UNITED STATES

NUCLEAR WASTE TECHNICAL REVIEW BOARD

SPRING BOARD MEETING

May 15, 2007

Crown Plaza Hotel
1480 Crystal Drive
Arlington, Virginia 22202

NWTRB BOARD MEMBERS PRESENT

Dr. Mark Abkowitz
Dr. William Howard Arnold
Dr. Thure Cerling
Dr. David Duquette
Dr. B. John Garrick, Chair, NWTRB
Dr. George Hornberger
Dr. Andrew Kadak
Dr. Ronald Latanision
Dr. Ali Mosleh
Dr. William Murphy
Dr. Henry Petroski

SENIOR PROFESSIONAL STAFF

Dr. Carlos A.W. DiBella
Dr. David Diodato
Dr. Daniel Fehringer
Dr. Bruce Kirstein
Dr. Dan Metlay
Dr. John Pye

NWTRB STAFF

Dr. William Barnard, Executive Director, NWTRB
Karyn Severson, Director External Affairs
Joyce Dory, Director of Administration
Linda Coultry, Program Support Specialist
Davonya Barnes, Staff Assistant

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PROCEEDINGS

8:00 a.m.

GARRICK: Good morning.

On behalf of the Nuclear Waste Technical Revenue Board, let me welcome you to our second meeting in 2007. The meeting is completing with a couple of our major meetings, something we didn’t know about when we were scheduling and planning the meeting. But, I think, still, we are going to be able to have a very interesting day.

As you know, our custom is to introduce the Board members individually. We are going to depart from that process a little bit today, and just identify the Board members and announce to the audience that we have gone to a little different practice here of supplying resumes or bios of each of the Board members, as well as each of the speakers. And, those will be available to anybody who wishes to have one at the table in the back of the room.

But, I would like to at least allow you to be able to match the face with the name, so I am going to name each of the Board members, have them raise their hand, so we can do that.

I’m Chairman of it, and my name is John Garrick, and the first one I’d like to have raise their hand is Mark Abkowitz. Howard Arnold. Thure Cerling. David Duquette. George Hornberger. Andy Kadak. Ron Latanision. Ali Mosleh.

William Murphy. And, Henry Petroski. We will be introducing the speakers as they speak.

I would like to recognize one distinguished group that’s with us today, and that’s the Advisory Committee on Nuclear Waste, which I understand is now the Advisory Committee on Nuclear Waste and Materials, something the current Chairman and Committee was able to get through, something the past Chairman and Committee was pushing for, but was unable to get through, so, we congratulate the Committee for its change. That’s something that’s very much needed in the Nuclear Regulatory Commission.

What I’d like to do now is, as a practice, to review some of the areas of major interest to the Board, not necessarily our current total priority list. We are usually influenced by the recent meetings, and topical meetings as well. I’ll try to highlight a couple of things that we are having to follow, and have a great deal of interest in, and I’ll do it in accordance with the categories that we have more or less associated our issues with, namely the preclosure period, the postclosure period, and the integration issues associated with the Project.

And, of course, as has been for quite some time, during the preclosure period, transportation and the design of the surface facilities stand out as areas of great interest to the Board. The Board was updated on both subjects at its last meeting in January, and we expect to return to these topics again in the near future, as more information becomes available.

The Board has also been particularly interested in the Department of Energy’s proposal to adopt a transportable, aging, and disposal canister. The TAD canister system could reduce the number of times individual assemblies are handled because the canister and its contents would be handled in pretty much a single action. This could also improve facility throughput at the repository and reduce the potential for accidents during handling operations.

The TAD canister system also has the potential to simplify the design and reduce the cost of repository surface facilities. However, as we have pointed out in our letter, in our reports, the success of TADs is very dependent on such uncertainties as the actual startup date of the repository, the availability of a Nevada branch rail line, and decisions on such spent fuel operations as the on-site storage at nuclear power plants and proposed spent fuel storage on pads at the repository site.

Consideration of these and other factors concerning the handling of spent nuclear fuel will be necessary to demonstrate the real value of the TADs concept.

Regarding postclosure performance, the primary interest of the Board is establishing a sound technical basis for a realistic assessment of the radiation doses that could result from the repository. During past Board meetings, we have often identified areas within the Total System Performance Assessment that could include varying, and sometimes unknown, degrees of conservatism. While this may be acceptable for purposes of licensing, we have indicated several times that a more realistic assessment would help generate a sense of confidence, both within the technical community and the general public, confidence that the facility would indeed perform its intended function.

An important component of a realistic dose assessment is knowledge of the chemical conditions within the repository system, especially in the near-field environment. The physical chemistry model of the waste mobilization process in the near field is a continuing area of interest to the Board. As a matter of fact, today, we have invited a guest speaker to speak about the use of depleted uranium oxide as a chemical barrier. And, our purpose here in inviting this presentation is not to promote the use of depleted uranium in a Yucca Mountain repository. It is to improve our fundamental understanding of the chemistry in the near field as a basis for enhancing our confidence in the dose assessment of the repository. This afternoon, we will also have a presentation by a representative of the Yucca Mountain project on the evaluation of the near-field chemistry.

A third major area of interest to the Board is integration, both within the project and with other entities, such as nuclear power plant operators that must interact with the repository, or with the project. Many of our presentations today support the Board’s choice to review the overall integration of project activities in addition to our interest in the technical validity of the specific activities or analyses discussed in each presentation.

A key factor in integration is the thermal management strategy that will be adopted for the Yucca Mountain repository. The subject of one of our presentations today, the proposed Global Nuclear Energy Partnership, has the potential to significantly affect thermal management, and we are especially interested in exploring how that proposal could affect design and operation of the overall waste management system.

Let me talk a little bit about the agenda itself. As is customary, we will begin with an overview, both of the overall Office of Civilian Radioactive Waste Management program and, more specifically, the Yucca Mountain project. That overview will be followed by the presentation I just mentioned on use of depleted uranium oxide as a chemical barrier. Following a break, we will hear presentations on the Inyo County drilling program and on the second generation waste package design work being done at the University of Nevada, Reno.

After lunch, we will have a presentation on the Global Nuclear Energy Partnership and, particularly, the waste streams that might be generated by the facilities proposed for the Partnership. Then, we will begin the Yucca Mountain presentations with talks on saturated zone testing and on near-field chemistry. And, following the mid-afternoon break, we will have a presentation on waste package design and prototype development, and then an update on probabilistic volcanic hazards analysis.

Now, as usual, following the presentations, we have scheduled time for public comment, an aspect of our meeting that is very important to us. If you would like to comment at that time, please enter your name on the sign-up sheet at the table near the entrance to the room. Of course, written comments are always welcome, and will be made part of the record.

In that connection, we have been asked to accommodate some public statements prior to our afternoon break to allow some people to attend another meeting. So, that will be a slight departure from the agenda as it now reads.

Some of you have asked about questioning during the course of the presentations. Our preference is for you to write down your questions and submit them to either Davonya Barnes or Linda Coultry in the back of the room, and we will cover as many as we can during the discussion period.

As we get into the presentation and discussion part of our meeting, it is important that everybody understand or is clear about the distinction between member opinions and official Board positions. Board meetings are very spontaneous. We want them that way. We want the exchange to be as free and as open as it can possibly be. We express ourselves pretty much as we like, and that is the process we'd like to continue. So, when Board members speak that way, or extemporaneously, it is important to realize that we are speaking on our own behalf, and not on behalf of the Board. And, we'll try to distinguish between the two situations.

Also, we urge that all of you put your pagers and your cell phones on the silent mode.

And, I have a reminder here too, especially for the Board members, that it's very important when you speak, to speak into the microphone, and not turn away from the microphone while you're making your comments. And, you also have to push a button in this case to speak. There is a speak button. If it's like this one, it's on the right side.

All right, I think with that, we will proceed with our presentations, and our first presentation today to give us the overview is going to be Chris Kouts of the Department of Energy. Chris?

KOUTS: Thank you, Dr. Garrick.

I have to say it's somewhat de javu, not only being before the Board, but as I was mentioning to attendees from the State of Nevada in walking over from the Metro, this is where I started my federal career in Crystal City working for the Environmental Protection Agency over 30 years ago. So, it was kind of a strange feeling walking over here, but nonetheless, it's the Ides of May, and I'm very happy to present a status as to where the program is.

I'm going to give you a status of the key project issues, and an overview of the areas of senior management attention. I will say that our Director, Ward Sproat very much wanted to be here, but Ward's duties essentially required him to be out in Nevada this week, and he sends his apologies, and I'm his substitute. I hope I will be able to answer any questions, and provide the information to the Board that they're interested in in terms of the status of the program.

The Board is fairly familiar, I believe, with the schedule of the program. It has not changed. We are still working very diligently to complete and submit a license application to the Nuclear Regulatory Commission no later than June 30th of next year. That will require certain steps along the way. We hope to finalize our design by November of this year, have LSN, the licensing support network certification no later than the end of December of 2007, in order to meet the NRC requirements.

We’re working on a supplemental EIS right now, and we hope to have that also completed prior to the time that we submit the license application.

The subsequent licensing dates are all basically in the hands of the Nuclear Regulatory Commission, and you are probably aware of the three year statutory requirement for the NRC to make a decision, and they can go to Congress, obviously, for another year, but, essentially, these dates, except for the rail lines, are essentially contingent on the ability of the Department and its application, to satisfy NRC issues and concerns in relation to the licensing process.

Also, as we should always caveat when we talk about 2017 in terms of the start date of the repository, that’s contingent on several factors. One is adequate funding for the program, and the other is enactment of the administration’s legislation that was resubmitted earlier this year.

If we could go to the next slide, I think the Board is familiar with these. Our Director has established four strategic objectives, the first of which I’ve already mentioned, which is to submit a license application no later than June 30th of next year.

The second one is one that he has a personal interest in, which is essentially to put the program in a position such that the skills and culture needed to design, license and manage the construction and operation of the project, if you will, with safety, quality and cost-effectiveness, are in place. And, he's basically attacked this issue with a great deal of interest and zeal, and I think we're seeing changes within the program. We've had a lot of discussions internally into the program, within the program about where we need to be, not only at the point that we submit a license application, but also at the point where we begin construction and operation, and that's a continuing effort on his part, and will be as long as he's with us.

The next strategic objective is one that I am routing very hard for since I generally have been the lead witness in these spent fuel trials, I've testified in all trials to date, there are nine of them. Someone has asked me whether or not I enjoy it, and I think my perspective is after the first couple of trials, the novelty really wears off. There are 55 more trials in the pipeline. We have four settlement agreements. I believe some of the Board staff has asked for some statistics in terms of what those settlement agreements cover, and so forth. But, this is certainly one area that I would love to see some progress made.

And, the last I think is to develop and implement a comprehensive national spent fuel transportation plan. I believe Gary Lanthrum, who directs that area of the program, is working very hard on that area.

If we can go to the next slide, which is areas of management attention. One of the reasons our Director is not here is that he’s conducting a progress status review, which he does on a regular basis. In addition to that, he has set up a group within the program, which I’m a member of, which is a Licensing Strategy Team, who reviews issues related to licensing strategy. We meet on a regular basis, and discuss issues about how the program needs to proceed. And, I think that’s helped the process a great deal from the license application standpoint.

I believe from an organizational standpoint, he’s also embarked on taking some lessons from the private sector and requiring each office director to develop business plans in terms of what milestones and what we hope to accomplish, how those milestones affect other areas of the program. And, we work that very hard within the program.

He’s also initiated the--actually, the hiring of a substantial greater amount of federal staff, which we’re in the process of doing right now. He feels very strongly that we need a much more active federal staff, and we’re hiring people on a regular basis in order to do that within our ceiling limits.

He’s also concerned about developing the management team that we already have, and we’ve spent a great deal of time on our different aspects of that with the individual directors.

From a culture standpoint, he’s very concerned about quality, and that’s--as he’s indicated on many occasions, that quality, schedule and safety are not mutually exclusive. He believes that they can be done at the same time. And, his basic issue with that is if a manager is having problems, he needs to ask for help. And, every manager who needs help gets it, and hopefully, we’re moving along to address quality issues.

In addition, the corrective action program, which is one that, again, is an item of special attention for the program, is receiving a lot of effort, and trying to streamline the process, and also make sure that when we do make a corrective action, that it deals with the issue and goes to the true root causes of the situation, so we don’t have recurrences of the same issue.

From a Congressional standpoint, Ward has spent a great deal of time on Capitol Hill educating staff and members of Congress on where the program is and where it’s headed, and he’s, from his perspective, building a great deal of credibility for the program. And, certain Congress is learning a great deal from those meetings.

The status of the budget, which everyone follows very closely who follows this program, our FY ’08 budget request is $494 million. As you’re probably aware, we received about $100 million less than we requested for FY ’07, and that’s required some reductions in staff. And, that’s been the history of the program. Typically, our requests are not fully met by Congress, and the program has to adjust when it finally gets its mark.

Status of the revision to the EPA standard? There’s really nothing new. That’s not a departmental issue. We are the regulated body, not the--we don’t have any true influence in relation to the issuance of those regulations. Nonetheless, that’s working its way within the government, and we’re hopeful to have that out sometime in the near future.

The e-mail issue, the USGS e-mail issue, hopefully now is somewhat behind us. It was a significant cost to the program in terms of the resources that it took to address it, and to redo the work. Hopefully, we’re beyond that now, and hopefully, we will not have another occurrence.

Dr. Garrick mentioned the Board’s interest in TADs, and since I’m responsible for implementing TADs, I put a slide in here of my own just to give you an update as to where we are. As you know, we issued a performance specification, a preliminary performance specification on November 29th of last year, which allowed us to kick off a process to have basically the four vendors identified and the slide to develop proof of concept designs to see whether or not that performance specification works.

We received those proof of concept reports, and we have reviewed them, accepted them, and paid the contractors for their efforts. And, we have also, as part of that process, asked for input from the vendors on ways that this specification could be improved, and we're evaluating that right now. The next steps in terms of implementation of TADs will be the issuance of a final specification, and the initiation of the next procurement to vendors to allow them to develop our designs under Part 71 and 72, and submit them to the NRC, generally, about the same time that we're submitting a license application for the repository.

I'd like to end up with basically a recitation of the Director's expectations of the program team. And, this is a presentation that he's given, or a slide that he's used in internal discussions with the staff.

First of all, compliance with quality assurance requirements is not optional. Safety, quality and schedule discipline are not mutually exclusive, as I mentioned earlier. The corrective action process is the process to be used for fixing problems, and I think the program is slowly turning to making that happen. We’re directed to work across organizations as a team to bring our A game, and I guess that’s in reference to our Tiger Woods. But, we’re not playing too much golf in the program right now. To be accountable for your performance, and integrity is a prerequisite of working on this program.

And, with that, I’d be happy to answer any questions that the Board might have as to the status of the program.

GARRICK: Questions from the Board? Andy?

KADAK: Kadak. You mentioned a number of areas, but you didn’t give us any information about where they stand. For example, the EPA rule you said will be issued shortly. Can you give us more definition about when that is?

KOUTS: We hope to have it issued shortly. It’s not within the control of the Department as to when that will be issued. That’s basically an intra-governmental process at this point.

KADAK: Are you guys part of that discussion?

KOUTS: We are part of it, but we don’t control it.

KADAK: But any sense of--are we talking a year from now, two years from now? I mean, this is pretty important, don’t you think?

KOUTS: Yes, it is. I can’t really give you a time frame on that.

KADAK: Okay. On the question of TADs, you said you got the preconceptual designs, or whatever you--concepts. Now what?

KOUTS: Well, as I just mentioned, the next step will be to issue a final specification. If you looked, originally, we issued a preliminary specification.

KADAK: Right.

KOUTS: The next step then would be initiation of a procurement for the vendors to develop designs and submit them to the NRC under Part 71 and 72.

KADAK: When would that occur?

KOUTS: I won't say specifically, but it's going to be in the very near future.

KADAK: And, your expectation for having TADs available for utilities is by when?

KOUTS: Assuming that we go forward with the program as it's currently planned, the earlier we could have them available to utilities would be about in the 2011 time frame.

KADAK: Two quick short ones. Is the DOE now working at identifying and developing interim storage facilities?

KOUTS: No, it is not.

KADAK: Okay. Last problem. Slide 9, what is--these are obviously very good goals. Now, why is it so difficult to do these things?

KOUTS: I think the program, to an extent, has done these things in the past. I think what we’re looking at is to raise the organizational behavior to hopefully all the same level in all areas, and I think that’s the best way that I think I can characterize it. I think that it’s been a long struggle within the program. I’ve been in it for over 20 years, and the understanding of the licensing process, the understanding of the CAP program, as you can see, we had some issues with USGS in terms of quality assurance, I think it took those speed bumps, if you will, to get people’s attention and to make them understand that you need to follow procedures. And, that’s the fact of the matter. We’re trying to elevate the performance of the program to be what we believe will be acceptable to the NRC from a licensee standpoint, and from an applicant’s standpoint.

GARRICK: Henry?

PETROSKI: Petroski, Board.

I’m interested in the corrective action process. Could you elaborate on that a little bit, and, say, give an example of a problem that is being fixed, and also explain how problems are identified to be subject to this corrective process?

KOUTS: Okay. Well, briefly, we have a corrective action within the program where anyone in the program can write what we call a CR, and at that point in time, those--that CR is evaluated by a screen team to understand its validity and so forth, and then it’s graded, and then it’s assigned to an individual manager. And, that manager essentially is responsible for evaluating it, if necessary, go through a root cause discussion, and at that point in time--at every step along the way, his actions are reviewed by other people to make sure that that corrective action and that condition report, if you will, the original CR, is addressed thoroughly, and such that this condition report would not occur if the aspects of the corrective action plan are fully implemented.

What has happened in the past is that where a condition report was submitted to the system, and a manager would develop a corrective action, that corrective action would be taken, but in many cases, the corrective action did not get to the root cause of the problem. It was not a comprehensive analysis enough to make sure that that condition report could not occur again. And, what we’re doing now is we’re essentially re-evaluating all our condition reports and making sure that all our corrective action plans truly are comprehensive, and will make sure that it’s not a continuing problem in the program. That’s taken a lot of time to go through, a lot of discussion with the managers, a lot of talk in terms of does this corrective action plan really address the root cause issue.

So, that’s, to give you an example, quality assurance is a concern within the program, it’s certainly a concern at the level of the Director, and he owns that condition report. And, it will be his corrective action plan in terms of implementing that to hopefully bring the quality assurance program and process up to the level that we need to be as an applicant and as a licensee.

PETROSKI: How long does this process take?

KOUTS: It depends on the individual subject. In some cases, they could be very minor. In other cases, they’re more substantial, and it can take hopefully no more than several months to resolve. But, in the case of we’ve had some very long-standing CRs within the program, and that’s one of the issues that we’re trying to resolve, some of which are over a year old, and we’re trying to do the best we can in order to close those as quickly as we can.

PETROSKI: Thank you.

GARRICK: Mark?

ABKOWITZ: Abkowitz, Board.

Chris, I was wondering if you could comment on the implications of the budget that DOE received with respect to the planning and the area of transportation. It was one of the four priorities, but my understanding is that it has taken a disproportionate share of the cuts that have come along. Could you elaborate on that, please?

KOUTS: That’s essentially a management decision and a value judgment within the program. The driving force right now behind the program is to, first and foremost, to submit a quality license application to the NRC, and basically, all other areas of the program are of secondary importance in terms of trying to accomplish that.

Transportation is, like other areas of the program that is not getting the funding that was originally intended, obviously, because we had a $100 million reduction. In terms of a disproportionate amount, you know, all I can say is that my areas of the program also took reductions, and other areas have to look very carefully at reductions also. So, yes, transportation was reduced, and typically when we get into that kind of situation, you can't have transportation unless you have a place to send it to. So, you've got to make sure that first and foremost that the repository and the license application is receiving the necessary resources in order to accomplish its objective.

GARRICK: Ron?

LATANISION: Latanision, Board.

Could you give us an update on the transition in terms of lead labs, the impact in terms of staffing, scheduling with reference to the license application, et cetera? What impact, if any, has this had, and is the transition more or less complete at this point?

KOUTS: My perspective, and Claudia Newbury is nodding to me, yes, the transition is complete. With any transition like that where you're taking a large component of work away from one contractor and giving it to a lab, obviously, there are issues. I think we have worked through that. Sandia National Lab, who is our lead lab, is fully at work, working on their aspects of the LA, which is basically postclosure performance, and they are coordinating closely with Bechtel in relation to the development of the license application, because it all has to come together and make sense from a preclosure and a postclosure standpoint.

So, a simple answer to your question is the transition is complete. There were, I will call them speed bumps along the way, but I think Sandia has the resources that it needs this year, as Bechtel does, in order to accomplish what they need to accomplish. And, all reports are that it's working smoothly at this point.

GARRICK: Bill?

MURPHY: Bill Murphy, Board.

I'm curious about the status of the performance assessment calculations to support the license application. Could you mention where that stands? Are new data being collected, or new models being developed, or calculations being conducted, or sensitivity analyses being done? At what stage is that?

KOUTS: The answer to all your questions is yes.

Essentially, I believe all the data feeds, or most of the data feeds for the TSPA are essentially complete, and, in the very near future, we'll start essentially running the model and doing the sensitivity studies that we need in order for our safety case in the LA. So, that's ongoing, and it's a continuous process, and will be a continuous process until we submit the LA.

GARRICK: Chris, the Board is under the impression that there are some schedule slippages, and that this is one of the agenda items this week in Nevada on the status of the project. Can you give us kind of a heads up on what slippages we're talking about, and what might be the possible impact?

KOUTS: Well, the slippages that you are referring to, from my perspective, don't impact the June 30th date in any way. We'd like to try to submit the LA as early as we can, and basically, the plan is to have the LA done early next year. So, to the extent that you are hearing about slippages, it's slippages associated with an early next year submittal of the LA, not the June 30th date, no later than June 30th that the Director has committed to Congress.

All I can say is this is typical project management. Whenever you have a great deal of activities being done by various groups, you've got to manage the process. And, in one case, if someone is behind, you need to find out what the reason behind that is, whether there are resources involved, whether there are decisions that need to be made. And, that's essentially what goes on at our monthly project reviews, which actually is going on today in Las Vegas. We go through in detail all the different components of the, especially of the license application, and identify what areas and what deliverables, where they are, and whether or not they're behind, whether or not they're going to catch up in the near term, and so forth.

But, the simple answer to your question is although you may have heard that we're behind, that's not in any way impacting a June 30th submission date for the license application.

GARRICK: Thank you. Are there any questions from the Staff?

(No response.)

GARRICK: All right. Thank you very much.

I guess our next speaker will be Ray Wymer. Ray is a consultant, retired from the Oak Ridge National Laboratory, and he's going to talk to us about depleted uranium oxide as a chemical barrier. Welcome, Ray.

WYMER: Thank you, John.

Thanks for the opportunity to make this presentation this morning. It's a little bit off the principal target of this group, but I think it's a chemistry of some interest to you, and it’s sort of, you might view the information I’m going to present as another arrow in your quiver that you’ll have available to you when you want to fire a bolt at the Department of Energy.

So, give me the first slide.

GARRICK: We don’t do that.

WYMER: I would never accuse you of that, John.

I wanted to point out that this talk is based on Russian work. It was started under the International Science and Technology Center, which is a program of the U.S. Department of State. It was created almost specifically for the purpose of occupying former nuclear weapons, Russian scientists in non-weapons research, but nonetheless, with a nuclear application. And, my role in this was as a technical advisor to this group, made a couple of visits to Russia, looked at their facilities, and gave them insights that they did not have on what the Yucca Mountain repository was all about, and some of the problems that might need attention. So, I didn’t do any honest work. I just simply followed what the Russians were doing and advised them.

The next viewgraph, that is something that is familiar to certainly all of the Board, maybe not to all of the members of the audience, it’s a picture of the cross-section of one of the proposed drifts in the Yucca Mountain repository. And, the purpose of this viewgraph is to point out some of the places that depleted uranium might be used in the Yucca Mountain repository, depleted uranium oxide.

One application is as a backfill around the waste package and the drip shield. Another application might be as material to fill in inside the waste package, packed in and around the fuel elements themselves. And, it also is meant to show that there are various places where there are various kinds of metals in the repository, in particular, around the waste package, or as part of the waste package, as part of the drip shield, and perhaps as part of the invert at the bottom of the drift.

Next one, please. Okay, now, the source of depleted uranium is the UF6 that’s stored in 14 ton containers, mostly at Paducah and Portsmouth. Now, it’s all that’s shipped off the Oak Ridge site, the K25 site. There’s about 700,000 metric tons of UF6 in those cylinders, and as I speak, there are two conversion plants being built, one at Portsmouth, and one at Paducah, Portsmouth, Ohio and Paducah, Kentucky, to convert that UF6 to mainly the U308. The process is based on what is fundamentally a French process for conversion of UF6 to oxide.

The way the process runs is it produces about 80 percent U308 and about 20 percent UO2. It could be fairly--the way we’ve talked to the people at Paducah, it could fairly simply be changed to produce completely UO2, if that were desirable.

I should point out that recently, there’s been a suggestion that that depleted uranium might be re-enriched. You know, it runs about .35 percent uranium 235. There’s still some good U235 in there. As UF6, it could be picked up by the U.S. Enrichment Corporation, or one of the two enrichment groups that’s currently building enrichment plants, to extract additional uranium 235 without having to go to all the cost of producing UF6. So, there may be some economic advantage to this, and that’s being looked at.

Next one, please. Now, these are what I consider to be the primary factors in the release of radioactivity, and you’ll see how these fit in with the UO2 discussion shortly. Most important, of course, at the top of the list is water. If you don’t get water into the waste package and into the fuel, you don’t have a problem. So, the ingress of water, one way or another, is of primary importance.

The next comes the potential failure of the drip shields, which is, as some of you may know, is the titanium palladium alloy that surrounds the waste packages. Next is the breach of the waste packages themselves, which are thought not to be breached very readily since they have Alloy 22 surrounding them, which is very resistant to corrosion.

Next, if you get through that barrier, then it’s the breach of the fuel rods themselves, some of which will have failed after a few thousand years and they’ll have cracks in them. Then, the fuel itself, the fuel pellets will dissolve, or if it’s the 10 percent of the content of the repository that’s devoted to vitrified waste from the plutonium production sites, it will be glass that would dissolve.

And, then, the next, the transport of radioactivity in water, either as dissolved radioactivity, ionic or as colloidal, either primary colloids or pseudo colloids. And, pseudo colloids are colloids that are sort of the second-hand ways of producing colloids. You have something like ions that will form a colloid, and then one of the fissure products will sorb on that, and that’s a pseudo colloid. And, that transports just like a colloid, just like it were a primary colloid.

And, then, the engineered and natural barriers, some of which are listed above, if they fail, and finally the primary factor in release is the chemistry actually in the drift in the unsaturated zone and in the saturated zone. So, you have these, these are the primary factors.

The next viewgraph, please? As far as things being chemically important in the repository in connection with DUO2 applications, application of DUO2 in the repository, the water, of course, is very important and its amount and composition is important. I’ll talk just a little bit about the composition of the water in a minute. Of course, air is important, and the most important things in air, in my judgment, are the oxygen and the carbon dioxide present in the air, and in the water.

And, in temperature, anything having to do with chemical reactions is temperature dependent. And, radiation, of course, the radiation produces, as most of you know, various kinds of oxidizing species, hydrogen peroxide radicals, and potentially, possibly, the production of nitrogen oxides from the nitrogen and oxygen in the drift to produce ultimately nitrite or nitrate. And, then, of course, the materials of construction, which I mentioned a little bit earlier, being a lot of metals in there which are intrinsically reducing agents.

Next viewgraph? Okay, starting at the top, the importance of air is that air will get into the drift in several different ways. Barometric pumping as the barometric pressure goes up and down, there will be either vacuums or pressures induced on the other rock, and it will pump air into the drift. Of course, diffusion of air down through the cracks in the Yucca Mountain, and water itself, of course, will have dissolved air in it, and most important will be, I think, the carbon dioxide that’s present in the air as far as the chemistry of uranium oxide dioxide is concerned.

And, I want to point out that there will be, despite the fact that the repository is inherently an oxidizing environment, there will be local reducing conditions due to the presence of metal. So, there are tons of iron in the repository, and, of course, there’s a lot of metal around the waste package. So, at a long time scale, you know, thousands, tens of thousands of years, even some of these metals that are very resistant to corrosion can be expected to corrode eventually, somewhat.

Next viewgraph, please. As far as the temperature is concerned, as you all are well aware, as long as the drift itself is above the boiling point of water, water will not get into the drift. It will be held out. And, that will be for a thousand years, give or take a few hundred years.

The solubility of the UO2, depleted uranium oxide, is of course temperature dependent. And, as you will see later, the sorption of radionuclides, and specifically, I’ll be talking about neptunium and technetium, is temperature dependent, quite strongly temperature dependent, and is obvious, I think, corrosion of the metals and of all of the ingredients in the repository, they are temperature dependent. And, the chemistry itself, both with respect to the chemical reactions that take place and the kinetics, the equilibrium are shifted as a function of temperature, depending on things like complexation formation, dissociation and dissociation of complexing species. These things are all temperature dependent. So, temperature is a big actor in the repository, as I’m sure you’re aware.

Next? This is data that was obtained in Russia, and these are the principal investigators. I don’t have information, I do not know information specifically on all the analytical techniques that were employed, although they are very competent and they have cutting edge technology, mainly with equipment that we have bought for them in connection with this program, and other programs. But, they are very well set up to carry out the kind of work that you will see discussed.

Next viewgraph, please? Well, you have to talk about the water that’s coming into the drift, and as you go through the literature, you will find several different rack-ups of the composition of J-13 water, which is sort of standardized. It’s been chosen in order to be able to fix on some kind of a composition of water, and it pretty much has this composition. You will find some tables that vary a little bit from this. Also, there are wells that have been dug in and around the proposed repository, which have compositions that are not the same as this. But, in order to have something to work with, J-13 well water had sort of been settled upon. It’s not to be believed wholesale, I don’t think, that this is the exact composition of the water that will be dripping into the repository.

The most important thing, in my judgment, on this listing is the carbonate ion concentration. It's fairly high in millimoles per liter, and it has a very profound effect on the chemistry of everything that goes on in connection with the UO2 and its potential use in the repository.

Next viewgraph? The Russian study, three samples, the most important features of which was the temperature at which it was prepared. Samples one, two and three are progressively higher in the preparation temperature. This has a distinct effect on the behavior of the UO2, on behavior of its chemistry, and in particular, on the behavior of the surface chemistry of the UO2. And, as you will see as I go through this, it's not the bulk of composition of the UO2 that really is the deciding factor, it's the concentration of the surface layer, what's the composition of the surface layer with respect to uranium 4 and uranium 6.

Next viewgraph? This merely shows you measurements of the particle size distribution for anybody who is interested in boring into that detail. They're very similar for all three of these samples, annealed at all three temperatures.

Next viewgraph? I found this to be a fascinating piece of information. This represents the permeability of thin layers of UO2 with respect to distilled water and J-13. I see the distilled water didn't come out. It's above, a little bit above the J-13 solution. But, you see, after half a year, or so, the water, J-13 water does not go through a thin layer, a millimeter or so thick layer of UO2, and this is the low temperature annealed UO2. So, I thought this was a fairly important observation, the fact that it inhibits the movement of water pretty appreciably, I would say, although to what extent it happens over 10,000 years is another story.

HORNBERGER: Ray, can you tell us a little bit about what that actually is? What kind of filtration rate? How is this experiment being done?

WYMER: How does it--well, this is in centimeters per day, and the experiment was performed on a cylinder of UO2, about a millimeter thick, and J-13 water was just simply put on top with no more pressure than just that exerted by the weight of the water.

Next viewgraph, please? Well, a very important consideration is the solubility of the UO2, and this is the phase ratio of the amount of UO2 with respect to the volume that it’s shaken up with. And, both distilled water, which we call DW here, and J-13 water were used, and it was filtered through three different porosity filters to make sure we weren’t looking at colloidal UO2 instead of dissolved UO2.

And, this is an extremely chemically complex system. I hope that will come out in the next viewgraph as we go along.

Next viewgraph, please? This just simply shows how fast the solubility measurements came to equilibrium, and you can see that the low fired UO2 in J-13 water had a solubility up here at around almost 5 parts per million, whereas, the higher fired material was less soluble under these conditions.

Next viewgraph? This is the intermediate fired and the highest fired UO2 were separated out and look at in J-13 well water at Page 7, and it sort of repeats what was in the other viewgraph, but in a little bit more detail. The equilibrium was essentially reached in a couple of weeks as far as these measurements are concerned.

Next viewgraph? Now, this shows that there was an effect of particle size in the water. The fairly porous shoulder showed a fairly high uranium content in this low fired material, and as you went to the very fine porosity filter, you got down to a factor of 20 lower, which means that there was some very finely divided UO2 from slow fired stuff. It was also true of the other two samples, the higher fired samples, but not to the same extent. They pretty much reached where they were going to get to with the 50 nanometer filter. So, you had to be very careful with these measurements that you’re not mistaking solubility for just extremely fine particles or colloidally dispersed particles.

Next viewgraph, please? I just put this in because I think it will help you understand a little bit later some of the sorption studies of neptunium that you will see. This shows the chemical--these are thermodynamic calculations, but these kinds of plots are common in the literature. You all have seen a lot of them if you've gone into this field at all. This is in J-13 well water, so there's a little bit of fluoride in the well water. That shows up here at the lower pHs, and the formation of UNO complexes. You manage to get to pHs where the carbonate ion can exist, where it's not dripping off the CO2 because of the acidity, then you begin to form carbonate species, and these will be the species that are present on the surface of the UO2, not in the bulk of the UO2. And, I think that they will play a big part in the explanation of some of the chemistry absorption.

Next viewgraph, please? Okay, we get in now a little bit to what's happening on the surface of the UO2 samples. When it just says UO2, that means the low fired stuff, and you see the function of pH here, how much U4 and how much U6 do you have on the surface, even though if you were to do a bulk analysis, it would still look like UO2. But, the Russians did measurements that just looked specifically at the surface layers. And, when you do that, then you see that the valent uranium on the surface varies as a function of pH a little bit.

