BULLEN: Degradation of waste package with respect to rockfall as a function of time is a deleterious event.

COPPERSMITH: So later rockfalls.

BULLEN: Rockfalls have an impact as opposed to late rockfalls in the other design may not have an impact.

STREICHER: Did somebody say large volume of corrosion product?

COPPERSMITH: Volume expansion. Yeah, the inquisitive look was in the case where you've got carbon steel inside, that hangs around in terms of mechanical stability for a long time until the C-22 finally goes.

PAYER: I think it's life when it's wet; your dripping is measured in tens of years.

COPPERSMITH: The general issue, even though I'd like to elicit that distribution on corrosion rate, is the issue that that's--in other words--

PAYER: No, it doesn't buy you much once it gets--under dripping conditions, it isn't much of a barrier at all. It's an excellent barrier when it's above 100 Centigrade.

COPPERSMITH: Okay.

BULLEN: I have one question to that, Joe. Is that an excellent barrier above 100 Centigrade if you're dripping on it at 100 Centigrade?

PAYER: Well, you won't be dripping on it at 100 Centigrade.

BULLEN: Why? I can drip on it if it's 100 Centigrade, can't it?

PAYER: You'd better not.

BULLEN: Well, I guess my basis for it is the large block experiment, you know, had fracture flow that could basically overcome any of the rock temperature. So, I mean, why can't I drip on it when it's 100 degrees?

PAYER: 105.

BULLEN: Oh, 105. Okay.

PAYER: No, once you have an aqueous phase on it, it's going to start to corrode.

MACDONALD: Just think of a hot plate and you're dripping water on it. You know, the bubbles are whistling around, you know, stuff is evaporating, you leave behind all sorts of crud. You know, the water is super heated, but it exists there because, you know, there's a finite rate at which the water can evaporate from the drop.

PAYER: The picture of how the drips fall onto the canister and the distribution of drips is an area of high uncertainty. but we do have a fairly uniform temperature in and around the drip. It's not like the canister is at 200 Centigrade or 100 Centigrade, and the drift wall is at 30. And I don't think--you know, I suppose you could picture piercing a perched water place and have a water fall, but that's not going to happen, I don't think. That's not the way the drips are, and there's going to be rocks that are dropped down there. You're not going to have a pristine iron surface. The drips are hitting a centimeter of iron oxide. They're going to be distributed and disbursed. I just don't think you're going to have a drip hitting steel more than twice. Because then it's going to hit iron oxide.

MACDONALD: The premise that you can't have, you know, a liquid phase on a surface at 105 degrees Centigrade I would--or above 105 degrees Centigrade, I would argue is false. Just go home and turn your burner on your hot plate and drip some water on it and have a look.

COPPERSMITH: It's transient; right?

MACDONALD: It will--bubbles will sit there, and it's quite an interesting physics problem as to why they quiver the way they do. But try it and see. This is for a long time because of the finite rate at which you can transfer the water molecules through the surface.

COPPERSMITH: Well, I think general modelling would say, at least the thermohydrologic modelling, you're not getting many drips, it's very difficult to overcome. It's the rock heat temperatures, too, that are very hot. So basically, the water that's coming down is staying away from the heated rock, not so much the package itself, and it can come through. So you have a condensate and you have basically a thermal boundary that ultimately will cool down and come down.

MACDONALD: But do we know the time over which the system exists where you can have liquid water coming?

COPPERSMITH: No, not well.

MACDONALD: I mean, from the ceiling.

COPPERSMITH: Not well, because maybe they develop sufficient condensate in the rock and have some very transmissive features from faults or other things, and it just roars on through and cools as it goes. So it's very uncertain, but I think the issue from the standpoint of this problem, it's not clear, I think what you're saying is that there's a dangerous period somewhere between boiling and maybe all the way down to 30 degrees where carbon steel just doesn't do well. That's where you're saying you're into ten, you know, maybe ten years for a ten centimeter thick wall, and that's pretty damn quick. I think that was the point he was making. I think the issue of localized attack or a more concentrated solution is a different one.

KESSLER: But I think that the issue here about how much above 100 C. you can get is this idea that is the carbon steel around long enough before you have dripping that you protected the C-22 by getting it down to this 103, 105 or all the way down to 80 temperature, below which then your C-22 is going to be more robust. And so the point is is that if the carbon steel hangs around above boiling and then disappears when you finally get down to boiling, and we have to define what boiling is, then maybe that's all we care about, just a matter of whether--I'm just trying to define why we're arguing back and forth about what the boiling point is here.

MACDONALD: But then why bother having the carbon steel if it's on the outside.

COPPERSMITH: Well, isn't it--I think it goes back to a comment I think Dan made earlier. This design works well for scenario one. This is the high heat. Even if it isn't exactly one, it's that type of thermal profile with a good bit of heat and dry conditions, low relative humidity for some period of time. And that's the period of time when dry oxidation is occurring in carbon steel at presumably slow rates. Then as it head down into or at somewhere near the boiling point and presumably down to much cooler temperature, then that's a period where the carbon steel begins to really do its thing. And then when we deal--what about scenario three? We're hovering around the boiling point. How does carbon steel work in that case, Joe?

PAYER: One other point before we leave zero is the microbial induced corrosion is more of a live issue, real issue, with carbon steel than the other deterrents. If you take carbon steel out of the system, you take a lot of the microbial issues off the platter as far as research you have to do.

SHOESMITH: I think if we're only interested in its function between about 105 and 80, I don't think you can actually rule out the microbes, but that's a fairly hostile environment, especially if you have concentrated salt in the drip water as well. So I think the feeling is that that's probably benign for MIC, but it's still arguable.

COPPERSMITH: Again, when we put Brenda Little through the--an MIC expert--through the course of this, we were looking basically at a scenario one type of scenario in terms of temperature, in terms of the moisture conditions are largely ones that are concentrated solutions, and subject to this mass inventory that Dave Stahl talked about, she felt that in fact this is not a conducive environment for MIC until you get well down below 100 degrees or so.

BULLEN: To get back to something that John Kessler said, I guess I have to be convinced that if I'm on scenario one, which says that the waste package surface is going to be a couple hundred degrees C., that it's always going to be dry in the waste package. Why can't I have a 500 year rain event that's going to gush water down the fractures and all of a sudden overcome the heating of the rock, cool it down and drop water on my hot container? Is that not a plausible scenario? If it is, then don't I have to worry about hot and wet at 200 C., just like I have to worry about hot and wet at 100 C.?

PAYER: I don't know that it's a plausible scenario. I don't know that it's a realistic extreme.

BULLEN: I guess the data that I get from the large block experiment makes me wonder about that, because I see something that's at 134 degrees C. that gets rained on, and everything homogenizes at 96 degrees C., which is suspiciously close to the boiling point of water.

PAYER: But that experiment's been done over a couple meters. And, you know, there's several connections there; what if it rains real hard for, you know--all those kinds of things. It's the 20 per cent ferric chloride issue. When you look closely at the 20 per cent ferric chloride, you cannot get that. And that's defensible on physical chemistry principles. I don't know the thermal hydrology well enough, but my guess is that what you said up there, it's not going to rain on these things when they're 200 Centigrade, that there's going to be enough rock mass that's heated up that you in fact cannot get enough flow through that. I don't know that, but I think we can't take that as a given and say that that's a realistic extreme condition.

COPPERSMITH: I was going to say there's a way out of this, is to identify I think it's identify research needs in another area. I guess this is a big thing and we should be sure that someone in the thermal hydrology program looks into that.

PAYER: It's not so much the rain issue as it's the mass of water that the boiling isotherm displaces. It goes up in the roof somewhere and cools down and then comes plunging down.

