UNITED STATES

NUCLEAR WASTE TECHNICAL REVIEW BOARD

SPRING 2014 BOARD MEETING

Wednesday

March 19, 2014

Marriott Hotel
Sandia Meeting Room
2101 Louisiana Blvd. NE
Albuquerque, NM

NWTRB BOARD MEMBERS PRESENT

Rodney C. Ewing, Ph.D., Chairman
Jean Bahr, Ph.D.
Steven M. Becker, Ph.D.
Susan L. Brantley, Ph.D.
Sue B. Clark, Ph.D.
Efi Foufoula-Georgiou, Ph.D
Gerald S. Frankel, Sc.D.
Linda K. Nozick, Ph.D.
Kenneth Lee Peddicord, Ph.D.
Paul J. Turinsky, Ph.D.
Mary Lou Zoback, Ph.D.

NWTRB EXECUTIVE STAFF

Nigel Mote, Executive Director
Karyn D. Severson, Director, External Affairs
Debra L. Dickson, Director of Administration
William D. Harrison, Systems Administrator
Linda Coultry, Program Management Analyst

NWTRB SENIOR PROFESSIONAL STAFF

Gene W. Rowe
Bret W. Leslie
Roberto T. Pabalan
Daniel S. Metlay

I N D E X

PAGE NO.

I N D E X
(Continued)

PAGE NO.

P R O C E E D I N G S

8:00 a.m.

EWING: Good morning, and welcome to the spring meeting of the Nuclear Waste Technical Review board. I’m Rod Ewing. I’m the Chairman of the NWTRB, and I’d like to start by saying how personally pleased I am to be back in New Mexico.

I first arrived in New Mexico in 1974 as an assistant profession at UNM. And, curiously enough, that was the year that, I would say, the WIPP project began in New Mexico. When I left the University of New Mexico for the University of Michigan in 1997; that was just before, if I recall correctly, the EPA certified the WIPP site for receipt of waste. And, of course, there was a little more controversy, so it wasn’t until 1999 that waste arrived at WIPP. During my time at UNM I spent twelve years on the National Research Council’s committee, which provided, I would say, continuous oversight and review of the WIPP project.

And so it’s a pleasure to be back in New Mexico, and it’s also very interesting for me to be dealing once again with the subject of salt as a repository medium.

Now, the Board meeting in New Mexico comes at a time when events at WIPP are creating even more interest than usual in nuclear issues, particularly for people who live in New Mexico. And I just want to say, as scientists and technical experts, Board members and Staff share this interest. However, it’s important to note that this meeting was scheduled and organized long before the recent events at WIPP.

The Board’s charge, as defined by the Nuclear Waste Policy Act Amendments in 1987, is to follow DOE activities related to spent nuclear fuel and high-level waste; and, of course, WIPP is a geological repository for transuranic waste.

And so our focus today is not on the incidents of recent weeks or on transuranic waste, but rather we’re focused on salt as a geologic medium for spent nuclear fuel and high-level waste. And, of course, we’re very interested to understand how the research and experience at WIPP might be applied to this new application or new possibility of an application.

The Board has organized an agenda that’s designed to inform the Board and the public of DOE’s analysis of salt as a potential repository medium for spent fuel and high-level waste. We’re meeting in Albuquerque because many of the scientists with the expertise in this topic are at Sandia or Los Alamos. And, of course, the public and the NGOs have a lot of experience and opinions on this topic, and we’re, of course, very interested in everyone’s opinion.

Now, let me help you understand the scope of the

Board's interest in these issues and give you a little bit of history on the Board. The Board, as I said before, was created in 1987 by the Nuclear Waste Policy Act Amendments. We are to focus on spent nuclear fuel and high-level waste. The Board reports its findings to Congress and the Secretary of Energy. There are eleven Board members. They are appointed by the President from a list of nominees submitted by the National Academy of Sciences.

There is a one-page handout on the table at the entrance, which describes in more detail the Board's responsibilities, and also on the back of that sheet there is a description of the Board members.

Even though we have that description, I'd like to introduce the Board members to the audience and say just a word about their backgrounds and affiliations; and I'd ask each Board member to just raise your hand so that people realize who you are.

Jean Bahr is a Professor of Geosciences at the University of Wisconsin-Madison. She is also a member of the Geological Engineering Program and is a faculty affiliate to the Nelson Institute of Environment Studies.

Steven Becker is a Professor of Community and Environmental Health in the College of Health Sciences at Old Dominion University in Norfolk, Virginia.

Susan Brantley is a Distinguished Professor of

Geosciences in the College of Earth and Mineral Sciences at Penn State, and she is also Director of the Earth and Environmental Systems Institute at Penn State and a member of the National Academy of Sciences.

Sue Clark is a Regents Distinguished Professor of Chemistry at Washington State University.

Gerald Frankel is a Professor of Material Science and Engineering and Director of the Fontana Corrosion Center at Ohio State University.

Efi Foufoula-Giorgiou is the Distinguished McKnight University Professor of Civil Engineering and Director of the National Center for Earth Surface Dynamics at the University of Minnesota.

Linda Nozick is a Professor in the School of Civil and Environmental Engineering and Director of the College Program in Systems Engineering at Cornell University.

Lee Peddicord isn’t here yet, but I think he’s on his way, traveling. He’s served as Director of the Nuclear Power Institute at Texas A&M University since 2007, and he’s a Professor of Nuclear Engineering at Texas A&M.

Paul Turinsky is a Professor of Nuclear Engineering at North Carolina State University and since 2010 has served as the Chief Scientist for the Department of Energy's Innovation Hub for Modeling and Simulation of Nuclear Reactors.

Mary Lou Zoback is a Consulting Professor in the Environmental Earth System Science Department at Stanford University. She is a seismologist and a member of the National Academy of Sciences.