Next viewgraph, please? This is another way of presenting that data. It shows the distributions of forms of valent states for the low fired and the high fired UO2, and you can see that the blue here is the U(IV), and it goes away as the pH goes up.

Next viewgraph, please? This shows uranium solubility, the theory versus the experiment. You can see that the low fired material here are--the black line is the theoretical solubility, the red line is theoretical solubility of U(IV) and U(VI) respectively, and the dots are the experimental data. So, they lie fairly close to the red line, which is the theoretical solubility of Uranium(VI). And, you can see that as the pH goes up, the solubility goes down.

Next viewgraph? This is meant to show that if you reduce, if you keep the uranium under reducing conditions, then the solubility remains low, at low pHs. But, it will be essentially the same once you reach a pH of about 4 ½ or 5. But, this does show that the presence of the uranium in an oxidized condition is a good deal higher, and is under reducing conditions, these are very low solubilities to start with in millimoles here. So, there is an effect. You can keep the system reduced, and reduce the solubility.

Next viewgraph, please? Now, I want to get into the sorption of neptunium. I wanted to point out that what

I’m going to be talking about is the sorption of neptunium on UO2, and its surface oxidized species. But, there’s a very good discussion of the speciation of neptunium in spent fuel, not with regard to sorption on UO2. It was done at MIT in the Nuclear Engineering Department. This is the, if you wanted to access this, this is the number you go to.

Next viewgraph, please? This is a viewgraph meant to point out something that most of you knew. It was taken from John Kessler, who sent this to me from EPRI, and I wanted to get away from either the NRC information or the DOE information, and present an independent view of the situation. And, it’s pretty close to both the NRC and the DOE model.

What it shows is the importance of neptunium with respect to contributing dose at the site boundary, which of course is what this whole thing is all about. The dose at the site boundary is the big deal. It doesn’t much matter what happens in between if the dose at the site boundary is acceptable. But, under some conditions, it may not be. So, this is the proposed, if you’re standard, this line shows what the dose would be without neptunium, and this shows what the dose would be with neptunium, according to John Kessler’s calculations. And, these are the contributors to the site boundary of technetium, iodine, plutonium, and this solid line is the total dose, of course. So, I just put that in there--oh, and this is John’s model of the failure of the fuel pin cladding. Once the water gets that far into the repository, then he assumes this “S” shaped curve. This is cumulative failure, and so adding something like 10 to the 5th years, it’s all failed, in his assumption. This is an assumption, of course. As you all know, that’s one of the big buggles of this whole business, is nobody knows for sure what’s going to happen in 10,000 years. You’re taking this based on the significant extrapolations.

Next viewgraph, please? Now, this shows how the valence states of neptunium in normal environment air changes as a function of pH. And, so, there is a distribution of valence states, according to the Russian data, as a function of pH.

Next viewgraph, please? Now, we’re getting at the sort of the mechanistic look at this thing. In acidic solutions, it’s assumed by the Russian workers, based on their experiments, that Neptunium(IV) absorbs in acidic solutions as Neptunium(IV) when there is a mixture of the uranium valence states out in the solution phase. But as you go to neutral solutions and higher, you get an oxidized uranium film, which is both, you could call it U4O9, a well recognized uranium species, also could be considered 3UO2 and 1UO3 if you want to not violate valence rules. And, so, there’s an oxidized layer which has the average composition of uranium 2.25 oxidized, and on that surface, it’s Neptunium (V) and not Neptunium(IV) that absorbs, and it’s basically just Neptunium(VI) out in the neutral solutions.

Now, the thing that doesn’t show here is the fact that there are carbonate species of both the uranium on the surface, and of the neptunium, which is an indication of the extreme complexity chemically of the system.

Next viewgraph, please? This is a plot of the sorption of Neptunium(V) onto the various--well, two of the oxidation states, the lowest and the intermediate oxidation state, in distilled water, J-13 water, inert atmosphere, and in air. And, as you can see, the sorption approaches, of neptunium, approaches 100 percent and in the case of J-13 well water and the case of nitrogen for the lower fired material. The sorption is not nearly as high for the higher fired material, and that’s because, as pointed out earlier, the surface is much more stable to producing the higher oxidation state of the uranium on the surface. And, you can see these are run over long periods of time.

But, the important point here is that you can get, with J-13 well water, you can get to 100 percent sorption of Neptunium(V) on the surface, which I think is significant.

Next viewgraph, please? This is on de-ionized water in the presence of nitrogen, and again, you can see that the sorption in this case of the UO2 is lower fired, which again shows up at 100 percent in the presence of nitrogen. And, it’s not as good as these higher fired materials. So, if you were to use this as a sorbant, you would want to use the low fired material.

Next viewgraph? This is a function of pH, and neptunium sorption here for Neptunium(V) is not as high, it’s a higher pH. This is not totally consistent with the other results, but nonetheless, it’s what they reported. And, I’m not sure to what they attribute the difference, but I thought you needed to know that there is some variation in the results with time, with experiment.

Next viewgraph? This shows the sorption again of Neptunium(V), after equilibrium has reached, and you can see that the sorption again is about 100 percent, except at this higher pH where it drops down, so there’s a strong pH dependency, which you saw in the last viewgraph, stronger than you might expect, based on the equilibrium for the carbonate complexation. And, the higher fired material again absorbs less well than the lower fired material.

The J-13 well water did not come out as well as the previous slide showed. So, lower pHs are desirable if you’re going to get good sorption of the neptunium.

Next viewgraph? This shows the effect of temperature, shown here 20 degrees, 70 degrees, and 95 degrees for the two higher fired materials. And, the important point here is that as the temperature goes up, the sorption goes up. And, the repository conditions for quite a long time when the temperature is well above 20 degrees or 70 degrees, there will be pretty good sorption of the neptunium on the UO2. And, this shows that in about a day, it’s reached saturation, 100 percent sorption. So, there’s an advantage of having a hot repository for as long as--with respect to neptunium sorption for as long as it can be kept hot.

Next viewgraph? This was just, I think, meant to point out the influence of the carbonate ion. When you get to 100th molar HCL, you’re going to certainly suppress the presence of carbonate ion in solution, and as you can see, over time, the sorption just drops off. So, if you get too acidic, like 100th molar, a pH of 2, then you get practically no sorption, which suggests that it’s a carbonate mediated sorption process.

Next viewgraph? Now, I want to spend just the last couple minutes talking about technetium. Technetium, as you know, pertechnetate ion is the stable species, and if you were to dissolve spent fuel in an oxidizing environment, the species that would go into solution would be the singly charged pertechnetate ion, which is very mobile in the environment, it’s a lot like the perchloride ion, it forms few complexes, it doesn’t attach to things that are in the soil well at all, and it tends to move, it would tend to move with the water.

However, in a repository environment where there are reducing agents locally, you could produce Technetium(IV), so that’s what the Russians concentrated on, was if you in fact produce Technetium(IV) perhaps by making the invert out of iron, and with all the other iron that’s in the repository besides, you know, there’s iron in the walls of the drift, there’s various kinds of metal alloys in the waste package, so, you could have a reducing condition in the water that comes in. And, so, if you produce Technetium(IV), I don’t assure that you would, but if you did, then the following information should hold true.

The next viewgraph, please? This shows that Technetium(IV) sorption is a function of pH in a nitrogen atmosphere, and as the pH goes up, the sorption goes up. So, there’s an advantage in having a reducing environment. Uranium dioxide itself is a mild reducing agent, wanting to go from Uranium(IV) to Uranium(VI). Whether or not this would have any influence has not yet been sorted out, sorted out by the Russians. But, at any rate I take this to be somewhat encouraging, that the pHs of about those that are going to be present in the repository, you’re likely to get very good sorption of the technetium, which if it didn’t hold it forever, would certainly hold it a long time. It would slow it down. It would slow down its release and transports through the environment.

Next viewgraph? This is meant to show that it doesn’t make any difference. If you had Technetium(IV) to start with, whether or not you’re going to have a reducing agent, does not change anything. It’s already Technetium(IV) and it’s not going anywhere. Iron 2 and hydrazine were both put into the solution, and you can see as you get to the pH of the repository, you’re not going to get much free Technetium(IV) in the repository.

And, finally, this is sort of a summary that the sorption increases as pH increases, and at pH 7 or above, the sorption of Technetium(IV) is essentially 100 percent.

Next? Because the conversion process at Paducah and Portsmouth produces about 80 percent UO3 instead of 100 percent UO2, we asked the Russians to take a look at sorption on U3O8. That has not yet been completed.

That’s it of my presentation. I want to stress that I’m not advocating that we pour a lot of UO2 in the repository. I merely wanted to point out to you that there is UO2 present in the spent fuel, of course, quite independent of what you might additionally put into the repository, and this is just meant to give you a little insight into some of the chemistry that might take place in the repository under various conditions that are likely to exist in the repository as a function of oxidizing conditions as a function of composition of the water, as a function of pH and a function of temperature, that all these factors that are present and variables in the repository influence the sorption of these two key species with respect to dose at the site boundary, namely technetium and neptunium. And, that’s my message for today.

Final viewgraph should say The End.

GARRICK: Okay, thanks, Ray. We have a number of questions. Ron, do you want to start it?

LATANISION: Yes, Latanision, Board.

Ray, I don’t see a number on this slide, so I’m looking at the--

WYMER: It’s in the right-hand corner--I’m sorry if it isn’t there, but--okay, well, just tell me--

LATANISON: Well, it’s the slide that shows that thin layers of UO2 become impermeable. On Page 3, but I don’t see--

WYMER: What about it?

LATANISION: Not the right one. Let’s try--

WYMER: What’s your question?

LATANISION: It’s the slide that shows impermeability to water.

WYMER: Yes, right, this one.

LATANISION: Yes. I don’t know which number it is.

There you go, just passed it. There it is.

WYMER: Yeah.

LATANISION: How is the UO2 introduced onto the surface, and what is its state? Is it glassy, is it--what--

WYMER: No, it’s a powder. It’s present as a powder, of the particle size distribution that the preceding slide showed. It was that powder that was put on as a thin layer onto a porous disk, and then it was through that layer of that powder that the water was allowed to flow. And, after a couple, three months, it stopped coming through.

LATANISION: Was the powder treated in some way?

WYMER: The powder was fired at 650 degrees, and it was just simply put on. It was the same material that you’d use in all these other experiments I showed the viewgraphs of, the very same material.

LATANISION: It’s certainly very intriguing to see that kind of data for oxide materials. That’s not common.

WYMER: As you probably noticed from the distribution, it was finely divided material, and the speculation is that it formed a hydrous layer, or a hydrous oxide layer right on the surface of those tiny particles, which essentially plugged the pores.

LATANISION: Yes, you’re going in the direction that I was going to head, and that was whether there was an evaluation of the change in surface chemistry of these powders as a function of time as well?

WYMER: They did not get that far. They merely made this observation.

LATANISION: Well, it’s very intriguing.

WYMER: I thought it was also very intriguing.

LATANISION: Thank you.

WYMER: I think it has implications with respect not only to the fact to maybe putting UO2 into the waste package surrounding the spent fuel, but also with respect to the UO2 in the fuel pin, which while it’s not the same as this low fired material, might in fact see some of this effect.

LATANISION: Latanision, Board.

I mean, I guess my concern would be, however, that if the powder is swelling, and, therefore, limiting the permeability of water, that could present another problem that would not be so attractive.

WYMER: And, the Russians did not do any experiments on volume change of the material. I would not expect that it would swell a lot. I think it would be just very much surface, and not--there would have to be some swelling, obviously, or it wouldn’t close the pores.

LATANISION: Yes. Thank you.

GARRICK: Howard?

ARNOLD: Arnold, Board.

I’m confused, I guess. Every time I’ve asked questions about the behavior of the spent fuel, the answers come back, well, once the cladding is breached, then the pellets dissolve because it’s an acid environment. And, I guess I’m not hearing that here. I’m hearing an entirely different story. But, I’m not knowledgeable enough to understand the difference. Are you implying that the UO2 pellets themselves will survive?

WYMER: I think there are some of the things that you’ve seen with these powders that will also happen on the surface of the UO2, depending on the degree of fragmentation, I would suppose. Of course, the UO2 pellets are all cracked. But, I don’t think you would see an effect of the UO2 pellets anywhere approaching the kind of things that you see, or to the degree that you see on these powders. I don’t think that the--I don’t know what the repository environment would be with respect to acidity. You see all kinds of discussions of whether or not radiolysis would produce nitric or nitrates.

ARNOLD: Maybe I misspoke. Maybe I should have said oxidizing environments. But, yeah, the question is the same one, are you implying a different behavior of the UO2 pellets than what I’ve heard in the past?

WYMER: I’m saying it’s conceivable. But, they did not look at UO2 pellets, which are much higher fired, and they are much denser than this material. You might get some, on a crack, as it starts to dissolve in the water that hits the crack, you might get some exfoliation where you form a powder material that would have some of the characteristics in this work, yes. But, we don’t know.

GARRICK: Thure?

CERLING: Cerling, Board.

On Slide 13, you showed this nice distribution of particle sizes, and so on, and then later, you showed a slide that showed that there was some colloidal material being absorbed when you are doing the filtering.

WYMER: Right.

CERLING: And, did they have a sense of whether that colloidal particle was re-suspension of this original material, or was it new and different, I guess, different than the original material?

WYMER: I think the assumption was that it was just this material suspending. There were no chemical treatments of it to alter its chemistry. They just simply looked at the particle size, and filtered the particles, and some of the particles are down at .19 microns. That’s pretty small stuff. So, the filter they used was small enough to catch most of it, and as you saw from the viewgraph, all three of the samples at the very smallest pore filter had about the same value, or solubility.

GARRICK: David?

DUQUETTE: Duquette, Board.

I’m not as mathematically challenged as my material science colleagues. Can I look at Slide Number 2? This looks like a very different concept than we’ve been talking about, with the exception of the titanium drip shield and the steel lined tunnel. Is this your suggestion? Is this a Russian suggestion? This is, I mean, even the package itself that’s a Cermet waste package, would be a totally different package that we’ve been talking about so far.

WYMER: This is based on work that was done by the Russians, and supported by the U.S. Department of State, and they are, the Russians are looking at a Cermet as the waste package. This viewgraph was made for a different selling job than today. Of course, it’s Alloy 22 for you people. So, this stuff down the left-hand side, especially that which shows the waste package, is different.

The filler material, the component of backfill that it shows there is really meant to be a Richard’s Barrier, which as some of you know, is a coarse material, topped with a very fine material, which by capillary action, redirects the water around the waste package and down the side. And, our thought was that if you made that out of UO2, you’d get a double advantage. You’d not only get the Richard’s Barrier advantage of diverting the water around the waste package, but any water that came in would become saturated with UO2, thereby, greatly reducing almost to zero the driving force for dissolving the UO2 in the pellet. You'd already have a saturated UO2 solution of depleted uranium by the time that water got to the pellets, so, there would be very little driving force except for the difference in the chemical activity of the UO2 pellet, based on the fact that it's now about 5 percent fission products, and it's fired for temperature. So, this would change the chemical activity a little. But, the solubility would be greatly reduced because you already have a saturated solution of UO2. So, it has a double, sort of a double whammy effect here, you decrease the solubility of the pellet, you brought it up against that, and if you had a Richard's Barrier, you'd also divert it.

The advantage of the UO2 saturation of the water before it gets to the defective pin would also be present in the case of packing UO2 around the spent fuel inside the waste package, which would be a much easier thing to accomplish than putting in a Richard's Barrier in a drift. That's not a simple thing.

DUQUETTE: Duquette, Board.

Why on earth would you put the titanium drip shield, given the engineering problems and cost of doing that, if you look at two pieces of data? One is that you're going to basically use the UO2 surrounding the package to absorb water, and secondly, I think you just showed data that indicated once it's saturated, it doesn't pass water anymore.

WYMER: That’s right. And, I think that the--it’s not out of the question that the drip shield could be left out of this repository. You know, these are personal opinions, but it’s a very expensive thing, and probably more expensive to make those drip shields with this titanium palladium alloy than it would be to pack depleted uranium oxide around the spent fuel inside the package. Now, there’s a question there, though, in that you have provided an insulator. If you pack the UO2 around the spent fuel, you effectively packed an insulator around the spent fuel, and it will get hot. So, you might want to store the fuel on a pad for a while before you put it into the repository and pack UO2 around it.

DUQUETTE: Thank you.

GARRICK: Bill?

MURPHY: Bill Murphy, Board.

I’m fascinated by the chemistry of UO2 added to the repository environment, and I think that--

WYMER: I knew you would be, Bill.

MURPHY: I think a lot of the effects that have been seen in these experiments are probably a consequence of the oxidation, rather aggressive oxidation of the UO2, and the formation of secondary uranyl mineral phases. And, you showed one diagram, this one, which is on composite Page 6, showing concentration approaching U(VI) solubility. I don’t know what phase that might be. Maybe it’s Shopite or some uranyl hydrate.

But, the experimental data on UO2 dissolution and natural urananite dissolution and spent fuel dissolution shows that under the oxidizing conditions of Yucca Mountain, it will oxidize fast. And, the depleted uranium that one would put in a repository, when exposed to the ambient conditions, would tend to oxidize fast. The surfaces would become coated with secondary uranyl minerals, which could very well explain why it becomes impermeable in a period of time. There’s a large volume increase upon hydration and oxidation of the UO2.

So, I’m a little dubious of the relevance of the sorption data on reduced UO2 surface, because by the time any of the matrix radionuclides could even encounter a buffer of UO2, the fuel itself would have to be oxidized. The pathways of ingress of oxidants and water and CO2 would have been established along the transport pathways within the EBS. So, I think while there may be a potential benefit in terms of diffusion barriers, or sorption characteristics of placing UO2 in the repository, it also is a, practical matter, just a place to get rid of the depleted uranium, which is of some value.

I think that its function is going to depend on how it behaves after it’s been oxidized, to a large extent.

WYMER: I know that this is a field of your expertise, Bill, these secondary phases. I think that a consideration that might come into play is that you get a surface layer, which then greatly mediates the subsequent reactions, rather than the entire material being oxidized so quickly.

But, yes, everything you say is right. We just simply do not know the answers to these things.

GARRICK: Thure?

CERLING: Cerling, Board.

One of the things that struck me in this presentation was, you know, the extreme fine nature of this stuff. It’s all, you know, sort of one micron and finer. Are they proposing to do further experiments on materials that are larger in size to be more practical to--

WYMER: Yeah, the size is up to as high as 7 or 8 microns. But, you’re right, they’re very fine. There’s a sad footnote to this, in that while the Department of State indicates an interest in continuing support of this, the work is in fact directed out of Oak Ridge National Laboratory, and people like myself follow the work, and advise them on experiments to perform that might be of interest. While the Department of State has continued to indicate possibility of support for this work, the Department of Energy has decided to withdraw the funding from ORNL, so there is no U.S. oversight of the work anymore. As a consequence, I see, personally see very little reason to continue to fund the Russians to do the work if nobody on this side of the ocean is telling them what to do.

GARRICK: Garrick, Board.

Ray, has there been any engineering studies done on just what it would take to implement such a process? I’m not thinking so much of redesigning the waste package with Cermet, or whatever, but just using it as an appropriate backfill. Has anybody looked at this from the standpoint of practicality and how it would be engineered?

WYMER: No. The only thing that’s been looked at, and that only in a very cursory fashion, is what I mentioned, namely the fact that it would be an insulator and the temperature would go up. You’d have to decide whether or not the spent fuel pins would tolerate the temperature without failing faster than they would ordinarily. But, there have been no cost analyses or engineering studies.

GARRICK: Yes. That was going to be my second question, was the trade-off between the accelerated deterioration of the fuel as a result of the increased heat with the advantages of the depleted uranium.

WYMER: No, this has not been done. The money has not been provided to carry out anywhere near that comprehensive a study. This is just meant to provide some interesting chemical insights that may have some relevance to the repository.

GARRICK: Has there been any attempt to quantify the effect of just the UO2 in the fuel with respect to this type of chemistry?

WYMER: No, they have not done any work with spent fuel at all in this program.

GARRICK: Andy, and then George?

KADAK: Kadak.

I guess I'm intrigued more by the holdup of neptunium and technetium as a useful application here. I wouldn't put any of this stuff in the canisters because that would complicate things even more, as you suggest. But, suppose--I remember at WIPP they have these bags of things on top of the plutonium canisters or barrels to absorb water, or whatever it is that control pH. Have you looked at something like that here? And, if Bill's concern is correct, namely the stuff oxidizing creates this layer of impermeability may not be helpful, but clearly, putting this on the ground, or in some place in the bottom of these drifts might be helpful.

WYMER: Yes, I think that's exactly right. I did not go into any of the other potential applications because of the emphasis of this Board being specifically on Yucca Mountain. But, yes, there are other potential applications that might be more practical actually than using this in the repository itself.

KADAK: I would suggest using it in the repository if and when the canisters are breached and begin to dissolve, and it falls onto this absorber, if you will, if it can be maintained.

WYMER: Well, that’s essentially what we were talking about.

KADAK: Well, no, you put in the cans, and then you also put it as a backfill. I’m talking about something more simple, putting it on the invert, or making the--

WYMER: Sure. Sure. There are a number of ways potentially to apply this. We don’t know how effective they would be. This is just interesting chemistry that has some potential application. But, yeah, there’s a lot of things you could think of, and that’s a good one. On the invert would be a good place to put it, as a matter of fact.

GARRICK: George?

HORNBERGER: This stuff that the Russians worked with has a huge specific surface area.

WYMER: It really does.

HORNBERGER: Surface chemistry effect. And, also, it’s pretty clear that you would never backfill with something in the micron size range.

WYMER: No, you wouldn’t.

HORNBERGER: So, then, given all that, the question is is there some extra advantage over backfilling with a larger aggregate, depleted uranium, versus some other getter?

WYMER: I think that the principal advantage might be towards that you would saturate the incoming solution with UO2. That’s the big thing that I can see. I think that’s pretty big.

HORNBERGER: What do you anticipate the basic effect would be? I mean, you would certainly reduce then the rate at which the uranium is dissolved. But, do you have a common ion effect that would slow down release of other nuclides?

WYMER: I don’t know. You get into the complexities of the chemistry because a lot of this dissolved uranium will have carbonate on it, and maybe some fluorides with the chlorides, as I showed there, because these are minor constituents in the repository, in the rock and in the J-13 water. So, I think the complexity gets out of hand, and that’s probably one of the major chemistry problems with the TSPA, is the chemistry is just out of hand. It’s far too complex to really model realistically. You’re going to have to step back a step and take a grosser look and look for overall moderating influences rather than detailed chemical effects, I would think.

GARRICK: Any other questions from the Board? From the Staff? David?

DIODATO: Diodato, Staff.

Dr. Wymer, I appreciate your presentation. I was intrigued by your assertion of temperature influence on sorbtion. And, you have the slide you showed, I think it’s about Slide 32.

WYMER: Yes.

DIODATO: So, would this experiment, can you tell me a little bit like was the possibility of out gassing of CO2 in this experiment?

WYMER: There was no purging with an inert gas, or anything of that, to carry the CO2 out. And, at a pH of around 7, or so, you would not expect out gassing of the CO2. It would be there as a carbonate ion.

DIODATO: So, a constant pH was maintained during this experiment?

WYMER: Yeah, as far as I know.

DIODATO: I mean, you could think of if CO2 was out gassing, then what would happen to the solution chemistry.

WYMER: It wasn’t boiled, and it wasn’t purged. So, there was no deliberate out gassing attempted.

DIODATO: All right. So, this is a constant pH, as far as you know?

WYMER: Yes. Well, so far as pH stays constant as you run the temperature up.

DIODATO: Well, that was my question, really.

WYMER: Yes, I don’t know.

DIODATO: Okay, thanks.

WYMER: Would be my answer.

DIODATO: I appreciate that. Thanks.

GARRICK: Any other questions from the Staff? Board?

(No response.)

GARRICK: Thank you. Thank you very much, Ray.

Our agenda says we're to a point of a break. I'd like to break until about 10 o'clock.

(Whereupon, a brief recess was taken.)

GARRICK: Before we turn the microphone over to John Bredehoeft, I want to indicate that there's going to be some minor changes in the program for this afternoon. All the subjects that are on your agenda will be covered, but we're going to change the order of a couple of them. And, in particular, the 1:50 p.m. Saturated Zone Testing paper will be given second, after the Near-Field Chemistry. So, Brady and Reimus are going to just switch positions.

The other thing is that we're going to crowd a little public comment time space in just after the Saturated Zone Testing, and before the break.

With that, we will turn the microphone over to John Bredehoeft, and John is from his own group called The Hydrodynamics Group, and will give us a progress report on the Inyo County Drilling Program.

BREDEHOEFT: Thank you, John.

I want to acknowledge my colleagues, Mike King, my partner, and Chris Fridrich, geologist with the U.S. Geological Survey, who has done a lot of the geologic mapping for us in the area.

Next slide, please? Now, we are the oversight contractor, technical contractor, for Inyo County. Inyo County, as many of you know, is in California. And, when we think about the repository issues in Inyo County, we’re concerned mainly with the potential for the movement by groundwater from the repository basically to Death Valley, to the springs in Death Valley. That’s our principal concern, and our program has been directed at looking at that concern. And, I will try to lay out the justification for the program, thinking behind it, in the next half our, or so, and show you some of the things that we’ve done.

Next slide, please? Now, we’ve just completed a well in this area right here. Actually, we’ve got two wells. We have completed one well to the carbonate aquifer, and then we drilled a second well into the Amargosa Valley fill into a reasonably permeable limestone at about 600 feet. The carbonate aquifer well is completed at 2800 feet into a permeable zone in the carbonate aquifer.

Now, what’s kind of interesting, I’ll point it out to you, is the repository is here. We have one well at the repository that went into the carbonate aquifer, UE-25P1. It penetrated the carbonate aquifer. And, there was an oil well that was drilled somewhere about here, drilled and plugged. And, that’s the extent of the drilling to the carbonate aquifer in the Amargosa area. So, this happens to be now the second observation well that we have to the carbonate aquifer after UE-25P1.

So, it’s interesting that we’re concerned about an aquifer here for which we have very little really subsurface information. We have other information. But, you know, in terms of drilling, there isn’t a lot.

Next slide, please? This is a log of the hole. This is the Amargosa Valley fill. We got into the Paleozoics here at 2400 feet, right here, and then drilled to 2800 feet, where we got quite a lot of water, lost the hole, actually, the first time around, and ran out of money. Then, we went back early this spring. We first lined the hole to this depth. We had to go in again and line the hole all the way to the top of the Palezoics, then completed this as an observation hole. We still have not pumped this hole enough to get it all cleaned up yet, get good water samples, and bring it on line as an observation well. We’ll do that in June. But, we’re not at that point yet.

KADAK: Could you help us with that? Where is the water table in that slide?

BREDEHOEFT: The water table is within 100 feet of the land surface. It’s right here. It’s in an elevation of about 2180.

KADAK: And, it goes down how far?

BREDEHOEFT: Excuse me?

KADAK: And, how far does it go down?

BREDEHOEFT: The well, we completed--

KADAK: No, I mean, is there water all the way from that depth that you mentioned?

BREDEHOEFT: Yes, to 2800 feet.

KADAK: It’s full of water?

BREDEHOEFT: It’s saturated.

KADAK: Okay.

BREDEHOEFT: All the way down. So, the water table is here.

In fact, not very far from this location as you go east through the Amargosa River Valley, the Valley is pretty wet, and there’s standing water in the Valley a lot of the time. So, the water table is very near the land surface as you go maybe two miles, or so, a mile and a half to the northeast of the particular location.

All right, next slide? Now, I want to review for you some of the thinking about how groundwater occurs in Nevada. When you go back to before 1950, the idea was that if you took any particular valley, there would be valley fill in this valley, and if you looked at the valley, it would be more or less a self-contained aquifer system, so that there was recharge from the mountain range. The recharge went into the valley, and then there would be discharge in the playa area. That discharge would also be in the form of free edified vegetation that went to the water table. So, these were thought to be pretty much self-contained systems. That was the paradigm when you thought about Nevada hydrology in the 1950’s, or so.

Now, there were a group of people, Ike Winograd--Ike is here in the crowd--working at the Nevada Test Site. And, Ike, Al Klept and his partner, Bill Thorasen, and they came to the realization that at least in the vicinity of the Test Site, that the valleys were underlain by this permeable aquifer, the carbonate aquifer, and the hypothesis was that this carbonate aquifer provided a kind of French drain underneath the valleys, and integrated the groundwater system from one valley to the next. And, that was the hypothesis that they developed in the 1958, the late Fifties basically, working, as I say, at the Nevada Test Site.

And, it turns out that hypothesis was very quickly picked up by most of the hydrologists working in Nevada at the time. So, there really wasn’t--and, it’s interesting to look back at this because they put this hypothesis out, and people jumped on it right away, said yes, that’s probably what’s going on. And, there was also a realization that some of these valleys, you could not account for the recharge and the discharge. There were discrepancies between the amount of water that's coming in, and the amount of water that was coming out of the valley. In other words, the valleys, not all of these valleys appeared to be self-contained.

So, this carbonate aquifer then was immediately looked at as this underlying large aquifer system which tended to integrate many of the valleys in eastern and southern Nevada. Now, this carbonate terrain is huge.

Next slide, please. Oh, back up just a second. One of the evidences that Winograd used, since there were not any drill holes to sort of say--you know, you couldn't trace this aquifer by looking at the drill holes, but the things they looked at were the water chemistry. And, they could see from the water chemistry that the springs and the water from wells that were developed in the Nevada Test Site had similar chemistry, and you could see that these are a group of springs here in the area of the Amargosa Desert and the Nevada Test Site, and you can see they all group rather closely together here, and, this cluster of, geochemical cluster, and that was one of their stronger evidences that this kind of under drain was occurring, and that things were moving, the groundwater was moving through the carbonate.

All right, now, the next slide, this shows the extent of this carbonate aquifer, and as I started to say to you before, it's huge. Here's California. I've got to be careful here. Here’s the Utah line. And, you can see we’re looking at most of eastern Nevada, and a goodly portion of western Utah, all underlain by this carbonate aquifer, and integrating many of the valleys in that part of the world into this sort of large aquifer system that underlies the whole area.

Now, this aquifer system, as you can imagine, it’s so huge that it’s broken up into kind of sub-compartments so that you, you know, it’s not all one big aquifer. There are compartments then in the aquifer itself. So, there’s one over in here that supplies the springs in the muddy river area, and then we sort of look at this area down in here where we’re interested, and this is kind of one big area here where we’re looking at the carbonate system as one entity.

Okay, next slide, please. Now, I want to come back to this one hole that we have at the Nevada Test Site. This is a kind of schematic cross-section of that hole. This is the hole, UE25-P1. It’s kind of the carbonate, Paleozoic carbonates here at about 1300 meters before the land surface. And, then, there was quite a large section of carbonate rock that was drilled down to about here. So, there was roughly 700 meters of carbonate that was drilled in this particular well.

And, it’s interesting that it encountered this Fran Ridge fault zone right at the top of the Paleozoics, and it at least appears to have encountered it right at the top of the Paleozoics. And, as in the usual situation, the Paleozoics here in this well were highly permeable. The other thing that was interesting at this well particularly is when you went into the Paleozoics, the water levels jumped by approximately 20 meters. So, we’ve got a 20 meter increase in hydraulic head between the overlying welded tuff material here, and the Paleozoic aquifer.

Now, there was some discussion about how--let me back up just a second. This water was relatively hot. It was 55 degrees, approximately 55 degrees centigrade. So, there was some discussion about was some of this head that we were seeing, difference in head attributable to the fact that we were filling the hole with lighter water, hotter water, therefore, less dense, and, in fact, you could probably in an extreme case, maybe account for 5 meters of head difference from the fact that you had this hotter water in the Paleozoics. But, still, there was still 15 meters, or at least 15 meters of hydraulic, higher hydraulic head in the carbonate here than there was in the overlying tuff aquifer.

Now, that’s important because what you’re saying to yourself is, you know, the potential here is for groundwater to move out of the Paleozoic aquifer into the overlying tuff aquifer, not for groundwater to move downward from the tuff aquifer into the Paleozoic. So, in fact, this higher hydraulic head is protecting the Paleozoic aquifer from downward movement of contaminants.

Now, there were a lot of interesting things about this Paleozoic aquifer. It had a very good earth tide, and the earth tide was analogous to the M-2, which is the lunar earth tide, the biggest one when you do the composition of the tidal components, the M-2, which is the big lunar 70 diurnal tide, and this particular well had an amplitude of 2.05 centimeters, which is easily observable. You know, you're at 4 centimeters up and down attributable to the moon. And, from that, you can calculate transmissivities. You can also calculate storage coefficients.

The storage coefficient calculated from that was extremely low. It was 10 to the minus 10, which we get for Paleozoic--confined aquifers like this, we normally see storage coefficients of something in the order of 10 to the minus 6, but this turned out to be 10 to the minus 10. That's extremely low. That suggested that the porosity of this material was very low, 10 to the minus 5, which is a very low porosity.

It was also highly permeable. They did a whole bunch of packer testing in this section of the hole, and it turned out there was 10 meters in here that had a transmissivity, which is the permeability, average permeability times the thickness, but there was a 10 meter section that had a permeability, or transmissivity, excuse me, of about 69 meters squared per day. I’ll come back and try to put that in context. I know it doesn’t mean much, just a number at this point. But, anyway, the hole was interesting.

All right, next slide, please? Now, as some of you probably know, there was two models being put together, one for the Nevada Test Site and one for Yucca Mountain, and they were modeling essentially this area of southern Nevada, and parts of California. A decision was made that that was redundant, and, therefore, the two models were combined into a single model, and it was decided that the USGS would complete this model. So, the USGS has basically built a model for this area here, and they’ve quite a large effort into that, maybe ten man years, maybe more than that, maybe as much as twenty man years into this model.

So, that model has integrated what we know of the regional geology, and pretty much the regional hydrology. So, it seems to me that this is a database which one ought to take advantage of in sort of looking at this particular region. And, we’ve done that.

Next slide? Now, one of the problems with trying to model this area is the geology is pretty complicated, and you can see here’s a cross-section block diagram through an area right in this area in the eastern part of the area they modeled. And, you can see it’s all broken up. So, there are all these fault zones in here, and it displaces the geology rather badly. So, if you try to follow any one unit in here, you see that the faulting breaks it up pretty badly.