BULLEN: Right. But the question I have is what is that temperature? If you can guarantee that I heat the rock above 210 degrees C., that I will never, or very rarely, you know, one in a thousand or something, drip water on a hot package, then I'm looking at carbon steel on the outside as being pretty darned good. But until you can tell me that, then I've got to assume that carbon steel on the outside is going to be rusty if I drip water on it, and so if that's the case, then I'm not going to have it, even if it's hot. And so I guess I'm looking for the research need or the calculation that tells me that yes, indeed, if it's above 200 C., there's no way in God's green earth that you're going to get any water to the surface during the time where it's going to be susceptible to rust. But if you can't do that--

KESSLER; Well, maybe you can, but the time that it stays at the surface is so short that you don't care. I mean--

BULLEN: Yeah, but I care about intermittent a lot. I mean, if you take a look at corrosion in a wet/dry environment, it's a whole lot worse than if it's always wet or always dry. So I mean, that's not a plausible argument from my point of view. I don't want it to be intermittent wet and dry. I want it to be always wet or always dry, because then I can understand what's going on.

SHOESMITH: But how much damage do you do if the rest of this time of the water droplet is ten seconds?

BULLEN: Don't know that. That's right. Unless the water droplet is ten seconds every minute for a hundred years.

COPPERSMITH: These are good questions to ask a thermal hydrologist. My guess would be if this was a room of thermal hydrologists, they'd be jumping up and saying there's no way you're going to get water coming through.

BULLEN: I'm sure they would, and I want to see the experiment by models that tells me where the water is going and why, and then I'd be a happy person.

STREICHER; And there's moisture in the air. You don't have to have drips, I don't think.

COPPERSMITH: Well, that's right, and relative humidities will climb once those temperatures start going up. Any other cons to zero, so we can move on?

Oh, yeah, we talked about colloids. Colloid mobilization. Okay, let's go to number three. We invert things, the C-22 outer barrier and then carbon steel inside. What were the benefits of that? Longer mechanical integrity, presumably?

SAGÜÉS: Longer mechanical integrity.

BULLEN: Limited ferric chloride.

BULLEN: It's limited to the concentrated J-13 that would sizzle on the waste package. Do any of the benefits of the previous one directly correlate, Carl?

DI BELLA: Mechanical strength.

BULLEN: Just better here, probably.

DI BELLA: Shielding. Cost.

BULLEN: I don't think there's that much difference is there? I don't know.

DI BELLA: Multiple barriers.

BULLEN: Don't have multiple barriers in this one.

PAYER: Not from a corrosion point of view.

SAGÜÉS: By the way, are we still on the pros? No need for shrink fit, I guess.

PAYER: Why is that?

SAGÜÉS: With the C-22 outside? Well, you were saying there not being a need for shrink on the--

PAYER: My understanding is the need for the shrink fit is to keep the inner can from rattling around and busting out of the outer can if you drop it.

COPPERSMITH: Can someone address that, Dave or someone? I thought it was for--

PAYER: That was certainly stated early on, that when galvanic protection went away, the shrink fit remained because it was still necessary; is that true?

DOERING: C-22, or just basically shrink fit, what we're looking for basically there was a nice sort of a tight fit, as tight as we could get to help us out on not only the structural, but also thermal. If we start introducing gaps, we start getting a rise in temperature, even though they're small, it's hard to predict when it's going to occur.

Shrink fit actually, from what we've seen from last year, and also this year's, really does impose a minimum of stresses into it, and the stresses that we do impose, especially in the C-22 wall, compress the stresses, and if you're looking at stress corrosion cracking, actually compresses stresses actually help you instead of hurt you.

The tensile stresses are the areas that hurt you and stress corrosion cracking. So we felt at that point in time there were a lot of benefits. It's relatively inexpensive to do. It takes as much to set up a shrink fit as it does a loose fit. In fact, looking at a loose fit, making sure that we have the right geometry to make sure the internals don't spin in there, to make sure we can handle it and manipulate it without any large center of gravity shift on it, causes from our understanding, at least from Jerry Cogar, to cost more because now you're doing a machine surface, the tighter tolerance than you normally would do a shrink fit.

Shrink fit is relatively easy, do fly cuts if you have to and just heat one up and drop it over the other.

Loose fit, you'd have to actually go to machine surfaces and actually make stops so it doesn't rotate inside, which causes a little more machining and fabrication time.

So those are our thoughts and processes of it. I probably can go into more detail on it.

COPPERSMITH: Would you envision having--it's maybe an unfair question--but if you went to number three where you had the C-22 on the outside, carbon steel inside, would you envision a shrink fit process as a reasonable way to do this?

DOERING: At this point in time, yes, you could do the shrink fit on either way.

SAGÜÉS: What would be the stresses, which one has the expansion coefficient, the C-22 or the carbon steel?

DOERING: I'd have to take a look at that number to be specific. I want to say that the iron has the larger expansion, but let me go back and check that to make sure. I was very close--they're very close. They're very close as we stand right now, so it's probably in the third decimal place that it shifts.

SHOESMITH: Is it easier to do the closure weld on the C-22 when it's on the outside than on the inside?

COPPERSMITH: While you're there?

DOERING: Not really. The configuration is different. The nice thing about the C-22 on the inside is that the carbon steel on the outside provides us a lifting mechanism that we can grab, and changes completely the way we would handle the waste package. I'm not saying we couldn't do it, but with the C-22 on the outside, now it's not thick enough to handle it from the skirts. Essentially when we're doing that, we'd have to go through different lifting and handling mechanisms, which not only affects the waste package, but would affect the surface and subsurface facility.

SAGÜÉS: If you weld the C-22, if you do the C-22 on the outside and you do the weld on it, wouldn't that give you a better opportunity to do some post-work that you wouldn't do before?

DOERING: On the heat treating process, essentially the heat treating process, what we're looking at it raising it to sufficiently high temperature, such that the cladding at 350 degrees on the cladding would be violated. But again, carbon steel and C-22 are very good heat conductors and we'd just simply with that temperature straight into it and overcome it. We did look at that early time frame to see if we could heat treat it, but at the temperatures that we'd have to go to, you know, Dave, was it 1100 or so? Yeah, it would just simply overwhelm the whole system. You'd essentially get a whole package close to 1100 degrees C. And if you're looking at the 350 cladding temperatures, you'd violate that very quickly.

SAGÜÉS: Even if you make the C-22 package a few inches long and then would put a cooling ring--

DOERING: We see very little distribution of temperature across lengthwise of the package because the C-22 and both the carbon steel are very good thermal conductors. So even if you would give it some air space, actually there is air space because you've got to remember you the fuel line for most assemblies is 140 inches, and the fuel assemblies are, you know, essentially have a foot or two on either side of that. So we actually have the heat sink out there, but it's still not enough to give us what we need.

COPPERSMITH: Cons to this design? We've already mentioned one in terms of potential lifting issues.

BULLEN: Galvanic protection, quote, unquote.

SAGÜÉS: You may have to make--for lifting, you may have to worry about special supports or legs for that.

COPPERSMITH: Yeah, the pedestals are--

KESSLER: Did we get the alleged expansion of corrosion products into the center?

COPPERSMITH: Yeah, because that will help define I think maybe a research need if this looks like a pursuable design. Any others?

SAGÜÉS: Maybe a greater susceptibility for mechanical damage on the side. In this case, it would be by some kind of--I guess it would have to be some kind of cutting actually.

COPPERSMITH: Bumping, scratching, lifting type damages?

SAGÜÉS: Yeah, or I don't know what would happen with rock fall, because now you have like two centimeters of metal on the outside.

PAYER: You would get the mechanical support of the inner barrier to distribute the stresses.

SAGÜÉS: It won't be able to squash it?

PAYER: I don't think so.

COPPERSMITH: And it would be just as strong certainly as the other. How about MICs in this case?

PAYER: It's a non-issue. Or it would be a plus.

COPPERSMITH: Should we go on to another design? Number one? Who was the proponent of this?