And, finally, I am a Professor in Nuclear Security in the Center for International Security and Cooperation at Stanford University and also a Professor in the Department of Geological and Environmental Sciences in the School of Earth Sciences at Stanford. And I look forward to the game on Friday night with UNM. My loyalties aren't entirely divided. I have to confess I'm pulling for Stanford.

So all of the Board members serve part-time, but we have a full-time staff. They are seated at the table just against the wall. They provide not only expertise but continuity to our efforts.

Please feel free to contact Board members. When we have breaks, we want to interact with you, but also interact with the Staff. So we look forward to those discussions.

Now, let me describe today's agenda. The first presentation will be made by Bob Neill, who many of you will know as the previous or the past Director of the New Mexico Environmental Evaluation Group, or EEG, which conducted independent review and technical evaluation of WIPP over many years. Bob will provide some context for our discussions that come later by giving us a short history of the WIPP project. And most importantly from the Board’s point of view, he’ll discuss the important technical issues that had to be dealt with during the development of WIPP as a repository.

Following Bob’s presentation, Abe Van Luik, Senior Physical Scientist and Director of International Programs at the DOE Carlsbad Field Office, will present insights gained from operating a repository in salt. Abe will also talk about some of the early heater testing that was conducted at the WIPP site.

After a short break, Kris Kuhlman from Sandia National Laboratories will discuss the technical basis for the disposal of spent nuclear fuel and high-level waste in salt, followed by a presentation by Florie Caporuscio of Los Alamos National Laboratory on brine migration experimental studies for salt repositories.

After the lunch break, we’ll have two presentations on models of coupled processes, the first by Phil Stauffer from Los Alamos on coupled thermal, hydrological, and chemical processes, and the second by Guadalupe Arguello of Sandia National Laboratories on coupled thermal, hydrological, and mechanical processes.

Dave Sevougian at Sandia will then describe DOE work on performance assessment modeling of a generic salt disposal system for high-level waste.

And, finally, Frank Hansen will describe U.S. and German collaborations on research and development investigations of salt as a repository medium for spent fuel and high-level waste.

We have set time aside at the end of the morning session and at the end of the afternoon session for those of you who want to comment or ask questions on the meeting topics. If you want to comment, please add your name to the list on the table where you entered, because I'll use that list not only to recognize you but to apportion the time so that everyone has a chance to make their comments.

If you prefer to make written comments or submit other materials, those will be made part of the meeting record. Written comments and materials, along with the transcript for the meeting, will be posted on our Web site, as we always do.

I also want to warn you that during the meeting Board members will ask questions that may be taken to reflect their personal views, and these are their personal views. Board positions can be found by looking at our report that represents the consensus of the Board on important issues.

And also we have the habit, the tradition, of summarizing our comments and impressions of the day's meeting in a letter to the appropriate undersecretary in the Department of Energy, and those letters are also posted on our Web site.

Finally, housekeeping details, please mute your cell phones. I'll do my own in a moment. And when you speak, please use the microphone, identify yourself and your affiliation, because we're recording everything, and your questions and the answers will become part of the permanent record.

So those are the opening comments, and it's my pleasure to turn the microphone over to Bob Neill to start today's presentations.

NEILL: Thank you, Rod. Well, it's a pleasure to be here to welcome this distinguished Board. I'm really impressed with the credentials that all of you have in addressing this seemingly intractable problem that has faced our nation for, lo, these many years. The only other group of academics that I think would have a greater probability of success are those that attend that are teaching at Harry Potter's school, namely the Hogwarts School of Magic, where they might be able to accomplish more.

In 1978 the State of New Mexico had a number of concerns about the WIPP project. This was a proposal by DOE to dispose of transuranic waste in southeastern New Mexico, but lacked the resources to address these highly technical issues. Secretary Schlesinger of DOE said, "Tell you what we'll do. We'll offer to fund an independent technical review by the State. Do it completely yourself. There will be no interference, no approval, or what have you." And although DOE gets criticized for many things, they really should be complimented for their willingness to do this. There aren't many agencies--federal, state, or what have you--who will fund a group to look over their shoulder and come up with some recommendations.

Now, the purpose of EEG, which was set up for that express purpose, was to do a technical review of the impact of WIPP on public health and the environment. Subsequently, we noted that there had been a nuclear weapons test called the Plowshare test in that area where we were using nuclear weapons for peaceful applications. The test called Nome (phonetic) vented and released radioactivity in the area. So it's essential to conduct monitoring in the off-site non-site areas to avoid the specter of being accused of that being the source of the radioactivity.

Now, the essential elements of the EEG reviews absolutely would be objective, neither pro nor con. It was essential to be independent with no review of the work. At one point one of the governors was unhappy, thought we were being a little bit too harsh on DOE, and took steps and--at any rate, both Senator Domenici and Senator Bingaman, who we regard as EEG's patron saints, reassigned a group from state government to New Mexico Tech to enable the group to function and complete the work.

Needless to say, it's essential to have senior knowledgeable people on this. There's nothing wrong with recent June graduates, but it is helpful to have people that are knowledgeable. And the disciplines include, similar to those on the Board, geology, hydrology, engineering. But the focus is still on radiation protection, to recognize it. I think that the approach of EEG is identical to the approach of the Board, namely to be totally objective and not stack the deck either in favor of something or in opposition to it.

Now, what we did was, rather than have meetings and express our concerns to DOE, we published 90 reports and gave widespread distribution of them to both the technical community, the governor's office, the legislature, et al. It's essential to have presentations at public and professional meetings, testify at legislature. In New Mexico we have a joint committee of four members from the House, four from the Senate, a joint committee. And I don't know how many times I did testify on it.