Now, the question is how to model that kind of system. And, what they decided was, in the next slide, that what they would do is basically slice this system into 16 layers. So, there are 16 slices here, and the slices are pretty arbitrary, so they basically took the land surface, made 16 slices, and then decided in each slice, basically, in each cell for the model, what geology would be there.

So, this is not the typical kind of model where you would say follow the carbonate. Rather, it’s an arbitrarily defined slice, and then you put the geology in there that the slice intersects. So, that allows you to create the model, but it makes some complications when you’re trying to interpret the model, because you don’t know whether when you’re in this layer down here, let’s say Layer 15, whether you’re in the basement or whether you’re in the Paleozoics. You’ve got to look all the time to see where you are.

So, this model, even though it integrates all this stuff rather nicely, it is difficult to use when you start to interpret the results. And, as you can see, it’s 16 layers, it’s about 250 kilometers across, 300 kilometers this direction. The cell size in the model is 1500 meters by 1500 meters, so it’s 160 by 194 by 16. There is roughly a half a million nodes, cells in the model. And, the model has convergence problems. I’m running this model, but it’s sticky when you’re trying to make runs with it.

HORNBERGER: John, is it fully 3-D, or quasi 3-D?

BREDEHOEFT: It’s fully 3-D.

All right, now, one of the things we did with the model is to say to ourselves, okay, let’s look at how much carbonate rock is in the model, and this is the model area here, this area on top, and this is the thickness of the carbonate that’s in the model, taken largely from the geology. What they did is they got the geologists together, created a whole bunch of cross-sections through the area, and then connected those cross-sections and created these sort of isopacks of what the carbonate would look like.

This is 5000 meters of carbonate. So, you can see that there are lots of areas in here where you’ve got more than 5000 meters of carbonate. You’ve got a tremendous thickness of this carbonate. But, it’s not everywhere. So, there are also areas in here where there’s no carbonate. So, anyway, we’ve got this very thick carbonate, which underlies much of the area.

Next slide, please? And, one of the things you can also do with the model is it can output the water table, and this happens to be the water table. This is Death Valley.

Death Valley is right in here, and then the Funeral Mountains are through here, and this is the Amargosa Desert back in here, and there’s this sort of flat area in here in the Amargosa Desert, with Yucca Mountain being right in here. And, Ash Meadows, which is another outcrop of carbonate rock, is basically there.

So, next slide? All right, now, one of the things that Chris Fridrich has done for us is to map the carbonate rock in the Funeral Mountains, and this is the PCA, this color here, pinkish color is the carbonate rock in the southern part of the Funeral Mountains. So, you can see we have this area here is all--the mountain range is basically composed of carbonate rock. It has this rather interesting older material stuck in the middle here. So, there’s carbonate rock here, carbonate rock all around, and then this carbonate rock area here.

The Big Springs and Death Valley occur right in this area. So, they’re on sort of the southwest side of the Funeral Mountains, and the hypothesis that the water is moving through the carbonate rock and the Funeral Mountains to supply the Death Valley Springs.

So, next slide, please? So, one of the things that Chris did in his mapping of the system was to project the base of the carbonate rock in the Funeral Mountain area. So, this is the bottom now of the carbonate rock, and he did that by looking at this cross-section. This is the carbonate rock here, PCA, here, here and here, and he basically looked at these faults where they outcrop, and then projected them into the subsurface. And, by projecting them to the subsurface, you can then create this bottom of the carbonate rock, and then we can hypothesize that that’s the bottom of the aquifer.

So, we’re looking now at a map of the bottom of the aquifer. So, it seemed to me one of the interesting things to do was to say to yourself okay, can we, given that this is the bottom of the aquifer, and we’ve got carbonate rock in here most everywhere, can we create a model which will flow water through this system, and supply the Death Valley springs, and we did that.

Next slide? And, yes, it’s feasible to do that. What’s kind of interesting about it is that the water has to go through this narrows right in here. This is this sort of older material that sticks up through the carbonate rock here that I showed you on the original mapping. So, the water has to be channeled through here, and then down to along the Furnace Creek fault zone here. And, the most difficult string to create is this one, Nevares here, where there’s this long finger of carbonate that sticks out to Navares. It’s actually in the carbonate.

The other springs here are actually in a syncline of alluvial material that’s associated with the Furnace Creek fault zone. But, we’ve got a model. The model looks like it’s pretty reasonable, the kind of transmissivities that we’ve got, that we solve for are out of the model, look like they’re kind of medium values for what’s the transmissivity. And, I’ll show you in a minute what the transmissivity values look like.

We recreated the spring flow. These are the elevation of the springs. This one is very anomalous. But, you can see these are the bigger springs, Travertine, Texas, and then Nevares here, and then we’ve got a couple of smaller ones. These are what we estimate the flow to be. We don’t have really good records on what the springs are, but we can recreate them with the model reasonably well, so this is what the model does. So, it’s a pretty good fit.

Next slide? All right, now, I should tell you that Las Vegas is having water supply problems. It probably doesn’t come as a surprise to you. You know, it’s growing, is a city of a million, and it’s growing like mad. And, its water supply comes from the Colorado River at the moment. They’re entitled to 300,000 acre feet a year from the Colorado River. They’ve been taking that for some period of time, but they’re looking for additional water. And, so, what they decided was that they are going to look to this carbonate aquifer and see if they can get another 150,000 acre feet of water.

They’ve gone as far north as Ely, Nevada, and they are going to pump groundwater in the vicinity of Ely, pipe that groundwater to Las Vegas. Ely is almost 200 miles north of Las Vegas. So, we’re talking about a groundwater system in the vicinity of Ely, and a very large transportation system from that area back to Las Vegas. The costs are in the billions, but they’ve got 60,000 acre feet out of their first application in Spring Valley. So, they probably will get this water.

At any rate, as part of that, they put together a dataset for all of the transmissivity determinations that we have from the carbonate aquifer through this entire very large area in Nevada and western Utah, and this is the frequency distribution of the carbonate transmissivities here.

The transmissivity is the ability of the well, basically, to supply water. And, that probably doesn’t mean a whole lot to you. We’ve got 218 samples in this frequency distribution. But, let me try to put this in some context.

If you say to yourself okay, a good well is a well that I can pump 100 gallons a minute with 100 feet of draw-down, so I’ll suggest to you that that’s a pretty good well. That happens here, right here, so 85 percent of the time I can get a well better than that from the carbonate aquifer.

So, in other words, only 15 percent of the time is it less productive than that. And, when you look down in here at the 4 or 5 percent level, these are the wells that are not very permeable at all. So, then, you have to say to yourself, okay, we've got this aquifer out here, is it permeable everywhere. And, it looks to me like--I have asked this question to all my colleagues, hydrologist in Nevada who deal with the system, and it looks to me like if you're willing to drill 1000 feet or so of carbonate aquifer, you're likely to be getting yourself a pretty good well, at least a well that's better than this 15 percent. You will be somewhere up in here.

Now, looking at the model that we created for the Funeral Mountains, we got a mean value right about here for the transmissivity. It was 100 meters squared per day. So, that's what--it was a mean value. The value that we determined at UE-25P1 was 69 meters squared per day. You know, that's very close to the mean. It's somewhere right in here. So, again, we're kind of a mean value for what we got from the one well that we've tested in this flow system.

So, my argument is that this thing is probably permeable, reasonably permeable everywhere, and, you know, there's quite a lot of it.

So, the next slide? All right, so, taking a smaller area here, here's Yucca Mountain, the Amargosa

Valley, and Death Valley, and looking again at the carbonate, this is the carbonate that underlies this portion--this portion that I’ve highlighted here--this is the carbonate aquifer that underlies it, the carbonate thickness of rock that underlies it. And, again, you see this is 5,000 meters. So, we’ve got carbonate in here--well, let me just--this is the Furnace Creek fault, which is the southwest side of the Funeral Mountains, and you can see that most of the Amargosa Valley looks like, at least the geologic interpretation is that we’ve got more than 5,000 meters of carbonate rock. There’s a lot of carbonate rock.

So, then, the next slide? This is a gravity survey that was recently completed across the Amargosa Valley, and you can see this is the Amargosa Valley fill, and then beneath the fill is this carbonate rock again. And, again, you see that we’ve got carbonate rock, it looks like it’s everywhere in there, at least from the gravity data. And, again, you see that the Amargosa Valley here is pretty badly broken up, but the carbonate rock is there. And, again, here is the Funeral Mountains with the carbonate rock outcropping. This goes from the Funeral Mountains to Devil’s Hole Ash Meadows, again, across Amargosa Valley.

So, next slide? So, I said to myself, okay, let’s do something really simple. Let’s say to ourselves the aquifer is there, carbonate is there, and we’ll take the carbonate as described in the USGS model, so this is the area underlain by carbonate rock in the USGS model. And, we say to ourselves, okay, we’ve got this carbonate rock, let’s give it a mean transmissivity so it’s everywhere has the mean value that comes from our frequency distribution, 100 meters squared per day, and create a model for moving water through the carbonate rock. And, we’ll say to ourselves we’ve got transmissivity, a good one aquifer, one layer aquifer, so, carbonate rock with a mean transmissivity, and create a model. And, that’s what this represents.

So, here’s Yucca Mountain right up in here. Here’s Nevares Spring. This is the Furnace Creek fault zone across the southwest edge of the Funeral Mountains right in here. And, you can see groundwater is moving in this direction from up here, down towards the discharge of these springs in Nevares area, and also from the Spring Mountains here through Ash Meadows towards the Nevares area.

All right. So, we’ve got this one layer model, and then we say to ourselves, okay, suppose we start with some contaminant at Yucca Mountain, put a contaminant in here, and ask ourselves how long will it take for that contaminant to go from Yucca Mountain, we’re assuming we’ve got the contaminant now, and we just put it in the aquifer, how long will it take for that contaminant to come from the vicinity of Yucca Mountain to Nevares Springs. And, these are years.

So, the calculation suggests we get it there in less that 50 years. Now, of course, that calculation depends on what porosity you put in the model. The velocity of groundwater movement depends upon the permeability over the porosity, times the gradient of the hydraulic head. And, what you assign here for the porosity is critical to how fast this velocity is, and, of course, how fast the contaminants are going to move. So, this calculation is based on a porosity of 10 to the minus 3, and you can argue that’s too high or too low. As I suggested to you a few minutes ago, the porosity that we determined at UE-25P1 may be as low as 10 to the minus 5. So, we’re a couple of orders of magnitude higher than what we saw at UE-25P1.

So, what it’s saying is, you know, if the stuff gets to the carbonate aquifer, it’s pretty well gone. You’re going to see it in the biosphere reasonably quickly.

Okay, next slide, please? Okay, so, now, we come back to UE-25P1, and you say to yourself, all right, there is this upward hydraulic gradient, you know, we’ve got 15 meters at least of upward hydraulic gradient between the carbonate aquifer and the overlying welded tuff aquifer. So, you know, that’s certainly a barrier to any kind of contaminant movement from the repository down to the carbonate aquifer. But, then, you have to ask yourself the question, you know, how permanent is that upward gradient. Because, you know, one of the more ephemeral things about groundwater hydrology is hydraulic head. When you start producing, and you start developing groundwater, hydraulic head is the thing that you lower, and you lower this regionally, and you do it really quickly when you’re developing. So, now, we’ve come back and said to ourselves okay, so what could happen to this hydraulic head.

So, anyway, the next slide? So, one of the thoughts was to say to ourselves okay, let’s take the USGS model, as I said, they’ve put a lot of work into this model, let’s take the USGS model, project it out into the future, and see what happens to the hydraulic head.

So, I have done that, and this is their 1998 hydraulic head map, and that includes a fair amount of development. So, this was calibrated, and it was calibrated for development in the Amargosa area, and in the Pahrump area. And, they got what they think certainly is a reasonable calibration. So, I said okay, let’s run it out for 1000 years into the future, and see what happens to the hydraulic head.

So, the next slide? The next slide shows the drawdowns that we would anticipate, using their model now, 1998 to 2998, almost 3000. And, this again is the--these are the wells. Now, what I did is I just took their pumping that’s in the model at the moment, and you can ask yourself well, how good is that pumping? It turns out that the State of Nevada, for every valley in Nevada, has a potential yield, and they say to you, the state engineer says you cannot exceed the potential yield--or, I shouldn’t say that. He tries to restrict the development so that it does not exceed the potential yield. That’s a better statement. Whether he does or not, is open to some question.

So, anyway, for the Amargosa Valley area, this area in here, the potential yield quoted at the moment is 24,000 acre feet a year. The state measures, they do an inventory each year of how much pumping is out there. They are estimating at the moment somewhere around 12,000 to 13,000 acre feet a year. But, I looked at the USGS model, and it looks to me like there’s somewhere in the neighborhood of 20 to 23,000 acre feet a year in the USGS model. So, the USGS model, as far as I can see at the moment, has about--it’s within 10 percent of what the state’s yield is.

So, this, assuming, you know, you’ve got to make some kind of assumption about how much development you’re going to allow, but this is pretty close to what the state would currently allow, within 5 percent, actually. So, anyway, this is a thousand year calculation. Yucca Mountain, you can see, is somewhere up in here. And, we’re calculating here for Layer 8, which is right about the top of the carbonate, about 10 meters of decline. So, if we had 15 meters there to start with, we’re calculating that there will be something in the order of 10 meters of drawdown, and we’ve lost two-thirds of what that upward hydraulic gradient is.

Now, you know, that’s kind of iffy, too, because maybe you could say to yourself, well, the welded tuffs are coming down as well as the carbonate, so maybe they’re coming down together, and you still have this relative difference between the carbonate rocks and the welded tuffs. But, you’re making a projection here way into the future, and it’s terribly uncertain, and you’re looking at a facet of the hydrology that’s probably the most ephemeral. And, you’re looking at an area that’s probably going to develop. I mean, it’s almost--it’s difficult to see that the Amargosa Valley will not develop, particularly when you look at Las Vegas and the spill-over into Pahrump, and now you’re seeing a spill-over into the Amargosa Valley. And, nothing seems to be there stopping it. In fact, the federal government looks like it’s going to sell 55,000 acres in the Amargosa Valley to private enterprise. So, it seems to me it’s iffy whether this upward gradient remains or not.

I think that’s my last slide. So, we can now draw some conclusions. You know, our thinking is that the whole area is underlain by this carbonate aquifer. It’s highly transmissive. It has a low porosity and high groundwater velocity. The aquifer extends to Death Valley. You know, if stuff gets in there, it’s going to get to the biosphere. I don’t think it’s--and, it’s going to go quickly. I don’t think there’s much debate about--well, it seems to me that’s what the data suggests.

Then, you’re counting on this upward head gradient now as a barrier, and that upward head gradient is vulnerable to future development. So, I think that’s where we stand.

Thank you very much.

GARRICK: Questions from the Board? George?

HORNBERGER: John, let’s see, it must be 15 years ago, or more, that you and I were on an NRC committee, groundwater at Yucca Mountain, and how high can it rise, and I recall that one of the recommendations we made in there was that DOE might consider better characterizing the carbonate in the vicinity of Yucca Mountain. Has anyone done any drilling except Inyo County?

BREDEHOEFT: Well, Inyo County did all that early warning drilling. Some of those wells got water in the bottom that looked like it was carbonate, carbonate geochemistry. But, they did not get carbonate rock. So--excuse me, Nye County, the Nye County drilling. But, Nye County didn’t get to the carbonate rock. So, the only program that’s done any drilling to the carbonate rock is Inyo County. That’s it. There has been nothing at the Test Site. We’ve got one well at the Test Site. That’s it.

HORNBERGER: So, how confident do you think we are that there is an upward gradient pervasive throughout that huge area of the carbonates that you showed us?

BREDEHOEFT: George, I don’t think we’re confident at all. I think we’re confident that it probably exists within the vicinity of the repository. But, let’s go back. One of the things that’s interesting, George, is that you see--notice how steep these gradients are? And, that’s that really steep gradient that’s suggested at the Test Site, at Yucca Mountain, and, so, you know, I think you can say to yourself with some confidence that probably that upward gradient exists in the vicinity of the repository. But, I would be reluctant to suggest that it extends, you know, even out into the valley very far. So, you’re thinking in terms of, you know, maybe an area up in here where you’ve got high gradient.

HORNBERGER: Of course, the real question then is that you showed a pathline directly from the repository, but the projections would be for the contaminants to move south.

BREDEHOEFT: Well, we still saw some upward gradient in our drilling down here. You know, we’re seeing a few feet of upward gradient between the carbonate rock and the overlying valley filled material, Amargosa Valley fill material. But, you know, that’s way down here. What happens in here? We don’t know. The only thing you could go with is you could go back to the model and say to yourself, okay, how much difference is there in the model. But, you know, we’re extrapolating based on very little information.

GARRICK: Yes, Bill?

MURPHY: Bill Murphy, Board.

This is fascinating to me. I really enjoyed your talk. You alluded early to Ike Winograd’s use of geochemical data to infer inter-basinal flow. And, I happen to have visited the springs on the western side of the Funeral Mountains a couple of years ago, and I was astounded by extreme differences in the water chemistry in springs that are very close to one another. Some precipitating sulfur and others, it looked like they’re perfectly oxidizing, good, high quality water. And, I wonder if you’ve made use of the spring water chemistry to constrain your carbonate flow model?

BREDEHOEFT: Yes, let’s back up a second and let me comment a little bit on that. We looked at the water chemistry obviously. Now, this spring, Nevares, comes, we think, is pretty well associated with the carbonate. It comes out through alluvium, but there’s carbonate rock very close to where the alluvium is. So, that spring looks like it’s very carbonate dominated.

These springs, notice these--okay, so the carbonate terminates along here, and then we have a syncline in here, and these are the two bigger springs, Texas and Travertine, and those are coming out of alluvial material. Again, they have pretty much carbonate signatures.

Now, there is a very peculiar spring down here, Navel, which is kind of sitting out here by itself, and that chemistry looks rather different, very different in fact. And, then, you have these two really small springs up in here, which--but, again, I’m pretty sure those two springs, I haven’t looked at the geochemistry, but I’m pretty sure those two springs look much like--this whole set here looks pretty much the same. This one doesn’t. It’s very different. It’s very different. The origin of that one, it seems to me, is--well, I’ve got questions about the origin of that one.

GARRICK: John, I’d like to ask you a whole bunch of questions, but I’m afraid it would reveal my lack of knowledge.

But, one thing that I’m curious about, aside from the work that you’ve done, are you aware of any basin and range community master plan with respect to the drawdown of these aquifers, and how that matches with the time at which there would be any contamination anywhere in the aquifers?

BREDEHOEFT: Well, obviously, the control of the development of the water resources in Nevada is up to the state engineer of Nevada. So, the state engineer of Nevada has a plan for the development of water resources in Nevada, and they are proceeding according to that plan.

That plan includes development in most of these valleys. And, that has--there is no relationship to the repository, zero.

GARRICK: Yes. But, what I’m getting at is when you try to ask yourself does this really matter, you have to deal with the question of timing. And, if you’re talking about contaminating something tens of thousands, hundreds of thousands, millions of years into the future, what are we talking about here?

BREDEHOEFT: Well, let me say this. It seems to me perfectly clear that there is a lot of water in this carbonate aquifer. This carbonate aquifer is now being attacked by the City of Las Vegas for water supply, and they’re reaching out a couple hundred miles to the north. Pahrump and Nye County are looking at the carbonate aquifer as a water supply for Pahrump Valley and the local area in the Amargosa Valley. So, I think you have every expectation that the carbonate aquifer will be developed for water supply.

GARRICK: Yes. And, I guess the question is the timing of that, because--

BREDEHOEFT: John, that’s going to happen quickly.

GARRICK: Yes. And, that’s good. That’s good.

BREDEHOEFT: Why is that good? Because that seems to me will destroy this upward gradient, almost inevitably. That will destroy the upward gradient.

GARRICK: Well, it depends upon how much of a drawdown we’re talking about. All I’m trying to better understand is if you have an aquifer and it is likely that the benefit of that aquifer is for the next few hundred years rather than for the next tens of thousands of years--

BREDEHOEFT: No, that’s not the argument. The argument is--it goes like this. These valleys have a certain amount of recharge.

GARRICK: Yes.

BREDEHOEFT: That recharge is discharged, and they say much of that discharge goes for non-beneficial use. Freadofites, which are basically brush, for the most part, in this part of the world, sagebrush, creosote bush, various different freadofites, and what you’re going to do is you’re going to draw the water table down, get rid of those freadofites, take some water out of storage, but ultimately, the sort of plan that the State has is that every one of these valleys will come into some kind of future equilibrium, and that equilibrium can be maintained indefinitely.

GARRICK: Yes. But, it’s kind of important to sort of understand, I would guess, what that equilibrium condition is.

BREDEHOEFT: Yes, that’s very true.

GARRICK: Okay.

BREDEHOEFT: And, John, the only way you can understand that equilibrium is--let me put it to you this way. The only way to predict that equilibrium into the future is to say to yourself, well, let’s take the--I’m not saying this well. The only tool we have to predict that equilibrium into the future are these models.

GARRICK: You said at the outset that the subsurface, hydrology was not nearly as well known as you like, or something to that effect.

BREDEHOEFT: We have very little--we have two holes to the carbonate, and one oil well hole that was plugged.

GARRICK: I’m just kind of surprised with the basin and range being such a water oriented area in terms of trying to get it, steal it, do whatever they can, that there isn’t more systematic and integrated, if you wish, between the regions investigation, the long-term resources here.

BREDEHOEFT: The State wouldn’t agree with that statement.

GARRICK: Okay.

BREDEHOEFT: The State feels like they’ve done a good job in identifying the potential yield of each one of these basins, and the State engineer is attempting the best he can to maintain the development within that potential yield. And, that potential yield is designed to maintain the system indefinitely.

GARRICK: Well, but one of the most interesting observations that I heard today from you was that these aquifers are not isolated.

BREDEHOEFT: That’s correct.

GARRICK: So, it’s much more than the State that has to really be involved here to understand these, it seems to me.

BREDEHOEFT: Like Utah.

GARRICK: Yes, like Utah, like California. All I’m getting at is that I’m trying to more clearly understand if there’s a problem here from the standpoint of Yucca Mountain. And that really still seems to me to depend on the time dependence of events, the demographics, and the one thing that you pointed out that I never fully appreciated, namely that the lack of--I thought this was pretty much a--so that’s the thing that I was trying to get at.

Yes, Bill, you had a question?

MURPHY: Bill Murphy.

Yes, I’m also interested in the relevance to Yucca Mountain, and I am not sure, but George noted that the contaminant transport models show that the contaminants move to the south in the tuffacious aquifer and in the alluvium. And are there any models for contaminant transport from Yucca Mountain that show that it gets anywhere near the carbonate Aquifer? There’s a kilometer of tuff you have to go through first.

BREDEHOEFT: I haven’t looked at those models.

HORNBERGER: But, they have an upward gradient, so they don’t even consider the carbonate. I mean, your whole point is that if you switch that and you now have a downward gradient, you now have a driving force to move the contaminants downward. You still do have to get through the tuff. There’s no doubt about that.

GARRICK: Andy?

KADAK: Kadak.

I guess I’m trying to understand the basis for these models. You have two data points, as best I can tell from your earlier discussion, and you were able to create a model based on those two data points?

BREDEHOEFT: Well, let’s back up a second. That’s not quite right. Let’s go to the second to the last slide. No, further. There. The next one. Okay, so they have put together a 16 layer model. Okay? So, you’ve got a 16 layer model.

KADAK: Now, who is they?

BREDEHOEFT: USGS.

KADAK: All right. But, they had data from where?

BREDEHOEFT: Okay, so they have data on--this is their array of pumping wells, and these pumping wells are pumping from the valley fill aquifer. Okay? So, you’re pumping here from the valley fill aquifer, and I haven’t shown everything. This is Pahrump down here, and they’re pumping from the, again, from valley fill there. And, there are more wells spread around. This is a gold mine up in the Beatty area. So, you’ve got wells spread through this area. So, you have data up in this section, so you’ve got data from the shallow system, from the valley fill system, and then in some cases, from the welded tuff section.

For example, you have that Nye County data, Nye County drilled all those wells along 395 here. So, that data is there. So, it’s not as if you don’t have data. You do have data, shallower in the system. But, when it comes to the deeper portion of the system, then we don’t have much data. So, in the deeper portion of the system, you are dependent upon your ideas of what the geology looks like, and we’re extrapolating the geology from the outcrop areas into the subsurface based on our based ideas of the geology and the geophysics. And, then, you’re fitting this model based on data that’s mostly collected in this shallower portion of the system. So, it’s not as if you don’t have any data. They have probably something of the order, I’ve forgotten the number, but there’s a couple hundred wells, for which we have--not a couple hundred--there’s probably a hundred wells, for which we’ve got pretty good histories, Twentieth Century histories for what happened to those wells during that period.

KADAK: But, these are, as you said, shallow, and the thing that you were trying to model was the deep ones; right?

BREDEHOEFT: That’s right. For the deep system, we don’t have much.

KADAK: So, I’m just trying to test the veracity of your comment relative to in 50 years, you’ll get stuff from Yucca in these wells. Where does that come from?

BREDEHOEFT: Well, you can question the veracity of that. What I’m saying to you is the geology suggests that there is 5 kilometers of carbonate rock down there. I’m saying to you that 200 wells that we’ve got that penetrated the carbonate rock suggest that it’s highly permeable. We’ve got one well that suggests it’s very--that the porosity is very low. So, if you combine, you’ve got big thicknesses of carbonate, you’ve got reasonable permeability based on a couple hundred wells, and then you don’t know what the porosity is. So, it’s uncertain.

GARRICK: Okay, John, thank you very much.

We’re now coming to our next speaker, and that’s going to be Sam Armijo from the University of Nevada, Reno. He’s going to talk about work that he has done on a second generation waste package design.

Sam, Welcome.

ARMIJO: Thank you, John.

First, I'd like to thank the Committee for inviting me and our team to present this paper. I'd like to acknowledge our co-authors, my co-authors, Professor Mizra, Dr. Karr, and also Professor George Danko of UNR, whose multiplex code was used in the thermal analysis in this work.

I'd like to make the point that the views we are expressing are the views of our team, and not necessarily the views of the University of Nevada, Reno.

First slide, please? This Committee is well aware of the standard of reference of what we would term a Generation I design for Yucca Mountain, consisting of the waste package--316 nuclear grade inner waste package, outer waste package of Alloy 22, and a titanium drip shield. And, we use the terminology 2nd Generation because we want to distinguish between this design, which is a reference design, which will get licensed, with a design that might be considered after the initial licensing.

And, there's an analogy between the licensing of Yucca Mountain initial design, and future waste or operated, and future packages. The analogy is with nuclear power plants in the United States. As you all know, the initial plants that we built in the United States had a lot of conservatism in them. And, in recent years, the industry and NRC has taken advantage of that excess conservatism and amended the licenses of the nuclear plants, increasing the lifetime of many plants from 40 years to 60 years, and there will be more license extensions as well.

In addition, the nuclear plants had sufficient conservatism that they could be upgraded in power. So, we have some plants in the United States that have been upgraded by as much as 20 percent in rated power.

With respect to the analog for the waste package, we have, for years, we have been introducing new fuel designs into our light water reactors base, and primarily, the purpose of those introductions are better performance and better economics. So, we believe that that sort of thinking would be valid for Yucca Mountain if you had something worth changing to.

Next slide, please? So, the 2nd Generation concept presupposes the initial, the current licensing application will be approved, and the system will begin to operate. And, at some point in time, could be amended to do the following things. First, permit operation of the facility at higher temperatures. Permit use of larger, lower cost waste packages. And, I'll get into details of what that waste package would look like. And, eliminate the titanium drip shields.

The benefits of such a move would be to delay the risk of aqueous corrosion by extending the post-closure period by thousands of years, and maintaining a dry environment. The second benefit would be to reduce the number of waste packages. If we use the nuclear fuel to create the additional heat in the drifts, then, of course, we’d require fewer waste packages.

We propose eliminating a lot of the conservatism in the current waste package design. I believe, and our team believes, that there insufficient credit has been taken for all of the materials that are protecting the fuel, including the pellet itself, the fuel cladding, the 316 nuclear grade material, the outer package, and, of course, the mountain itself.

Now, we have done some very simple estimates of what this could save if this approach was proven to be valuable, and it’s in the billions of dollars. Our simple little calculation demonstrated $5 billion would be pretty easy to achieve in savings.

Finally, and I think since Yucca Mountain is sort of a moving target with GNEP and with TADs and a number of other things changing, this design is robust enough to actually be compatible with all those concepts.

Next chart? This is a chart, and I borrowed liberally from various Yucca Mountain project publications and presentations, and I’ll use those through this talk, I don’t want to imply that we generated this ourselves. This chart demonstrates, or is supposed to show the range of vulnerability to Alloy 22 to localized corrosion. And, as you can tell from the chart, the red box is the range of vulnerability. If you stay below 85 degrees centigrade and below 50 percent relative humidity, this material should not be vulnerable to any type of localized corrosion.

On the other hand, if you stay above 120 degrees centigrade and less than 50 percent--say above 120 degrees centigrade, it wouldn’t be susceptible to any type of localized corrosion. My personal opinion is that this is a very conservative criteria, and just to make sure that everybody understands, I believe Alloy 22 is a superb material. I think it’s just a little too expensive.

Next chart? Okay, our first step is to get the heat. We do this by increasing the number of assemblies that go into each package. In the case of the BWR packages, we could fit in up to 64 assemblies by a very small increase in the outer diameter, and save about a 1000 waste packages, and their related titanium drip shield.

With the same diameter change in the PWR assembly, we could increase the number of assemblies from 21 to 29. Now, of course, that all requires the whole system of work, we have not studied the mountain itself. It’s way beyond the scope of our study.

The Alloy 22 and the titanium drip shields would be replaced by a material called Core-10. It’s commonly known in the industry, in the construction industry, as weathering steel developed by the United States Steel Company, and is widely used for unprotected steel applications for wet and dry environments, alternating wet and dry environments. This material is well understood, and has a lot of favorable characteristics.

We would replace, we propose replacing the Alloy 22 and the drip shield with a very thick Core-10 steel by ASTM grade of A588. And, I’ll show you later, that’s a very--with that thickness, it’s sufficient to meet the strength requirements of the packages and the drip shields.

Also, we need, in order to get the temperatures that we’d like to extend the dry period, we need to add backfill. So, in our calculations we’ll describe, we add about 20 centimeters of backfill to get the temperatures we want.

Just for reference, just on a raw material cost, and this is from a little Alloy calculator from the London Metals Exchange, Core-10 would be about 21 cents a pound, compared to Alloy 22 about 10, and titanium in the $13 to $16 range. Now, that does not include the fabrication costs. And, one thing about low alloy carbon steel is they do not require complex post-low heat treatment, shop peening techniques, laser peening techniques, and are very fabricable.

Next slide? This just gives you a little idea of what Core-10 steel looks like. There are a variety of different steels. We picked A588, but there’s no reason to believe that the others wouldn’t work just as well. And, they are superior to regular carbon steel as far as corrosion resistance. They form a dense protective oxide film, and it lasts--well, we don’t know how long it lasts. But, our estimation is it will grow very, very slowly over the time period of interest. You can tell it’s about 98 percent iron with a little bit of copper, a little bit of chromium, nickel and vanadium. All of these elements contribute to the oxidation resistance of this material.

Next slide? This is kind of a busy chart, and I don’t want to waste your time on it, except to point out that the reference--let’s take the BWR reference design. We have the 316 nuclear grade, and its mechanical properties, the Alloy 22, and the titanium drip shield. And, these are the thicknesses of the various components of the package. We eliminate the drip shield, and we keep the 316 nuclear grade exactly the same. And, so, we wind up with a net of 15 centimeters thickness as compared to 17--I’m sorry--15 versus compared to 22.

But, when you do take into account the thickness and the mechanical properties, the combination of the 10 centimeter wall, A588 plus the nuclear grade, will meet the--

will meet or exceed the strength requirements of the titanium, plus the Alloy 22.

Okay, next slide? Now, this is a chart from one of the early Yucca Mountain studies that actually got us interested in this idea. And, the reference is down there. I don't remember the author. But, basically what was done, this was for, I think, about a 30 year ventilation period, and then closure. And, there were two temperature pulses, one without backfill, and one with quite a lot of backfill. I don't recall how much backfill was put in there.

But, what interested us was that even though you can't see it, what this backfilling process did is extend the dry period above 120 degrees centigrade, and that's our goal, to stay above 120 degrees centigrade as long as possible, because in that condition, the packages dry and there's no risk from aqueous corrosion of any type. Below 120 degrees, you can get into aqueous corrosion phenomenon, but we want to avoid that, and delay that for as long as possible. If we do, of course, we have taken advantage of radioactive decay and minimized the risk of any potential breach of the package.

There was a problem here that if you take the waste package surface temperature up to 400 centigrade, you would exceed the fuel cladding maximum temperature of 400 centigrade that's the current guidance for storage from the

NRC. So, we started to look at ways to achieve a high waste package surface temperature without exceeding the 400 degrees C. fuel cladding temperature limit.

Now, we were quite impressed with ourselves until we discovered prior work done by the project about eleven years ago, which in this case, the project was looking at carbon steel as an outer package, again, with 316 or some other stainless steel as the inner package. This was work done by Henschel, and what he did is he took--there was an analysis of what the--this is on a linear scale now, and what he did is he took an earlier analysis of what the thermal pulse would be, and he just added 100 degrees centigrade to every time step, and he came up with this extreme temperature history curve. And, he came up with an equation that described that curve.

Then, he calculated, based on existing data of the oxidation behavior of Core-10--not even Core-10 steel, just plain carbon steel, had no alloying elements at all, and there is quite a bit of data on the behavior of this material in dry air as a function of temperature. It corrodes by parabolic mechanism, which diffusion controlled, and had the well-defined Arrhenius equation, which describes it over a temperature range of probably 400 centigrade up to as high as 900. But, we’re of course interested only in the lower temperature range.