PAYER: That was the one I--

COPPERSMITH: Okay, titanium outer wall; C-22 inner wall.

PAYER: I guess I said that.

COPPERSMITH: Have you got your checkbook for this one?

PAYER: Yeah. The benefit is it's passive in the repository environment. So the life when it's wet is measured in thousands and tens of thousands of years. That's the advantage. Titanium is the ultimate drip shield.

COPPERSMITH: So you have that drip shield in contact with the CRM material?

PAYER: You bet.

STREICHER: Fluoride, in J-13 there are two parts per million of fluoride, and fluoride is not compatible--or the other way around. Titanium is not compatible with fluorides.

PAYER: Because of potential degradation. I'd certainly agree with Mike. You'd have to look at it and see if in these environments, if that caused a problem. It's not a straight fluoride environment, so there may be some mixed--or whether in fact it was not an issue. But it's something that--there were some things we need to talk about in titanium are fluoride attack for one, and tigride.

COPPERSMITH: Let me go back to fluoride. What's your feeling about that in terms of this ionic mix, concentrated

J-13?

SHOESMITH: Well, let's be careful a little bit. Fluoride will in fact break down the titanium. It will give you a low class corrosion process. But if you paid your money and went to the higher alloys, you know, the nickel containing or the palladium containing, it's highly unlikely that it would propagate. It's present in our Teflon spaces when we induce the crevice corrosion. But you still can't get it to propagate on 12 or 16, 16 being the low palladium containing allow. We haven't looked at 7. Seven's got too much palladium to contemplate, and it's too expensive. But I think it's not likely to cause it to degrade, but it is something that should be checked.

STREICHER: And it isn't going to be easy to check. To simulate what might happen here--

SHOESMITH: I don't see why it would be hard to investigate. You know, you're interested here in determining properties titanium are as the fluoride concentration increases. I don't see that as a difficult thing to investigate.

COPPERSMITH: We'll debate that when we talk about research needs. What wall thicknesses are needed to have a mechanically viable waste package with this type of design do you think?

BULLEN: That was a con; right? Because there were going to be probably four or five centimeters; right?

PAYER: I don't know.

KESSLER: At the break, I was asking Tom Doering about how much C-22 was needed for structural, and he was saying that they can do five and a half, and that seems to be enough.

BULLEN: But that's going to be costly. I mean, structurally, you'd want to use something else, and you'd just want to use C-22 for the corrosion, wouldn't you?

PAYER: I would think so.

BULLEN: Don't want to pay the big bucks for that.

COPPERSMITH: Is there anyone who has a feel for just the--I remember one point in a TRB meeting, it was on the waste package degradation elicitation, and I think you were asked whether you had titanium containers, and I think someone asked you what would the cost be, and I think you said that it would probably be double the cost--maybe it was someone from Morrison-Knutson or someone at M&O.

SHOESMITH: I doubt whether I estimated the price. But where you put the titanium would dictate which particular grade you use. If you want to go on the outside, then you'd pay money to get the corrosion resistance. So you might have to go to a more expensive alloy if this fluoride was a problem. If you put it on the inside with something else, then the criteria for what you use is nowhere near as stringent because the temperatures are down. So which particular alloy you would chose would depend on whether you went on the inside or the outside.

COPPERSMITH: But this was clearly a case of a dual barrier type of system, a redundant system, in terms of both--are there corrosion modes that are comment modes of failure here? Other problems that--and I know Dan has brought this up before, that in fact if you have a dual wall system and they're susceptible to the same corrosion modes, the same mechanisms, then it's not so dual.

PAYER: One of the concerns about titanium is high drive formation. So if it's on the outside, it's very unlikely it's going to see that. If it's on the inside, you have to worry about if there is some retained water in the canister, which some folks say there won't be because it's specified. Other people have suggested even though it's specified, it's a likely scenario that some cans will have water in them. And so it could be a problem from the inside compromising the titanium. The repository environment I think favors titanium. That's what titanium likes, oxidizing environments. Does it not have a higher critical temperature in most cases?

SHOESMITH: Well, for the Grade 2 and the Grade 12, the critical temperature to avoid crevice corrosion is about 70. For the Grade 16, they will tell you it's 200, and we certainly couldn't start it up to 150. And Grade 16 would also be somewhere up there in order to initiate crevice corrosion in strong chloride media.

COPPERSMITH: What's the medium?

SHOESMITH: Well, it's a little bit peculiar in it's chloride behavior. It seems to peak somewhere around 2 mol per liter, which is about three to four times sea water concentration. But at higher chloride concentrations, it doesn't crevice corrode.

COPPERSMITH: It sounds like additional research may be--do you have a summation for that?

SHOESMITH: Possibly. There are two possible reasons for why it doesn't do that when the chloride concentration gets very high. One is that the chloride complexation of the dissolving titanium species within the crevice prevents the hydrolysis that drives the pH down. And, therefore, you just never get sufficient acidity within the crevice to start it.

The second one is for the poorer grades of titanium, that you've just activated all surfaces inside and outside and, therefore, you have no potential driving force for the crevice to go, so you just have a slow general corrosion on all surfaces. So that latter situation is not a good one. It's a higher rate than you would expect--than you would want to tolerate, but you would avoid that by changing to a higher grade of titanium alloy. So it's not--the chloride concentration is not a real issue. Plus, if it's a mixed anion situation, as expected, it would have the same beneficial advantages of the sulfate and nitrate around as it does for the C-22.

SAGÜÉS: On the potential cost, I think that the mixed metal crevice with the titanium on one side and the C-22 on the other would be something that should have to be looked at, having two corrosion resistant metals in intimate contact with long distances.

SHOESMITH: Well, you'd have to get acidity, and titanium doesn't mind having all those--I mean, it's used industrially in strong acids when you've got contamination from oxidizing cations. That's one of its bigger advantages.

SAGÜÉS: But then the C-22 on the lid--but then the other metal may get it.

SHOESMITH: Only if it can establish its own environment. The titanium itself will not interact with the C-22. Anything where you have confined environment, you could potentially get acidity, but in the absence of--it's hard to imagine the C-22 breaking down within that crevice. The Germans looked at C-22 in concentrated brines, 100 degrees, 46 per cent magnesium chloride, which is a pH of 2 to 3 environment, and it generally corroded. And that was C-4, not C-22. So it's hard to imagine that in the absence of a gross oxidant, that the C-22 will break. And I think

Gustavo Cragnolino would support that based on his own work. So I can't--well, I'm not suggesting that it shouldn't be looked at as a potential crevice site between the two materials, but I think there are good grounds to believe that it will be benign.

COPPERSMITH: Any other cons besides cost? We're not sure about structural integrity as well; right? And what would be required for that. So I think that might be something that would need to be looked at.

Any other pros or cons for that type of design? Any titanium vendors in the--no. Let's go on to the next. Titanium or C-22 outer wall, structural material inner wall. This could be anything as long as it's strong inside; right? Is that the idea?

SHOESMITH: I'm sorry?

COPPERSMITH: Who was the proponent for number two?

SHOESMITH: Number 2? Yeah. Well, that just loads up the outer barrier for the corrosion resistance. You would be fairly confident that you were going to get thousands of years of containment, by which time the temperature on the inside should be low. Now you're probably below the threshold temperature to initiate localized corrosion on--so this is sort of a low grade nickel alloy material, because the localized corrosion probability is that much lower. So you would not have to put huge amounts--and you could use that one as the thick layer to give you mechanical stability.

BULLEN: David, did you want to go to something like Monel 400? Do you want to go that low a grade, or do you want to stay with the nickel alloy?

SHOESMITH: Well, I wouldn't say that I've given consideration to what material you might choose, but I don't see why you wouldn't think about it.

COPPERSMITH: Any feel for cost?

BULLEN: It's more than cast iron.