Also, field trips. Dr. Ewing participated in a number of the field trips that we had with the NAS and the University and Agency experts, people of dissimilar views--and strongly-held views, too, I might add--and also encouraged staff for key roles in professional societies. A Dr. Jim Channell, who is with us today, was the president of the Health Physics Society of New Mexico; and after I retired the new director, Dr. Matthew Silva, hired George Anastas, who was the president of the National Health Physics Society, which is quite an endeavor.

Well, we concurred that DOE had met the standards, recommended disposal, and I think part of the success of WIPP is public confidence from our independent evaluation of the impact on public health. It's to be noted that from the get-go, the Carlsbad officials staunchly and strongly supported this project. Senator Joe Gant, who is the number two ranking person in the Senate, was really the ramrod for this. The mayor, Walter Gerrells, as well as Representative Jim Otts on the House side, were also supporters.

The governor and legislature committed to give the project a fair hearing. In fact, Secretary Schlesinger asked Governor King point-blank, "If you're really violently opposed to this project, say the word and we'll pack it up and leave tomorrow." The governor said, "No, we'll give it a fair hearing," and that was done.

When I say that we were really good and objective, it's nice to hear other people say it as well. And the Blue Ribbon Commission made the recommendation that the health and welfare interests of the people in the State of New Mexico were being protected, and their concerns were being heard and adequately addressed.

Now, just a real quickie here is that there needs to be recognition that radioactive waste disposal is not unique in exposing people to ionizing radiation. It's a beneficial tool that we're not about to abandon, and I'm speaking now for medical, nuclear, all kinds of things.

For example, on food preservation, I was on a panel for the World Health Organization. And in parts of the world where food spoils on the ground, people are literally starving to death, that you can irradiate food and increase the shelf life from a couple of days to several months. And the cost to save a life is a dollar in contrast to several million that we do for this.

Now, what is unique about--well, we all know this--that predicting naturally occurring and man-made intrusions in the distant future. When you tell your own brother what's going to happen in 10,000 years, and this is the manner in which people will dig down into this area for mineral extraction or what have you, there needs to be a little more humility on all our parts and also candor in discussing problems that have come up--you never see papers being presented where six out of the ten low-level waste sites had to be closed in the first decade of their existence--and the assurances that the standards will not change substantially.

In 1957 in the Public Health Service and also in the Pacific with Joint Task Force 7, the allowable radiation exposure during a test series was 3.9 roentgen, 3900 milliroentgen. Contrast that with numbers today that we're using of 10 millirem or 25 millirem annual exposure to people. These are somewhat dissimilar. That's 100 ergs per gram of stuff for rad; whereas, a roentgen is 83.8 ergs per gram of air. But they are roughly comparable in that concept. The cost is substantial. We know that. And there is no system to verify that what you've come up with is correct and/or incorrect. And that needs a little bit more candor in leveling with people.

And there is a demand, whether it's reasonable or not, for greater standards or concerns about waste disposal than on other environmental or public health hazards. I mean, witness cigarette smoking of 420,000 deaths a year is continuing, you know, and we accept this in our society.

Now, the most succinct summary I can come up with on WIPP, which essentially doesn't tell you the project isn't quite fair. It's a $19 billion repository for the disposal of 6.4 million curies of defense TRU waste, which includes 12.9 metric tons of plutonium-239. Note that from the very beginning in the draft impact statement in 1979, DOE included spent nuclear fuel. That was their desire to have it in there. The chair of the House committee said, "Look, I don't want NRC licensing on this defense project," and told DOE to either get the high-level waste out of there or get another committee. Well, it was deleted.

One thing to note on WIPP, in contrast to the high-level waste, the waste is highly heterogeneous in contrast. It ranges from overalls and contaminated clothing to particulate fines less than 10 microns in diameter.

I want to note that, out of the 6.4 million curies, about 1 million curies are remote-handled. And, as you know, that means you have to handle it remotely; you can't put your hands on the container. The remainder, about 80 percent, is on the actinides as well as some fission products that are present in there.

It is contained in a Type A DOT container, which is carbon steel, and it's vented. It has to be vented because of the problems associated with the potential generation of hydrogen gas in the WIPP waste from the plutonium-238 disposed, and that's why the drums are all vented. Also, that's why right now where you have TRUPACTs parked down in Carlsbad at the WIPP site, there is a limit that NRC has imposed that you either have to get rid of it down in the mine to dispose or figure out something else.

Now, the lid they have for these drums are required by DOT to stay on for a 30-inch drop test. The drums are stacked three high, so recognize that this is potentially problem. And also it's a fact that some years ago, out of the eight rooms where waste is to be disposed, there was a

300-ton roof fall came down one time and landed in there. Fortunately, no one was in the room at the time, although there was a Sandia employee there a few days a week earlier from that.

The waste at WIPP is respirable. It has not been fixed in an insoluble matrix. That issue was debated, and basically Sandia really found that you could model the stuff with the solubility of the waste and meet the standards. The standards on WIPP are probabilistic in nature where you have to show that the probability is less than 1 in 10 that less than 100 curies of plutonium-239 would be released per million curies over 10,000 years and also 1 in a thousand to 10 times that wouldn't occur.

The isolation at WIPP is fundamentally based on containment in the salt beds. There was no credit for engineered barriers taken in the modeling and the supporting evidence that EPA would certify that they met these requirements.

Now, some of the technical issues that we have addressed. From the beginning DOE had the responsibility and the authority to self-regulate the disposal of these materials. They've set up waste acceptance criteria. For example, one of them initially was to stay two miles from a deep borehole. Well, it was apparent that if you're going to have a repository, you have to have a shaft to bring workers right down into it in the immediate area. And the law requires you to have a second shaft so workers could escape if there was ever a problem. Similarly, you need holes to bring the air in and also boreholes to release the air to discharge it. So that was deleted.