What he did with this extreme temperature history, he calculated that in 5000 years, you would only get about 1 ½ millimeters of metal loss. That’s 1 ½ millimeters out of 10 centimeters. So, it’s very small metal loss. He also added a conservatism in the treatment, where he said okay, at every 20 microns of oxidation, we’ll assume that the oxide cracks, and is no longer protective, so what you’ll have is a whole series of parabolic oxidation steps each time a new passive film grows to protect the material.

So, that was pretty good calculated oxidation resistance. We wanted to learn more about that. But, we wanted to learn it based on our concept of a higher temperature package, but without exceeding the 400 C. limit. This chart shows our calculation using Professor Danko’s multiplex code for a 2nd Generation design, containing 29 PWR assemblies, and 20 centimeters of backfill.

As you can see, we can keep below the 400 centigrade, this is the fuel cladding temperature, and that’s for assemblies in the middle of the package. But, we can only achieve somewhere around 280 degrees C. outer maximum package surface temperature.

But, the important thing--I should say we would like to do more work because we have some ideas to minimize the Delta T between the package surface and the cladding maximum temperature, but we have not done that work.

What is important at about 100 is that we can extend the dry period to about 6000 years by this technique, and we think that’s a valuable benefit because it gives us, you know, about 4000 years more of radioactive decay to minimize the risk in case we had a breach of the package.

We can skip this one. Our calculation only went out to 5000. This is on a linear scale. So, we just extrapolated to where our 6000 time period is.

The next one is a key chart. And, this is the well-known radio toxicity as a function of time curve. And, this concept takes the referenced Yucca Mountain package, and extends the dry period from about 2000 years to about 6000 years. Now, it has reduced the impact of actinides, but it hasn’t brought them down to the natural uranium ore standard.

But, more important things are going on than this study, and that is GNEP. GNEP will take out many, if not all, of the actinides, and change this curve substantially, and will bring us to a much lower, shorter time, that we need to worry about. And, so, it’s my hope that GNEP moves forward and that we make use of this valuable resource, rather than just burying it forever.

So, with that concept, we tried to visualize what will happen using the temperatures that we calculate, and the oxidation that we know for carbon steels, and this is what we would expect, assuming 60 years of ventilated operation preclosure, and low humidity environment, and we would reach 150 C. peak surface temperature. Now, this is a little bit of an artifact because in our calculation, we added backfill even during the preclosure. So, it won’t get that hot, but if it did, if we added backfill right on top of the waste package, we would reach the 150 degrees C. So, it would oxidize a little bit in the first 60 years.

The next chart, from 60 to 6000 years, we know that water--it’s possible, although it’s never been demonstrated unless somebody can correct me, it’s possible that water will be coming down, but it will evaporate in the environment above the drip shield--I mean, above the waste package. I didn’t show the backfill itself. I didn’t know how to do that. So, in the backfill, water will evaporate or be combined with the backfill material, but it will never reach the package surface. And, during that time, we calculate, and very good agreement with Henschel’s work, about .01 to .2 centimeters of oxide thickness. It may be small, but it turns out we believe it--we’ll tell you why we think it’s important.

The next chart is okay, we get past 6000 years. The package has really cooled down. It’s now down in the range from 120 to 85 degrees centigrade. The environment is, we assume, very humid. And, there is some possibility of some liquid phase reaching the surface of the package. Well, the first thing that will happen is the liquid phase will boil dry. It will evaporate and leave an oxide--a mineral scale on top of the iron oxide scale. But, you know, the issue is would that damage the package? And, of course, that depends on how much water is reaching the package. We think it’s going to be very little water. And, so, that’s not a great concern.

With time, next slide, we actually could have liquid form and stay there as a liquid, either as a deliquescent salt saturated liquid, or just well water liquid. And, again, the damage to the package depends on how much liquid there is, and what are the characteristics of the oxides that are protecting the package. And, I want to emphasize the “may” because I think that’s central to the whole discussion of Yucca Mountain, how much water can really reach the package.

And, even more importantly, if the package breached, how could it really, mechanistically, how could you get it through the 316 stainless steel container, get it through the zirconium alloy cladding, which I know is a very, very corrosion resistant material, dissolve some UO2, and then come out through this same tortuous path and contaminate Nevada. I just think that’s unrealistic.

So, why is this material--why do we think Core-10 is such a great material? Well, just for comparison, Alloy

22 is a great material. It’s great for just about any environment you can think of, sea water, salt water, chemical industry, you name it, but what protects it is a very thin passive film. And, you can argue whether that’s nanometers or angstroms, but it’s a very thin passive film, and in the right environments, if it’s fractured or damaged, it will reform and protect the material. In the wrong environments, the passive film could fail, and you can get into localized corrosion, or stress corrosion cracking, or other mechanisms.

This material is protected by two mechanisms. First, we will form a passive oxide film. We know that forms because of the parabolic nature of the corrosion. In addition, over that long period of time, we form a very thick, we’re talking of the order of a .1 millimeter to 1 millimeter iron oxide, it’s a dense oxide scale, but it could be cracked. But, the fact of the matter, on top of that, is a mineral deposit. And, so, any water, unless it comes in in large quantities, will have to get through these layers, somehow damaged passive layer, and somehow attack the metal. And, I believe that’s really an additional barrier, which makes a lower cost material do the job of a higher cost material.

Now, what happens when temperatures get really low? Of course, we can’t duplicate Yucca Mountain times and temperatures in our laboratory. As everyone who has worked on this project knows, it’s one of our biggest problems, how do you predict long service life for our model? How do you do accelerated testing? And, we’re going to try and study that in our future work at Nevada.

But, we do have artifacts from history that tell us a little bit about how the material behaves in the event you have it exposed in a water containing environment.

Next slide? These are iron spikes dug up in Scotland. The longest spike is about 15 inches, and they are nails, and there were--apparently, the Romans were having a lot of trouble in 87 A.D., and they had to leave England and Scotland, and go pass by the rebellious Gauls. And, they had all this iron and they couldn’t transport it back, and, so, they buried it. And, anybody who has been to Scotland knows that is not a dry Yucca Mountain type environment. It’s wet and dry.

Well, it was forgotten. Nobody--someone, I don’t know how they found it, dug it up after 1900 years. And, these were just wrought iron. These were not alloy steel. They were not even good carbon steel. They were just iron spikes. So, these unprotected, unalloyed nails, suffered negligible corrosion during this totally unprotected exposure. We would expect a Core-10 material in a Yucca Mountain environment to perform far, far better than that.

So, in conclusion, and I didn’t go into how we came up with the cost savings. That’s in the paper, and it was published in Nuclear Engineering and Design last year. We think eliminating waste packages takes out about $2 billion, and that’s based on a--well, maybe we can get into that in the questions and answers if there’s interest. But, it’s a lot of money.

Maintaining the dry environment and using more spent fuel helps create these savings, replacing the Alloy 22 with a much cheaper and much more fabricable material, which in our opinion, will do the job. That’s A588, or one of those classes of materials. And, of course, eliminating the drip shield.

Our analysis shows we can achieve higher waste package surface temperatures without exceeding the peak cladding temperature of 400 C., and that will extend the dry period by thousands of years. And, we believe that our literature data on oxidation and corrosion of these steels, plus the archeological data from things like these wrought iron artifacts, supports another look at carbon steel for the Yucca Mountain application.

Thank you.

GARRICK: Thank you. I should have announced at the beginning that due to our mix-up, we don’t yet have the viewgraphs, or the copies of the viewgraphs for this presentation. But, we are getting them, and they will be made available.

David, do you have a question?

DUQUETTE: Duquette, Board.

I think the Gauls that the Romans went after were probably my ancestors. But, with a little less levity, I'm sure you're aware that the Core-10 steel in wet ionic, wet electrolytes, doesn't behave any better than regular carbon steel, and, so, it can be used, for example, on surfaces of buildings, and things like that, but if you attempt to use if, for example, for guard rails, has failed entirely because it corrodes off at the ground level at the same rate as regular carbon steel does.

ARMIJO: Yeah, but--

DUQUETTE: Let me finish my comments. So, that's number one. Number two, the environment you're talking about does contain both chloride and nitrate and the Alloy 22, the project would like to take credit for the nitrate, but in this environment with carbon steel of any kind, the nitrate and chloride are going to act synergistically, neither one is going to protect the other from happening. They're both going to add to the ionic concentration of the water. Your temperature of 120 flies in the face of what we've been told about the mountain, which we'll see deliquescence of 150 to 160. I don't know of any work presently, and I presume you're going to be doing it, that looks at the corrosion by the way of Core-10 at 150 or 160 degrees Celsius. I don’t think there’s very much work out there on that. Even using the Arrhenius equation, I think you’re going to have some problems putting that together with your environments.

So, I think it’s a nice idea. Of course, it’s revisiting something that was done a long time ago, because the original package was going to be carbon steel, and you simply let it corrode away. You simply took the allowance for it, and let it corrode away. But, I don’t think Core-10 is going to be the answer in this particular case, because of the specific environment that’s a fairly strong electrolyte, and you will have--well, it’s almost surely going to have some deliquescence. Maintain the temperature at a higher temperature to guarantee evaporation is probably a good idea, period, if you’re going to use carbon steel.

But, I think some of your arguments would have to be proven because I just don’t know what the oxide is going to be like at 150. We do know that in the mountain today, that carbon steel is corroding rather rapidly, as we speak, and that some of it had corroded at quite a high rate, to the point where it’s, I believe, had to be replaced, or at least had to be retired because of--but, things like rock bolts are corroding, and there are some doors that seal off parts of the vault, and those are corroding. And, so, there’s corrosion going on in the mountain at room temperature at the present time of carbon steel.

So, I don’t have a problem with using this if we’re going to use a corrosion allowance, and allow the container to disappear. But, I don’t think it’s going to be a replacement for Alloy 22.

ARMIJO: I’d like to respond to that.

DUQUETTE: Sure.

ARMIJO: First of all, I assume this is a corrosion allowance method. You know, I don’t expect that this material would be unharmed and pristine, and I really don’t believe it’s only going to have a tenth of a millimeter of oxide at the end of even 10,000 years. But, I know there’s plenty of margin, and I know that this material is not acceptable to many of the localized corrosion phenomenon that affect the stainless steels.

And, then, if we could just go to the backup Slide 21? The thing that stimulated us a lot was the issue of how much water are we really worried about? If Yucca Mountain was a swamp, I could see the argument. We’re talking about long periods of dry, maybe some periods of wet and dry operation. If it’s going to be submerged, then Yucca Mountain is not the right material--not the right place for this material. So, again, nobody knows for sure, but the international peer review that was done in year 2000 asked the question and made the statement, the natural dripping of groundwater hadn’t been observed, and they also made the point that there was a great evaporation potential from the waste packages from the decay heat, and that would decay, you know, boil away up to a thousand liters a year per container. And, even at 10,000 years, it would boil away 100 liters a year.

Do we expect 100 liters a year per container to be hitting the waste package? I don’t--I hope that’s not the expectation. And, I hope it’s not greater than that.

The other point, this was made by the Secretary of Energy in 2005 when he recommended that the President approve the Yucca Mountain facility, and he repeated the point that there are very small amounts of water, and, you know, there’s no indication that water is actually dripping into the tunnels.

I just say that our concern is overstating the threat, you know, water is corrosive, you know, water is corrosive to Core-10 just like any other materials. But, it’s a matter of how much, and how much and how long, and if you have a generous corrosion allowance, you could dissolve that whole carbon steel thing away. I doubt it would ever happen, but you’d still have the package, the fuel protected by 316, and the zirconium alloy cladding. And, I’ve been in the zirconium alloy business for a long time, and that is a very corrosion resistant material. It’s not going to fall apart spontaneously. There are mechanisms that have to act for the cladding to fail, even at those long times. And, they're not going to be much of a threat.

But, the whole idea of this thing is can we make Yucca Mountain perform its mission at a lower cost, and this was our contribution to it. In the event that GNEP moves forward, I think the use of Alloy 22 will be more in question, because the lifetime duty is going to be much shorter. So, that's really my response.

GARRICK: Go ahead.

LATANISION: Latanision, Board.

I would generally agree with Dave Duquette's concerns, Sam. And, I would also add that it is known that with constructional steels, hot nitrates will induce stress corrosion cracking. And, so while we have this perversity of nature right now in this context that, well, the current generation of alloys, C-22, the external of the waste package, we're looking at taking advantage of nitrates in terms of reducing the potential for localized corrosion. You would have the converse situation with a constructional steel that you would have to worry about another form of localized corrosion, and perhaps a more severe form in terms of stress corrosion cracking.

ARMIJO: Yes, I agree with you, Ron. That's amenable to testing. You know, if you came up with some realistic or even aggressive, a more conservative environment with nitrates, you know, the first thing you would do is you’d test wells. But, these steels, even though they are weldable, don’t require the heat treatment to reduce the residual stresses that would be the cause of the cracking. You know, it would be just testing to see if it is susceptible.

But, you know, again, I keep going back to the amounts of water and the concentrations, and I just think it’s hard for me to see a real threat to a lot of packages, unless Yucca Mountain is floating in water. And, so, I think if that’s the case, then this would not be the material to use.

GARRICK: Any other questions? From the Staff?

(No response.)

GARRICK: Well, this is wonderful. The speakers have been great this morning. We’re right on time. Thank you very much, and we will adjourn until 1 o’clock.

(Whereupon, the lunch recess was taken.)

AFTERNOON SESSION

GARRICK: I want to remind you again of a couple of minor changes in our program. One is the changing around of the two presentations on Saturated Zone Testing, and the Near-Field Chemistry. And, the other is to make sure we allow a few minutes before the first break to have some public comments from people that are going to have to leave early.

And, now we're going to hear from Jim Laidler of Argonne National Laboratory, who is going to tell us a little bit of what the Board has been very anxious to hear about, the Global Nuclear Energy Partnership. Jim?

LAIDLER: I've been selected to provide the light after lunch entertainment on something non-controversial. So, I'll talk about GNEP.

I'm not going to cover the motherhood things about GNEP even though I believe in them very strongly. But, suffice it to say that GNEP is about ensuring the sustainability of nuclear energy in the future. It's about closing the nuclear fuel cycle. And, it's about a complex system of light water reactors and fast spectrum reactors. I'm not going to talk today about the closure of the fast reactor fuel cycle, because that's something that's off into the future. But, our immediate priorities are on the treatment of light water reactor spent fuel. And, so, I want to talk about the perceptions that we have about what the waste management aspects of the GNEP separations are.

I think we can just charge into it, so let me show you the first slide. There are projections of significant growth in nuclear power in the world. By 2050, we may have something like 1100 reactors in operation. There are a number of reprocessing plants currently in operation. Perhaps the most recognizable are the French plant at LaHague, which has the capacity, name plate capacity of around 1600 tons per year, the new Rokkasho-mura plant in Japan, the Sellafield plants of the UK. But most importantly, I wanted to point out some of the planned reprocessing plants in the world.

The Chinese are talking about building a UP3 type reprocessing plant maybe by 2020, 2025. The Russians are planning a replacement for the Mayak plant, which would go into operation in roughly 2025, at a scale of 1000 tons per year. And, in the GNEP program, we are looking at the deployment of a reprocessing plant that we call the Consolidated Fuel Treatment Center, the CFTC, and have that in operation by sometime between 2020 and 2025 at a very large scale, a scale that’s really dictated by the present generation rate of light water reactor spent fuel.

So, we’re talking about some major new installations in the world, in addition to those that are shown on here as planned plants. We anticipate that the French will construct a replacement plant for their two LaHague facilities, and that that would come on line in 2035 or 2040.

The main point of bringing this up is that we are trying to exert our national influence on what those plants do. And, in GNEP, one of our precepts is that we will not separate pure plutonium, and we want the rest of the world to do the same. And, so, what we are aiming to do in the GNEP program is to demonstrate that a reprocessing plan can be operated efficiently, economically, and not act like a PUREX plant, and not separate pure plutonium.

Next, please? And, this is what PUREX looks like. If you’re not familiar with the process, it involves the chopping and nitric acid dissolution of spent fuel. Currently, in the commercial plants, there are a number of releases during that operation. Iodine is released to the ocean in France and the U.K. Tritium, Krypton and Carbon-14 and CO2 are released to the atmosphere.

The PUREX process then follows the chopping and dissolution step. This is a co-extraction of uranium and plutonium. There is a separation of the pure stream of uranium, and a pure stream of plutonium. In the French plant, at least, the plutonium is sent as the nitrate solution to precipitation step where plutonium oxide PuO2 is produced for use in mixed oxide fuel thermal recycle.

Uranium is converted to UO3, and presently, it’s stored in France. There has been some recycle of the uranium, both in the enrichment plant, and in the thermal reactors. But, the balance of the fission products and the minor actinides, neptunium, americium and curium, with all the fission products then, other fission products, are sent to a vitrification step, where they are put into glass and packaged for ultimate disposal in a geologic repository.

Next, please? And, we have issues with that process. Even though it’s been around for a long, long time, 50 years or more, and has been successfully refined over the years, and optimized, it does produce a pure stream of plutonium. And, that is contrary to our national policy, which states that we will not use plutonium in the civil fuel cycle.

Minor actinides in the PUREX process are sent to the waste, which increases the volume and radiotoxicity of the waste material. The heat generators, the cesium and strontium, the americium and curium, go into the waste stream. So, there is no benefits to repository heat management. And, frankly, you can make a number of minor modifications to the PUREX process and call it proliferation resistant, but any good chemist can change that process very easily to extract a pure plutonium stream.

Next, please? So, we began in the GNEP program to design a separations process that was really aimed at optimizing waste management. Maybe the first time that that’s ever been done in the nuclear industry, but we had a lot of bad examples that we could point to. One, of course, is all that high level tank waste. And, so, one of our going in criteria was we’ll generate no high level liquid waste that requires extended tank storage.

We want to limit the emissions from the plant because at the size that we’re talking about, the releases could be significant, and might make the plant a little hard to license. So, we want to recover the iodine, krypton, tritium and Carbon 14.

We also want to hold the cost down. And, as you can see, we’re working at counter-purposes, because by doing all the things that help waste management, we add complexity to the reprocessing plant. And, so, we have to be very, very careful that the things that we do are the right things, so that we don’t drive ourselves out of business. We want to keep the increase in busbar cost of electricity to more than just a few mils, four or five mils would be nice, to accomplish the reprocessing of the spent fuel.

We want to get very efficient recovery of the important radionuclides. For example, iodine and technetium, we have a recovery target of 95 percent. Now, that’s about a factor of 20 reduction in the source term. We think we can do better, and we're working on that.

We want to get about a ten-fold reduction in high-level waste volume relative to direct disposal. And, in the integrated process, that is, the whole plant process, we're targeting for more than 99.9 percent recovery of the transuranics and the cesium and strontium.

Next? And, this is our reference process that we call UREX+1a. And, you will see where this came from in just a few moments. We begin by--and, this is something only a chemist or a chemical engineer could love, so I'm not going to waste a lot of time on it, other than to say that we have a lot of process steps that are targeted at specific radionuclide to removal. We have a four step--four extraction step system, where we successively remove cesium and strontium, the transuranic elements together with Lanthanide fission products, and then we separate the transuranics from the Lanthanides. This is a so-called group extraction process, where we're pulling out all the transuranics in one stream, and then sending that stream to fuel fabrication for irradiation as fuel in a fast reactor. This is not a thermal recycle operation.

Next? We've tested this process. It works pretty well. We get very high recovery efficiencies. Of course, this is that lab scale, so we need to demonstrate this at a larger scale in the future. But, we do get very high purification of the transuranics. One of our major concerns is the removal of the lanthanide fission products, because we have evidence that they can cause fuel cladding chemical interaction in fast reactor fuel. The purities of these recovered elements are at least three-ninths, in some cases more than that.

Next, please? Now, this is what we call the suite of UREX+ Processes. If you recall, the initial flow sheet I showed, the first extraction step is called UREX, where we separate out the uranium. And, then, all the other process steps that are added just make it UREX+ those other steps. And, we've designed the processes so that they can be tailor made to fit whatever our national policy evolves into. And, so, the first ones, the UREX 1 and 1a, are designed for a group extraction for irradiation of the transuranics as a collection in a fast reactor.

The others are designed for what we call heterogeneous recycle, where we split the transuranics into two groups, plutonium and neptunium as a fast reactor fuel, americium and curium, perhaps even separating the americium and curium for use as targets, or in the case of curium, to store the curium until it decays.

I just want to emphasize the reason for having these heterogeneous recycle systems is that in the homogeneous case where we do the group extraction, it forces you into remote fabrication, hot cell fabrication of the fuel, which is, we think, an expensive proposition. So, we’re looking at heterogeneous recycle, this kind, because you can fabricate that fuel, the plutonium and neptunium fuel, in a glove box, and the costs then become comparable to what, for example, the French are doing at their Melax plant.

Eventually, we can evolve into a group extraction system, once we develop the technologies that can make remote fuel fabrication economical.

Next? Now, I wanted to show you this flow sheet again, because now I’m highlighting the different waste streams in red here. In the initial step, we perform a low temperature voloxidation with just heating the fuel in an oxygen partial pressure to convert the UO2 to a higher oxide, which expands, opens up the cladding, fragments the fuel, and if it’s done at the right temperature, will release only the tritium. We want to do it in the dry step before the tritium gets into the liquid phase and goes all through the plant and becomes very difficult to recover. So, there’s our first recovery step, to get the tritium out.

Then, in the dissolution process, we’ll remove the xenon, krypton, CO2. And, in addition, there is a certain amount of fuel, material in the fuel, that will not dissolve readily in nitric acid, and forms undissolve solids, or sludge, if you will, at the bottom of the dissolver vessel. Our intention is to recover that sludge, to wash it to make sure that we’ve got the transuranics out of it to the greatest extent that we can. And then also left behind in the dissolver operation are the cladding hulls. And, that’s a valuable material. So, we want to recover the cladding hulls, wash them with a combination of nitric and hydrofluoric acids, make them non-transuranic, less than 100 nanocuries per gram, and use that as one of our waste streams, in fact, to form the matrix for a metal alloy that contains the sludge, the undissolved solids, and the technetium that we recover at very high efficiency, in the UREX process step.

We convert, in this step, we convert the technetium to a metallic form, combine it with the hulls and the sludge, and make a metal alloy out of zirconium. So, we’ve got to locally very strong reducing environment that retains that metallic technetium in the metallic state, where it has limited solubility and limited mobility in groundwater. And, it’s contained in the matrix. It’s pretty corrosion resistant.

We recover the iodine in this step, in the dissolution step as well. I’ll show you what we’re going to do with that. The cesium and strontium I mentioned, our intention is to place the cesium and strontium in a dedicated waste form in some facility. We have not arrived at a decision on what the facility ought to be, it could be a remote part of the repository, the Yucca Mountain repository, and just let it decay. It doesn’t have to be in an exotic container because after 300 years, it’s going to be gone. So, we’re looking at different ways to immobilize that product.

The TRUEX process separates the transuranics and lanthanides from the other fission products, and those fission products in that stream are largely the transition metals. They don’t make a very good glass, but they do make a pretty good metal alloy. And, so, we’re thinking about combining those transition metal fission products with out alloy that immobilizes the technetium as well.

The next step, TALSPEAK, separates transuranics from the lanthanide fission products. The lanthanides make a very nice glass, and, so, that’s our present intention for that.

Let me see the next slide, please. I mentioned the alternatives of heterogeneous recycle. And, I’ve said these things. Because of the need to move ahead and not get hung up on a prohibitively expensive fabrication technology, we’re going to process, or are looking at a process, let’s say, that’s called UREX+3. And, this is one where we recycle uranium, plutonium, neptunium as fast reactor fuel. Separate americium and curium and perhaps the americium from curium, that’s a fairly difficult separation, but it’s feasible, and transmute the americium in probably an epithermal region of a reactor, a fast reactor. And, then, separate, if we can, the curium and just let it decay. It will decay to plutonium and americium, and then that could be recycled.

Next? And, here’s what UREX+3 looks like. It looks like it’s getting a little more complicated, and, sure, it is. We’ve got two product streams now, but the other products are exactly the same as coming out of UREX+1a. The only difference really is that we’ve got U-Pu-Np stream and an americium and curium stream as our products separately.

Next, please? So, I wanted to talk a little bit about what we envision as the waste forms, and you will see that we are talking about a variety of different waste forms. I’m not sure there’s anything wrong with that, because we can tailor those very precisely to do just exactly what we want them to do.

The tritium will collect as water, and put it in grout. The cladding hulls, the bulk of the cladding hulls we will wash and then just compact. A portion, maybe 10 percent of the cladding hulls, are diverted for use as the matrix alloy for the metal waste form where we immobilize the technetium and the undissolved solids, the sludge, and the transition metal fission products.

Technetium is recovered as a metal, and that’s done by absorbing technetium pertechnetate on an anion exchange resident, and paralyzing that resident to convert the oxide to a metal. Now, we have done experiments with that, and we find it forms nice little bb’s that are free flowing and metallic. So, we think we’ve got something there.

And, I should point out that that’s one of the big considerations in all of our process design work, and that’s the chemistry, the extraction process is fairly easy. It’s designing the plant to operate with a variety of different products to be able to recycle the reagents, to be able to manage the affluence, and to efficiently produce the desired products.

The xenon, krypton, we would like to separate those two, the xenon from the krypton. Xenon is not a problem. It can be vented, because it’s all stable xenon. But, the krypton 85, we’re going to try to capture, and we think we can immobilize that in a zeolite like structure.

Carbon-14 is simple. We’ll just pass that through a scrubber, a caustic scrubber, and make carbonate out of it.

Next, please? Iodine, conventional method is to trap it in a silver-coated zeolite. We may convert it to a more stable compound, like potassium iodate.

Uranium, we recover at a very high level of purity, contaminants in the part per billion range, and it’s pure enough that we could re-enrich it. The uranium coming out of our light water reactors is worth re-enriching. It may have some 236 in it, but that may be a perfect source of fissionable or fuel material for our international partners, so that they don’t have to get into the enrichment and reprocessing business.

Cesium and strontium, we plan to go into decay storage. Now, whether that’s near surface or deep burial has not been determined yet. We’re looking at various options there.

And, then, I mentioned the residual fission products. Lanthanides, we’d put into glass, the transition metals into a metal alloy.

Now, this is all a work in progress. We’ve really only scratched the surface on how best to prepare these waste forms, and that’s one of our main areas of emphasis this year and into 2009. But, our intention is to have the best possible set of waste forms that meet the objectives of the national program.

The next one? These are the estimated volumes. Now, this table is different than the one that’s in your handouts, so please, I’m assuming that the Staff will make this correction available to you. They certainly have it available now. But, we’ve gone through a major exercise recently on trying to get our best possible estimate of waste volumes, and you can see here, Alan Croft hates this column. I don’t know that it’s all going to be high-level waste. Some of these things are fairly benign and may not need to go into a repository.

But, what I’m showing here on the far right column, is the unpackaged volume of these different waste forms. The bulk of the volume comes from cladding hulls. The next largest contributor is the uranium losses. We’re now projecting a 99 percent recovery efficiency for uranium--I’m sorry--99.9 percent, and once we lose that additional tenth of a percent, it amounts to a lot of material. So, that we’re presently projecting to go into glass. I think with a reasonably conservative estimate for the waste loading, we can probably get it down to the lower number here, 1 cubic meter per 100 tons.

Just for comparison, that 100 tons of untreated LWR spent fuel has a volume of about, unpackaged volume of around 45 cubic meters. And, if we’re smart and do the right things, then we get at least a five-fold reduction in unpackaged waste volume. And, then, a significant reduction when we come to putting it into canisters for ultimate disposal, because we’re no longer, in this case, limited by heat generation. So, we can cram these waste forms in pretty compactly. So, we’re thinking that probably a factor of 14 to 20 reduction in packaged waste volume.

There’s another option here, and that is to look at the disposition of the zircaloy cladding. We have some industrial firms that may be interested--I think it’s in the next slide--to look at recycling of zirconium. We could go into a chlorination process, make volatile zirconium chloride, and then go into the coal process, recover pure zirconium and recycle it. And, we’ve got some companies, Wachang (phonetic) is one of them, who have said that they will be delighted to accept that material for refabrication of cladding.

Now, the question is how long do we continue refabricating, or fabricating zircaloy cladding if we’re going to deploy fast reactors. And, I don’t know. It depends on the mix of reactors that we have in our system, and we’ll probably have light water reactors for a long, long time. They work pretty well.

I guess the bottom line is whether we do that recovery and recycling of zirconium is an open question. Now, it may be that a ten-fold reduction in waste volume is just fine. The cladding hulls and the fuel assembly hardware are not really significant contributors to the heat load imposed on the repository. So, it may be that we could just pack them in tightly and say fare thee well to them.

I believe that’s the end of my presentation. Thank you very much.

GARRICK: Questions? Andy?

KADAK: Thank you. You know, our interest obviously is in the waste forms, and as you know, we are struggling even with what we know to be the waste form in terms of how it will be, you know, dissolved, or whatever in the repository. How do we deal with that? Because the waste forms aren't really defined into just sort of, I guess, analyses, we don't know what the packaging requirements are, and we certainly don't know how these waste forms will dissolve in water, if you will. What do you advise the Board to do, given where you're going with this, or potentially?

LAIDLER: Be patient.

KADAK: We have plenty of patience.

LAIDLER: We are working very diligently to produce waste forms that are realistic. We've done a lot of preparation with surrogates. But, now, we're trying to get into, because now we're beginning to do demonstrations with the processes and larger scale, we're accumulating more actual fission products, and part of our demonstrations in the next few years will be to complete the fabrication of the waste forms, and then get into the testing of their behavior in a representative environment.

The fact that we have a lot of different waste forms may become an issue. I don't know. But, I'm convinced that we can make them cheaper than if--this collection of waste forms cheaper than if we had to put it all in glass. And, that’s one of our drivers, is to reduce cost.

GARRICK: Garrick, Board.

One of the issues in repositories is waste acceptance criteria. And, going in the direction that this goes, with large, much larger number of waste forms, there’s going to have to be a lot of attention paid to what constitutes a reasonable criteria. Is that right?

LAIDLER: Well, absolutely. And, we think that you can use the same kind of tests that are presently available as measures of the behavior of these different waste forms. We’re not really getting that far afield from what’s already being produced. The metal waste form may be something new, but there are ways to test that, and we’re pretty sure that that’s going to be a winner as far as retention of the technetium and the other fission products.

GARRICK: Besides proliferation, what are a couple of the other principal drivers for GNEP?

LAIDLER: Well, we’re trying to prevent proliferation by non-weapon states, and, so, the organization of the program is such that there would be a so-called group of supplier nations, which consist of the current weapon states, plus Japan, and those are countries that have established and significant nuclear infrastructures.

Now, what we’re trying to come up with is a system where we would provide an assured fuel supply to any other country as long as that country agrees to forego enrichment of uranium and reprocessing of spent fuel. So, that’s the motherhood part of GNEP.

The other intentions are to make sure that we have a strong nuclear energy program in the U.S. One of the advertised features of the program is to be sure that we have optimized the waste management so that we have the need for only one repository in this century.

GARRICK: Andy?

KADAK: Just a follow up on that last comment. The Board has heard that depending upon how you design the repository, it could be as many as four to eight times the current legislated capacity. And, I’ve heard this comment made many times about only one repository. But, when you look at your waste volumes, and you mentioned the hulls and the sludge, depending upon how efficient you are, that number could be much larger than the number that you pointed out. In fact, if you do it wrong, it could be larger in terms of total waste volume requiring geological disposal. What is the sensitivity of your 99.9 to that question?

LAIDLER: Well, we’re actually giving ourselves quite a cushion. These processes can be made very, very efficient. And, the interesting thing about a reprocessing plant is that the separations part, the chemical separations part, is really the easy part.

KADAK: That is the easy part. It’s the other stuff.

LAIDLER: It’s the other stuff, yeah, it’s the head end and back end of the process. And, that’s where we have to place our emphasis. And, we’re counting on input from the industry, and as you probably know, there’s a funding opportunities announcement that recently was published to seek industrial engagement in the program.

KADAK: One other question. What are you going to do with all the separated uranium? It says storage, but what does that actually mean?

LAIDLER: Well, there are two options. One is to re-enrichment, which we can do, because it does have value. It’s over natural levels. The other is that it can become a blanket material for fast reactors in the future to breed plutonium.

KADAK: You said the magic “B” word, and it wasn’t burner, and in your opening comments, you talked about sustainability, and it’s pretty clear that just the burner reactors are not sustainable, and you would need to do breeding, I would assume, as part of the long-term future for this program.

LAIDLER: I think everybody recognizes that ultimately we have to go to breeder economy. The burner reactor, as I see it, is really the evolutionary step toward that.

KADAK: Can you do breeders without separating plutonium?

LAIDLER: You can do the same kind of group transuranic separation, or a plutonium, neptunium separation.

KADAK: So, yes?

LAIDLER: Yes, the answer is yes. But, breeders imply reprocessing. There’s no way around that.

KADAK: Yes.

GARRICK: Ron, and then Howard?

LATANISION: Latanision, Board.

Accepting for the moment that the chemistry can be performed in the way you describe it, what is the path forward in terms of geo-politics, I suppose, for lack of a better word? This whole concept is derived on the notion that other nations would become partners in this exercise. So, in the best of all worlds, what is your path forward? When would you see something like this taking form and handling waste?

LAIDLER: We have been engaging a number of other countries in the past year and a half. We have met with all of the potential supplier nations, and at the ministerial level, gotten their agreement and their support and their commitment to go ahead with the partnership. We had a meeting in Vienna with, I don’t remember, something over 30 different countries, to get their opinions on participation in the partnership. And, most of them are favorable. There are some countries who are suspicious of it, that we’d be forming a Cartel that would dictate prices, like OPEC, and we tried to assure them that the supplier countries, the potential supplier countries are very strong competitors, and the intention is to have it be a competitive market, that the client countries, or the user nations, can shop around for the best price. And, each supplier nation would set its own price, depending on what they want to do. And, the processes may be slightly different. The only constant is going to be no plutonium separation.

LATANISION: Just a follow up. Given all that, what’s the time frame that you look at for all this to come about?

LAIDLER: Well, I’ll be a little bit older than I am now. I’d give it 20 years.

LATANISION: 20 years.

ARNOLD: Arnold, Board.

I have a similar question. What year closer to then do we have to make a major decision on building the first major facility of this program? Because that’s when it’s going to get crunchy.