COPPERSMITH: Any other pros that people can think of for that? You potentially don't have the same type of iron oxides available.

BULLEN: Corrosion products and nickel alloys are going to be somewhat--products of the outer barrier, aren't they? I mean--

SHOESMITH: I would think so. If you went with C-22 on the outside, yeah. Titanium has no corrosion product that will affect anything else. It's too insoluble.

PAYER: I think the cost is a big advantage. A con would be in how to know with some certainty where that environmental extreme limit was. Did you have enough of a lower cost nickel alloy corrosion resistance that it just doesn't give you--I just don't know how you could define the environment specifically enough to be really certain about it.

COPPERSMITH: Does anyone have an idea about things like fabricability or weldability or other issues related to that type of design?

SAGÜÉS: By the way, when Steve mentioned again the mixed metal crevice possibility in any of these cases of corrosion resistant metals, I think--especially if they are less corrosion resistant than the others--

COPPERSMITH: Any other pros and cons of that type of design? I guess the mechanical aspects come up in this, too, and I'm just not sure who will answer that question. Dave, have you looked into any of these types of designs with corrosion resistant materials, dual wall? I'm just wondering if in fact it's a real challenge for mechanical integrity, either during the lifting and loading or later on in rock fall, whether or not those types of alloys, do they have that type of strength? Because again, I think the--I don't know, maybe it's because I just went through the near field panel with four rock stability guys, but there's a high likelihood of a few thousand years, of having a good bit of rock fall.

PAYER: I recall from Tom is the same thing that John Kessler mentioned earlier, is that if you look at the current design requirement, a drop requirement and current understanding of rockfall, you need about 5 1/2 centimeters of metal for that--

COPPERSMITH: As a minimum?

PAYER: Yeah. That that's the lower limit. And so you either relook and sharpen the pencil on those criteria and see if there's another way around them, or those are the kinds of thicknesses that they start talking about, and that all I think is based on no backfill.

SHOESMITH: For these kind of fuel corrosion resistant material waste packages where you anticipate very long corrosion lifetimes, why would you not consider the backfill? Now you don't really care about the cladding any more. You know, you've loaded up on the outside of the dual barrier the two materials that's you're going to do at least as well as even the most optimistic cladding model, so now why would you bother about the backfill? Why when you finally decide 100 years down the line, or whatever it is, that the decision to close is being made, why would you not just shovel this stuff back in? Now the rockfall issue is not a huge problem, but you've been watching it in the meantime if it occurred anyway.

PAYER: I think that, to my mind, would reduce or eliminate the rockfall requirement. Then it gets down to how stringent is the dropping, just getting it emplaced, requirement. And if that can't be relaxed or provided by some other carrying mechanism that would provide the protection until you get there, but the current design requirement, as I understand it, still says you have to be able to drop this thing.

BULLEN: And we can be gentle and we can put impact limiters on it and we can do all sorts of things engineering-wise, that if it's--you know, if you're worried about it, you can do that and if it's a question of the impact limiter, then obviously we've got to reload it.

PAYER: At this meeting, I can do that.

BULLEN: That's right. At this meeting, you can design anything you want, Joe.

PAYER: Well, those are the kinds of things that if it's that fragile, we could probably, you know, during the movement, worry about it and then just backfill it and not worry about it after that.

COPPERSMITH: One of the questions that came up that dealt with the mechanical integrity issues were dealing with some of these designs that are all either one or two walls of just corrosion resistant material. What is the sort of minimum wall thickness that would be required for mechanical integrity? Joe's memory was about five centimeters of metallic material would be needed.

DOERING: We looked at the C-22 for both rockfall and also tip over, and there was--it was right around 55 to 60 millimeters. Titanium helps us a bit, so you can remove, you know, if you add a centimeter of titanium, it can kind of back off a little bit. There's not a straight ratio because titanium has a different stress limitation. Again, the ductility is very important, especially this is where the NRC brings both water and also--water for criticality, and they also say that you devise a system such that the package cannot tip over or fall, but please evaluate what happens when they fall or tip over.

KESSLER: Tom, did you evaluate the cost of a 55 to 60 millimeter C-22 versus the base case?

DOERING: Yes, it's about double the cost of the base case, or a little bit higher. Two and a half times.

COPPERSMITH: That was all C-22?

DOERING: Yes.

KESSLER: So if we allow--

DOERING: Oh, just simply the C-22 was double.

KESSLER: Okay. And then how much titanium is that in the other cost?

DOERING: Two centimeters of titanium.

COPPERSMITH: So two and a half--

SHOESMITH: The issue here for this container is kind of past the drop test, and that's the only criteria by which it will be judged, the wall thicknesses.

DOERING: We have right now, the design basis events that we have is a tip over event where you essentially let it tip over, and that actually is a very high load because essentially the more mass you put on it, the higher load values you get as you tip over. And then we also have right now a two meter horizontal drop.

What we're doing to work those issues right now is that the NRC has allowed us now in the industry to go to a drop on a yielding surface if you know what the surface is. And so what we're doing right now is evaluating that with sort of bounding evaluations. We're trying to move away from the unyielding surface as has been done in the past. That is something that is relatively new in the industry. Until recently, they also had an unyielding surface. If you go to 10 CFR 71, which is transportation, that is unyielding surface because you don't know what surface you're going to drop it on. We're hoping with the design of the repository, we can get away with it a little bit. That will help us significantly, but still that's going to be in the 50 millimeter kind of range. We hopefully can drop it down to maybe 40. That's what we're anticipating. But that's really the ranges that we're dealing with right now.

COPPERSMITH: Okay, thanks. So those types of cost differentials look like two times, or maybe two and a half times.

PAYER: And I think for the purpose of this panel, just by the way the panel has understood, most of us are corrosion type folks. I believe all the things Tom said there, but I couldn't comment on, you know, what alternatives might be, that sort of thing. But David said something earlier that we have to remember that if it's straight corrosion resistance and we believe what we're saying about these materials, then several millimeters of these materials are enough to give you tens of thousands of years life that remain passive. And so then the trick is how you give the mechanical integrity another way.

SHOESMITH: That was my reason for proposing the second option. I don't think there is a viability for this waste package with these two corrosion resistant materials if they have to be fabricated with the thicknesses required to withstand the tests that were just described. If that is the overriding criteria for the acceptability of the waste package, then the cost is, to me, will be outlandish for just a combination of titanium and C-22. So if that criterion is the absolute standard, then it doesn't matter how good of corrosion resistance it has.

COPPERSMITH: Yeah, other things override. I guess than that's where the number four would come in, which was yours, Dan, wasn't it.

SAGÜÉS: Of course we don't have a--which is the double outer wall corrosion resistant with an internal strengthener of something cheap?

COPPERSMITH: No.

SAGÜÉS: We're not considering that?

COPPERSMITH: I think we should add that, double barrier corrosion resistant--what's your number seven right now? You have a seven--oh, here is it. We're talking about adding one that looks very much like two prime, a two prime.

DI BELLA: So two prime is Ti and C-22 over something cheap and strong.

COPPERSMITH: It may be that in fact the research needs associated with these different designs are very--overlap quite a bit. So we may not need to go into a whole separate suite of research activities for each design.

Are there other negatives to the design number two that people can think of?

BULLEN: I think cost might be the biggest overriding negative of all, just because if it has to be structural, the cost is going to be--

COPPERSMITH: Where are we on the licensability? Where are we on the nature of corrosion modes and localized corrosion rates and other things that feed into--

BULLEN: I think we understand the fundamentals of how corrosion initiates on both of those.

COPPERSMITH: How about propagates?

BULLEN: Well, basically, how about if I say we know where it doesn't initiate, and that that's how we're going into the licensing argument.

COPPERSMITH: But I think the licensing will have to incorporate not only initiation, but propagation and--

BULLEN: Sure. But I'm going to have to get to a fluoride environment and a chloride environment in both cases that's going to be--and then a temperature regime that's going to be such that I don't think I'd be there.