The limit on respirable fines is one percent. And this is critical, because the root of exposure which is most likely is inhalation, not ingestion, and the inhalation limits are quite stringent. But how do you measure or determine that there is only one percent respirable fines? Well, it would be unacceptable for the workers to remove the lid, paw through the contents, and confirm that it’s less than one percent. So that limit was deleted. The ten-year drum longevity had to go out the window, because the drums were all considerably more than ten years.

Three of the raging geological issues that came up initially were on the significance of brine reservoirs. This is one--the brine reservoirs--we had an example of 200 feet below the waste horizon that was proposed, we found a 15-million-barrel brine reservoir. And the issue of dissolution, namely whether or not these soluble salts or the sodium chloride or calcium sulfate or the phosphates in the potash would be readily dissolved, and that was really a raging issue as to whether or not this could cause the overburden to be removed in time. And, of course, breccia pipes were--you had these cylinders of brecciated rubble stemming from the dissolution of more soluble materials coming on down.

In fact, we held meetings on this--in fact, Dr. Ewing participated in a number of them--where on each of these issues, like on dissolution, you have one proponent who is very, very concerned about the ravaging effects, and one said it wasn't that bad. Ten minutes apiece should do that, but then left a good forty minutes for discussion by the panel, by the group, of the significance of that.

Well, we redesigned the monitoring equipment in the stack; and, as I mentioned, the offsite, there never have been issues--standards developed by DOE for the 10-100 nanocuries per gram alpha emitters, which are low-level waste. And DOE has chosen to deal with those as TRU waste. You take nine drums of 50 nanocuries per gram stuff, put it in with one bona fide drum of 15,000 nanocuries per gram, the average for the 10 is greater than 100, therefore, it would qualify; and the EPA said this was acceptable.

One thing we argued with DOE on, and they succeeded, was to delete the double containment requirement for the CH-TRU shipping container. We believed it was a requirement of NRC--and I still do--that this would enable DOE to increase the payload, because you could increase the diameter of the shipping container by perhaps three-quarters of an inch, which didn't amount to much on six feet. And also you could increase the payload, because you would no longer have to have the inner containment vessel. Weight has never been a major factor in the shipment of material to WIPP.

Now, the 600-pound elephant in the room--I had mentioned this thing on the release--today, again, there was an announcement in the newspaper that there was another small release. You can see the plutonium and the americium, those two radioisotopes of 800,000 and 500,000 curies total projected. Note that the annual worker limit of 5 rem, as set initially by the ICRP, adopted by NRC and DOE and everyone, is not a dose that if a worker gets is going to be fatal or induce an injury or, you know, morbidity statistics. It's not. It is a prudent value that you get off the job and do other things.

But a note that the becquerels per year for plutonium-239 and/or americium is 370 per year. A becquerel is one disintegration per second, so the allowable exposure from plutonium-239 would be one radioactive atom a day decaying, roughly 370 per year. In microcuries it's .01, and a microcurie is one-millionth of a curie. In terms of the weight of that, it's something like .117 micrograms. You take a gram, divide it into a million pieces, and then take one-tenth of one of those. So even though it's recognized that the releases that have been announced in the paper are quite low--and indeed they appear to be--the allowable limits are also similarly low.

Now, the past work in high-level waste, which I think all of you are familiar with, is a system of screening sites by listing, rating, and comparing the favorable characteristics where you say, you know, the absence of water, the absence of mineral resources and other things, you list the criteria and come up with a list of five candidate sites, then further sharpen it by getting it down to three and then to one.

I have jokingly said at times that you don't do that in selecting a spouse where you come up with a list of desirable characteristics and narrow the field, and you tell the candidates that you don't qualify on this or that. And radioactive waste disposal is an equally serious business.

At one time we appointed a negotiator, Dave Leroy, to negotiate with states to have one volunteer. Well, Leroy was never given any authority to negotiate, to assure them of jobs or what have you. Now, in 1982 Congress required, as Dr. Ewing indicated, to evaluate the need for a second repository. And DOE said, "You know, it's a lot easier to authorize the increase in the first rather than develop the second." I believe that. I was on the DOE advisory committee on crystalline rock with Frank Parker and Susan

Wilcher-Kylee (phonetic), and we saw the enormous concern and opposition raised in the eastern part of the U.S. on crystalline rock. So even though it was easier to do that, this is only true if there was a first repository. But since there isn't a first repository, we really don't have a second.

Another thing to note is that, up until 1970, DOE had the authority to self-regulate the disposal of these transuranic wastes, which are now coming to WIPP, and dispose of them in shallow land burial in Los Alamos under three feet of ground cover.

There is, I believe, an excellent paper by Dr. Helen Neill, who is a professor at UNLV, currently an associate dean out there and also is my daughter, and I'm the junior author on this paper. But we recommend that DOE evacuate or excavate the 20,800 curies of plutonium-239 currently under three feet of topsoil and ship it down to WIPP. Now, understand that DOE is entirely, totally, and correctly on this where they have the authority to do so, and they exercise those authorities and responsibilities. And I'm not inferring in any way that it was done poorly or shabbily.

However, it's to be recognized that there is an inconsistency here when you address a group of fifth graders and say, you know, it's necessary to put this stuff 2,150 feet underground because of the hazard to people's health in the long-term future, but it's okay for this stuff that's under three feet of topsoil. As a minimum, we've urged that DOE do the modeling to calculate what fraction would be released in 10,000 years, which is the requirement for WIPP, and see whether or not it's acceptable. We do know that at other sites under three feet of topsoil, the waste has leached out from that.