LAIDLER: Exactly. Yes. We have selected a June of 2008 decision point by the Secretary on how to proceed with GNEP, whether to go just R&D, or to go full blast into large scale facility construction. It’s hard to say what form that would take, but our target is to have, if we go to an industrial scale plant, to have it in operation by 2020.

ARNOLD: Which would mean a decision to build it would have had to have been yesterday.

LAIDLER: Almost. If we go in in June of ’08, we’re probably a year and a half late.

ARNOLD: So, you’re saying fiscal ’09 would have to have construction at least anticipated, and environmental impacts and licensing and all that?

LAIDLER: Well, we’re working on an environmental impact statement at the moment. We’ve had expressions of interest by a number of industrial concerns. We know that there are some companies out there that would just love to build a big reprocessing plant. In fact, my company would like to build one.

GARRICK: Jim, you mentioned a 2020 production plan. Is there a plan to have a pilot plant between 2008 and 2020?

LAIDLER: What a good question. I think not. If we’re on a 2020 schedule, we don’t have time. We’re operating pretty much in the French mode right now. The French claim that piloting at a few kilograms per year or a few kilogram batches is adequate to go up to 1600 tons per year. I’d feel a lot more comfortable if we had a pilot plant, but I don’t think we can do it on that schedule. We are now at the one to five kilogram batch in size.

GARRICK: Andy?

KADAK: What part of this international agreement is to take spent fuel from other nations and to reprocess it and ship them back fresh fuel, I assume, or reprocessed fuel? Is that correct?

LAIDLER: Yes.

KADAK: Do we keep the waste, or does the waste go back to the country?

LAIDLER: It depends. If it’s the French participation in the program, and the French have a law that prohibits the acceptance of waste from other countries. So, they would have to find another of the partner, the supplier partners, to find a repository for the--or a disposition path for the waste stream. And, that is something that’s not resolved. What we’re trying to do in our program is show that the waste that we produce is comparatively benign, and maybe in fact the French could change their law.

KADAK: But, the U.S. will retain possession of the high-level waste?

LAIDLER: No, that decision has not been made. But, I’m going to make it good enough that you could put it in my back yard.

KADAK: The high-level waste? Really? Because, clearly, that’s a question about capacity of the repository and needing only one. If we’re going to be the sort of host of whatever deals we can make for reprocessing, those numbers may change a little bit in terms of only needing one repository.

LAIDLER: Yes, if you look at the--there’s been a number of studies strictly based on heat management in the repository, and it can be fairly convincingly shown, if that’s the only criteria, is heat management, that you can expand the effective capacity of the repository, of Yucca Mountain, by maybe 200 times.

KADAK: 200 times? So, it wouldn’t be a problem for us to store international spent--waste, I should say, not spent fuel?

LAIDLER: I’m not going to get into that one.

KADAK: But, that’s a question, an important one.

LAIDLER: Sure, it’s a question, and it’s a policy issue that we have to deal with. It’s not going to be dealt with by the technicians like myself.

KADAK: When do you think the fuel for these burner reactors will be ready to put in a fast reactor for reliable operation? These actinide fuels.

LAIDLER: One of the facilities that is part of GNEP is called the Advanced Fuel Cycle Facility, and that is basically a research laboratory on a grand scale, where we would have the capability to fabricate the fast reactor fuel, either from--for the heterogeneous case or for even the homogeneous case. But, that will be ready by about 2017.

KADAK: With tested fuels that are suitable for, quote, unquote, burning?

LAIDLER: Well, that’s another problem. We don’t have a fast reactor in this country. We can test fast reactor fuels in thermal reactors with some filtering, you know, to get a sort of hard spectrum. But, our alternatives are go to the Russians or the Japanese who do have operating fast reactors.

GARRICK: Okay, Ron, and then Carl DiBella.

LATANISION: Just a short question on budgets and cost. Do you have a sense of what it would cost to put all of this together and, given that sense, what is the current budget for this effort?

LAIDLER: The current budget in ’07 is, what, 230 million. The request for ’08 is 405 million. And, once we get into the industrial scale plants, of course, it goes on into major system acquisitions and line items. So, I don’t know, there have been estimates for a plant the size we’re talking about, the 2500 ton per year plant, with fuel fabrication, that combined total may be 15 billion, or so. It’s a big business.

GARRICK: Carl?

DI BELLA: Carl DiBella, Board Staff.

First of all, as a chemical engineer, I want to thank you for those wonderful blackboard diagrams. I’ve got a question on your overhead Number 5, just a clarification.

LAIDLER: Okay.

DI BELLA: It’s about costs, and you said added fuel cycle costs, and then you said four or five mils is sort of what you’re shooting for. What is that added to? I mean, today’s fuel cycle cost has a disposal fee as part of it. Are you adding even to that, or--

LAIDLER: It’s a mil per kilowatt hour.

DI BELLA: Yes.

LAIDLER: What I’m thinking in my non-professional estimate is it’s probably--we’re probably going to add four or five mils per kilowatt hour to the busbar cost. But, if you look at--well, I’m from Chicago, and COMED just increased their rates by almost 40 percent, so I’m paying way more than that right now. I don’t think you’ll even see that effect.

DI BELLA: Thank you.

GARRICK: Thank you, Jim.

LAIDLER: You’re certainly welcome.

GARRICK: We’ve been looking forward to this presentation.

All right, our next presentation is going to be from Pat Brady, a senior scientist at Sandia National Laboratory, and he’s going to report on work to characterize the near-field chemistry of the repository.

BRADY: Okay, we’re going to shift gears a little bit here from the chemistry of the waste that’s going to go into the repository, and instead, focus on chemistry of the water that will move through the overlying rock and possibly come into contact with that waste.

For the next 20 minutes, I’m going to describe for you the near-field chemistry model. This is a rough outline. Essentially what I’m going to do is build a hydrologic model, graft onto that a rock-based geochemistry model, consider what that implies for the chemistry of the fluids going into the repository. And, then, I will say a few words about validation of that model at the end.

All right, I’m going to talk about the top, the near-field chemistry model, which is--this takes the water from the PTn/TSw contact and moves it down into the drift. Inside of the drift, a model called the physical and chemical environment model, the P&CE model, takes the seepage compositions, evaporates them, and then considers their impact on the drip shield and the waste package. Corrosion is considered in a different model.

Downstream further is the consideration of the engineered barrier system, flow and transport. This is where we take those fluid chemistries, put them in contact with seepage that comes out of the waste package, and then consider the overall controls on radionuclide mobility.

All right, the first and most important point to be made here is that this is not work I did. This is largely the work done by Charles Bryan and Kate Helean. Charles Bryan is here with us today. I hope you will take the opportunity to talk to him later on as we go and explore the particular details of the model.

Now, conceptually, what we do to calculate what hits the engineered stuff is we take water, move it from the PTn/TSw contact, down into the drift as seepage. Now, once the water gets there, we do an explicit consideration of the ratios of, for example, calcium to alkalinity to make predictions as to what type of phases are going to form, whether it’s calcite or something else. And, that’s done by classical geochemical divide approach.

Ultimately, after you get the water through this cascade of chemical processes, you are able to make some estimations as to the chemistry that finally hits the engineered materials, and ultimately contacts the interior of the breached waste form.

I’m not going to talk about this section today, because I believe you all have all heard about this. Instead, I’m going to focus on this part up here, water moving through the tuff, because this is where we’ve come up with a new model, and this is what I’d like to focus on today.

Now, conceptually, you can imagine--well, conceptually, the processes that affect the chemistry of water moving down towards the repository are fairly simple. The water is going to stay in contact with something that’s basically two-thirds feldspar, one-third silica polymorphs, quarts, Cristobalite, Opal CT, what have you, and a very small amount of clays and zeolites, plus, there’s some calcite.

Now, that means that, in essence, we’ve got a reaction going like this. As you go down from the contact down towards the repository, the feldspar goes down, as the primary minerals alter to more stable secondary phases, clays and zeolites. And, the process, you would expect that the components of the feldspars would accumulate in solution, potassium, sodium, some silica, such that the TDS would go up a little bit. There’s some--this is kind of an over simplification, the silica, those values are also going to be controlled by interaction with the other silica phases as we go down.

What complicates this otherwise straightforward picture is the temperature gradient being changed by the thermal pulse. If we take our present day temperature gradient, and then you put a thermal pulse through it, you’d make it less steep as you move out this direction, and then you’d come back down. And, that’s going to affect all these reactions through here. Now, what I’m going to do is show you how we do a hydrological geochemical model to anticipate what that net impact is.

All right, I’m going to do this in two steps. First of all, I’ll describe the hydrologic model in this viewgraph, and then the geochemical model.

What we do is we take the repository footprint here. We’ve got seven drifts. Along each of those drifts, we have 16 locations. Okay, so, we’ve got 7 times 16 times 41 time slices. Essentially what we want to do is calculate what the temperature as a function of distance up above the repository is, at ultimately 4592 spots. Okay, this comes to us from the multi-scale model. This doesn’t come out of our calculations here. But, the first input we have is a temperature history as you go through the repository.

And, up there on the northeast corner of the slide, you can see one of the--conceptually, what it looks like. You have--there’s ambient. As time goes on, and the thermal pulse moves through the rock, the temperature gradient moves out, and it comes back through.

In your handouts, you will see these things are contoured. That’s the units of time there.

All right, so, the first step of the modeling is we take a thermal model generated on the southwest portion of this graph, and we graft onto that a percolation flux. Okay, since we can predict the temperatures from the multi-scale model as you go up, now we want to take water and move it down through that thermal gradient. And, that comes independently of this model from the percolation calculations.

Now, we have--I'm going to show you in just a moment how that varies over time. But, what you will see in the southeast corner of this slide is where we have combined the water moving through the thermal gradient, and we've tried to predict the arrival times of water as it comes into the drift. And, we've got it for five different percolation fluxes here. Each of these unattached points are the water arrival times. The lines here that are anchored by a data point right there, those were calculated independently with FEM, a finite element heat and mass transfer code that does particle tracking and interaction between the fractures and the matrix.

The first point to be made here is that there is a pretty good agreement between what we have done, which is essentially a plug flow approximation. We have just taken percolation fluxes that are independently calculated, and moved them through a thermal gradient.

Now, the plug flow approximation hits pretty close to what you'd get with the more elaborate FEHM calculation. Typically, the disagreement is less than 10 percent. The disagreement is larger at the higher fluxes.

So, to cap the hydrologic model, we take the plug flow results, these unattached numbers, we multiply them by that deviation, just to bring us up to what the more elaborate FEHM model would predict, and we basically have our fluid travel times, more importantly, the fluid residence times as they go down through the rock.

Now, the other application of the close agreement there that I would like to emphasize is the fact that those numbers are fairly close suggests that equilibration or exchange between the matrix and the fractures happens more rapidly than the actual vertical movement.

Okay, so, up to this point, there has been no chemistry. This is when the chemistry starts to come in. The primary reaction that you see in the rock, like I said in Slide Number 3, is feldspar altering to clays plus zeolites. We need to come up with a rate of feldspar degradation. And, the way we do it is by looking at the rock itself. These are mineralogic abundances in the units we care about. Basically, there’s roughly, I’ll call it 2 percent smectite plus zeolite, about half a percent, or a little bit less of sorptive zeolite. We can add those up and you have about 2.5 grams per kilogram of rock is alteration product.

If we assume that aluminum is conserved, as you turn feldspar into clays plus zeolites, we can use the alteration product abundances to back calculate out the feldspar, the amount of feldspar that’s been dissolved. In other words, we just assume local conservation of alumina. That is the least soluble component of the feldspar.

And, so, we can calculate how many molls that 2.5 grams equals. And, so, we end up with the amount of feldspar in the rock today. To turn that into a rate, we need to spread that over the age of the rock. The TSw is 12.8 million years old. So, we could take the modal abundance, divide it by that number, and come up with a maximum feldspar degradation rate. And, that’s not the value we use, though. 5.9, 4 times 10 to the minus 9 molls per kilogram per year. That’s the highest value we could expect. That would be--that is a rate calculated if you assume that all this stuff came out of the mountain, and it sat there at ambient temperatures for 12.8 million years, which really isn’t the case.

The temperatures quickly decayed, as the unit cooled, and then as the overlying rocks were erupted and adjacent bodies were erupted, temperatures wet back up, and then came back down again.

So, the first thing we have to compensate for is the fact that temperatures over the lifetime of the rock had not always been ambient. So, when you do that, we end up with a rate that’s about a third of that. It’s 1.7 times 10 to the minus 9 molls per kilogram of rock per year.

Now, we’re pushing water down through a column. This temperature is changing. We’ve established what the base rate of feldspar degradation is, the rate at which the components of feldspar have turned into solution. To be able to anticipate the rate at which the feldspar dissolves into those packets of water over time, we have to be able to predict the feldspar degradation rate of temperature. And, for that, we need an activation energy.

The activation energy, this is taken from literature values, 49 kilojoules per mol, it’s a little bit over 10 kJ per mol. Although there’s an enormous debate about what silicate weathering rates are in nature, and whether you measure them in the field or in the lab, you tend to see--the activation energies you measure in the lab tend to be fairly close to the ones you measure in the field.

So, what we do is we take that number and we use it to scale our feldspar degradation rates up temperature. And, what you see here is, and I’ve done it just an Excel spreadsheet calculation, from 25 degrees up to 95 degrees, you get an increase in the silicate of a feldspar degradation rate of about a factor of 50 or 60.

All right, so, at this point, we have a hydrologic model that moves fluids through a thermal gradient. We have a baseline feldspar degradation rate that we know is a function of temperature. We map that onto the first one, and we can calculate at a point in the repository, at a point in time, how much feldspar--how much potential feldspar, how much dissolved feldspar is going to be seeping into the drift.

Now, that unwieldy description, the amount of feldspar seeking into the drift, is called the WRIP, the water/rock interaction parameter. That’s nothing more than the number of molls of feldspar dissolved into a liter of water. And, I’ve plotted here the WRIP value for one of the median thermal paths as a function of time. All of the lines are rated out because these are cumulative distribution functions over the percolation fluxes.

If you take the central one, that would correspond to 7 millimeters per year for the present, 11 for the Monsoon, 11 for the glacial, and 21 for the post-10K.

Now, what this graph shows you is if you were sitting in a drift for a million years, and you were collecting water that seeped through that point, and you measured how much feldspar dissolved along the way, those are the numbers you’d see starting from zero, up to the -5, peaking, and going.

Note that the--if you look, go down on these cumulative distribution functions, you’re going to be increasing flux. So, what that says is that the faster the percolation, the lower the mean residence time of the water in contact, which means less feldspar degradation.

Okay. I’ve only got three or four slides left. At this point, I’ve talked about water moving through the rock, and rock being dissolved into that water. Neither of those are used. TSPA needs more than that for the calculation of the corrosion potential, for the assessment of is this chemistry good or bad for corrosion. We need the aqueous concentrations of the components in the rock. And, the way that is done is shown up here. Now that we know about--since we can estimate the amount of feldspar for every packet, now we have to back calculate what that does to the water chemistry.

And, the way that is done is with EQ3, EQ6 reaction path code, which makes our lives somewhat easier. But, I want to show you pretty much what the boundary conditions of that calculation are. Most notably, we assume equilibrium with calcite. That’s the additional constraint on our water chemistries. There is between .01 to .41 percent calcite in the TSw. If you look at geothermal systems throughout the world, typically, you see that most waters stay close to equilibrium with calcite. So, this helps us when we balance out where the elements go.

We use a similar boundary condition for silica. Again, there’s no shortage of silica, and silica containing phases in the TSw. It’s not immediately obvious which ones control the solubility of the amount of silica in solution, which is kind of what we see in geothermal zones. If you look in the southwest corner, you will see with temperature, the measured silica concentration is 10 to track in between some of the silica polymorphs.

We use amorphous silica to set the aqueous silica concentrations, which probably leads to an over estimate in silica. When it comes to the reactions that control the pH, the alkalinity, things that we really need for the corrosion assessment, silica is not that important. So, this little bit of uncertainty isn’t a huge one.

All right, if we take our feldspar degradation reaction to the equilibria with silica and calcite, and what you see with increasing alteration are the following three reactions. Starting off, the feldspar dissolves and you end up growing stellarite, a calcium bearing zeolite. You increase this calcite--silica is also used up, as is calcite, and you produce--basically, you shift from consuming just silica to silica plus calcite. As the water/rock interaction parameter goes up, the amount of silicate weathering increases. You start moving into the formation of clay minerals, which is shown down at the bottom.

Next slide? Okay, at this point, this bounds our fluid chemistries. One of the things that is not bounded by this calculation and which is indeed very difficult to bound is what are the partial pressures of CO2 and equilibrium with the fluid as they come into the drift. This is very important because it tells us something about the inherent buffer capacity of the fluids.

We have two bounding approaches to give us a minimum and a maximum in-drift CO2 level. If you look down at the bottom, you will see the minimum in-drift CO2 level calculation. And, that’s estimated if we take the seepage water that we’ve calculated independently. We bring it into the drift, and evaporate it. The H2O vapor takes up a large volume of the drift, leaving a little bit of space left over, at the same time contributing CO2 to that. So, that part started off with 10 to the minus 3 atmosphere, CO2 the ambient. It picked up a little bit more as the water was evaporated. That’s the minimum CO2 calculation.

The maximum CO2 is just assume that the seepage as it moved down through the TSw, it carried with it its own PCO2. PCO2’s go down because of the silicate degradation, they go up when clay minerals form. We keep track of both of those, and the maximum CO2 level is just that, that CO2.

Next slide, please? Okay, so, in a nutshell, you’ve seen what the model is. You’ve got a plug flow, hydrologic calculation, followed by a field based feldspar weathering rate, one put on top of the other, for two different bounding CO2 scenarios.

How do we validate the model? We validate it in three different ways. This is the--well, two of them are done, a third is in the process of being done. The first one was to compare the feldspar degradation rate with independently derived degradation rates inside the TSw. Second approach is to examine the evolution of the PTn waters. And, the third is to look at the drift scale test results. I’m only going to talk about the first one here.

All right, what’s shown are data from Bryan Marshall at the USGS. What they have measured are pore water compositions and rock compositions, strontium 87/86 ratios in the rock and in the pore fluids. The strontium comes from the feldspars. These are just the pore fluids. Imagine, if you will, a fairly vertical--imagine if you pushed this whole thing out to here, and you had a vertical line. That would be the strontium 87/86 in the rock.

What this means is that as you go down in depth, what happens is the slanting of the pore water trajectory, if that’s the rock and that’s the pore water--I’m exaggerating, but as my elbows get closer, that’s reflecting the strontium 87/86 exchange with the rock that occurs in dissolution. The steeper the--let’s see--the pointier my elbows are, the higher the radius. If you had a vertical--if you had a semi-vertical one, that would suggest that the approach of the pore water to the rock is very, very slow. And, the rates would be, that you’d calculate for mass balance, would be low.

Doing it like this would give you a fast rate. That gives you an idea of how the calculation is done. It’s a lot more complicated than that, but you get the gist of it.

All right, so, the end result is we’ve got--we can calculate an 87/86 exchange rate that can be converted into a feldspar degradation rate, an in situ feldspar degradation rate, and we can compare it with ours.

This is the value we used. I want to point out the units here. Molls per second per kilogram of tuff, and this is logs, rates get faster this direction towards me, and they get slower towards you all over there. All right, so that’s the value we get. The uncertainties that go into this are--well, there are uncertainties in the model analysis that gave us the amount of clays and zeolites. There’s uncertainties in the water/rock ratios. So, you get about an order of magnitude spread around it.

From these two boreholes, you calculate rates that are about a factor of five faster. The uncertainty here comes from--Charles, you’re going to have to correct me if I say this wrong--a parametric uncertainty of--there is a parametric uncertainty in the percolation fluxes. The extra uncertainty here, that reflects uncertainty in the leaching of the strontium out of the rock.

Okay, the upshot, though, is that--we're fairly close--a factor of five in the silicate degradation world? That's quite notable.

All right, next slide? Okay, so, let me summarize all this. The near-field chemistry model, it's what we use to build the major element composition of the seepage fluids. That, we pulled out of independent thermal field calculations and percolation fluxes. We've grafted onto that a rock based feldspar degradation rate that gives us seepage chemistries. And, it looks like we got it right the first time out of the gate, because so far, our validation efforts, in particular the strontium 87/86 comparison, looks like we're in the ballpark for both the reaction mechanism and the rate.

Next time I talk, or Charles talks, we'll talk about the drift scale test in the PTn waters. I think that's my last slide.

GARRICK: Ron?

LATANISION: What have you learned in this study that has implications related to corrosion of the waste package? You made a comment at the outset that this--I think you made a comment at the outset that this is part of the motivation.

BRADY: Well, that was the goal, was the come up with a better prediction for what those values were. The values are being written up right now in the AMRs, so I don't know if we're at liberty to say. Charles?

BRYAN: Well, of course, also within that AMR, we talk about our new pore water selection scheme, which in general yields waters that have higher nitrate to chloride ratios than the previous several year old results did. I think we have a better understanding now of the changes that are occurring as the water percolates downward, especially with respect to pH and the PCO2. So, I think what we have developed is a little better understanding of the general geochemical processes that are going to affect the water over time.

LATANISION: Just a clarifying comment. This had no functional affect on the chloride/nitrate ratio, ionic ratio, which is clearly of importance in terms of--

BRYAN: I can address that as well. We are actually sampling chloride and nitrate ratios differently this time around. What we are doing is we have divided the waters, the pore waters, into four different groups, and now in TSPA, we’re actually sampling the four different groups. Okay. We’re sampling the chloride to nitrate ratio. It’s measured over all of the waters within those groups. So, we’re no longer just using a single chloride to nitrate ratio to represent each group. So, I think we’re doing a better job of incorporating the uncertainty in chloride to nitrate ratio that we believe exists.

BRADY: And, Charles referred to the four different water types. The chloride/nitrate ratios are right here. That may be part of the answer to your question.

Let me make a point here. The calculation I did--excuse me--that Charles did that I described, that’s only going to tell you the silica--well, it’s going to tell you the major element compositions as they come into the drift. Chloride and nitrate don’t show up in any of those reactions for feldspar dissolution. The water comes in and they’re just along for the ride. So, when you do this calculation, it’s two parts. One, to get the major element chemistry as it moves and goes down the pipe. But, two, is to get the right waters in the first place because that’s where the chloride to nitrate ratios are going to come from. And, this was a summary slide that Charles hit the higher points of. I believe it’s in your packets.

GARRICK: Thure?

CERLING: Cerling, Board.

I think you have answered some of my unasked questions, but it had to do with your modeling was to basically understand feldspar chemical dissolution. But, that doesn’t give you any anions except bicarbonate, and so it seems that what you’re trying to describe is just confirmation that the water chemistry you’re observing is compatible with the fluxes. And, so, you’re trying to get an estimate of flux; is that right?

BRADY: That’s part of it.

CERLING: Which will then give you other anions that are so important to Ron’s question?

BRADY: Yes, those anions start off, they go into the calculation at the beginning, and there are no reactions that happen to them until you get inside the drift. But, pH and the buffer capacity of the fluids that hit the waste package, those are also important, and those do come from, as you said, bicarbonate. Those are one of the outputs from the feldspar weathering reaction. So, it’s a little bit more of the feldspar. It carries a little bit more of a load.

BRYAN: With respect to the changes, the feldspar alteration does change what the waters eventually evolve to when we evaporate them within the drift. Even though we’re assuming equilibrium with calcite, calcium concentrations in the water do go down, mainly because bicarbonate goes up. So, for some of those four pore waters, for instance, if you take them initially and evaporate them, you will end up with a calcium chloride brine. As you increase the amount of feldspar dissolution, those waters evolve into sodium, potassium carbonate, and chloride and nitrate brines. So, the composition of the brine does change. The composition of the final end product, once that brine drips onto the waste package and evaporates, changes potentially with the amount of feldspar dissolution.

GARRICK: George?

HORNBERGER: Pat, you mentioned that your calculations depend on the hydrologic model going through a thermal gradient. And, you mentioned that you're basing this on the multi-scale model. We've seen some results recently that would question how good that multi-scale model is, because it's a two dimensional model and it doesn't take into account vapor flow along the drive, and you may have significant amounts of vapor entering the drift. How do you think that would affect your calculations?

BRADY: I'm going to pitch that one to Charles.

BRYAN: First of all, I wanted to expand on what Pat said. We're not using the multi-scale model results. We're using results which are compatible with those, actually model results that are consistent with their approach used in the condensation and convection MR. Basically, we're using that model just for generating the thermal field around the drift, which is a conduction only thermal field.

Within the drift, we're sampling the upper and lower bounds for PCO2, and then in the TSPA model, we'll sample between those two. One of the primary reasons we're sampling a range is because we don't know the degree to which--how well we're modeling water vapor within the drift or how much oxygen will diffuse into the drift, or CO2 will diffuse into the drift at the ends. Because we don't have a good three dimensional model, we think we're bounding the CO2 range, but we're not trying to predict a single value to represent that range.

GARRICK: Bill?

MURPHY: Bill Murphy, Board.

Pat, as you know, these are problems that I've thought a lot about and am quite interested in, and, so, I'm curious to see your results, and I'm not sure quite where to start with my comments. But, I think I'll summarize some of the observations of the ambient system. Well, first of all, I commend you for trying to characterize the ambient system. I think that's the key to developing a water chemistry model that could potentially be extrapolated to characterize the thermal conditions. And, so, looking at the feldspar rates under ambient conditions, and considering the reactions, I think that's an appropriate first step. Unless you can model the ambient system, I don't think you have a chance of modeling the perturbed system, frankly.

And, so, perhaps I'm anticipating some of your future work on PTn waters, but one of the characteristics of the unsaturated zone groundwater chemistries reported by Al Yang, and others, is that there's really a very wide diversity in cation ratios. The ionic strengths are all about the same, but there's really a big diversity in these, and that's a peculiarity that I've wondered about a lot.

But, we saw more or less the same variations in the seepage that came into the south ramp in the winter of 2007, once again quite a big range of cation ratios.

And, that's led me to think that a lot of the unsaturated zone water chemistry is controlled very early in the soil zone, or in the vitric part of the Tiva Canyon, and if you look at the mountain, the vulnerable phase among the rock materials is the glass. It's the volcanic glass that's been altered to zeolites a depth, and even higher up in the mountain. I think glass dissolution has a predominant control on unsaturated zone groundwater chemistry, even though it exists--it doesn't exist in the Topopah Spring, particularly, but up above that.

Also, a lot of the components that enter the unsaturated zone are introduced by reactions in the soil zone, dissolution of dust and coleche, and the waters that infiltrate are very highly conditioned by that infiltration. And, I think the south ramp data in relation to the unsaturated zone water chemistry tends to bear that notion out. So, that's one observation.

I'd like to see your model compared to unsaturated zone chemistry and to rationalize the sorts of--

BRADY: Let me point out that that was foremost in our minds when we set it up, because you're right, there's glass up above the TSw, and glass is far more reactive, and you would expect that the fingerprint of the fluids that ultimately make it through the TSw are going to be--they are going to reflect that interaction. That's why when you see that backup slide, the four different waters, those are all TSw waters. They aren't PTn waters. In other words, they implicitly carry that fingerprint of glass interaction with them.

So, in a way, that was our attempt, to make certain that the glass got into the water before we sent it to the TSw where there wasn't glass. But, I agree with you about glass being the important thing.

MURPHY: Somewhat along those same lines, in making the effort to calibrate your model to the observations of the system, there's some smectite and I thought it was a very nice piece of work to estimate average alteration rates on the basis of the smectite and zeolite, and I think you produced a realistic looking number.

The predominant alteration phases, though, are mordenite, clinoptilolite, and I'm aware of the challenges and characterizing the properties of those for this kind of modeling, but I think it would be nice to see the actual phases that occur in the mountain represented in the model, as opposed perhaps to stellorite.

BRADY: Obviously, the thermodynamic properties of those minerals become very murky, but, yeah, I agree with you.

MURPHY: And, one final observation is I think you probably answered this in addressing why you decreased your feldspar alteration rate due to natural thermal perturbations. I think that petrographers at Los Alamos always say that a great deal of what alteration you see at Yucca Mountain occurred very shortly after the volcanic rocks were erupted during the cooling of the rocks, and, so, a calculation of an average rate may be fine as an average rate. But, there could be--a lot of it could have occurred early.

In contrast, we have the very nice rate of precipitation data from the calcite or opal, the opal dating from Jim Pace’s rates of reaction.

BRADY: These are all good points, Bill, and you’d think the limiting case would be that it all happened right after the rock was formed, in which case the rate is zero. But, you can correct me as soon as I--but the fact that there are a couple of items that kind of gave us confidence that we were in the ballpark. First of all, it’s the strontium 87/86 comparison.

Also, when you convert those rates to a molls per centimeter squared per second value, I want to say it’s about 10 to the minus 20, which is sort of the low end that we’ve measured on plagioclase in Hawaii and other places. And, so, I think we’re in the ballpark on it.

BRYAN: I agree with you. But, the actual range, if you consider that all of the alteration that we observed occurred in 12 million years under ambient conditions, that would give you one rate. The other bounding rate would be to assume that the rocks had been at elevated temperature, 100 degrees C. for 12 million years, in which case, you could calculate a rate from the observed alteration, which would be a 100 degrees C. rate.

Now, we know from the activation energy that the 100 degrees C. rate can only be a factor of 60 above the ambient rate. So, what we actually have is, at most, a factor of 60 uncertainty in the actual rate.

Now, when you consider that most of these rocks did form, most of the alteration did form early, you can actually significantly reduce that. And, that’s what we did in the model. So, we actually have on the order of a factor of five or eight in uncertainty in the actual rate, let me include that.

So, we have considered the fact that the majority of the alteration did form at elevated temperature early in the history of the rock. Early being actually about 3 to 4 million years. The rock remained hot for several million years because apparently, according to Bryan Marshall’s modeling, intrusion at greater depth.

MURPHY: Okay, let me make one more point, please.

Prefacing this by saying I think it’s important to demonstrate that you can characterize and understand the controls on the chemistry and the ambient system, the real effect is how is this going to affect corrosion and waste form dissolution and perhaps hydrologic characteristics of the system in the long term. And, as was pointed out before, what you really need, what’s used in PA is ionic strength and pH and CO2, and those are related to this in some ways.

But, also, I’m curious about the effects of the thermal period on the hydrologic characteristics of the near-field. Can you calculate during the thermal excursion that may last a thousand or a few thousand years, if the permeabilities or porosities, will there be a silica cap formed, will there be channelized flow developed because of the precipitation and dissolution? Are there coupled hydrochemical effects that you can draw from your model that really lead us to evaluating how the emplaced wastes are going to run to respond?

BRADY: We don’t, but we could, to a point. Specifically, we could calculate the masses of the feldspar dissolved, minus other stuff formed. I think it’s going to be murky though when you spread that out in fractures, or matrix porosity. So, we haven’t gone down that path.

GARRICK: Okay, I think we are going to have to move on. Thanks, Pat.

Our next speaker is Paul Reimus. Paul is a senior scientist at Los Alamos National Laboratory and he will give us a presentation on testing in the saturated zone. So, we go from the near-field to the far-field.

REIMUS: Yes, we'll move about 300 meters down now.

Next slide? This is the outline of my presentation today. First, I'll give an overview of the new Nye County early warning drilling program wells that have been drilled since 2005, basically since the last time we had an informational meeting with the Board. These are the Phase V wells in the Nye County program.

I will also provide an update of the hydrostratigraphic framework model, which is the underpinning for the saturated zone flow and transport models for Yucca Mountain, and the site scale models. But, I will provide an overview of the hydraulic and tracer test results at Nye County Site 22. These tests were in progress a couple years ago when there was last discussions with the Board on saturated zone testing, and these are all now concluded tests, and interpretations are being documented in an AMR.

And, finally, I'll discuss some innovative methods of identifying flowing intervals and measuring ambient flow velocities in Nye County wells that have been implemented in the last year and a half, primarily through the Nye County program. All these activities have benefited from or been a direct result of close cooperation between the DOE-funded Nye County early warning drilling program, the DOE-University of Nevada Cooperative Agreement, and Yucca Mountain Project scientists. And, I should also add OSTI, your science and technology program scientists have been involved in this as well, although there's certainly some overlap with the Yucca Mountain folks there, particularly with this last bullet, that was an OSTI inspired investigation.

Next slide? Okay, the blue circles on this map are the locations of the four new Nye County early warning drilling program wells in Phase V. There's actually a fifth one over at Site 22 added to two other--or three other wells that were there. The two wells south of Highway 95 here, 33-P and 32-P, were drilled at the location of magnetic anomalies, looking for volcanic explanations for the magnetic anomalies. 13-P was drilled up to the northwest here. There was an additional well also drilled at Site 24 that I'll talk about in a little bit more detail.

The two circled locations here, Sites 24 and 22, are the locations of tests that I'll be discussing in this presentation.

Okay, first, I wanted to briefly go over changes to the hydrostratigraphic framework models since 2004. In 2004, the base case model was based on what was called the HFM-19, which had built into it regional model efforts from the mid to late Nineties. The 2006 base case model basically incorporates all the observations in Nye County wells through Phase IV. So, the updates, particularly the southern portion of the site model area are largely due to the Nye County drilling program.

And, then, of course, the boundaries and other changes to the model have come about primarily as a result of changes to the USGS regional model that occurred in 2002 and 2004.

I do want to point out that the major change here, this is a slice at the water table, basically, over the site model domain in both cases. The big change in terms of performance of the saturated zone is that whereas in the 2004 model, we had fractured volcanics, all these darker colors are fractured volcanic units. The units are shown over here in this legend, and that legend is expanded and explained a little bit better in the very last slide of your handout. But, fractured volcanics basically were in the flow pathway, almost all the way up to Highway 95. Now, the Nye County drilling program has identified at least a third or so of the flow path from the repository horizon to the 18 kilometer boundary, is in saturated alluvium at the water table.

Let’s see, I also wanted to point out these black lines here are particle tracks derived from the northern part of the repository, the southern part of the repository and the central part of the repository in the current site scale transport model. And, if I may, I just wanted to point out that Site 22 where the tracer testing and hydraulic testing that I'll talk about next, occurred, is located right along the project flow pathway from the model, basically right along Forty Mile Wash, just a few miles north of the 18 kilometer boundary.