COPPERSMITH: Does it simplify the licensing argument? Does it make it easier/

PAYER: It removes the ferric ion from consideration.

COPPERSMITH: Yes, ferric is gone.

PAYER: From that standpoint, yes, and you've got all the other unknowns.

COPPERSMITH: But aren't you going to have to deal with fluoride and the potential for--

BULLEN: How is that different than what you have to do anyway?

COPPERSMITH: I think, you know, if you had--one of the arguments that John Scully has made many times is that in fact the argument should be--you should have an alloy, but your total argument is you're never going to go into a window of susceptibility, and then you don't have to worry about a growth rate or anything else. You just flat out make sure the conditions never allow you to move--pH and ionic content or anything else will never drive you into that window of susceptibility, so it doesn't matter what happens once you're in it because you're not going to get in it. And that's a wonderful case. It's a deterministic licensing case if it can be made.

PAYER: And mike just brings up the very real issue that you have to consider where that fluoride boundary is. We don't know where that is. Dave is saying there's titanium alloys where that window, he believes, will be far enough out that there's--if not, then it has to be rethought.

I don't know that that complicates any more; it's just another environment.

COPPERSMITH: Exactly. I didn't know if it was a benefit or if it was something that switches from ferric ion to another. Okay, we can revisit that. And I guess there's a mechanical issue, some of the negatives there in terms of its structural integrity.

Number four moves us over into the idea of having the resistance.

SHOESMITH: Kevin, I'm sorry. I'm a slow thinker. I didn't catch up with you there fast enough.

But just to address that issue of fluoride on titanium, that is no different to the issue of ferric ion on C-22. Both materials are designed to resist the propagation by repassivation, and that has always been the thing that you have to demonstrate for C-22. It will be what you have to demonstrate for titanium in the presence of fluoride. So conceptually, there's no difference between the two.

COPPERSMITH: I agree. The problem is I again, and maybe Dave Stahl or somebody can correct me if I'm wrong, but the focus--it's very difficult to deal with the repassivation issue. The focus has been on one of propagation, initiation and propagation. And for example we asked you, okay, give us your probability of initiation given a set of environmental conditions.

SHOESMITH: You forced us to give probability.

COPPERSMITH: Right. Exactly.

SHOESMITH: I will say right from the beginning that the criterion that you had to work on was that this material is designed such that it will not propagate. It will initiate.

COPPERSMITH: Right. But when it came into what's the stifling criteria, what moves you out of the window of susceptibility, when do you think it repassivates? Which is critical. I think that's just as important as when do they initiate.

PAYER: And they have rationale for that.

COPPERSMITH: Exactly. There's electrochemical rationale. But there aren't a lot of tests, and maybe--that tell you when you slide back out and what temperatures do you slide back out, what pHs, what conditions will allow you to repassivate.

PAYER: And if you don't do those tests, you won't license your repository.

COPPERSMITH: Well, that's why--

PAYER: If you do the tests, you've got a good chance to do it, and that's why everybody has been asking for those kinds of tests.

COPPERSMITH: Exactly. And maybe some of these other designs allow me to answer that question more clearly or more easily.

MR. PAYER: I don't know any clearer or any easier; it's the same kinds of tests are required and they just have to be done.

STREICHER: There's no guarantees--excuse me--they're not a guarantee. Those are kinetic tests; not sort of thermal kinetic values that come out of those tests. And in that sense, they're time dependent.

SHOESMITH: I mean, we'll never establish the thermodynamic criteria for these materials. This is an oxidizing environment. I mean, the potential that you could get from oxygen and water will always be in excess of, from the thermodynamic point of view, the stability of any of these materials. We'll never get a thermodynamic criteria. We'll only ever get a kinetic one.

STREICHER: Right. But there will be, in other words, time dependent tests. I mean, if you run them for a year versus ten minutes--

SHOESMITH: Oh, correct, but that's why I think it's more important to rely on a criterion which says even if this material starts to corrode by this mechanism, it has been made with the composition of micro-structure that will force it to repassivate. That's why I believe that's a better criterion than saying what is the probability that this material will start, because the probability that it will start is tied up with the thermodynamics. The probability that it will repassivate is tied up with its metallurgical properties, and I think that's the criteria that you have to use to defend the material.

STREICHER: There are indicators, but not really predictors.

SHOESMITH: No, I think it's a predictor. It's not a guarantee, but then there are no guarantees. There's absolutely no absolute.

COPPERSMITH: It is getting late, isn't it. I think again these get to the heart of maybe some of the research needs. But I think the appeal that I want to make in this is that all those aspects, all three aspects, initiation, propagation and repassivation or stifling, I think are going to be key in a licensing type of environment. Right now, I haven't seen any sign of taking credit for stifling, for example, other than a growth law that has an end value that's less than one. And there has been no explicit criteria that says because temperatures are decreasing, the probabilities of repassivation are going up. I haven't seen any type of model like that, and that may be difficult to do given the lack of data at the present time.

Everyone feels that it physically should occur that way, but I haven't seen it incorporated into any of the rate laws that have been developed so far. And I think again it's not necessarily a mechanistic understanding, but it's one that is based on a certain amount of observation and understanding of the mechanisms that go on.

Okay, let's go down to number four that deals with--it's becoming very similar to some of the other ones we looked at. It's just the internal material. I think, Dan, this was yours?

BULLEN: Yeah, this was mine. The only difference between four and three is four is cheaper. Carbon steel is the inner barrier. Four is just a casting. It doesn't have the basket in it. It basically has channels that you put the fuel in. If you wanted to consolidate, then you'd move down to number six. If you did that, I think you'd probably almost want to go with two prime instead of four. The reason is you'd want to have both barriers for a multiple redundancy, and what you've saved in casting, you've probably spent in a couple of millimeters of titanium over your C-22. But basically that gives you a redundant barrier.

Four and six are essentially, if I were doing four in cast iron, I'd probably try and go to self-shielding containers just so I could go in, have access to the drift. I wouldn't necessarily let the surface temperature go any hotter, let the wall get any hotter, which is why it would have to be ventilated. I mean, six is a big design paradigm shift.

COPPERSMITH: It changes quite a bit. A lot of coupled processes involved.

BULLEN: Yes. And it casts an entire different light on how the repository is expected to perform. But four and six are sort of closely related, and in fact, I don't think four is any different than three, except for cost. And if you wanted to take a look at evaluating, we'd probably want to just look at six where it's case with rod consolidation, shielding, ventilation, all required.

PAYER: Does nodular cast iron have the ductility to tip over?

BULLEN: I wasn't expecting it to be able to take any of the impacts. Nodular cast iron always looked bad when you wanted to try and put any kind of impact on it. I just picked it because it was cheap.

PAYER: Yeah, but it will die because of the tip over test. The tip over test is real.

BULLEN: Yeah, what's the cast steel that you have, David? 827 or--I mean, you could go A516 cast, is what it is. I mean, I don't really care. The casting I'm looking at as being cheaper primarily because of the fact that you can fabricate it in place and put a channel in there for the fuel, I'm biased again by my trip to Europe a couple of weeks ago. But that always looks so nice when they cast those materials.

COPPERSMITH: One of the things that we guard against in expert elicitation is the idea of availability. The thing you've heard about most recently becomes more credible because you've looked at it recently.

BULLEN: True. It looks so nice and when they build it it's just--

SAGÜÉS: If we're talking about waiving mechanical requirements on impact, what is the possibility of thinking in terms of like your high silicon cast iron, something like that, that would have high corrosion resistance? But I don't know--first of all, I don't even know if you can weld that stuff or what.