Now, the status, as you know, that we've--I think the figure of 22 billion is probably low. I've been retired for a number of years now. That's gone up considerably. The efforts to date, all of them unsuccessful--and I don't argue about whose fault it is and what we should do about it, but there are some recommendation for the Board to address on here. We lost a year and a half due to jurisdictional disputes between EPA and NRC whose turf was involved. Those things should be resolved promptly.

Bear in mind that states do not regulate radioactivity, only the non-radiological constituents, be it delegation by EPA--and Jim Channell did a very good paper here, which was published, where the hazards associated with the non-rad constituents are a factor of about a thousand less than the radiological risks associated with it.

Now, the most important recommendation we've got here is that the requirement to predict a radiation dose from the particulate resuspension and inhalation over a million years is meaningless and really does not do anyone any value. I was on the National Academy of Science's committee on uranium mill tailings, and we found that emanations from the pile after about 25 years that the measured concentrations of radon daughters varied from the predicted values of the concentrations by a factor of two orders of magnitude. So if after 25 years you get this kind of stuff, imagine what it would be like for a million years. I think that the time period should be more meaningful, perhaps like the one at WIPP of 10,000 years.

Plutonium is considered to be the most hazardous radionuclide at WIPP, although there are four other radioisotopes of plutonium that are present: a 238, which generates a considerable amount of heat, which is the one associated with the problem of the generation of hydrogen; 239; 240; and then the beta-emitting plutonium 241.

Note that another argument against the million years, the inventory, according to what I looked at, the plutonium-239 on the high-level waste would be something like 25 million curies. That sounded 10-1/2 microcuries, a millionth of a curie, and that's a needlessly restrictive reduction. Note that basically radioactive decay does become innocuous with time or it becomes innocuous--the toxicity decreases, but a bucket of lead is as hazardous today as it was a million years ago.

Now, another strong recommendation is to do two sites. There are various proposed sites that have been found to be unacceptable, certainly the one up in Lyons, Kansas, right now; they wanted Yucca Mountain; the one in Deaf Smith in Texas. For various reasons we’re not pursuing those. And I think the nation can ill afford to restart the clock decades later, so we urge to take a fresh look at crystalline rock, bedded salt, basalt, and tuff. In other words, do this thing correctly, properly, and through the front door.

Now, the other problem is that the high-level waste for disposal is bigger than the authorized capacity of the first one. So common sense would dictate that if you’ve got more stuff than the first repository could take, you really ought to get looking for a second one. And this provides the nation with a double benefit. It’s a home for the second and a backup for the first. And this recommendation is one that all of our grandmothers would make, namely, don’t put all your eggs in one basket.

It’s essential to have an independent state review. I don’t want to belabor that; but of the 90 reports, 4 of them were co-authored by Thomas Sargent, who is known for his mathematical rigor. These were on calculating the potential of a catastrophic release from the hoist system at WIPP, and Dr. Sargent and Dr. Greenfield co-authored this. NRC was so impressed with the first one on this low-probability/ high-consequence event that they not only reference it in guidance, but they reprinted the report in its entirety, saying, “Hey, follow this position.”

Now, Congress set up a really good system for high-level waste disposal and then subsequently abandoned it. You know all that. One recommendation: That Congress and the administration need to agree to a system and stick with it. Some of them might say, Neill, out of all your recommendations, that one is the most absurd. But certainly Congress ought to get cracking and hold hearings to specify incentives for a state to volunteer as a candidate and look at the--you know, the BRC report came out two years and two months ago, and I’m not aware of any efforts by Congress to address those recommendations.

Don’t ask Congress to solve technical problems that you can do yourself. DOE wanted to bring waste to WIPP before meeting the EPA standards, and they said, “Well, we can conduct experiments, which would be very useful in providing confirming data for our predictions.” Well, Congress required it and agreed to this. They were without merit. We recommended they be discarded, these bin and alcove tests, and DOE had a Blue Ribbon or a Red Ribbon Committee, which also agreed, and DOE did cancel them.

The only trouble is, the law stated you had to do experiments in order to bring waste. So you had to go back to Congress and ask them to change the law. Secondly, they didn't like the EPA requirement of 10,000 years to predict the behavior. Congress asked the Academy for views. They believed 1,000,000 years to be more appropriate.

The moral is: Don't ask Congress to solve technical problems that can be readily solved by the technical community.

Also note that engineers and scientists should present papers at meetings showing the merits of disposing high-level waste in their home state. The paucity of such research--there are some examples like Tennessee was willing to consider a monitored retrievable storage facility to store high-level waste in Tennessee. But, really, there have been very few examples of where people publish reports saying that, you know, the rock formations of crystalline rock are really eminently suitable.

When I've told this to my friends in various states, they say, "Hey, Bob, what are you trying to do, get me fired?" You know, the paucity--and it's essential that the public believes in the objectivity by their technical community in order to have confidence.

This is one that's an interesting comparison. The NCRP published--I think the slide in your Viewgraph is incorrect--in 2006--for over two decades the medical radiation exposures stemming from diagnostic applications for CT scans, mammography, nuclear medicine increased by a factor of 7.3 to 900,000 Person-Sieverts. That's an indication of the population dose or insult via radiation, weighted by the population, but where the nuclear power plants went down by a factor of 5.

Now, the question comes up: Why would the public accept this astonishing increase? The reason is--the belief is that the benefits clearly outweigh the risks, and people at times focus solely on the risks and not also considering the benefits, and we need to discuss this.

And lastly is a point to note that the public acceptability of activities in defense of the country is much greater than acceptability for commercial high-level waste. That's a fact that we all recognize.