Next viewgraph? Okay, the testing at Nye County Site 22 involved both hydraulic tests and tracer tests. The objectives were to test and validate conceptual models of flow and radionuclide transport in the alluvium south of Yucca Mountain, the saturated alluvium, and to obtain estimates of all these various flow and transport parameters.

The site layout is shown here in the lower left, 22S is the large diameter production well. It had four screened intervals in it, and 22PA, 22PB and 22PC are all dual nested peizometers that are completed to different intervals. There's about 650 feet, or so, of saturated alluvium at this location. It's underlain by what's classified as a non-welded volcanic breccia.

The hydraulic testing involved isolated zone pumping of each of the four zones in 22S, looking at responses and recoveries in the observation wells, the peizometers, and the tracer testing was conducted in Zone 2 with 22S as a production well, and both 22PA and PC as tracer injection wells and cross-hole tests.

Okay, this slide summarizes the methodology for hydraulic test interpretation in the testing at 22S. The observation wells drawdowns and recoveries were analyzed in both the pumped interval as well as intervals above and below the pumped zone. This was a little bit of a departure and an improvement over previous modeling that had been done, which had primarily focused on looking at responses in the pumped zone.

Three-aquifer semi-analytical solution was used, basically simultaneously solved for the drawdowns, or simultaneously fitted the drawdowns and recoveries in three intervals, the pumped interval and intervals above and below, and you can see here an example of fits to the drawdown and recovery data for the test in Zone 2.

And, what came out of that was obtaining estimates of storativity and transmissivity in either three aquifers or in the case of the upper or lower zone, it would be two aquifers, the two-aquifer analytical solution was used. And, also, vertical hydraulic communication parameter was estimated in the intervening layers between the zones.

Next viewgraph? This viewgraph summarizes the flow conceptualization and the parameter estimates that came out of the hydraulic testing. A lot of numbers here. I’ll try to summarize really quickly by saying that the upper two intervals essentially behaved as a combined aquifer that had weak vertical anisotropy that was a hydraulic conductivity, a horizontal hydraulic conductivity to vertical conductivity ratio of about two to one, based on the test interpretations.

The horizontal hydraulic conductivity composite over this upper portion of the alluvium is about 10 meters per day. The next interval down definitely exhibited more confined behavior. There appears to be some sort of a confining layer or semi-confining layer between Zones 2 and 3. And, then, again, there's confinement between the bottom of the alluvium, or the lower zone in the alluvium, and the volcanic breccia.

If you look at the entire alluvium as a composite here, you get about a five meter per day horizontal hydraulic conductivity. Again, ten meters a day in the upper portion. And, just for comparison, the calibrated site scale flow model has a horizontal hydraulic conductivity at this location of about 17 meters per day. So, within a factor of two, or so, of the estimate obtained from hydraulic testing, and that's assuming a 10 to 1 vertical anisotropy ratio, which we do observe deep, but not shallow at this location.

HORNBERGER: Can you give me an idea of how thick the what you have marked as aquifers are?

REIMUS: The total saturated thickness of the alluvium here is 650 feet. But, you're talking about the individual screens? The screens are on the order of 100 feet, or so, 60 to 100 feet. The gravel packs for the screens are maybe slightly longer.

Okay, moving on into the tracer test results. There were two single well tracer tests conducted in 22S. They both involved the use of two conservative tracers with different diffusion coefficients, a halite fluorinated benzoate. Iodide was the halite in both cases. The difference between the two tests was the amount of time that the tracers were allowed to sit in the formation in the aquifer before being pumped back out. On the left here, we see the tracer responses, the break-through curves, normalized to injection mass, as a function of time, after three days of sitting in the aquifer. On the right-hand side, we have the responses after 30 days of sitting in the aquifer, and you will see there is a difference.

And, what was interesting here is that the iodide the tracer with the larger diffusion coefficient actually had the higher normalized concentration in both of these tests. The fact that there is a difference in the concentrations of the tracers does indicate some dual porosity character or diffusion into stagnant water. However, the fact that the iodide is coming through at a higher concentration also suggested that diffusion distance, or diffusion time scale, was fairly short. And, we would expect this dual porosity behavior that’s observed at these sort of time scales to convert over, or transfer over to more of a single porosity behavior over a much longer time and distance scales.

Next viewgraph? Okay, the responses of the tracers in the single well tests were also used to estimate specific discharge in the alluvium at Site 22. That was done by comparing and analyzing the responses of the same tracer and the two different tests. You see iodide here, the responses in each test shown with a linear scale on the left, with a log scale on the right. The differences in the peak arrival times, the mean arrival times, and the arrival times associated with a high recovery, nominally about 97 percent, as I recall, were used to estimate flow velocities, and, in turn, estimate specific discharges. Those estimates are shown right below here. You get slightly different values depending on whether you analyze the peak or the mean or the high recovery times.

Those values range from .5 to 5.4 meters a year. You get the higher estimates with the high recovery time difference. By comparison, single well testing at Site 19-D, which is located a few kilometers further southwest along Forty Mile Wash, range from 1.2 to 9.4 meters a year. And, also, by comparison, other specific discharge estimates are listed at the bottom here. The discharge estimated independently, just using head data along Forty Mile Wash and the hydraulic conductivity estimates from the hydraulic testing in Zone 2 range from 3 to 12 meters a year, and the estimate from the site-scale flow model is about 21 meters a year.

Okay, there were two cross-hole tracer tests that were conducted at the site. I’m showing here the results in terms of normalized concentrations of tracers as a function of time. From the first test, these are all normalized to injection mass, so we can compare them fairly, the first thing I want to point out is the red curve is the responsive of 2,6 difluoral benzoate from 22-PC. It was the only tracer injected into 22-PC, which was located almost due east of the production well. The other tracers all came from 22-PA located the same distance away from the production well, but due north. Clearly, there’s a faster response coming from the north direction as opposed to the east direction. We see clear evidence of flow anisotropy from those responses, or from the comparison of those responses.

We also see that there is a separation between the 2,4,5 trifloural benzoate in this case, and bromide as a halide. Again, that is indicative of dual porosity behavior. There is some uncertainty in the normalized concentrations of not only the bromide, but the trifloural benzoate as well, primarily because there were some discrepancies between masses that were measured to be injected, and concentrations of the injection concentrations. But, even taking into account that uncertainty, we basically see what appear to be dual porosity effects with either the bromide being significantly lower in concentration than the benzoate, indicative of diffusion to stagnant porosity, or we see a slightly delayed peak, which is also indicative of diffusion to stagnant porosity, just a little bit less stagnant porosity, or shorter time and distance scales.

The lithium response, lithium was used as a reactive tracer here, injected with all the other tracers. It’s very attenuated in concentration relative to the conservative tracers. However, it does peak early. This behavior is also consistent with a dual porosity system. And, finally, carboxide modified latex microspheres were used as colloid surrogate tracers in this test, and you can see their response is quite low. But, nevertheless, there was a recovery from which colloid transport parameters could be estimated.

Next? Okay, this just shows a quick summary of the interpretive aspects of this first cross-hole tracer test. By taking the two cases, the two end member cases of a minimum difference between the floural benzoate and bromide break-through curves and the maximum difference between the normalized break-through curves, there were two sets of interpretations done. Both interpretations involve invoking three flow pathways to fit the response curve. That was driven in part by the fact that it was just not possible to get a good fit with the single advective dispersive pathway. But, it was also driven in part by the fact that just looking at the derivative of the concentrations of either the bromide or the trifloural benzoate as a function of time, exhibited multiple peaks, which is suggestive of multiple tracer arrivals. So, there were a few different lines of evidence to suggest multiple pathway behavior. And, at least two of the pathways involved invoking dual porosity character of the system. And, in the case of the maximum difference between the benzoate and the bromide, all three pathways had dual porosity character.

Next viewgraph? Okay, the second cross-hole tracer test is shown here, the responses of the tracers. In this test, only iodide and Perrhenate were used as tracers. They were both injected at 22-PA, the same interval that the other tracers were injected into, with the exception of that 26-DFBA. Perrhenate was used in this case as a surrogate for pertechnetate, as was mentioned this morning, pertechnetate is expected to be the form that technetium 99 will be in in the saturated zone, at least it will be under oxidizing conditions, and Perrhenate is just below pertechnetate in the periodic table, so it serves as a very good surrogate, has similar redox behavior and general chemical behavior. So, it was used as a surrogate for pertechnetate.

And, we see the two responses of the tracers. A few things to note here, first of all, when you compare these responses to the bromide response in the first test, you see there is quite a difference, a much higher normalized peak concentration and the shorter tail. Basically, what appeared to be the case here is that the third flow pathway that was activated in the first test was not activated in this test.

The second point of interest is the Perrhenate has a lower peak concentration and a longer tail, ever so slight, but nevertheless apparent, than the iodide, which does suggest that there’s some sort of retardation behavior of the Perrhenate in the system. This might be explained by diffusion, but the problem with that explanation is literature values of Perrhenate diffusivities are lower than iodide, so in that case, if it was all diffusion, you would expect the Perrhenate to have a higher concentration than the iodide. In this case, it doesn’t, which does tend to point toward some sort of a retardation mechanism, or sorption mechanism.

Next viewgraph? Okay, this table summarizes all the parameter estimates obtained from tracer testing. I won’t go into all the details here. You can read them in your handouts. Each of these has a lower bound and an upper bound, and in a few cases, there’s best estimates provided.

I did want to point out some of the comparisons of these estimates with what’s coming out of the saturated zone flow and transport modeling in cases where you can make a direct comparison. The best estimate of effective flow porosity that we get out of the tracer test is about .12 in the alluvium at this location. The flow porosity distribution in saturated zone modeling assumes that there’s a normal distribution of flow porosities that are sampled, and they have a mean of .18. So, the best estimate is a little lower, but keep in mind also that the tracer tests were interpreted assuming dual porosity behavior, but it was dual porosity behavior with very short time and distance scales of diffusion. So, if you accounted for the stagnant porosity, as would be expected to be valid, over longer time and distance scales, this estimate of flow porosity would go up to around the 20 percent level.

Horizontal anisotropy, we get a ratio of north-south to east-west horizontal flow anisotropy of about 3 to 1. There is no explicit horizontal flow anisotropy in the flow and transport models, but there is a zone of enhanced permeability along Forty Mile Wash in the direction that we see the principal axis of conductivity. And, the conceptual model, flow and transport models, is that the alluvium behaves as a porous medium, and basically, these tracer tests confirm that assumption, although we do see dual porosity behavior, again, the time and distance scales of diffusion are very short. So, over longer--the performance assessment type time scales, that should be, a single porosity assumption should be pretty reasonable.

Okay, going back to the outline, I'll hit on the last topic, these innovative methods of measuring ambient flow velocities and identifying flowing intervals in Nye County wells.

Okay, a couple different methods were employed, inspired by, as I said, the OSTI program. I really want to give Barry Friefeld of Lawrence Berkeley Laboratory a lot of credit here. He was the one that was the PI on this project for OSTI, or science and technology, and worked closely with the Nye County folks doing this.

The idea with flowing electrical conductivity logging, if you're not familiar with it, I won't go into all the details, but the basic idea is you pump water out of a well, run it through a de-ionizing unit, and reinject it at the bottom of the well. And essentially try to fill up the well with de-ionized water by running this circulation, and you look at the extent to which you can do that replacement of de-ionized water as one indicator of where there's cross-flow occurring in the well. And, then, you can also stop this recirculation and watch the ambient formation water replace the de-ionized water in the well over time. And, you do this, of course, by logging up and down with the simple conductivity probe, and looking at how the profiles develop over time.

Next one? And, these are the results from 24-PB, which was one of the new Phase V Nye County wells completed just last year. This well was completed primarily in fractured volcanics. The water table was right at the base of the alluvium, right at the top of the Bullfrog Tuff that occurred at this location. And, there’s some Fran Tuff below the Bullfrog Tuff.

What you see here are profiles over time and also model fits to those profiles of conductivity as a function of depth. The blue line at the bottom, or the blue dashed line at the bottom of all these curves is basically the baseline profile that was obtained when trying to recirculate de-ionized water into the bottom of the well at eight gallons a minute. So, this essentially was as good as the well could be de-ionized at eight gallons a minute. And, you can see as you’re flowing up, there is this one interval here at about 230 meters where you essentially go from being somewhat de-ionized to hardly de-ionized at all. So, there’s clearly a lot of inflow occurring at this location, a fairly high flow zone is identified.

Then, the pump was shut off and these other profiles that go upward in time reflect the flushing of the de-ionized water out of the borehole by the formation water. We can see there’s an inflow zone here. It doesn’t really show up very well from the baseline profile, but you can see it as you watch the de-ionized water get flushed out. Flow rates are backed out of these model fit calculations that are shown here, the solid lines, and they indicated a cross-flow at this particular depth here, this 230 meter depth where there is this high flow zone of about two gallons a minute cross-flow in the well, which was very interesting to see.

Next viewgraph? Okay, the other technique that’s been employed is called distributed thermal sensor logging. The idea here is you run a constant wattage heater down the length, the entire length of a borehole, and then also equip the borehole with a sensor array that can look at and record temperatures as a function of time over the entire length of the well. And, this, as opposed to the electrical conductivity logging, which is done in an open hole, at least in this case it was, this is more amenable to being done in a shut-in borehole, where you eliminate the possibility of upward and downward flow in the borehole, by either plugging the borehole or packing it off.

The idea here is you heat up the well bore for two or three days, and in all these cases that were done by Nye County, it was a two day heating period, and you look at specific locations in the well, where the temperature increase tends to lag, and take those as indicators of cross-flow, where the water that’s flowing in the vicinity of the well bore is carrying heat away. So, anyplace in the well that the temperature increase looks like it’s lagging, or once you have heated the well and you look at the cooling, where the cooling occurs fastest, would be indicators of flow zones. And, the higher the flow, the more lag you would tend to see.

Next one? These are profiles from this same well I just talked about, 24-PB. This is now with a grouted-in borehole as opposed to an open borehole a couple months later than the flowing electrical conductivity logs. We see here in the dark blue line, the baseline temperature profile before any heating, and then we see a whole bunch of curves here going up on the plot in the upper direction on the plot, are going out in time during the heating period. And, it goes up to 48 hours, I believe, and we see that in this high flow zone, or what was identified as a high flow zone, with the flowing electrical conductivity logging at about 230 meters is clearly lagging in heating up. So, that’s another indicator that this is a high flow zone. And, again, doing some thermal calculations, it’s a little bit less directive a measurement, but by doing some thermal calculations, heat transfer calculations basically, you can deduce a flow rate in this interval.

Next one? Okay, Nye County has used this thermal logging method in several wells, six or seven by now, I believe. And, I did want to show the heating profiles in 22-PB, which was one of the wells at Site 22, where the hydraulic and tracer tests were conducted. It’s kind of interesting here, this is the baseline profile and here are the heating profiles. It’s interesting here that the lagging seems to be a little greater in the temperature increase down a little deeper than the zone that was tested for tracers. So, that might indicate there’s a little bit more flow there, although it is always a little uncertain to interpret these thermal logs. They’re dependent not only on water flow, but also on thermal conductivity, porosity of the rock, how uniform the well bore completion is, things like that all factor in. But, nevertheless, it’s interesting to see that there’s a possible indication of higher flow, slightly lower than the tracer test interval.

Okay, finally to summarize and conclude, the hydrostratigraphic framework model updates that I showed were based on both Nye County wells that have been put in in the last ten years, and the regional flow model updates in 2006. These have resulted in much greater predicted transport of radionuclides through the alluvium in the performance assessment model than in the saturated zone, in particular. That’s important because alluvium has a lot more surface area for sorption, and also flow velocities tend to be a lot lower. You have a higher effective flow porosities in the alluvium. So, flow velocities tend to be slower.

The Site 22 hydraulic tests exhibits vertical anisotropy shallow, and stronger vertical anisotropy deep. Just comparing with the site scale flow model, we get a good comparison for vertical anisotropy. When I say vertical anisotropy, again, I mean ratio of horizontal hydraulic conductivity to vertical hydraulic conductivity. You get good agreement deep. We see more of the uniform, or more of the homogeneous system shallow.

The composite horizontal hydraulic conductivity is 5 to 10 meters per day based on the hydraulic testing, versus about 17 meters a day coming out of the site-scale calibrated flow model.

The tracer tests indicate a dual porosity system with short diffusion, distance and time scales, single porosity system is assumed in site-scale saturated zone transport model. However, these are not incompatible because the dual porosity behavior again appears to have very short diffusion scales that would definitely transition over to single porosity behavior over longer time scales.

Effective flow porosity estimates from pressure testing, .12 versus a mean of about .18 in the site-scale transport model.

Specific discharge estimates ranged from .5 to 5.4 meters a year, versus about 20 meters a year in the site-scale flow model. And, the flowing electrical conductivity logging and the differential thermal perturbation logging do indicate a high flow zone in the Bullfrog Tuff at Site 24. And, they show promise for possibly being applied at other locations as well.

And, I think that was my last viewgraph, except for the legend for the hydrostratigraphic framework map.

GARRICK: All right, questions? Yes, Thure?

CERLING: Cerling, Board.

If you go to Slide 4, I was just curious because it seems like there’s some large changes between these, but not until--getting that last slide where we can see it--the legend, and really not knowing exactly what all these are, are the differences between the two maps really significant in terms of flow characteristics for most areas? I know you pointed out one area where it’s in alluvium, and it was in volcanics. But, in some areas, 18 has been switched to a 19, and so on. Are there really significant differences as that appears to be?

REIMUS: Well, by far the most significant difference is the increased amount of alluvium in the flow pathway. That’s really been recognized primarily as a result of the Nye County early warning drilling program. I do, as backup slides, have a few vertical cross-sections, and, you know, those show some differences. There's some changes in the number of units that are used in the different models and a little bit of difference in the mapping of the units from the old framework model to the new framework model. But, the changes for the most part are not great with respect to affecting performance, other than this increased amount of alluvium.

There's certainly been a lot of increased understanding of the structure out there, and that's been incorporated into the later version of the hydrostratigraphic framework model. So, if you're interested in seeing those vertical cross-sections, we could pull those up, I think.

CERLING: Well, I guess more as a follow-on question, what would you, if you had your wish list of what you could do to improve certain areas, you know, where is improvement needed? Because clearly, there was, you know, what you said is one very big improvement. Are there other areas where you could possibly improve things with an additional well placed boreholes?

REIMUS: Well, certainly there are. There is still what's called an alluvial, or alluvium uncertainty zone, I guess is what it's called, but it's basically a zone of uncertainty in this region here where the water table transitions from volcanics to alluvium. This is the way it's shown in the current hydrostratigraphic framework model, but there’s definitely an area that I’m roughly outlining here with this laser pointer that is considered a somewhat uncertain zone where that transition occurs.

There has been some discussion of trying to put a well in over here. Unfortunately, that’s on the western side of Forty Mile Wash, but still on the Test Site, which is a bit more of a logistical challenge for locating a well. But, certainly some more information there would help. That’s probably the big thing.

I guess the other thing is there is, in the flow model, this zone of enhanced permeability that I referred to along Forty Mile Wash, which, you know, could probably be characterized a little bit better if there were some more wells. Right now, all the wells are pretty much right along Forty Mile Wash, so you really can’t look at a contrast between properties of the alluvium in the wash zone versus a little bit away from the wash zone.

GARRICK: Ron, and then George?

LATANISION: Latanision, Board.

If we could go to Slide 21? Physically, what does dual porosity system mean? What does that mean, physically?

REIMUS: Okay. Yes, dual porosity system is a system that has basically porosity that’s flowing, and porosity that’s full of water, but not flowing. So, essentially, the flowing porosity is your primary porosity for transporting contaminants, radionuclides, and the stagnant porosity, or what’s often referred to as the secondary porosity, is porosity that is in mass transfer communication by diffusion with the primary porosity, but it’s not actually flowing. So, it’s essentially acting as a storage porosity that tracers or contaminants can diffuse into and sorb within surfaces in that porosity, and then they have to diffuse back out to a continued transporting along the pathway.

LATANISION: How would you distinguish such a site physically? I mean, can you look at a particular type of pore and determine that it’s going to be essentially a trap for water, and the other a mobile? I mean, what’s the distinction?

REIMUS: In alluvium system, you know, you certainly have internal grain porosity that would be dead-end porosity that should behave, you would expect to behave as this secondary porosity that’s not flowing. However, there’s varying degrees, I guess of dual porosity character. You could think of, in a layered system, which these alluvial deposits certainly are, you know, you can have layers of coarser material that have a much higher hydraulic conductivity than other layers that are like clay, much finer materials. And, so, those layers may actually--those layers of a clay, finer grained material, may have a slow enough flow velocity through them that they essentially act as a secondary porosity that behaves more as a storage porosity than part of the flowing porosity.

LATANISION: That helps. If we could go to Slide 20 next for just one moment? Your first bullet does not sound like particularly good news. Am I reading it correctly?

REIMUS: What doesn’t sound like good news?

LATANISION: Well, I mean, greater predicted transport; is that a characteristic that gives you comfort or gives you concern?

REIMUS: Well, I’m sorry, when I say greater, I’m talking about a greater portion of the flow pathway is in alluvium. Don’t take that to mean that there’s faster transport through the whole system, but, no, it’s poorly worded, I guess. It is intended to convey the idea that the new hydrostratigraphic framework model has a much greater portion of alluvium in the flow pathways.

LATANISION: One of the hazards of the English language, isn’t it? Okay, good. Thank you.

HORNBERGER: As Thure said, you could have said it as less transport in the tuffs.

REIMUS: Yes.

HORNBERGER: The FEC logging, was that done only in the Bullfrog? You didn’t log any of the alluvial wells?

REIMUS: Well, the water table was at the base of the alluvium, basically right at the top of the Bullfrog in that well.

HORNBERGER: But, that’s the only well that was logged that way?

REIMUS: No, there was one other well that was logged that way, 32-P was logged that way. It was one of the wells that was drilled where there’s a volcanic anomaly. That had some very interesting results, which I didn’t present, some interesting what appeared to be flow up out of the--let me back up. In that well, there was a basalt layer intersected by the well, which was sort of suggested by the magnetics, and alluvium above and below that basalt layer. And, a screen was put into the well, both in the basalt layer and in the alluvium above and below the basalt. It would appear to be the case there that there was flow coming from the alluvium below and the alluvium above, into the basalt, and then flowing out in the basalt. And, that’s a very preliminary result that hasn’t been analyzed to the extent that the result from 24-PB has.

HORNBERGER: So, for 24-PB, Barry calculated specific discharges. You didn’t present those?

REIMUS: Right.

HORNBERGER: Do you recall offhand what the numbers are?

REIMUS: No, I don’t. I don’t recall offhand what they are. He primarily calculated linear velocities based on a number of assumptions, and then tried to back out a specific discharge.

HORNBERGER: My recollection is they are 10 to 100 times bigger than the ones you cite for the alluvium wells.

REIMUS: Well, of course, again, this is in, you know, fractured volcanics, not alluvium. But, as far as where the linear velocities--I mean, if you take the specific discharge that’s assumed, or the range of specific discharges that are assumed in the performance assessment parameter distributions, and then you also look at the ranges of effective flow porosities in the fractured volcanics, those linear velocities that you back calculate out of like to 24-P result, actually do fall within--they’re at the upper end of the range you would expect, but they do fall within the range of that combination of parameters. You know, it’s the combination of the specific discharge and the flow porosity that give you the linear velocity. And, it’s certainly within the range that’s considered in TSPA.

HORNBERGER: So, if we go to Slide 19, you showed a thermal log for? Now, this is the alluvium?

REIMUS: This is all--well, except for Screen 4, is this volcanic breccia. But, yes, this is all alluvium.

HORNBERGER: And, so, the Screen 3 results, as you pointed out, suggest that you have a higher flow zone there. So, have you done the calculation? I mean, my recollection is that you said the calibrated flow model had a specific discharge of something like 20 and you were estimating from the tracer test something on the order of 10. Is that just a factor of two, or is it more than that?

REIMUS: I haven’t done that calculation.

HORNBERGER: And, you can’t eyeball it? Your eyeball isn’t that well calibrated?

REIMUS: Well, you know, as I said before, it’s--you know, I think before you write down a number, you have to also consider the uncertainties associated with the well completion, and the different, you know, porosities encountered at the different depths. And, so, there is some uncertainty associated with it, but no, and, in fact, it’s kind of interesting that Screen 3 actually was a poorer producer than Screen 2 in terms of hydraulic conductivity determined from hydraulic testing, just from looking at the responses, the cross-hole, hydraulic responses. So, that actually contradicts this thermal perturbation look at the well. You, of course, have to recognize again, too, this is a very local measurement. I mean, we’re talking interrogating only, you know, tens of centimeters at most into the formation from the well bore, whereas, a cross-hole hydraulic test is, you know, in this case, 18 meters. And, then, the flow model is trying to do things at, you know, kilometer scales.

HORNBERGER: Yeah, but I mean your interpretation of the thermal isn’t that you’re just interrogating ten centimeters. You’re interrogating the flow into the borehole.

REIMUS: You’re interrogating flow, in this case, around the borehole. This is actually, you know, a closed off borehole.

HORNBERGER: Yes. Right.

REIMUS: But, yeah.

HORNBERGER: To go back to Ron’s question about the greater predicted radionuclide transport through the alluvium, the thrust of that slide is you now have a longer predicted path through the alluvium?

REIMUS: Right.

HORNBERGER: And, did I hear you correctly; is it about 30 percent longer?

REIMUS: Well, it’s about 30 percent or so of the total flow path away.

HORNBERGER: Oh, of the total flow path?

REIMUS: right. However, previously, there was no alluvium in the flow path up to the 18 kilometer boundaries. So, yeah, it’s 30 percent.

HORNBERGER: My recollection is from performance assessments, that that can be pretty important in terms of calculation of radionuclide transport, because that’s where you get the sorption of things like neptunium; is that correct?

REIMUS: Right. It’s very true, not only do you get much greater surface area for sorption, but, you know, that 30 percent--I don’t know the exact numbers, but I’m sure that that 30 percent of the distance accounts for a much larger percentage of the travel time because of the higher flow porosity.

HORNBERGER: Right. And, so, these new results are being fed into the TSPA?

REIMUS: Yes.

HORNBERGER: As we speak?

REIMUS: Yes.

HORNBERGER: Okay, thank you.

GARRICK: All right. Any other questions? I want to move to the public comment as soon as we can. Dave, go ahead.

DIODATO: Diodato, Staff.

Thanks for your talk, Paul. On Page 4, you had the--on Slide 4 there, you had the two different hydrologic flow models. I just want to look at this for a second. On the left, if we look at the particle tracks, you know, in some of the previous models, they would--Busted Butte, the particle tracks would have to divert around Busted Butte, and then reconverge. And, here, it seems like they don’t see Busted Butte at all. It’s going to go ripping right through.

So, that’s one observation I’d like you to help me understand.

And, the other part of it is on the 2006 hydrologic flow model, framework model, those particle tracks there don’t reflect that flow model. They reflect the old; is that correct? I mean, they’re identical, so it seems like--

REIMUS: Yes, I’m sorry, I should have mentioned that. Those particle tracks are both actually from the 2006 flow model. I just showed--I just superimposed them both on the old hydrostratigraphic framework model and the new HFM. So, those are both--those are identical particle tracks from the 2006 model.

DIODATO: So, what change, I guess that would suggest to me that the Busted Butte hydraulic parameters of the Busted Butte now don’t have that much of an influence on the particle tracks. It looks like it really just kind of rips right through, doesn’t really see it, you know?

REIMUS: Yes, I don’t have a good answer for that. I’m not directly involved in the flow modeling. I actually don’t recall the diversion around Busted Butte. Maybe I could ask Charles Bryan to come up and answer that one.

DIODATO: It’s been noted that--so, then on Slide 11, you had three pathways postulated here, but then in a subsequent test, you only had two active pathways. So, what’s your hypothesis of why in one test, you could see three flowing pathways, and in a subsequent test, the same wells, only two pathways would show up?

REIMUS: Yes, good question. And, that caused a lot of pondering for sure. Can we go into my backup slides? I don’t remember which one it is, but I do have a backup slide that addressed that.

DIODATO: We’d like to get these backup slides, by the way, just so we could have them.

REIMUS: Two back. That one, okay. Okay, so this is a few working hypotheses. First of all, something I didn’t mention was that the tracer solution that was injected in the first test had a very high density. It was basically, you know, on the order of 5 percent more dense than the groundwater, which is pretty hefty, you know, we’re talking a few hundred thousand parts per million concentration of tracers. The second test was much, much lower concentrations, so there certainly was a difference in the density of the tracer solution, even though it was the same volume injected, and it was chased with the same amount of water, and the pumping rates were all the same. Everything was the same in the two tests, except for that.

So, clearly, density driven flow is something that you would look for as part of the explanation, because you would definitely expect it to occur in the first test, and not in the second test.

And, so, there’s a couple explanations here. The first site, the explanation on the left here really just accounts for the two pathways that we see, which, you know, appear to be the case in both tests. There appeared to be two flow pathways contributing to the transport in both the first and the second test. It was the third pathway that was different between the two.

One possibility for explaining the two pathways is that we did only have a very small amount of tracer solution, and then a small amount of water that chased it out of the borehole. If there happened to be a higher conductivity channel somewhat near the injection borehole, but not exactly directly intersecting it, it’s possible we could have pushed a small amount of tracer into this high K channel. That would have, in effect, broken off and flowed quickly toward the production well. And, then, what remained behind that wasn’t initially pushed into that high K flow pathway would have eventually bled into that, and resulted in the second peak.

The other possibility in the cross-section view is that there--you know, this is clearly a layered system. This is 100 foot interval. We could have had a high conductivity narrow layer, with a small amount of tracer mass, and that first flow pathway only had about 5 percent of the tracer mass in it. A small amount of mass could have transported quickly through a high K channel, and the majority of the rest of the mass would have moved through a much lower K stratification or portion of the system.

I speculate that the third flow pathway occurred in the first test, but not in the second test, because there was quite a bit of tracer mass that sank out the bottom of the borehole because of that density contrast. It would have eventually deluded and worked its way back to the production well under the influence of the pumping. But, that is my explanation for why there's a third pathway in that first test, but not in the second test. There wasn't that density contrast at all in the second test. It was basically the same ionic strength almost as the groundwater. So, that's a couple of speculative explanations.

DIODATO: Okay, thank you.

GARRICK: All right, I think we're going to move on because we want to make sure that people that have to leave at 4 o'clock have an opportunity to make the comment we had promised that they could make. So, if we have to, we can come back to this. But, thanks very much.

And, as we promised, I guess, Judy, you were going to make your comment, give us the benefit of your comment at this time.

TREICHEL: Judy Treichel, Nuclear Waste Task Force. I'm not sure how beneficial this is. But, it may be interesting.

One of the things that is so amazing, and I know I’ve been doing this for as long as the Board has been around, is the way things change, and the fact that the site that was recommended is so very different from the site that’s headed, they say, toward licensing. And, there just seems to be a sort of a chaotic thing going on here, and I thought part of that really showed up in the GNEP presentation, because it always seems like something that you don’t know too much about really looks good. And, I think that that was really clear when Mr. Laidler said that he would just as soon have the waste from that in his back yard. And, I always have the urge to ask what his address is.

But, we just--we’re just racing toward something that’s 20 years old, and there’s such a disconnect between site recommendation and site licensing now. And, I’m not going to be here for the PVHA presentation, but in part of that, it says that they’re going to get the results out of that in June ’08, which I guess will come careening in along with the license application, or probably, the way things go, even after that time.

But, since the time that this started, when DOE was promising that if every single rule at the time wasn’t met, they were going to walk away, we were talking about inches of transport, groundwater travel time during centuries, and now we’re talking about meters per year. And, I know it depends upon if you’re talking about alluvium or if you’re talking about the carbonate aquifer, but when you talk to the guys that worked for a long time at the Nevada Test Site when they were doing a lot of drilling out there, you hear stories about really rapidly moving water, and you hear stories from people who have been around Amargosa Valley for a long time about the various temperatures in the water, and all of the questions. And, we just keep coming up with so many more questions, and I think the Board really needs to be thinking about that.

And, I guess I get really offended as a Nevadan who like almost every other Nevadan, really doesn’t want this thing to happen, and to be looking at it and not to be even an alarmist in order to be alarmed about it. The discussion that was carried on here about the water use when you had the Inyo County presentation regarding the carbonate aquifer, and the discussion about well, what year does that hit. Okay, great. Let a lot of people drink for a while, and then they can give it over to the Yucca Mountain project, and that’s bizarre. We have been battling with EPA, who of course still has not put out a standard, over the us versus them kind of thing. And, you’d have one standard for the people now, and one standard for the people way down the road.

Well, I think this is the epitome of that, that you would have water available for people now, and then the people in the future would have to accommodate or include Yucca Mountain in their ability to be able to drink water. And that’s where Nevada has really been upset over this thing, and that’s where we’re always going to be upset about it, and that’s why we’re not going to quit until we win, no matter how long it takes.

And, this situation just gets set up for that. The battle lines are continually drawn, and there’s always this line in the sand, and we’re not going to cross it. So, it would seem to me that the Board, in looking at the scientific validity of this whole thing, really needs to look at the changes over time, and not give them a pass on this. A lot of the stuff you’re hearing is crazy, and it should not go any further than presentations here.

Thank you.

GARRICK: Thank you. Thank you, Judy.

Steve, were you going to make a comment, or--

FRISHMAN: Steve Frishman, State of Nevada.

I just wanted to go a little further on this question of the discussion that went on over future water use in Nevada. John, I got the impression that you thought that there was something going on in the water planning area in Nevada and California that was somehow detrimental to this program. And, that’s not the way it goes. It’s Nevada’s sole responsibility, and California’s, to plan the use of their water. And, it’s perfectly reasonable in this part--or in southern Nevada especially, and now we’re beginning to see in Inyo County as well, it’s perfectly reasonable to assume that there is going to be very rapid growth, and the basis of that rapid growth is going to be available water, primarily the carbonate aquifer.