BULLEN: Well, I don't even think I'd want to weld it. I think I'd just want to bolt a lid on it, wouldn't it? I mean, if I had to weld this cast iron to this material--oh, no, I'm looking at six with two prime on it. The reason I say that is because basically what I want to do is I want to get a shielding component in there. I want to get a compressive structural component, and if I drop something on it, I may crack the inner barrier, but I'm not going to compress the structure itself. Right?

COPPERSMITH: Let's go back to shielding. How important is that?

BULLEN: To me, a lot. But that doesn't mean it sells very well. I think the opportunity to be able to inspect, to be able to not have to have those big robots in there moving this stuff around makes it a lot more palatable from a licensing perspective, but I could be wrong. My concerns there deal with the fact that I'd love to have access to inspect, access to repair. 300 years is an awful long time, and I think it's going to be costly to move everything out and repair a tunnel for 300 years as opposed to have somebody stand on top of a waste package and, you know, slap the plaster back up on the ceiling. I'm sorry, that's a very rudimentary image of tunnel repair.

COPPERSMITH: I'm sure ground support people will--

BULLEN: I know. But as I mentioned before in previous times, you want to be able to go in there to inspect and, you know, go in and kick the tires, so to speak. And if it's ventilated to a temperature of 40 degrees C. and radiation field is a couple hundred MR per hour, that's a typical radiation worker field and you can send a guy in ten hours a year and not have to worry about it. And so I think that gives you tremendous advantage. Now I'm going to step over the bounds here and say--

PAYER: That's 200 Centigrade?

BULLEN: No, that's just surface temperature. I'm blowing air by this thing so that the near field is 40 or 50 Centigrade. I mean, that's the kind of ventilation that I had envisioned when we talked about ventilation. I can be next to my hot plate and, you know, not get burned as long as I'm--

PAYER: You're not standing on it.

BULLEN: Well, I can put a shield over the top of it if I want to stand on it. I want to have shielding so I don't have to worry about irradiating the daylights out of myself while I'm standing there.

KESSLER: Since we're speaking of temperatures, Tom, I hate to call on you again, but we're talking about rod consolidation. I'm guessing you did some thermal analysis of rod consolidation. We're now talking about ventilation and how much cooler the air is. Any words of wisdom for us?

DOERING: What we've actually done is looked at--what we do is develop--we've developed an effective thermal conductivity for the fuel assembly itself. Rod consolidation actually lowers the effect of thermal conductivity of the rods because essentially we're removing radiation, which is a major removal of heat. What we'd be looking for; our viewpoint of rod consolidation is that we would still limit the kilowatts per package again so we don't overheat.

Again, if we go to the person standing in the drift and you can't be with this wind moving by you to keep these drift walls or yourself wind cool, but it's like standing in Las Vegas in 50 degrees Centigrade or 120 degrees during the summer and with the wind blowing it's still hot because you get the radiation coming on you. And that's the dominant factor and the drift is radiation. Essentially, the conduction is overwhelmed by the radiation, so even if you would have air moving through the drift, irrespective of what the mode is, you still are going to get hot on that one side that's facing the waste package.

PAYER: Thermal radiation?

DOERING: Thermal radiation. The other radiation will, depending on the thickness of the shield, we have calculations of those too, and that's in the back of your handout that we gave to provide you with understanding of how thick that might have to be. Even there's some composite materials in that. Does that answer your question on that one?

PAYER: Thanks, Tom.

COPPERSMITH: The reason I brought that up, if this is a big pro, shielding, then we need to specify that and then go back and really think about whether or not these others provide that, and I think most of them don't that we've talked about so far. And I'm just not sure that in fact it is something that we'd be considering. Even though you would, I'm not sure that in general, it's something that's needed for the repository, and we'd just need to make sure and make that clear, in the same way that something like cladding may not be important unless you're going to take credit for it, or some other aspect.

STREICHER: Could we spent quite a bit of money for alloys to avoid ventilation, cost of ventilation?

BULLEN: That analysis, yes, would need to be done; you're right. You could spend a whole lot of money, and if you ventilate for 300 years, it's going to cost you, you know, billions of dollars.

STREICHER: And you're making the people down the road, so to speak, pay for it.

BULLEN: Unless I'm smart and get a passive design of some type, but I'm not sure that that's doable either, unless you listen to, is it Nye County that's going to generate power?

So, I mean, there are ways that you can deal with it, but I guess in taking a look out of the box, and this is about as out of the box as you can get, this is pushing the envelope all the way to the edge, and as you know, as I make it shielded, I make it more expensive, and as I make it more expensive, then I want to make fewer packages, so I consolidate, get as much money as I can out of each package that I'm building. And if I'm going to shield it, maybe I don't care that it's hot. I mean, I don't want it to be 500 C. or anything like that in the container, but the concern that I have is, for example, the Germans don't care about 450 degrees C. for cladding because they're not going to take clad credit after they emplace. And maybe if we backfill, we're not going to either.

COPPERSMITH: How much space do they have? Do they consolidate rods? How much--physically, how much space is there?

BULLEN: The biggest can that they consolidate is a 12 PWR can. So that's as large as it is.

COPPERSMITH: But are they looking at waste form oxidation and volume change there?

BULLEN: I don't think that's a concern of theirs that they've looked at. They're looking at consolidation.

COPPERSMITH: Isn't there a significant volume change?

BULLEN: Well, yeah, but that only occurs at high temperatures. It's another kinetic one. It depends on when the air gets to the--

COPPERSMITH: Exactly.

BULLEN: If the air gets to the fuel and it's hot, then it's a big problem.

SHOESMITH: But the eventual fuel conversion to U-03-

$2H_2O$ or the secondary phases is an even bigger volume change.

BULLEN: Yes, sir.

SHOESMITH: The volume change will occur.

BULLEN: Right. But when, is the question.

COPPERSMITH: You rely heavily on that integrity of the waste package. So just juvenile failure or something like that could lead to this large volume expansion and ultimately problems within the--

BULLEN: Potentially, sure, which is why you don't want to have any juvenile failure. They have a double coated resistant barrier outside of the Pollux container.

SHOESMITH: The Europeans have been like we are. Their in canister environment should be reducing, therefore, the fuel reactivity--chemical reactivity is a lot lower and it takes a long time for that calculation. It's not quite the same here. I think presently, they perform an assessment 400 to 1000 years for total oxidation of the fuel. I think that's unreasonably conservative, but that would be a very rapid volume expansion.

COPPERSMITH: That's in their environment, 400 to 1000 years?

SHOESMITH: That's our environment. I was going to say Yucca Mountain environment. So, you know, 400 to 1000 years for that total--is a big volume change.

COPPERSMITH: So you just need to have a good container in your scenario then. In fact, I guess early breach in any of these is going to be that--that will be a problem, high temperatures and a lot of oxygen.

KESSLER: I guess I just wanted to complicate matters even more being a PA person. One of the things that we haven't talked about in terms of--I hope this doesn't get into the PA paradox problem of PA needing things that process models can't provide and process models provide things that PA can't use. One of the things PA would like to have is this issue of okay, you've got a pinhole. Well, do we care about that. We need to care about the container failure distribution, which I haven't heard any discussion about. We care about how big the hole is in some way of being able to assess how much flux actually gets through the container. That's very important to PA. And I haven't heard any discussion about whether any of these scenarios will provide us any information.

Another thing that was brought to my attention last week in one of the talks at the Las Vegas meeting was when we're talking about container failure distribution, we need to make sure that we're separating uncertainty from real variability in conditions that lead to failure distribution. We don't want to have our uncertainty broaden that failure distribution unnaturally, so to speak, that would cause--I mean, because the point is the wider the failure distribution, the lower you might actually show that you have any release rate.

And so one of the things that we may need to get into are variability and conditions, variability in actual container that would cause this failure distribution under a constant set of environmental conditions. And then ultimately, PA is going to have to worry about the variation in environmental conditions as well.