Funding, basically we've dropped 13 billion on Yucca Mountain to date, and I think there's 26 billion been collected from rate payers, probably more. A number of the utilities have sued and have won in court because of the failure to take title to the spent fuel. And so it really needs to be moved more quickly or rapidly, and the waste requiring disposal--and this is hardly news--future funding is going to get more difficult all the time.

Recommend certain things to the DOE also. Let's get on to these potential rock formations in different areas of the country; identify the incentives for states to volunteer. It may well be that Tennessee or Michigan would find very, very much in these economic distressed times to do that, get better cost estimates for it.

Also, lock in on the maximum inventory. That also will require you to address the issue of a second repository. Publish a report about the status of funds. The DOE official in charge of this at a symposium I was at in Tennessee said that the money is not there any longer; it’s an IOU in the till. It’s been spent to mask in part the deficit that we had. And we need a greater candor and directness on it.

And it’s very important to say, okay, if we don’t do something on this, what are the consequences if we continue to fail to take title? Will this stop any future power reactor from being built? Does this leave a site as a terrorist target to fly an airplane into it?

Now, one question that is in the--this 600-pound gorilla again--is this site suitable for New Mexico for disposal? We don’t know. The site has not been geologically characterized for high-level waste disposal, and Congress gave that job to EPA and NRC. It’s not an ad hoc decision.

I noticed the local newspaper last week said, “Hey, bring the high-level waste down here,” very casually without any reference to--note that the mineral resources in the area are substantial. Many of the lists for the high-level waste disposal say, “Stay away from areas like that.” The thermal loading for high-level waste is really high and the effects of that on bedded salt, and the total curies are a factor of, I think, a thousand greater than the 6.4 for WIPP.

This is an old map, 64 square miles. The area at WIPP in the center, 4 by 4, around 16 square miles, covers that, and it shows the footprint of the repository itself. But it’s ringed with either known potash reserves, mineral, gas, and oil extraction.

And also in 1975 the National Academy of Sciences recommended bedded salt for disposal of high-level waste. And that is quite true, and some people are fond of quoting that fact. However, that report also was dealing with some other things that haven’t been recognized. It was for liquid high-level waste. That is off the table. We no longer are considering it. They said, finish all your geological work before you authorize construction; locate it in an area near the power reactors to minimize transportation risks--that essentially hasn’t even been considered--and select cavities at shallow depth to reduce room collapse; and check out a large number of sites.

Well, those are the main points that I wanted to cover this morning. I think it is an admonition here that the selection of a high-level waste repository is going to be a very difficult, complex business. There are any number of cogent reasons and also imagined reasons why it is either acceptable and/or unacceptable. So the challenges that the Board has in addressing this problem are appreciated and recognized, and we thank you very much for your efforts here today to do this. Thank you.

EWING: Thank you, Bob.

So, just to remind everyone of the procedure, we'll first allow the Board members to ask questions and then Staff and then, time allowing, perhaps questions from people who are in attendance.

Okay, from the Board? Jean?

BAHR: Jean Bahr. You mentioned the issues related to brine pockets and dissolution. So as we're thinking about salt as a repository medium in general, what was learned from the WIPP experience about how do you identify the potential hazards associated with brine pockets? How do you identify where those are? Not thinking about WIPP specifically, but thinking about bedded salt in general, what have we learned about what needs to be done in site characterization and how did they resolve the questions that were associated with those risks at WIPP?

NEILL: A very good point, Dr. Bahr. Correct me if my numbers are off a little bit, but over the years I think in that area we have discovered or noted the presence of about eight to ten brine reservoirs that have been picked up in different locations in that area. We never did understand exactly how they were created, but the resolution of the problem was, if the modeling said, okay, you have a brine reservoir here, what, if any, was the effect on the performance assessment calculations or the modeling to increase the likelihood or the quantities of radioactivity released because of this?

And in that way we sort of--I don’t want to say bypass it, but address it by saying, well, how bad is this or how much of a problem would it be? But I don’t know if we have any mechanism today--certainly back in 1980--of being able to detect each and every brine reservoir in the area. But at WIPP-12 this is just a couple of hundred feet below the proposed repository horizon.

EWING: Other questions from the Board? Steve?

BECKER: Becker, Board. You mentioned that it’s important for the technical community to win the confidence of the public, to establish confidence. I’m wondering--you mentioned a couple of factors. I’m wondering if you could elaborate for us on that. What kinds of things do members of the technical community need to do? And, more broadly, what sorts of things do agencies involved with waste management need to do in order to win that public confidence?

NEILL: A very good question, but it’s a real tough one. One of the things that has to be done is to get the data out there. Today there was an announcement in the newspaper of another small release at WIPP, americium-241, no numbers. And it’s essential to get the numbers out there. I realize that they are numbers that people are not familiar with. A curie is 3.7 times 10 to the 10th disintegrations per second. A Becquerel is 1 disintegration per second. We’ve gone from this absurdly large number to absurdly small.

But it’s essential to publish and get the data--you know, in 1958 when I came back from weapons testing in Nevada and the Pacific, I was given--and there was a great concern about the numbers from fallout on St. Louis, strontium-90 in milk. And the AEC was opposed to releasing these numbers. Well, we published the results of all the measurements in air, water, milk, and other biota and got it out there to the public to see it. And that is essential.

I think that scientists and members of the community really need to publish some papers. The last one that I recall people talking about problems that we had was a symposium in the Health Physics Society back in--oh, it must have been 1978 when I first started the job, where they gave a couple of papers on how the low-level waste facilities were leaking, major problems, and there was a directness and a leveling on it. And it’s essential to provide this information to the public, that the appropriate officials really ought to be testifying before the legislature, giving the governor information on it.

And in the absence of it, when somebody says, hey, don’t worry about it, it’s a low number, that’s really not good enough for the public today. And that does provide ammunition for some people that are violently opposed to even considering this, and they say, see, they’re not leveling with you, and it’s a simple thing to resolve and to address. It doesn’t take a rocket scientist to get this stuff out there.