And, to somehow imply that we in those two states don’t have the right to plan for that future development and the right to use that water as our laws permit and as our societal needs require, to somehow imply that it’s wrong to do that in the face of the fact that somebody else wants to possibly contaminate this aquifer is just totally outrageous. And, I think it shows maybe the--it’s symptomatic of the level of concern that we’ve had all along in Nevada about the fact that there is some federal right that we’re not supposed to interfere with, when, in fact, we have our own rights.

I recall when I was working in Texas and Guthsmith (phonetic) County was under consideration. And, it was, well, the second richest farm county, agricultural county in the country at the time, and there was a lot of concern about the salt pile that would result from mining out a repository in the salt beds beneath Guthsmith County. It would have been a large salt mound. And, in that area, people were growing wheat, growing sugar beets. The immediate area was, in fact, the seed farm that developed the genetic red winter wheat, which is the staple of wheat in this country. And, people started saying well, this salt pile is going to result in increasing salts in our groundwater. We already have that problem from irrigation. We have salt build-up and we have to deal with it. Now, you're bringing salt to the surface to be blown around, and the Department of Energy's answer was well, you people should grow more salt tolerant crops.

And, I see the same kind of thing in the assumption that Nevada and Southern California don't have the right to have a planning perspective over a very long period of time over the use of their water, and that somehow, we do not have--or somehow, we're preempted because there's some greater plan. That just can't happen, and it isn't going to happen. And, I think it's particularly disturbing to see that outside of the Board's responsibility, there's even concern about what our water resources planning is in the State of Nevada, and in the State of California, and how we would intend and attempt to protect those water resources for their currently legal uses.

Thank you.

GARRICK: Yes, just on that. My question was only informational. I was raising the question about what studies had been performed more than I was concerned about who should do the studies. And, the fact that these aquifers were as interconnected as they are, which was something that was not generally discussed in the past, would seem to indicate that the regional boundaries are quite extensive here, and was there not some consideration of that in the studies that were either underway or being planned, and that’s all I was really trying to better understand.

FRISHMAN: Well, there’s a difference between the regional studies you were talking about, and the implication that I got, which was regional--the concern over whose responsibility and right are involved in regional water planning.

GARRICK: I would never get into a State’s rights or--no, that was not--

FRISHMAN: Well, you damned near started one.

GARRICK: That was not my perspective at all.

KADAK: Kadak. I was trying to connect the morning presentation with the afternoon presentation. And, I didn’t see water--I didn’t see how they were connected, frankly. The water flow paths looked like they were, as I think George said, going south, and the other one was going like southwest. Is there any clarity in where this water from the Yucca Mountain area is going? Perhaps both of you can answer that.

FRISHMAN: Well, I think in the specifics of where it’s going, there’s a lot to either be agreed with or not agreed with in the last presentation. It’s certainly new information, or a compilation of newer information than was out there even for the 2004 model. But, there’s also, at least as was talked about this morning, this sort of unknown about the relationship between the carbonate aquifer, the tuff aquifer and the alluvial aquifer. And, we’ve really, through all these years, as was pointed out, we’ve really only had one data point for the higher head in the carbonate aquifer. It’s become sort of a core in everything all the way to transport.

Now, there’s apparently another data point that suggests, but doesn’t show the same level, but I think what it all comes to is we have a pretty good understanding that the discharge point for water that flows under Yucca Mountain is Franklin Lake Playa, and also very likely springs in Death Valley. And, the exact movement of that water, I don’t think we have enough data on. But, I think it also relates to some unknowns between--in the relationships between those three aquifers, and a much greater unknown about the carbonate aquifer in that particular area.

So, in some discussion that we’re going to have with the California Energy Commission in a couple weeks, we’re going to be in this same discussion again, where Inyo County’s study is going to be discussed. I’m going to be talking about some things, and it’s--there are two different perspectives. But, we’re talking about maybe not the same water, and we’re talking about two discharge points, one of which I feel pretty confident is an important discharge point. The other I think is growing in importance as being a discharge point, and I think the discussion about the head reduction is an important one, because it makes the possibility of the Death Valley discharge case probably as important as the Franklin Lake Playa discharge case if you are within--at least within the calculational range that it is possible.

So, it’s probably that there is confusion to some extent about discharge, but I don’t think it’s as much as saying it’s either there or there. I think it is very likely both, but for very different reasons. And, anybody else can try to explain that if they want.

REIMUS: This is Paul Reimus.

I guess I just wanted to point out that the flow pathways or the particle track pathways shown this morning, and the ones that I showed, I think are actually completely compatible. We’re talking about two different depths in the flow system. We were talking about the carbonate aquifer this morning. Now, if I’m not mistaken, I believe a particle was just put into the carbonate aquifer below the repository this morning, and flowed through the carbonate aquifer, based on, you know, the regional flow model, straight toward Death Valley. Is that correct?

So, that was all carbonate, and in the Yucca Mountain models that my particle tracks, or the particle tracks in my presentation reflected those particles never see the carbonate aquifer. There’s that strong upward gradient that was discussed this morning. The particles, once they hit the saturated zone, they stay in the fractured volcanics, relatively shallow actually. There’s a vertical upward gradient within the volcanics as well. And, these two aquifers, it’s fairly well recognized, at least in the vicinity of Yucca Mountain, are quite separated from each other. I mean, you wouldn’t have this 20 meters of head difference if there was good communication between the two.

I mean, we are talking about two different things here that aren’t incompatible based on the understanding of the flow system and how separated the volcanics are from the carbonate aquifer, at least in the vicinity of Yucca Mountain.

KADAK: Is there a graphic that you can show us that shows where these two aquifers are relative to where the waste is?

REIMUS: Actually, the best one I have at least was in my backup slides, showing the vertical cross-sections, if you wanted to see that.

KADAK: Perhaps. I mean, I’m trying to--you’re talking two links; right?

REIMUS: One high, one low. And one is going towards the southwest towards California, and the other one is going sort of south, from what he showed; right?

KADAK: I’m sorry, I’m confused, but maybe that one slide will help me. Can you find it? Is it that one there?

REIMUS: There we go. So, this is a vertical cross-section now of the base case model in 2004. This was actually an alternative model in 2004, which by then, you know, certainly was recognized. There was differences based on the Nye County program. And, then, this is the 2006 based case. But, this blue color here is basically the carbonate aquifer, and the dark blue is up-thrusted carbonates. But, the carbonate aquifer is very deep. The water table is this line here.

This actually, the earlier model actually was cut off at the water table. The water table is the dashed line going across here in the two lower figures. So, the blue and the dark blue is the carbonate aquifer, and that certainly is the main aquifer in a regional sense that John was talking about this morning, and it underlies all these volcanic units. The repository is here. The 18 kilometer boundary is here. Death Valley is way out of this cross-section here. But, anyway, all the particles in a vertical profile coming out of the repository are going to hit the water table and because of that strong upward gradient, are predicted to remain shallow, go through the various volcanic units until they hit this alluvium out here. And, then transport in the alluvium to the boundary, and they remain separated from the carbonate because of that upward gradient.

KADAK: So, your suggestion is that even if the repository hundreds of thousands of years from now, or sooner, starts dissolving the waste, you’re staying that it will stay in that upper aquifer?

REIMUS: That’s what all the data suggests, that, or is used in the saturated zone flow model, the site-scale saturated zone flow model. There’s both the head data, the geochemical data, certainly suggests those aquifers are very compartmentalized with respect to having any communication with each other. And, so, that’s basically what the data supports.

KADAK: Is the flow path in those two aquifers the same direction, or is one, as you suggest, going south and the other one, as the earlier speaker suggested, going to Death Valley?

REIMUS: I certainly can’t speak to the deeper one, especially in the vicinity of the repository, where, as was mentioned, there’s only a couple of wells. But, you know, that one basically reflects the regional flow model that the USGS has done. It reflects, you know, the information in that regional flow model, the deeper one, and exactly what direction the flow goes in the vicinity of Yucca Mountain, I can’t speak intelligently to.

I can speak to the model that the project has developed, which is based on water levels and geochemistry, a number of different lines of evidence in the volcanic units and in the alluvium to the south.

KADAK: Okay, thank you.

GARRICK: Okay, thank you. All right, I think we’re going to take a 15 minute break. We’re not off schedule as much as it sounds, because we have had probably the substantial part of our public statement, so I think we’ll be all right. 15 minute break.

(Whereupon, a brief recess was taken.)

GARRICK: Our next presenter is Mark Johnson from Bechtel SAIC. Mark is the Project Engineer for Subsurface Engineering. So, without further ado, here’s Mark.

JOHNSON: Thank you, Dr. Garrick.

Yes, I’m going to talk a little bit about the waste package design, prototyping. I’m also going to show in a little bit of the prototyping, what’s going on up at INEL, about a 50,000 foot high-level, take a look at some of the things they develop there.

Just as an outline, we’re going to go over the design requirement for the waste package, description and major features associated with the waste package, we’ll go over some of the changes that have gone to the waste package with the implementation of the TAD concept, touch on the design code for the waste package, and then I’ll hit on the prototyping programs.

Okay, from design requirements, we have both preclosure and postclosure design requirements. Obviously, we have to fulfill safe loading of the canister, the canistered spent fuel and high-level waste. It’s transportable within the surface facilities, it has to be transportable and placed in the underground. We have to safely and remotely close it. That’s closure cell. Retrievable, and it’s got to meet its preclosure safety requirements, that’s breaches from drops, some handling equipment, and rock fall scenarios, run-away scenario in the emplacement vehicle, that sort of thing. We’ll talk a little bit about preclude criticality, however, that’s really transferred over to the TAD, and not the waste package anymore.

And, postclosure. It is important to barrier capability and it must meet long-term dose performance requirements.

Some features here. We talked about it earlier today in the other presentations, but, we have an Alloy 22 outer corrosion barrier. We have sleeves on each end. They provide stiffness and also contact points. One of the changes with the new surface handling facility, the TAD, there is no longer removable trunnion collars here. It’s all handled via the pallet. This is the inner stainless steel 316 inner vessel. Also, there’s a two lid instead of the old three lid design that’s new to this.

We touched on it briefly. We removed the internals from the fuel bearing waste packages. We increased the volume to take the TAD. We changed the number of configurations down from ten different configurations, there was a BWR, there was a PWR, there was a 12-PWR, down to six configurations, and the criticality is, like I mentioned, part of the TAD now.

We removed the inner Alloy 22 lid. We also added a shield plug to the design of the waste package. Both the TAD and the naval waste packages have integral shield plugs to them. In order to get the dose level down on the closure cell equipment, consistent with those two, a shield plug has been added to the DOE high-level waste bearing waste packages. And, I mentioned we removed the trunnion collars to facilitate the handling approach in the surface facilities.

This is just a quick view of the different configurations. Right now, the 21 44-BWR TAD and the naval long are identical. That will probably change as TAD information comes in from the vendors. But, right now, that’s what we’re using. We’ve got the DOE waste package, and then also the naval short as well.

For criticality control imposed on all canisters. Waste package doesn’t have that feature anymore. Neutron poisons in the DOE canisters are tailored to whatever type of DOE fuel there is, and that comes with the canister.

The TAD is performance based into the spec., and it can either be neutron absorber plates, tubes, of borated stainless steel, and it can do more of an analytical method and show the postclosure, how they meet criticality. I’m probably going to focus on this one. And, that is all provided in the performance specification for the TAD that was discussed this morning.

An exploded view. This is the TAD naval waste package, real quick, exploded view of this, you’ve got the inner lid, with the spread ring. There’s a couple fillet welds on it. The outer lid has a full depth closure weld on it. Thicknesses, the outer corrosion barrier is a 1 inch thickness, the inner vessel is 2 inches, and those are consistent with the lids. Basically, the same thing, except you’re seeing here the integrated shield plug that is on the DOE package. It provides a shielding on top of these, and that varies between 8 or 9 inches, depending on the waste package configuration. I think the 2 MCO2 HLW is the 8 inch, and then the five packs are both the 9 inch shield plug, but the configurations are the same.

One of the things we're working on now is putting a taper on this to make remote placement a little easier, and then as well as where we place this. The old concept, or current concept, I should say, is to place these lids on in the closure cell. Since this is a lot thicker, and the shield plug integrated with the lid, the INEL equipment doesn't quite handle the weight of that, so we're looking at placing this in the--using the canister transfer machine in the facility, and then taking it to closure cell, where the spread ring will be spread out and the fillet welds made.

Codes and standards that we're applying to the design of this, I think it was in 2005, BSC along with the code consultant, put together a position paper on the code approach to the fabrication. The inner vessel is fabricated to the ASME Code, Section III, Division 1, Subsection NC, and we'll have an N Code stamp affixed to it.

Outer corrosion barrier, basically the same code, Division 1, NC, however, it won't be N stamped. Materials are specified in Section II, and NDE Section V, and welding to Section IX.

Part of the reasons we've gone that route, you know, in the position paper, we discuss--there's a lot of different things, but I think your heavy hitters is Subsection NC, this is precedent for NRC licensing of basically storage casks as of late. There is also more vendors that are qualified to do NC work than such that the NB, a little stricter subsection, and that’s why we’ve gone that route.

From the prototyping on the waste package and components, program objectives are to develop and confirm the fabrication methods. I’ll go over some lessons learned that we’ve got with the first prototype. Inform design of design alternatives. Part of the program allows us to change some things with the prototype, is we go out and investigate how well they worked, and then if it’s successful, we can roll that back into the design of the waste package. We want to look at some commercial vendor capability. It’s hard to do with sending procurements of one waste package out there, but we’re basically getting the waste package out there, getting companies familiar with fabricating them, and the issues with it.

Along the same lines, we’re developing qualified vendors, gives us an idea of future costs and fabrication durations. Prototyping costs are naturally a little higher, but it gives us an idea of the ballpark, especially when you start buying Alloy 22. If you’re familiar with the market and nickel right now, it’s pretty expensive stuff. We look at process variability. We want to go out, have a couple different manufacturers prototype the same waste package, and we’ll compare them and see how much variation there is. And, we also plan on using these to do start-up testing, factor acceptance testing, provide for training, as well as we do some destructive testing of the prototypes to get some information on residual stress, those sort of things, and get that information over to the lab.

I touched on a little bit informing design. We figure out tolerances, how well things, we can fabricate, we provide samples for destructive testing and ensure handling techniques are achievable. This is just a little picture of the prototype we just finished up here last January.

I have here the different things--we’ll go over what we can do with these prototypes. We have, I don’t know if you guys have seen the transport and emplacement vehicle, but I’ll have a little animation here that shows an emplacement activity. That vehicle picks up the waste package in the surface facility and transports it all the way down underground. Maybe it’s a newer concept you haven’t seen yet. But, with these prototypes, we can check different things. We can look at receipt inspection to look at hey, we shipped this thing across the country, how much damage did we get to it. We can use these waste packages for handling in the surface facilities, as well as in here. We can use them to test fit run and loading TADs into them, loading the naval package, loading the DOE waste.

We can test them, maneuver them around on the emplacement pallet, and handling the waste package after closure, emplacement. And, then, also shipment to the vendors to do some extensive factory acceptance testing.

Go ahead and click the video. This will show you the--it's basically an emplacement activity. It comes in, opens the doors. You're going to see the bed plate rolled out. That's the mobile shielding plate. It's lowering the waste package, backing off. It will close, and exit the emplacement drift. But, the reason I'm pointing that out in here is that needs a pretty extensive factory acceptance test. I would like to test it with actual waste packages. So, those are the sort of things we can use these prototype, after we have manufactured it, for. Also, in the surface facility, they have a rather unique piece of equipment that grabs that, tilts it up, so we can provide the different waste packages to these vendors to support some extensive factory acceptance testing.

This is a picture of the annealing process of the first prototype. It was a 21 PWR, so it had the fuel basket in it. That's the annealing of the outer corrosion barrier. It's essentially upside down, so it's going in with the open end. What you see here is a snorkel to get the steam out of that as it is placed into the clench tank. And, my numbers on the top of my head, I think we heated it up at about 100 degrees an hour, up to 2050 plus or minus 50. I can’t remember the whole time, but the quench time was supposed to be under two minutes, and the water was at an ambient temperature, to get us the results we wanted for the annealing.

It finished in January of this year, and it’s presently being stored at the fabricator awaiting implementation of the testing program. This first waste package has a life to be tested, cut up and some results for the science people.

Residual stress measurements. These are the testing we’re going to do to it. They’re going to measure stress fields due to the solution heat treating. We want to look at, after we test it, is it consistent, did we have any dimensional changes through shipping.

Mechanical and corrosion properties. We’re going to damage this waste package. We’re going to actually go measure the energy, the impact with different, you know, ball bearings, different things to try to figure out how does damage that may happen in the facility affect the residual stress, and then maybe hopefully develop a criteria, saying, if in mechanical handling, it gets dinged up, what type of marring is acceptable. That’s part of the planned testing.

Some samples are being provided for corrosion testing. We’re also planning on heating it up and then placing the lids to see if there’s any problems with placing the lids on a waste package that’s at the temperature it would be after it’s loaded with waste, and then the lids are going to be placed on it.

Some lessons learned. On the welding, had pretty good results with the welding, and didn’t get any defects on it. Did find out we missed a fillet weld when they did the code analysis at the fabricator, and, so, that’s been added.

On the machining, we found the guy that--the machining sub tried to go a little too fast, he broke some tools, and had to slow it down. It took a lot longer than they anticipated. So, we’ve added that. We don’t tell them how to machine it, but we add in that machining Alloy 22 takes more time, to consider that in your schedules. And, then, we give some recommendations, but we want to make sure we’re not trying to take the responsibility for manufacturing this away from the vendor.

One of the things we had thought was the ability to machine the inside of the outer corrosion barrier after the annealing. When we annealed it--well, just go to the next one, and I’ll get to that. When we annealed it, we had some distortion. We put essentially spacers in it to keep it from deforming, but we still had some distortion at the top. We originally planned on the vendor having the capability to machine it out if that was the case. Before we annealed it, we placed the inner package and it fit. Afterwards, we measured and knew it wasn’t going to fit, so we had to do some machining to get it in there.

So, we have added extra fabrication stock to the outer corrosion barrier to allow some machining. But, the vendors out there, not everybody has the ability to reach all the way in. Now, the distortion we noticed was at the outer trunnion sleeve. We’re going to try to anneal the outer trunnion sleeve this time separately prior to installation. We think that the stress that was in that actually deformed it during the annealing.

Also, we found out we have the thermocouples lined up in there that we had a few hot spots where the steam vapor wasn’t getting removed by the snorkels. So, we’re going to introduce a spray system in there to help mix that up and remove those steam pockets.

Follow-on prototyping, we’ve had a change. Like I said, the first one was a bare fuel type. We’re going with TAD-bearing, which is also right now identical to navy, and then also the high-level waste. We do have the ability to change this around as we see fit, but we wanted to go with three of the similar ones so we can look at some vendor, different vendor manufacturing variances with the same type waste package.

This one now is planned to go out for--the design documents are done, the spec is done, the code spec is done, probably go out for bid in the 2008 time frame.

On the component side, we do have the pallet and the drip shield that we do plan to prototype. Right now, we’re looked at two pallets, long and short. I don’t think pallet is too complicated, but they will be used to support factory acceptance testing and start-up testing for the waste packages.

And, the drip shields, two of them, we want to confirm the connection/interlocking feature, as well as support the factory acceptance test and start-up testing for the drip shield emplacement gantry.

This one just hit the street. We’re going out to procure the mock-ups to support the actual closing demonstration at the INL facility for the closure system. These are going to be full diameter, but much shorter height mock-ups to support that, and we’re scheduled to complete procurement of that and have it to INL by July 2008 to support their testing of the system in October of 2008.

I’m going to give a brief overview of the closure cell prototyping. This one is not under my area of responsibility, but I’m going to hit some of the high points of it.

This prototype doesn’t just look at the welding system. It also has in it the NDE, the plasticity burnishing, the purge, and inerting of the waste package. So, it’s more than just welding. It’s the whole system. They’ve got the robotic arms. They have they call it the remote handling machine. And, also, equipment for different sort of recovery. But, they’re going to go over the welding, NDE, leak testing. They also are putting together the process ops for that, and this unit would eventually hopefully support start-up testing, and also be a training unit to train operations staff, then operating the actual system.

This is a picture of the current welding end effecter for the closure weld. It’s going to be mounted on the end of the robotic arm. The plan is to have two of these arms that will each be doing 180 degrees of the weld.

Just an example of the end effecter for the NDE. The visual inspection was on the welding end effecter, but this one does the eddy current and the ultrasonic. It is also attached to that same robot arm I showed in the last slide. As you can see here, this is a little mock up of the top of the closure weld of the waste package, and how it travels along the waste package.

Inerting and leak testing. The inner lid has a purge port on it, so the plan is to purge and fill the cavity space between the TAD and the inner vessel with helium, and then they leak test using this took, leak test to see if they have any leaks. You can see essentially in this little blow-up, which is actually a TAD, but there's your spread ring, you fill it welds, you've got a gasket here and here, and eventually pull a vacuum on it, looking for any helium leakage.

So, in summary, we went over the waste package design requirements, how the waste package is changed to accommodate a TAD. The criticality has been moved out of the waste package into the TAD as a specification on the vendors. And, then, we went over the prototyping program, talking about the first one, the plan for the future, and then just briefly hit on the closure cell.

So, with that, I can take some questions.

GARRICK: Okay, Henry, and then Howard?

PETROSKI: Petroski, Board.

I'm pleased to see that this is underway. I was wondering how long you expect to be doing this? You said you contemplate about six prototypes. I'd like to know how long that might go?

JOHNSON: From the waste package side, the plan as scheduled now is basically from now to the 2011 time frame. It's all dependent on funding, but that's what's in the plan. Your pallet is probably in the 9 to 11 time frame, and your drip shield is coming after that. That's currently what we have in the plan.

PETROSKI: Now, this first prototype that you describe, and you indicated that you would cut samples from that for corrosion testing. Will those samples incorporate the damage that you will inflict on the prototype?

JOHNSON: I don’t think they do for the corrosion testing. Those were going to be separate. So, you’re saying inflict the damage, take the residual stress and then give them to corrosion?

PETROSKI: Well, there’s some concern. I’ll let the corrosion people speak to it more directly, but certain types of damage may affect how this sample responds to corrosion testing. So, I would think it would be important.

JOHNSON: That’s a good point. I’ll take a note of that. We get the requirements from that. We develop the testing spec jointly with Sandia, their waste package, corrosion people, to try to just say hey, what would you like from this, and we can get it for you. It’s no problem. We cut coupons, tell a size, how many you want, and that sort of thing. So, if that’s something that they would like, that’s not a problem.

PETROSKI: I’m glad to hear that you’ll take that under consideration. What is the cost of this program projected?

JOHNSON: Claudia, is that something I can--

NEWBURY: Newbury, DOE. If you know an answer, I--

JOHNSON: Well, they each--I’m going to go, I don’t know the total, but I can give you a unit cost. The budgeted cost and the actual cost was pretty much the same of each one, is around a million dollars for a waste package. The pallet in the 200k range, and the drip shield I want to say is in the 800k range. Well, testing program, it's going out for bid, so I'm not going to discuss what that one is.

PETROSKI: Well, it's still relatively small potatoes compared to the whole project, and I would hope it would be given high priority for having its funding maintained.

JOHNSON: Thanks. Next question?

ARNOLD: Arnold, Board.

You talked about doing marring tests, and then measuring the effect on residual stresses just in the same vein as what Henry was talking about. My question is what is the original--I mean, what is your specification on the finish itself when it's delivered to you?

JOHNSON: I'm going to pull that number out of my head.

ARNOLD: Do you require a special polishing and--

JOHNSON: Part of the program is looking at the polishing. We've got, one of the things we're going to test is a frit versus electro polishing. I can't remember the number on what type of mill finish we want. But, it wasn't anything out of ordinary. We do want a matt gray, because that's what we have modeled for thermal on the emussivity. But, the number on the smoothness escapes me.

ARNOLD: This goes back to the corrosion issue?

JOHNSON: One issue that we're working with the lab on now is that oxide film left after the annealing. That is what they would like us to remove. They don't really have a preference. The requirement there, they're putting in what we call our postclosure parameters document, is a removal of it without any specifics. And, when we talk about a strip blasting, a concern a customer has is the incremental cost of frit blasting 11,000-and some waste packages. So, they're looking at do we really need to do this? What are the effects of that oxide layer on the long-term corrosion? But, in the prototyping program, we've added in the ability to go and say hey, on this one, we want you to frit blast it so we can see about what it takes, what kind of surface does it leave.

A lot of things have been thrown around, walnut shell, I think that's gone by the wayside because of the organics that are impinging on it, and things like that. And, electro-polishing is a little overboard for what they need, and that's kind of a spendy little process step to get a really nice finish on it. But, right now, I think the one we're going to go forward with in this prototype just to try it out is a frit blasting.

ARNOLD: All right, thanks.

GARRICK: Ron, David, and Mark?

LATANISION: I had another question, but just a follow-up on Howard’s. So, there is a plan to cut the type--cut sections and explore their corrosion resistance relative to what’s been done in laboratory samples?

JOHNSON: I’m not familiar with what the lab wants to do. They have asked us for coupons from this one to go do testing. So, that’s what I’m providing them. But, I’d have to have someone from Sandia, Neal Brown, or somebody, to say what they’re planning to do with it. I’m just saying how many do you want, and we’ll have them for you, and you can do with it what you want. So, I can’t answer your question. I can get an answer for you, but--

LATANISION: Yes, I’d like to have that, because I think it does make a difference. I mean, all of the laboratory tests have presumably been done on polished surfaces, and we’re now looking at the real world, and the real world isn’t a polished surface.

JOHNSON: Right. And, this prototype did not have that heavy oxide layer removed from the annealing process.

LATANISION: Okay. And, the question I wanted--

JOHNSON: Are these actions getting captured? Because I don’t want to--Okay. I don’t want to tell you something and not deliver.

LATANISION: Right. I couldn’t tell from your discussion whether post-weld anneal, stress relief anneal is part of a plan.

JOHNSON: The only weld on the outer corrosion barrier that’s done post-annealing is the final closure.

LATANISION: Yes.

JOHNSON: In the past, they have talked about either a laser peening or a low plasticity burnishing. I did a value engineering study. Both methods were reasonable. I think low plasticity burnishing won out. INEL is now taking that. It is going to be doing that in that mock-up to do that low plasticity burnishing. So, yes, a good point, sorry I missed that.

LATANISION: Okay, thank you.

DUQUETTE: Could I go to Slide 14, please? I just want to understand what’s happened here. This is everything in place, obviously not the waste, but everything in place, including the baskets, and it’s been totally sealed, it’s welded at this point?

JOHNSON: No, this is just the outer corrosion barrier. That’s the only one that’s annealed. The baskets and the inner stainless steel vessel are separate. They are not annealed. So, this is your Alloy 22 that’s basically, you can see the trunnion collars on it, and they weren’t machined yet, but basically, I think the only thing left to do on this was the machining of the trunnion collars to put their receiver grooves in for that.

DUQUETTE: Okay. I was confused by the fact that it said PWR absorber plate waste package.

JOHNSON: Sorry. That was the configuration, but this is just the OCB.

DUQUETTE: Right. Can you tell me something about the kind of distortion that you got, and you indicated you’re going to try to machine distortions out, so when you put the inner package--slide the inner package--

JOHNSON: That’s one method. The other is try to prevent it up front. So, we are going to look at both ways. But, yeah, I can get you exact details on what sort of distortions. Essentially, though, they were primarily in this area right here, and we thought it was induced from that thick trunnion collar on there. That’s just one of the speculations. So, we’re going to go play with it.

But, I’ll tell you what. I’ll send you the exact dimensions that it was off. It was significant enough where before the annealing, the inner package was placed, it went right inside. Afterwards, it wouldn’t fit. So, we had to machine it to get it to fit.

DUQUETTE: Okay. Duquette, Board.

Just to clarify for the record, since we are making a record, it’s 2150-F, not 2150-C.

JOHNSON: Oh, excuse me, did I say C? Yes, it was actually 2050-F.

DUQUETTE: No, you said 2050. You didn’t indicate which kind of temperature it was. But, I saw a couple of eyebrows raised because you would melt it at about 2050 C.

JOHNSON: You’re right, it is F.

GARRICK: Mark?

ABKOWITZ: Abkowitz, Board.

First of all, I wanted to share my sentiments with Dr. Petroski’s about what you’re doing here with the prototype program. I know it’s something the Board has been anxious about for some time, and it’s good to see the plans and how it’s coming into fruition.

My question had to do with the whole process as you’re getting into prototyping. And, I guess the easiest way to ask the question is what happens when one of your prototypes fails a test?

JOHNSON: That’s part of prototyping. So, we’ve learned something that didn’t work. I would say that probably the distortion on that was when we were discussing, you know, they don’t fit, how are we going to fix this, was originally thought to be a failed test. We got it fixed, but you learn from your failings as much as you do from your successes. That would be rolled right into the next one. So, we haven’t had that happen yet, but that’s kind of my answer, is, well, why did it fail, we’ve got to go figure that out. How can we prevent that failure? And, then, let’s take that information, roll it into the next one.

ABKOWITZ: Is there a formal process for corrective action when there is a prototype failure? Can you describe for me the inner workings of the Department when a prototype fails, and how--

JOHNSON: I’d be speculating.

ABKOWITZ: --it gets rectified? Because you will have failures, obviously.

JOHNSON: Sure. Sure. As part of the program, we would probably document one way to handle that would be--I don’t know if it would be captured in the CAP system, or your typical non-conformance report. Non-conformance report would be justified as, you know, accept as is, rework, that sort of thing. Your non-conformance reports are then rolled into the next spec on how you, you know, essentially just like something that came to the field and didn’t conform, you fixed it, and then what your fix is is as built into your specification or drawings or as into this case, the next prototype. Or, if it was the last one, the final prototyping spec that we will go out and procure with.

You could also handle it with a condition report system, but, you know, the NCR process is in place to capture fabricated items or constructed item, non-conformances, which this would fall under. So, that’s kind of my speculative answer of how we would handle it. Put it like any piece of equipment coming to a facility that didn’t meet its spec’ed requirements.

GARRICK: Any other questions?

(No response.)

GARRICK: Okay, thank you. Thank you very much.

JOHNSON: Thank you for your time.

GARRICK: Okay, our final speaker for today will be Kevin Coppersmith, who is a consultant to Sandia National Laboratories, and he will report on activities to update the probabilistic volcanic hazard analysis, and also give us some background information about the original PVHA. Okay?

COPPERSMITH: My talk will actually entail both, the probabilistic volcanic hazard analysis done in 1996, which continues to be the basis for the license application. I’ll call that PVHA ’96. As well as an update which is underway, and I’ll go through the process that’s being followed for both of those.

Next? In the presentation, I’m going to talk a little bit about formal expert elicitation methodologies, for those who are not familiar, the steps that are involved in any formal structured expert elicitation, in particular, drawing on the guidance that we follow in the Yucca Mountain project. Review the context in terms of the technical information available for the PVHA done in ’96, as well as information we now have for the update, and to go through the activities for the PVHA update. We are well along on that process, in fact, had a workshop that ended I think about 72 hours ago.

Next?

KADAK: Could I just ask why are you doing this again? What prompted this--

COPPERSMITH: I have a slide that will summarize that. It’s basically new information that became available subsequent to the ’96 study.

KADAK: Was there a volcanic eruption somewhere nearby, or what are we talking about?

COPPERSMITH: No, that would be more profound information.

KADAK: Okay. I want to gauge the significance of your presentation.

COPPERSMITH: Yes, it’s definitely new data, it’s aeromag data, and I’ll discuss that.

People have asked the question though on stability. It was a question that was asked I think by Leon Reiter at the workshop a few days ago, asked for long-term stability, since this was done after ten years, what will lead to stability over the next ten years, and those types of questions have been asked.

When it comes to looking at the types of guidance that we follow on this study, the PVHA, we look to a couple of documents. First is the Kotra et al. It’s a branch technical position developed by the NRC specifically for this project, provides the steps that are needed, and should be followed in a formal structured expert elicitation.

We also lean on and use the so-called SSHAC guidance study that was sponsored by NRC, EPRI and DOE, that has very similar overall steps, but some differences in the roles that experts play on the panel. And, I will talk a little bit about that.

The applications of these methodologies have been two, the probabilistic seismic hazard analysis that was completed in ’98, and the PVHA that was completed in ’96.

Next? In terms of the steps of the elicitation, this is from the NRC branch technical position, and, for those that are familiar with the decision analysis literature, these are common steps in studies of this type. We begin with a discussion and description of the objectives of the assessment. Of course, selection of experts. Decomposition, identification and decomposition of the issues into the salient elements that allow for assessment by the expert panel. A big part of the studies these days is the assembly and dissemination of a database. I’ll talk a bit about that in the PVHA.

Pre-elicitation training, to have the experts familiar with many cognitive biases and other issues related to the probability assessments. Elicitation of judgments which can occur in a private setting, interview setting. Post-elicitation feedback, which is the stage that we just completed last week, and the update, is very important, provides opportunity for the experts to understand the implications of their assessments and identify the most important issues. Aggregation, which of course is a big part of any expert study, is the combination of the assessments of the panel, and final documentation.

Going to the SSHAC guidance, most of the steps that are recommended are identical. I wanted to point out a couple of differences related to the roles of the experts themselves. This particular panel spent a lot of time dealing with the issue of the roles of experts. Do they represent individual representatives, are they representatives from the larger technical community? If so, how can a panel represent that larger community?

And, the bottom line evaluation, or bottom line recommendations in the SSHAC study was that members of a panel of this type should act as evaluators, as opposed to proponents. Proponents are a much more common scientific role that we're used to playing, that we could be a proponent or advocate of a particular technical position, we often publish that position and talk about it at professional meetings, and we look to the larger community to provide a critique and review of that position. And, we allow others to develop other positions. And, we have an opportunity then for discussion and dialogue and disagreement.

The role of an evaluator, though, on a panel of this type is to capture that range of views, to listen to proponents and advocates of different positions, and ultimately to capture that range of the community distribution, if you will, if they were to have gone through the same process.

So, this issue, and of course the technical facilitator/integrator is the terminology that SSHAC uses for those who have to or are responsible for facilitating that process. But, in the end, also for integrating the assessments across the panel.

So, this role of evaluators is ones that we have implemented in the PVHA. Ultimately when we finish, the representation of their range of uncertainty will be deemed to be representative of the larger technical community.