BULLEN: Could I ask for clarification? You don't want the variability to narrow the distribution? Because the narrow distribution is worse.

KESSLER: A narrow failure distribution is worse.

BULLEN: Right. I mean, if we went to a million years and they all failed at a million years, it's still not going to be good.

KESSLER: Well, it depends on what your criterion was.

BULLEN: Yes.

KESSLER: One of the things that I think has been happening with some container failure distributions is that uncertainty and variability have been getting mixed, which will tend to broaden the distribution.

BULLEN: Unrealistically.

KESSLER: Unrealistically.

COPPERSMITH: So it depends on what direction I go. Does that mean if you take all of what's perceived as uncertainty and assume a spatial variability, then it will lower the peak?

KESSLER: Right.

COPPERSMITH: But if you keep some components and say we're just uncertain about it, it's knowledge uncertainty about the corrosion rate, it isn't the distribution of rates among canisters, it's just saying we just don't know. Then if you maintain that as uncertainty, then that's real uncertainty, to be reduced--

KESSLER: All I'm saying is is that real uncertainty, the way it's usually used, will broaden the peak.

COPPERSMITH: That sounds like a PA guy. Who invited the PA guys? Let's go back to his first issue. This is always one that--this has to do with corrosion modes, the size of the hole, the transport problem is important.

PAYER: And I think the rationale that's being developed here is with these materials, titanium over C-22, or C-22 over titanium, that is the scenario that Carl had us start with here, is that we're talking about packages getting dripped on all the time, or by the episodic events. And what we're saying is that there's a very realistic possibility to show that these materials remain passive under those conditions, and measured in thousands and ten thousands of years.

Now, what's going to make the distribution of the failures different then is how you distribute the drips across those, and where they're going to get dripped upon.

COPPERSMITH: What I'm asking is for a difference--let's assume the environmental conditions are exactly the same, you have exactly the same environmental conditions, aren't they all going to fail at exactly the same time?

PAYER: No, they won't.

KESSLER: What's cause that?

PAYER: You'd have to--well, again, the distribution. You're not going to have the drips even distributed. It would be unrealistic statistically to say you're going to have 100 waste packages that see the same drip distribution in the same time.

COPPERSMITH: No, but it's--you're right. There's variability in both the environment and in the waste package characteristics. But assume for a minute that nature is exactly the same, that the location of drips, frequency of drips, the humid air conditions, the temperature, everything is exactly the same across the repository forever, now what would the variability be or the distribution of failures of those waste packages be? Would they all fail at exactly the same time?

SHOESMITH: No, but it would be relatively narrow compared to what you will get if the environmental factors are incorporated.

COPPERSMITH: That's been the assumption so far.

PAYER: That's a tenable--

COPPERSMITH: The real variability comes from the environmental variability and not so much from the waste package. That's been the assumption.

BULLEN: But if the PA time step is 500 years, the answer is yes, they'll all fail in the 500 year time step, whether they fail 100 years, 200 years, 300 years, 400 years is not going to be relevant. But your PA time steps, particularly in the long term, your PA time steps are like 10,000 years; right? So, yeah, they're all going to fail at the same time in PA terms.

KESSLER: But is it variability or uncertainty?

PAYER: I don't know.

COPPERSMITH: We asked the waste package people that and I think only two people responded, maybe three, with their arms twisted and they basically had about a fifty-fifty split between uncertainty and variability. And actually John Scully made it a condition of whether or not you're dealing with the CAM or the CRM. He has much higher uncertainty for CRM, real uncertainty, not variability, but uncertainty. He feels there's very low uncertainty in the CAM, but there will be variability, and right, just this location versus that location, but it's very well described. I feel very certain about rate, but I know it varies. I have seen it vary in test results. But this issue comes up, and I'm not sure how--I wanted to ask you where does that go. How do we use this for the discussions here? It's important, but will it affect these designs?

KESSLER: Well, I think the point is you're trying to identify research needs in the end; right? And if the feeling of the corrosion people here is that if the external environmental conditions are exactly the same and they're all going to fail within a 500 year time step, then the research needs do not--don't fall in the corrosion area. They're going to fall in the environmental area.

On the other hand, if the point one to one range is due to some other uncertainty, then now you might want to do research need that's in the corrosion area.

PAYER: I think if you got three corrosion experts in a room, you could reach a majority opinion.

COPPERSMITH: If it was four, you'd be up the creek.

PAYER: Well, you might have a tie.

PAYER: It's difficult to respond. I don't have a good enough appreciation, although I've heard this several times, the distinction between variability and uncertainty. But the physical phenomena of how this metal will thin, I think you could get a consensus amongst the corrosion community and how you distribute that and how you deal with it. But it's the same thing the modelers had problems when, you know, if these things don't corrode and fail, we don't know how to model that, so you have to change the design so they fail. That doesn't seem to make sense.

COPPERSMITH: But right now, the distributions let's say that were developed for the CRM localized corrosion and general corrosion rates are assumed to be spatial variability unless told otherwise, they'll be out of 10,000 waste packages. The 99 percentile will apply to a few waste packages or a few patches on waste packages. So it's distributed out spatially and assumed to be variability. It's easier to do a calculation, and it goes back to this notion that in fact, you know, uncertainty isn't that large in this problem compared to environmental. But the problem is there's still a variability and if it's not reducible, then you say forget research needs, you'll never reduce this uncertainty. I think what John is saying if there's some real uncertainty, and we think it can be reduced and rate information, stifling criteria, the passivation, other things that relate to these materials, then it's worth looking at. But if it's irreducible, that's not real uncertainty and it's irreducible, then don't waste your money.

PAYER: My guess is that the action will be in the environmental range and the distribution of the drips and things like that is really--

COPPERSMITH: Let me go back to the first point that

John made, and this has to do with the size of penetrations, you know, the failure criteria and so on. This could relate to the--excuse me--the mode of failure of these things, they fail in general corrosion, they fail when they finally do go through, in other words, the passive dissolution is the process, the primary process under the conditions we expect, whether they fail as pits or crevices, localized corrosion, in which case you have a totally different--potentially a different transport problem, diffusion problem, and I think those could be taken into account in the pros and cons of these designs, too.

You know, for example, I guess the results that whoever showed, Dave showed, Dave Haught showed on the TSPA round this time shows that the general corrosion rate, or general corrosion is the dominant corrosion mode for CRM right now, and those are big holes. Now, you can say, well, they don't happen for a long time, which is a good thing, but there are also much larger penetrations than high aspect ratio pits. And I don't know if these--you know, how does titanium fail in these conditions?

SHOESMITH: It will fail by a passive process. So there's no point in using it if it--it's like the C-22. That's the reason for going to it. Passive corrosion is not something that most people have given any thought to. You know, the feeling is that it will just be a single layer working its way evenly through. But it probably won't. There will be some distribution of sites based on what kind of inclusions you encounter in the process, what kind of recrystallization processes take place. You know, you can make a case if you drip and introduce calcium silicate, in the end, that particular site will become ceramicized and will react less rapidly than one that is not. But all that stuff is speculation.

PAYER: But on a grander scale, one model of what it would look like, the perception would be it would look like the corrosion allowance material would look like. Not all surfaces of the can are going to corrode. There will be patches and areas and moving out primarily due to where it gets wet and what kind of deposits form. And you're going to get penetrations over tens of square centimeters, not a sharp pit necessarily.

COPPERSMITH: So those are the dominant corrosion modes that we're talking about for all these alternatives then, is the general corrosion type of failure, large patch.

SHOESMITH: For the corrosion resistant metal as in outer barrier. You had the ferric chloride and you could get the localized corrosion issues, so that's a different mechanism. So you just take the corrosion resistant material outside. Then that was what we expected.

COPPERSMITH: If we completely eliminated localized corrosion--right now in design zero, that possibility exists that has a very low probability. Does that possibility exist, localized corrosion, if it's on the outside?