EWING: Other questions? Jerry?

FRANKEL: Jerry Frankel. I’d like to just follow on to Steve’s question. It sounds like the EEG should be applauded for its role in developing trust with the public over these many years. And I’m just wondering if you could hypothesize what the situation would have been had you not had strong support at various levels of government, say, if you were in a state where maybe the senators weren’t strongly supportive of the activity even though maybe the local officials. How would that have affected your experience and the effectiveness of the EEG in performing its tasks?

NEILL: A really good point. The answer really is, I don’t know. But in some ways one could say, you know, the greatest credit for WIPP going ahead was the support of the local officials in Carlsbad. Senator Gant, Representative Otts, the mayor, Walter Gerrells, really pushed the project from the get-go. I like to believe that you really--in providing this information to the public--and say, you know, that we don’t have all the answers; we’re doing the best we can; we believe in the concept of multiple barriers so that if one fails, there are others. However, in WIPP you really don’t have engineered barriers other than magnesium oxide to pick up the CO2, if you define that as an engineered barrier.

But it is a complex in a relationship. And although EEG’s role and the Board’s role is to address technical issues, the technical issues are only one part of the total. It’s economic, certainly the fact that there are 1,000 employees down in Carlsbad who may well have their jobs jeopardized if that mine is not able to reopen. So the community does have some major concerns.

And so there’s economic, there’s social, political, and it’s a composite of all of these factors. And it varies, and it certainly varies within various groups.

My daughter had a grant for looking at the Nevada test site and as an economist came up with a unique idea--not unique--but why not poll the people in the offsite communities of their views? And she found there was a bimodal distribution. There was one group in violent opposition to a proposed high-level waste repository, and there was another group roughly similar in size that said, “Okay, let’s consider it and go ahead.” You do have 200 million curies from weapons testing in Nevada there, and you have fairly complex issues that are seemingly difficult to reconcile.

For example, for a year I've seen editorials in the Vegas papers saying that, "We are violently opposed to high-level waste disposal, but keep the work force there for weapons testing if we continue with it." And it's almost as though when you produce fission products with a bomb going off, it's socially acceptable; but if you put it in a box, no way.

And so we are inconsistent, but that's really the price one has in a democracy, and it's a fair thing.

I didn't answer your question, but I'm not sure if anyone else can.

EWING: So, Bob, let me follow that question and ask it in a slightly different way and give you a chance to maybe speak a little more to this issue. So the Blue Ribbon Commission recommended a consent-based process. With your experience, how would you, moving forward, blend a consent-based process with the technical review process?

NEILL: Well, the ability to go ahead with a repository is a mixture of technical, which we've all been looking at, political, social, economic, and other factors. And that's just the way it is. I think that you do have to have the consent of the people in that community in that area to proceed on stuff, but I'm not sure that that should be the sole--in fact, it should not be the sole basis for making a decision to go ahead, where people just say, hey, we're going to be laying off everybody on the TRU waste facility when we finish up placing the rest of the TRU waste, and your real estate is going to be on the market, no jobs that potash has offered railroads. But these are things that need to be discussed and debated, argued, and explored fully amongst all the different groups that are involved.

EWING: Okay, thank you. Paul?

TURINKSY: Paul Turinsky of the Board. Could you comment on one or two areas of scientific information that you've learned from the operation of WIPP, advances from the experience?

NEILL: That we've learned? Well, we've certainly learned quite a bit about the deformation of the salt beds after you excavate a cavity. One of the problems in the room where we were doing the experiments was that the floor was coming up and the ceiling was coming down, and the bins and containers would tilt. We certainly learned that. We've certainly learned a great deal about the real estate for the disposal of TRU waste. I'm not sure how much of it is applicable for high-level waste. The numbers are so dissimilar to heat loading. It was incredibly greater, and certainly the number of curies involved has been greater.

But I have always been sort of disappointed--like in Nevada, as I said, when 200 million curies are underground as a result of nuclear weapons testing in both underground and atmospheric, and I always said this is an ideal place to model, to measure the behavior of actinides, fission products, neutron-induced activity. We never really have pushed that out there in Nevada, to my knowledge. Somebody may jump up and say there's 17 reports on that, but at least in the past they haven't been doing it.

EWING: Jean?

BAHR: Back to another technical issue that you mentioned, the roof collapse that happened early on. So what was learned about the mechanical properties of salt, and what was done in response to that in the design of WIPP, if anything?

NEILL: Good point. In fact, the NAS in '57 said, "Hey, go for shallow burial to reduce the possibility of room collapse," and that's not a viable alternative. But what has been done since then after that was to put much longer roof bolts in the roof of the rooms. Initially there were 12 feet and then 16 feet, which would provide greater stability and reduce the probability of a roof coming down. So the approach has been to do that.

Of course, the reason that salt is a candidate or desirable is that it does eventually enclose and deform. It deforms without fracturing. When you have to model the behavior of a radionuclide in a fracture of granite, it's a tricky business; whereas, in bedded salt, hopefully it deforms and forms a homogeneous matrix, which would minimize the migration of a radionuclide.

EWING: Mary Lou.

ZOBACK: To that point on the salt deformation, several of the rooms have been closed for quite some time now; is that correct?

NEILL: Yes.

ZOBACK: And do they have strain meters in the room that are being actively monitored? Is the rate of deformation occurring at what was predicted by modeling?

NEILL: I'm going to beg off that one. I retired in 2000, 14 years ago, and that would be more appropriately answered, I think, by a DOE official as to what the current behavior is.

ZOBACK: Okay.

NEILL: But we keep learning from this stuff all the time.