KADAK: Excuse me. Could I just ask the selection of experts, that’s the toughest part of this solicitation process, because as people are people, many of them have positions, especially if they’re volcanologists and seismologists. I mean, there aren’t that many of them, frankly, and I’m just wondering how you were able to find some that were not proponents of something, because they have all written papers likely, and they all have views about the frequency, consequences of these kinds of events. So, how did you create evaluators from this subset?

COPPERSMITH: Well, we do have--you're right, there aren't that many. We identified about 70 in this particular case, and we ended up with a panel of ten. Part of the charge to them, that has to be part of their overall scope, is the ability to act as an evaluator, be able to put on--

KADAK: That's the charge. Now, how did you--out of the 70--

COPPERSMITH: The selection process is asking for their colleagues, as well as them individually, if they were able to play that role. So, it is part of what we ask them to do and they are aware of that coming into this.

KADAK: And, this is a different group than the first group?

COPPERSMITH: I'll show the two panels that we, over ten years, we lost two of them, passed away, we lost two more due to retirement, and we added two to replace. So, we have presently eight on the current panel.

But, the issue of--you're right, the expert selection process is very important.

Next? In terms of what we're covering in the PVHA, I want to make it clear, if we go to the risk triplet as being these three things, we're dealing with only the first two. What can occur and how likely is it to occur? So, it’s loosely called the probability part of the igneous issue. The third is the consequences, so, it would be what are the consequences, given a volcanic or igneous feature intersects the repository, either a dike intersecting or a conduit for eruption.

So, what can occur is broken down typically into these types of things, either intrusions, volcanic dikes that would intersect the repository drifts, and, if so, those are described in terms of their dimensions, geometry, complexity, and so on, and eruptions, which basically is the intersection of a conduit, volcanic conduit with the repository tunnels. The implications there are actual eruption through the repository, and deposition of volcanic ash and other materials at a more distant site. So, these two things are what can occur in terms of nature, either the igneous eruption or the intrusion.

Next? And, looking at how likely, the volcanic experts divide their time between spatial models that deal with the relative likelihood spatially of different volcanic features occurring. This would be a relative intensity of future events. Obviously, it’s based largely on the pattern of past events, but also other information, spatial information. And, secondly, temporal models that deal with the likelihood and time of occurrence of igneous features.

Common models are homogeneous Poissonian models, but as I’ll talk about, there are other episodic models, models with memory, time volume models also come into play.

In this whole process, throughout aleatory variability and epistemic uncertainty are captured. The aleatory variability are those things that truly vary in nature and are not reducible with additional information. And, of course, uncertainties that are epistemic are those that are knowledge based, with additional information, would be reduced.

Part of the epistemic uncertainty element here, are alternative conceptual models about how the system works, different temporal models, whether or not it’s episodic or not. Those, we capture and quantify and wait, and include in the assessment. So, we are rather than considering alternative conceptual models and choosing one, we incorporate both of them into the assessment.

Next? So, let me talk about PVHA ’96. The purpose of this study was to have a probabilistic assessment of volcanic hazard at Yucca Mountain, with particular emphasis on the quantification of uncertainties. And, this was done in the ’94, ’95 time frame.

The product was the probability distribution of the annual frequency of intersection of a basaltic dike with the repository footprint. That is, and that product continues to be, the basis for the inputs to the TSPA and for the license application.

Next? These are the steps and the methodology. You can track them through with the guidance that I talked about earlier. In addition to developing data, compilations and dissemination, we also had an opportunity to go out in the field a couple of times with the panel for them to have first-hand observations, and to look at the features in the region, in Crater Flat and some of the volcanic features nearby.

Part of the process here is bringing together this group, this diverse group, but also bringing together a series of data experts, resource experts, but also proponents, and we had a wonderful time listening to and juxtaposing proponents who could offer their alternative views of exactly the same data. But, in some cases, they are different views of different datasets. For example, age dating at that time of some of the features of the Lathrop Wells volcano to the south of the site, was a hotly contested discussion at that time. It has since subsided over the ten years with additional data. But, this was an opportunity for the panel to listen to and consider the pros and cons of the different geochronologic techniques themselves, and the data that have been gathered.

Next? This is the panel. It’s a mix of

researchers, academics and some consultants.

Next? And, this is the result of the PVHA ’96 assessment. This was the product. The annual frequency of intersection, shown here as a probability mass function. You can see the individual expert assessments are shown here, with their means and medians and fit to 95th percentile, entire probability distribution across the panel here, that is used in the TSPA. The mean of that distribution is 1.6 times 10 to the minus 8. That is the annual frequency of a dike intersecting the repository.

Now, that probability distribution then goes on to its application and consequences. Given that this happens, what are the effects, and so on.

KADAK: If you took out the highest and the lowest, what would the result be?

COPPERSMITH: It’s fairly--actually, the mean estimate is fairly stable, but we are using the entire probability distribution. You can see it’s a good, you know, the spread here is three orders of magnitude, 10 to the minus 7, or 10 to the minus 10, not a surprise. This is a--when you look at the basic problem here, our basic problem of course in modeling this area is the dearth of volcanic features.

For those that have done PVHA’s in other parts of the world, and volcanic fields that have 300, 400, over a thousand centers, the uncertainties are significantly reduced. The mean hazard, of course, is significantly higher. So, the disadvantage of having very few data points is a broader spread in distribution. But, the mean estimates are lower.

Next? This gets to the question, why did you redo it? Well, three years after the PVHA was complete in ’99, the USGS did an aeromagnetic survey, and some ground magnetic data had also been gathered by the center and others working out there, and those surveys identified anomalies that existed out in the areas that were alluvium, covered by alluvium, down in the Amargosa Valley, over in Jackass Flats, and those anomalies were--their origin was not known. Some of them clearly looked like they were dike bolts or it looked like they would be magnetized bodies, presumably the salt bodies at depth. Others, not so, and at that point, the DOE and those of us involved in the PVHA, did an analysis of the potential impact that this new data would have on the PVHA ’96.

We looked at those anomalies, and assessed whether or not they would be added to the existing number of events that the experts had had, looked at the implications to the temporal models primarily, and a bit of the spatial models. And, the bottom line conclusion in that sensitivity study is they would all be a marginal effect on the mean estimate of dike intersection frequency.

That was sent to the NRC, and the NRC did a review, and they disagreed, said that basically, yes, what you’ve done would only lead to a marginal assessment. But, in fact, this information needs to be interpreted by experts, and not by the Department of Energy, but by the experts themselves, not so much because they would disagree with you, but in fact the information may lead to alternative conceptual models that you’re not able to consider, it might change the review of what’s happening from a process point of view. And the bottom line assessment is that we did not provide an adequate technical basis for the conclusions that in fact this led to an insignificant difference.

So, DOE made a regulatory commitment to complete several things: a program of field studies, they go out and gather additional geophysical data, to drill several of the anomalies and to age date those, and look at the chemistry, and to go through a process of updating the PVHA. And, that process is what we are in now. And, that will, the planned end of that actually is--it will occur in June of 2008, which happens to be about the same time the license application goes in. The PVHA ’96 is supportable and defensible and will continue to be the basis, but we’ll have the results of this study at that time.

Next? So, let me talk a bit--I think jump back one. Oh, I’m sorry, go ahead. Let me talk a little bit about the PVHA update. As I mentioned, two of the members of the original ’96 panel have passed away. Two other members have declined continued participation. They decided to take retirement seriously. And, we replaced those with two other members, and went through the process of expert selection.

Next? Let me talk a little bit about the aeromagnetic survey and drilling, because it’s all part of the same package and commitment to carry this out. A high resolution aeromagnetic survey was carried out, with resolution significantly better than the original USGS survey. Seven of the anomalies were drilled to look at, particularly looking at whether or not in fact they were related to the salt, and if so, how deep, are they buried by alluvium? The depth of burial of course can give an indication of age, various types of age dating, potassium Argonne. The focus on Argonne, Argonne and geochemical analyses were done, and obviously all of this information provides information that can be used in assessments of the age of these buried anomalies, the alignment of vents, the nature and geometry of the sources, and so on.

Next? This is just an example of the aeromagnetic, the high resolution aeromagnetic survey that was done. You can see the--this is the repository footprint up here, Yucca Mountain. You can see the sort of distinctive nature of the known sub-aerial, like Lathrop Wells Volcanic Center, Red

Cone, Black Cone over here. These are some of the anomalies, down in Amargosa Desert, G,F and H, for example, O,N,M and L over here, some anomalies up in this area. And, the drilling as well as a re-examination of the existing boreholes, was done really with these in mind. What are these anomalies? Are there buried basalts over in Jackass Flats, which was determined to be a very important assessment, and so on.

Next? This is a map that provides an overall summary of the interpretation coming out of the aeromag interpretation and the drilling, as well as age dating. And, the bottom line is that the assessments, these are shown in red, are quaternary volcanic centers that are known. Shown in the pink are those that are Pliocene in age, typically about 3.7, 3.8 million years old, and Miocene, interpreted Miocene basalts are shown in green. And, we have now some drill hole information that we can use for interpreting these older features in Jackass Flats. More information to tell us about whether or not these anomalies were in fact tuff or basalt, and this is the interpretation that comes from that.

The other advantage of the high resolution surveys is we were able to make interpretations of the faulting pattern as well, and it turns out that much of the location and the features related to these younger centers are related to the shallow pattern of faulting. And, that is, in fact, part of the assessment that is being made now.

Next? So, the issues that are being addressed in the update are these. We talked a bit about spatial evaluations before. Source zones were used previously. Now, there’s more emphasis on the concept of smoothing the locations of past events, depending on their age.

Next? Temporal evaluation. These types of assessments that range from simple homogeneous type models, to those that have either a time aspect to them or a volume aspect, or are non-homogeneous nature.

Next? And, the event definition part of it is in fact very important as well. The intrusive event geometry, what do these dikes look like, in terms of whether or not they occur singly or in groups. Also, they consider the potential impact of the repository opening itself on the localization of intrusive events.

Next? In terms of extrusive event geometry, we looked at the potential for conduits developing, how many, what are their geometries, how large would they be, all assessments that ultimately can be used in subsequent modeling. And, because of the potential change, we make these assessments for both the 10,000 year and 1 million year time period.

Next? As I mentioned previously, one of the big issues, or one of the big steps involved in one of these assessments is developing a database, a uniform database for all the experts. And, of course, over the last ten years, with computational ability, GIS, and so on, our ability to develop and deliver databases to the experts is much enhanced, and particularly having some national labs involved, like LANL, they have been able to develop some wonderful maps and other layered products for the experts. And, this is just one example of an isostatic gravity map that’s been imposed, that has--on a digital topographic base, with all of the faults identified, the interpreted aeromagnetic anomalies identified, and so on.

Next? In addition to the available information in the Yucca Mountain region proper, information was also developed at a number of analog sites throughout the southern great basin. Information that could be useful to the experts in all of these ways, in terms of helping them to understand and make assessments of future events at Yucca Mountain, we developed information at a number of analog locations.

Next? This is a listing of those analogs. We also had a field trip out to many of these sites so they could see first-hand the information of these analogs.

Next? This is an example of--one of the assessments that we asked them for is an assessment of conduit geometry at depth. Of course, the repository depth would be about here. This is an example of an older 8.8 million year conduit that’s been identified and actually is exposed in a large cliff sequence that shows the nature and geometry of the feeding conduit to this particular feature.

This type of analog, as well as there’s many others, of the type they consider in making their assessments of what would happen at the Yucca Mountain area.

Next? This is where we are in the project. May 10th and 11th, Thursday and Friday of last week, we had our feedback workshop, which provided back to the experts information that came from their interviews, their elicitations. We presented information that identified the issues, the important issues that they have to deal with in finalizing their assessments, which will go on over the next couple of months. And, then, we’ll get into final hazard calculations and aggregation of the expert assessments and documentation.

Next? I just want to show a couple of examples of the types of information that we talked about last week. This is an example. One of the experts has looked at and divided spatially the Amargosa Desert volcanic domain, he calls it, from the Yucca Mountain fault domain. Two areas that are separated in terms of the tectonic manifestation of extension, either accommodated through a dike injection and developed in the volcanic features here, or through fault displacement and extensional faulting in the faulted domain.

Next? This is an example of some of the

assessments that were made on individual characteristics, dike length in this case. Dike length is an example of what’s almost purely an aleatory variability. They expect almost all dikes--dikes of all these lengths to occur, and this is the relative frequence of occurrence of those dikes. The maximum dike length tends to be an uncertainty, tends to be an epistemic assessment. And, it is usually treated that way.

Next? This is another example of the number of conduits that might develop along a dike, and this particular expert makes it a conditional assessment on the length of the dike. The longer dikes, the larger number of conduits that are part of his assessment.

Next? This is an example of feedback. One of the issues here, I won’t get into the details of kernel smoothing, you’re probably all aware of that, but one of the assessments that needs to be made, if you are going to use a kernel smoother and smooth the locations of past events, is you need to consider the smoothing distance, or essentially the standard deviation of that kernel. And, the assessment is difficult to make without looking at the potential influence, and this is an example of two smoothing distances, a kernel of 5 kilometers, and a larger kernel that would obviously lead to a more uniform map, less topography.

What this says is that the past location of events provides more resolving power in the future location than a kernel that’s longer like this. These are assessments that are typically in a seismic hazard analysis as well.

Next? Another example of sensitivity, the expert was considering two alternative conceptual models, one that says that I will wait. The location of future events by the inverse of their age, in other words, the younger they are the more likely the future events will be near them. This is another model that says that the volume of those events is really a strong discriminator, and the larger events, the future events will be closer to the larger. And, so, there was examining the potential influence of different weights on those alternatives, either a fifty-fifty weighting or a seventy-five twenty-five weighting. In this case, not much difference in the predicted spatial intensity.

Next? And, finally, this is a case where the actual centers that are being used in the smoothing are different. Either going to use quaternary centers only as in this case, or the Pliocene only, or the combination of the two. And, you can see the implications here in terms of the predicted spatial intensity. This is the type of information, I don’t see either Bill Melson or Leon Reiter here, but they were there on Thursday and Friday, and we went through about two days of this type of evaluation.

Next? So, in summary, the methodology that’s been followed is consistent with our guidance, NRC guidance, as well as the guidance that's been developed by other groups. We're taking advantage and the PVHA update of the lessons learned and the opportunities for refinement of the basic methodology. But, the process, just like it was in '96, is structured around workshops and expert interaction. This is part of the overall elicitation process. And, we will be documenting this in fiscal year 2008 during the license application review.

Thank you.

GARRICK: Okay, thank you. Thank you very much. Any questions, please? Yes, Andy?

KADAK: Kadak.

I'm trying to understand the significance of this again. And, how much of the expert solicitation is databased or subjective, in the sense that if your best estimate is an event occurred 1.X number of--10 million years ago or 100 million years ago.

COPPERSMITH: The oldest events considered are usually about 9 million years ago. The ones considered very closely, occurred say in the last 3 million years.

KADAK: 3 million years, you had a couple of dikes occur; is that correct?

COPPERSMITH: In this area, over that time period, probably more like about 15, 10 to 15.

KADAK: 15 dikes of meters or kilometers in length; is that correct?

COPPERSMITH: What you see are evidence of a volcano itself. You see the actual cinder cone. You do not see the plumbing system in terms of the actual dikes.

KADAK: But, these anomalies are the dikes; is that right? No?

COPPERSMITH: No, the anomalies are actually the buried cinder cone.

KADAK: Buried cinder cone. So, tell me how this relates to the dike question?

COPPERSMITH: Well, all of these features presumably, they’re all basaltic, are fed by dikes. So, the question is, in our case, we are looking at fundamentally of dikes for that reason. They are the most important now. When along a dike, if it localizes down to, and you watch this process happen in real eruptions, if it localizes down to an eruptive center, that is usually the location where the cinder cone or the volcano will develop. So, dikes can be longer than the localization process. We’re asking for both. We’re asking for the dike, the location and orientation and length of dikes, but also conduits. Where will they be? What is their dimension?

Remember all this is occurring at the repository depth, where understood in the hazard at the repository horizon, which is say about 300 meters below the surface. So, the features that would exist at that depth are either going to be a dike or a conduit on a dike, and those are the two features we’re concerned about.

KADAK: So, your expectation is at some point within the million years, there will be some confluence of lava activity below the mountain that will erupt in and disrupt the storage of this stuff.

COPPERSMITH: It’s a hard question to answer. The hazard--right now, getting back to where the--what is the process right now? We know that Yucca Mountain is composed of volcanic tuff. We know that there were large caldera complexes to the north, and that process of large scale, you know, salicic volcanism occurred many years ago, say 12 million years ago. As we moved into this process--and, again, I’m not saying anything that the experts haven’t. In my real life, I work on earthquakes. I’m portraying what the experts tell me. As we move into a few million years past and starting at about 9 million years ago, we start seeing nothing but basaltic volcanism. We started seeing lava pores, and that type of thing.

As we moved into about 3.8 million years ago, we see, and still have at the surface, some of these basaltic volcanoes, and there we do see, since about 3.7 million years old, we see them dissected. The cones are gone, but we see some of the plumbing system and we see some of the dikes that gave rise to those.

If we move to the quaternary, say a million year centers, now we see nothing but the constructional features. We see the cinder cones, and so on, that are out in Crater Flat and are down at Lathrop Wells. So, the process of tectonically what’s happened is we move from large scale salicic volcanism to smaller scale basaltic volcanism. And, that change not only in its, you know, geochemical nature, but in volume, and so on, is being used by many of the experts in their temporary modeling. The have time volume models that take into account what happens over time.

KADAK: So, the answer to my question is?

COPPERSMITH: Okay, so what can happen at the repository in general will be basaltic volcanism, which basaltic would look like the types of volcanoes that we see in Crater Flat, and--

KADAK: The little cones--or, actually, big cones probably.

COPPERSMITH: Right. The little cones. I mean, the volumes of these per event, just looking at quaternary, are very small geologically.

KADAK: I mean, is this something that we really should be concerned about at Yucca Mountain, I guess is what--

COPPERSMITH: I think we have to be. The information is such that it could have a large consequence, and the probabilities are high enough that it's something that needs to be considered.

KADAK: Okay.

GARRICK: Ali, and then Bill.

MOSLEH: Mosleh, Board.

I'm trying to understand this frequency graph that you have on Slide 11. Is this--the numbers coming from the experts, do they represent some sort of calculation, computation, and, therefore, a compound event?

COPPERSMITH: This is a single event. These are definitely--

MOSLEH: Single.

COPPERSMITH: The numbers that you see here, the distribution and again these are 5th to 95th fractals or percentiles, they're entire distribution take into account their spatial and temporal models--

MOSLEH: Oh, so it's an aggregate of--

COPPERSMITH: Yes.

MOSLEH: So, a related then question is that is this, I think from what you were saying before, there is such a notion that we have in the seismic hazard, it's frequency versus magnitude.

COPPERSMITH: Exactly.

MOSLEH: And, this represents--

COPPERSMITH: This is not the same. The temporal model would be the frequency, magnitude analogy to PSH.

MOSLEH: Okay.

COPPERSMITH: PSHA then goes to the next step saying given that an earthquake happens, where does it happen. How far away is it, and the result is then a hazard curve, which is the frequency of exceeding ground motion levels. Okay, this goes to that next step also. The spatial model says the relative likelihood of it occurring at different locations, given that a volcano occurs, so it’s a conditional spatial intensity, it combines that with the temporary model in an absolute sense says how many occur, what is the rate. So, the convolution of the two is what you have here.

MOSLEH: Yes.

COPPERSMITH: Actually, I’ve got to say there is a third, which is given that an event occurs somewhere, it has to have dimensions, it has to be close enough that the dimensions of the dike would intersect the repository, because this is intersection frequency.

MOSLEH: That’s right. So, compounding events.

COPPERSMITH: So, roughly, that third part is like attenuation of law in PSHA.

MOSLEH: Okay. So, given that, how do you go from the ranges provided by the expert to do that aggregate distribution? Is that--

COPPERSMITH: We have an explicit, or goal at the beginning of this study that we will be in a position to defend equal weights. The SSHAC guidance spends a lot of time on aggregation methodology.

MOSLEH: And, you follow--

COPPERSMITH: We have people, George (inaudible), Peter Morris, and others on the panel who spent a lot of time on what we call integration in that study, and the goal from the beginning is to provide a basis for being able to defend equal weights. So, the distribution of a common database, exposure to all of the same series of proponents and advocates, a process of training so that they understand the issues related with probability and coding, and so on, in other words, we--and, the fact they're acting as evaluators, that whole process leads to ultimately our decision to equally weight. We leave open, and SSHAC guidance, the issue is left open then in fact you may be in a position where you need to provide differential weights, and we leave open that option. But, in fact, in this case, we have provided, combined them through a process of equal weighting.

MOSLEH: So, it looks like you have, you know, a set of ranges from the experts, and then you end up with the frequency distribution that you have--

COPPERSMITH: Actually, they're not ranges. They are a series of distributions, just like--

MOSLEH: Oh, is that right? Okay. Okay, And, then, you sample from them to generate--

COPPERSMITH: Exactly. It’s the combination of the two with equal weights. The ten in this case.

MURPHY: Bill Murphy, Board.

This is really interesting, and I was paying close attention to Chuck Connor’s work about the time the first elicitation was done, and so I’m real curious about what’s changed, and tell me if my impression is right. The new volcanoes that have been identified are all in the northern Amargosa Desert in the old Miocene basalts out in Jackass Flat, and ten years ago, the problem was that there was an extremely steep gradient in the probability across the repository horizon, because there were volcanoes in Crater Flat, and none to the east. And, so, it was very hard to pin the probability for the repository because it was on that very steep probability gradient.

COPPERSMITH: Well, if you notice the plots that I showed for sensitivity, we’re still on a gradient.

MURPHY: I know, and plots like Slide 29.

COPPERSMITH: Right.

MURPHY: And, that makes me wonder when I see plots like Slide 29 and the locations of the triangles, which I guess--

COPPERSMITH: Those are the--

MURPHY: I don’t see them for the Miocene basalts in

Crater Flat.

COPPERSMITH: The most important part of the new data collection part, with the analysis of the aeromag as well as the drilling information, is the absence of Pliocene or quaternary centers in Jackass Flat. That was postulated at the time of ’96, it was discussed, and perhaps that would be something that could be there. Everything has been done since shows a factor of--what’s over there on Miocene, and in most cases, you can see the distribution of Miocene events. Their volumes are much larger. In most cases, the experts give much lower weight to the spatial distribution of Miocene features in predictions of the future.

MURPHY: Okay, thank you.

GARRICK: George?

HORNBERGER: Kevin, just to follow up on that. So, my understanding is that the investigation or the aeromag and the drilling has not produced any surprises that might lead someone like me to expect that there was going to be a tremendous change in the PVHA?

COPPERSMITH: That’s right. But, if the next question is what will the answer--

HORNBERGER: No, I know better than to ask that. I know better than to ask it. I just--

COPPERSMITH: I’m asked that daily. So, I thought maybe you--

HORNBERGER: No, I phrased my question--

COPPERSMITH: I think in terms of the new data, to me, the strongest impact from what I am seeing in the elicitation we've done so far is the lowering of the number of undetected events, in other words, the potential for events that exist up there, but we just don't see any case. That's simply due to higher resolution, period, either drilling, aeromag, mapping. The other part is I think a better and maybe more sophisticated modeling of temporary aspects, homogeneous Poisson processes were believed to be a default last time. People on the panel now that have studied around the world say we see much evidence for temporally clustered activity. And, when they take those models here, they say hey, potentially we see the same thing at about a million years, about 3.7. You know, that type of modeling is much more sophisticated than it was a decade ago. Now, the effect of that, I don't know.

HORNBERGER: The other question I have there, sort of falling on what Andy was trying to get at. We have a lot of tunnels at Yucca Mountain, and they've been mapped, and all the faults. How many dikes have been counted?

COPPERSMITH: Right now, there are no dikes that have been encountered.

HORNBERGER: And, how does that enter into estimating the probability of a dike intersecting?

COPPERSMITH: Well, the youngest and closest dike that has been mapped is the Solitario Canyon dike, which sits to the north. I don’t know, one of those--go to the next slide.

HORNBERGER: And, what age is Solitario Canyon?

COPPERSMITH: That doesn’t have it. It’s about 10 million years old, it’s arguably between 10 and 11, depending on which lab. But, that is a--that was located up about right here, and that of course is subject to a lot of discussion, and was part of our field trip. And, the bottom line, though, is that features that old are rarely used to make assessments of future distribution of igneous features,

As you go back, literally as you go back, the processes change. And, so, it’s considered by all on the panel, but it has very low weight in terms of spatial distribution in the future.

GARRICK: Well, let me ask a question as a practitioner actually. I want to know if you have observed the same thing that we observed many years ago before this process was formalized as much as it is now, and that is that we discovered that this business of trying to calibrate the expert, and this goes to Andy’s question as well, is not nearly as important as understanding the evidence supporting the expert’s opinion. As you expose the expert’s evidence and supporting database, or as the various experts begin to converge on the same database, there was a convergence of opinions and this tends to confirm the Bayesian concept that given the same evidence, we’re all basically wired the same, and, therefore, we will assign the same probabilities. Did you observe this kind of phenomenon?

COPPERSMITH: Well, number one, the criteria of both that Jack has and the branch technical position has for when you carry out these types of elicitations, has to do with large uncertainties that are very significant to start with, and that are not amenable to reduction, significant reduction with new data collection. And, this is your classic case. We obviously can gather information related to location and nature of past events, but can do very little more than that. So, uncertainties are significant in this case. They have been shown to be potentially significant in the TSPA as well.

The process of convergence, SSHAC spends a lot of time on consensus. And, of course, we do not push for that process. But, what we do see is in the course of consideration of proponent views, for example, and the discussion that follows, we see quite a bit of convergence in terms of unintended differences in interpretation, in definition. Ultimately, when we get down to exactly their assessments of what event types we’re dealing with and the types of models they’ll implement, there’s still a good bit of divergence and their view of the world in terms of what is happening out here, temporally and spatially. And, I think that is reflected in this three orders of magnitude in this intersection frequency.

GARRICK: But, the idea is not to push for convergence.

COPPERSMITH: Right.

GARRICK: The idea is to come to an understanding of what the basis of their technical--

COPPERSMITH: I want to go back to that point, because I just had an argument in Switzerland a few months ago with a fellow who likes strong differential weights on the basis of calibration tests of all types. And, I take strong issue with that. In fact, in SSHAC, we spent about a year on this issue with those on the panel who were experts in this particular area.

There is a large burden that's associated with the process of education, of data dissemination, of interaction that goes along with this type of process. It's expensive and it takes a long time. If we are able to bring in experts, give them a questionnaire and get scores on almanac type questions that we could then use in the aggregation scheme, with strong differential weights, it would be an easy life. But, it isn't the way this process works.

In fact, the discussion and interaction process is just as important as any other mathematical aggregation scheme. So, we spent a lot of time on what's called the behavioral aggregation, and, in fact, they will work out and argue some of the unintended differences in the process.

GARRICK: Okay, Andy? How is your brain wired?

KADAK: My brain is wired. Slide 11 again, I think just as a follow-up to this, it appears to me that I don't see much of the science of volcanism coming out--

COPPERSMITH: You don't see any science in a plot like this. This is a calculated result.

KADAK: Okay. Now, you said you were a--now, for me, it would be really good to understand how these things actually occur. And, do we see any evidence of it occurring in this area?

COPPERSMITH: Let me just show--give you an idea what a typical model looks like for one expert.

KADAK: Okay.

COPPERSMITH: And, I can't get all the complexities for those--anyone here who was there last week, and have some sense of the nature of these models. Let's start out with a description of the spatial distribution of future events. Well, how do you do that? Well, they decide first which past events they're going to use. So, they study and provide tables and evaluations of the location and age and use of past events, quaternary events, the Pliocene events, or they consider the Miocene events. That provides the basic database, looking back and now, I'm going to need that to look forward.

The spatial distribution of future events is what we care about, not the past. They use that past in different ways. One is to say it will occur uniformly over some zone. What is that zone, what’s the tectonic basis for that zone, or other bases? In some cases, they will say this is tectonically part of this same trough, it responds structurally in the same way. Others will say this is part of an isotopic zone that tells me--its isotopic signature tells me about what’s going on at source depths, depths for the user of 60 to 80 kilometers below the surface. Others will have different reasons for their zonation.

They could then within those zones, talk about the spatial distribution. Is it uniform? Is it non-uniform? Will it follow the location of past events? They can have all those. If it’s non-uniform, they can then define a spatial smoothing operator that tells you how uniform or non-uniform it will be. They can describe that distribution.

Those all go into the relevant spatial intensity of future events. I haven’t even talked about their likelihood. So, the temporal evaluation tells us something about that. So, they sit down and say okay, is it uniform over time? Well, now, they need to study past events, study other locations around the world where they’ve looked, and apply different models, either a Poissonian model that they have their events defined, or we can have those uniform over different time frames. They can have non-uniform, episodic models that accelerate for a period of time, go for a long-term Poissonian rate, and accelerate.

KADAK: But, there’s no data.

COPPERSMITH: Yes, they have data.

KADAK: To support these things?

COPPERSMITH: They have data. They started out with the events in the region.

KADAK: Like a million years ago, they knew how these things were working on?

COPPERSMITH: These events that are defined over in many cases the Pliocene and quaternary, provide a basis for that assessment. Our chief scientist won’t--

KADAK: Well, let me finish to the point. You do seismic hazard analysis; right?

COPPERSMITH: Yes.

KADAK: Are you comfortable with numbers, 10 to the minus 8, 10 to the minus 9, 10 to the minus 11 for some event, like a seismic event?

COPPERSMITH: Right now, seismic events are different. The seismic--the maximum earthquakes on faults occur much more frequently.

KADAK: Right. And, that’s where your comfort zone is. But, are you comfortable with numbers of that order of magnitude?

COPPERSMITH: Yes, because these are not directly assessed. These are the product of a series of assessments. For example, the average recurrence rate for volcanism in this area is about 3 to 500,000 years. That’s the average rate, about a half a million years. So--

KADAK: And, where was the last one again?

COPPERSMITH: The last one was about 80,000 years ago.

KADAK: Where?

COPPERSMITH: Down at Lathrop Wells.

KADAK: And, there was like a--

COPPERSMITH: Yes, volcano.

KADAK: --Discovery Channel volcano?

COPPERSMITH: They are assumed, because all the cinder is being trucked away as we speak. So, the recurrence part of this by its nature the average recurrence rate is hundreds of thousands of years to start with. Now, those are in the more active areas. And, now, we have to deal with the probability of the fact those areas, or features there are occurring up closer to the mountain, and the probabilities get lower still.

SWIFT: This is Peter Swift from Sandia.

Do we have Slide 15? No, 16, one more. The reddish orange dots on here, Yucca Mountain is the yellow area, and the reddish orange dots, and I see three of them, one, and then--yeah, you had three of them, there’s a big one down there. There’s a little cone up at the north end, that one up there, too. Those are the actual cinder cone volcanoes you see at the land surface from the crest. Those are, unarguably, real volcanoes. The one down at the bottom, Lathrop Wells, that one there, that one is about 80,000 years old. And, the simplest possible model, geologic model for recurrence of volcanoes, would be to draw a circle with Yucca Mountain at the center, and capture those three or four red dots, determine their age, and you get an aerial frequency. That would be pretty much an uninformed, but we’d be well within the range with what the experts came up with, but it will be an uninformed model. So, we asked volcanology experts to do a better job of drawing the circle and counting the volcanoes inside it basically.

COPPERSMITH: Well put. Some of the others, just while this is up, this is before--these anomalies here, for example, showed up in the aeromagnetic survey. This one was drilled and found to be basalt, 3.8 million years old. These are model anomalies at the same depth, and interpreted to have the same age. So, now, we have information on these we can use. Likewise, this anomaly has been drilled. It’s again, another Pliocene basalt. So, these are the events in the region that are used, they are past events, but they are used to develop spatial models for future. It’s an extremely low probability.

SWIFT: I agree, but I think the components, it’s easy to show how the components lead to that, derived probability, and the largest component that drops the probability the most is the long recurrence interval. Typical recurrence interval, I don’t know if we have anyone here from the Armenian plant. We worked on a plant in Armenia, and they have, you know, recurrence intervals that are on the order of several hundred years, maybe a thousand.

GARRICK: Okay, any other questions? Board? Staff? Audience? Going, going--okay, well, thank you very much. And, I also want to--pardon? Did somebody want a question?

FITZPATRICK: If you don’t mind, I’ll be real quick. Charles Fitzpatrick from the State of Nevada.

I was just wondering with an eight person panel, and six of them with the same--that were on the ’96 panel, the first one took from ’94 to ’96, and this one has taken from 2004 to 2008. And, it looks like the expert’s work will be done in July of ’07. Couldn’t it be ready sooner than a year from now?

COPPERSMITH: The short answer is no. The reason it took longer is all the data collection, the drilling, aeromatic, and so on, that were done this time as opposed to the last time. No, we have every code that’s used has to be qualified. We have to go through a process to button this all up so that it’s completely QA.

FITZPATRICK: The second quick question was I believe I read in the first one in ’96, that the experts were strictly limited to a 10,000 year window because of the EPA standard at that time. Was any such limit provided in this exercise as far as the scope?

COPPERSMITH: We are doing both 10,000 and 1 million years on this one.

GARRICK: Okay, thank you. Thank you very much.

I also want to thank all of the presenters today. We did an excellent job of conforming to the 50 percent rule, 50 percent briefing time, and 50 percent question time. And, we got done on time, and we appreciate that very much.

And, unless somebody has an announcement or wants to make a comment, I think we are in a position to adjourn this meeting.

Not hearing any, we are adjourned. Thank you.

(Whereupon, at 5:43 p.m., the meeting was adjourned.)

CERTIFICATE

I certify that the foregoing is a correct transcript of the Spring Board Meeting of the Nuclear Waste Technical Review Board held on May 15, 2007 in Arlington, Virginia taken from the electronic recording of proceedings in the above-entitled matter.

May 28, 2007

Federal Reporting Service, Inc.
17454 East Asbury Place
Aurora, Colorado 80013
(303) 751-2777