SHOESMITH: C-22 I think there is still some issue as to whether the drip deposits will give--C-4, for instance, does develop pits that deposit signs that I think the C-22 won't do, and the other issue then also is that these materials have shown some sensitivity to radiolysis. Again, the dose rates are relatively low. This probably would only be a minor short-term process. There would not be failure mechanisms for thick wall, but there are things that would give some distribution and penetration sites. It should be evaluated. But titanium is hard--I presently do not know of anything except long-term hydrogen pickup which would lead to a hydrogen induced crack, which would give you a local failure for that material, and that's unlikely under oxidizing conditions.

BULLEN: Is that a radiolysis issue again?

SHOESMITH: It's nebulous to say that radiolysis is good for titanium. It blocks local corrosion. It increases film thicknesses, decreases corrosion rates, blocks hydrogen absorption. Oriani has some data, the Germans have some data, we have some data. So radiolysis is not a factor with titanium. It's a benefit, if anything.

SAGÜÉS: This may be a good time to bring up I guess the fact for this work that collectively there's no engineering experience with any highly corrosive resistant passive metal for more than--operated for more than what, 70 years or something like that? Stainless steel since the 1920's perhaps, and I think that there is very little scientific evidence of this type, or rather I should say engineering evidence of this type that the passive or the successive generations will last any more than, again, say 50 years or 100 years.

You can--we have some experience on this. We have some mechanisms, some of them developed with different degrees of sophistication over the last generation or two, and Digby is I guess one of the exponents of some of the more sophisticated mechanisms that exist. But there may be a lot of questions about what happens over long periods of time. Yesterday they were discussing--just to measure one thing as being consumed, you're hitting more and more defects with the passive layer retaining some kind of memory of that over long periods of time. You know, I'm saying because basically we're sitting here--we are betting now that that is going to be the main way of protecting against corrosion over long periods of time.

PAYER: But we're not betting with no knowledge. There's also no precedent in those 30, 40, 50 years of a material that's remained passive for tens of years and all of a sudden, starts to behave like Alka Seltzer. That's unprecedented.

The only time that has happened has been if there's been a drastic change in the environment. And so all of a sudden somebody started bringing in, you know--so again, the bigger bet is on the environment. It seems to me--and we have a lot of short-term experiments, though, on how these materials behave during passivation, repassivation cycles under various conditions, and you can look at a bunch of cycles. Now, I don't know how many you have to look at. So I don't think it's as bleak. It certainly isn't unknown, you know, the sun comes up most mornings, and we don't know about tomorrow, but--

MACDONALD: You know, if you take a passively corroding metal and you do relatively short-term tests and you calculate a rate constant, and you start looking at the consequences of a small error in that rate constant which reflects your experiment conditions and see how that propagates through the thickness of much longer times, then you get a tremendous variability in the thickness.

What I'm saying is that in order to take a rate law and extrapolate it from ten years of experimental data through 10,000 years of extrapolation, there's a real question as to whether you can ever measure a rate constant with sufficient accuracy or precision to get any meaningful extrapolation. That to me is the principal problem. You get a mind effect of the simple rate laws that are used, parabolic, for example, probably incorrect.

PAYER: Granted, but you're still dealing with something of the passive materials. If the upper bound is a micron per year, and then you don't know the difference between a tenth of a micron and a hundredth of a micron, at that rate, it's all going in the right direction. The worry we would have would be would you be predicting at the micron per year and all of a sudden something kicks in and gives you, you know, 100 microns per year, and most of the things I think we see in short term polarization tests are that these kinetic rates tend to go down below where we can measure them. We have real difficulty measuring these things.

MACDONALD: I'm going to say it's a trivial issue. I think it's something that ought to be really dealt with, but I wouldn't say just because we don't have 40, 50, 60 years data, that it's an untenable problem.

I'm not an expert on C-22, but can you get C-22 to pit in an aggressive electrochemical test?

STREICHER: Yeah, in ferric chloride above 70 or 80 degrees Centigrade, or somebody said 100. It pits at--

PAYER: You go through a pitting regime and then you can go so aggressive that you just activate the entire material and you get what looks like a general corrosion penetration.

MACDONALD: Then I would, if that's the case, then I would argue that we're probably on fairly shaky grounds in assuming it won't pit under less aggressive conditions, because it's merely a matter of time.

COPPERSMITH: Again, I'm not sure that that is the assumption. I think the assumption is that in fact if there are--

MACDONALD: No, the assumption that we seem to be making is that the C-22 is going to corrode as a passive material.

COPPERSMITH: But I think that there is an allowance for--

SHOESMITH: This is a very definite issue for experimentation. I don't argue with you that this has not been demonstrated or that the few tests that now exist out there prove the point. But to drive hundreds of microns or even milliamps of current through something has such a positive potential that you can guarantee that the chromium molybdenum will go to the sixth state is a little bit different than saying your environment can only push it so far, that even if you open it up, the metal that you block the active dissolution site, you have to handle the question of the mechanism to answer that question. So experimentation is required. But you're right. If you really kick the buck hard enough, it will go, and there's hardly any material out there that won't if you really give it the right drive. You have to know what the environment can sustain.

COPPERSMITH: Okay. Dan's looking at his watch here. We've got a couple of other designs, and we'll hit those first thing in the morning when everyone is sort of passive.

I'd like to thank everyone today. I think this has been very useful in at least brainstorming the designs. I think we'll need to rethink some of this stuff overnight and particularly think about some of the differences that these designs or conceptual designs would imply in terms of the research requirements. And don't forget some of the modelling issues. I think John's here to sort of remind us that some of this stuff has to actually be used in a performance assessment environment.

And now Dan is going to take control.

BULLEN: Thank you, Kevin. Russ, would you step outside and see if anybody signed up for public comment? At break time, there had been no one that had signed up.

In keeping to true form of the NWTRB open meetings, we would like to request anyone from the general public, anyone from the audience who would like to ask a question of the panel here, or to make a public comment, to feel free. Identify yourself and ask your question, please.

AHN: Tae Ahn of the NRC. Carl earlier presented the base case of environment. Based on that, design was from the consideration of anticipated scenarios. If you really needed to consider the waste package failure on destructive conditions as well, that includes volcanism, seismistic and faulting. Under those conditions, corrosion will not be--rather creep or mechanical failure dominant processes. In your design, do you consider those creep or mechanical failures perhaps in other panel discussions?

BULLEN: Okay, actually I'll try and take that one. As we set up this workshop, we identified the three scenario environments that were to be discussed, and we wanted to limit the waste package design concerns to those scenarios, assuming that we would look at corrosion, pitting and those types of failures.

Disruptive events, and I'm going to use this for the first time today, we would have put right in the parking lot, and the reason is it was beyond the scope of what we wanted to study with this design. We saw some very nice presentations with respect to, for example, the volcanism disruptive event, and the types of intrusions of magma into the repository, how fast the velocity would have to be to lift particulates of fuel into the environment to have them entrained. That is all well beyond the scope of what we've done here, and you're right, the design that we have right now is just for the mechanical and corrosion integrity of the waste package, and it's beyond the scope of what we'd want to do here. Any design that would come out of this would have to follow through the disruptive event analysis, but that's not what we had these experts here today. But that's an excellent question, and I'll immediately defer it to the "Parking Lot."

Other questions from the audience?

(No response.)

A silent group. I can't believe this. No one has any questions that we might be completely way off base, particularly that Bullen guy; right?

Well, if not, then you guys are going to get off early. We will reconvene tomorrow morning at 8:30, which I assume is so that the people in Washington, D.C. can make the commute; is that not correct?

So we'll be here at 8:30 tomorrow morning and we'll start with the other waste package designs, and then talk about experimental needs.

Thank you very much for your attendance.

(Whereupon, the meeting was adjourned.)