ZOBACK: Sure. As long as we collect data, we learn.

NEILL: Right.

ZOBACK: And publish it, too.

NEILL: Okay. And one last--oh, excuse me.

EWING: I was just going to ask if there are any questions from Staff. Okay. And you had what?

NEILL: Just one last comment. I appreciate the opportunity to give some of our perspectives. I don't know how well this has served to aid you in your deliberations, but I recognize the complexity and the difficulty of all of these issues. And all I can say is, I hope that we certainly do this properly and correctly and consider all of the alternatives for it. Thank you.

EWING: Okay, Bob, thank you very much. We appreciate it.

So the next speaker will be Abe Van Luik from Carlsbad.

VAN LUIK: It's a great pleasure to be here in front of the Board. It's a brand new Board. I only really know one person out of all of you and--well, Jean I've met before. But I do know quite a few of the Staff. And it's always been a pleasure for me to be able to address the Board. I used to work on the Yucca Mountain project, and so I met the Board quite a few times.

In fact, Bob Neill's talk was very interesting for me to listen to, because it reminded me of my own past. I started out in the nuclear business as a consultant at headquarters, reading the reports on the sub-seabed disposal program, and then it went down the tubes. And then I got on the crystalline program, and it went down the tubes. And I'm the author of several documents on site selection in the Northeast and upper Midwest of the United States from Argonne National Laboratory.

And then from there I, of course, had to survive, so I moved to the Yucca Mountain project for 24 years, and it went down the tubes. And now I've moved to the Waste Isolation Pilot Plant.

EWING: So we've found the root cause.

VAN LUIK: Anyway, it's interesting. By the way, if you go to the WIPP Web site, you will see the number for the latest disclosed release, and it's thought to be something that basically was deposited by the original release on the ductwork that has come loose and come into the filter. I think it said 62 DPM was the number found. Just prior to that there was no detection, and after that there was no detection. So it's a one-time, very small particle. One of the problems is that we know how to basically measure an atom, and it's very difficult to put these things into perspective.

But let me get on with what I'm supposed to be talking about.

This is the valiant description of WIPP, a national solution pilot. It has two meanings. It can mean something small that then becomes commercialized. It also means the ship that brings the larger ship into port, and so it's a way to steer the nation into a solution. It's currently the world's only operating deep geologic repository for permanent isolation of any type of radioactive waste. And WIPP, as you know very well, is restricted to defense transuranic waste.

In this particular picture you see what we have proposed is adding to panels here, because right now we're filling Panel 7; Panel 6 is done; all of these other panels are done; and we have Panel 8 yet to be excavated. And then we have paperwork in with EPA and the New Mexico Environment Department for permission to do panels 9A and 10A there. The experimental area is very far away from the waste area, and the experimental area is here where we have physics testing. And all of the testing has now been suspended, of course. Nobody's going underground.

So what have we learned from the disposal of our remote-handled waste that could be of some use or some insight in disposing of other waste types? What we have discovered is that our original idea, what we were doing--and I'll show pictures later--is we would first open an excavation, we would put boreholes into the walls, and we would first take the remote-handled waste, the higher activity waste packages, and insert them with remote controlled--not remote controlled--but with shielded mechanical devices into that wall and put a plug in. And then we would come in with a contact-handled waste, of which there is much more, and fill up the room after the room has already got its capacity of remote-handled waste. This is a great idea, and it worked to some extent. But the problem is, there is so much more contact-handled waste than remote-handled waste that there is actually competition for space.

The operation, it takes about 10 to 12 hours from receipt to emplacement for remote-handled waste. It's much more efficient for the contact-handled waste. The equipment size, the physical equipment, that's shielded and can be operated at a distance. It actually dictates the excavation size, and the excavation size dictates the stability of the room. So there is all kinds of issues.

The other thing is that it blocks access to the drift. You cannot be disposing of remote-handled waste in the side wall. Because that machinery takes up the whole drift, you cannot bypass it and at the same time be remote, putting contact-handled waste in the same room. So because there is pressure, much more contact-handled waste coming in, it caused a lot of boreholes to be passed over and go unused.

So what we have learned is that if we can do on-the-floor disposal in dedicated rooms for remote-handled waste and by implication other higher activity waste, it would enhance operational simplicity and efficiency.

This is the emplacement experience. You see at the top a container coming in with a shipping--this is the shipping container. Inside is a container that has the actual remote-handled waste. That container has to be rotated up to be put down to a lower level and then rotated back to put onto the facility cask that then brings it--and here you see it on that device that then shoves it into the wall and puts a plug behind it. Quite a complicated procedure.

And this is what the rooms look like. Very nice room right here with the MGO, the magnesium oxide, on top as an engineered barrier to control CO2 if there is a brine release into the repository, which is a very unlikely event. And then we have the remote-handled waste in the walls right here, and you can see this gentleman is standing right next to it, because this concrete plug is very large and basically blocks all radioactivity from that remote-handled waste package. The problem is that all of these have to be done before you can bring in the contact-handled waste.

So we think that a lesson that we learned, it's preferable to have a very basic waste handling concept, nothing so fancy as all of this equipment that's just hard to maintain and difficult to operate. We would like to have a system where you unload and transport the shielded waste in single horizontal orientation--none of this flipping it up and back--eliminate emplacement in walls or vertically in the floor; emplace it on the floor unshielded and then backfill with run-of-mine salt--that's our concept now--and accept that retrieval of thermally hot or highly radioactive waste would be possible but difficult. And I think, you know, we're talking very possible but very difficult.

So the basic mining approach is minimal mining, single pass when possible, if you can make the room so that your mining machinery can just go in and out and be done instead of right now we go in several times to basically take out the roof and then take out the floor; angled entries so that you--you know, that determines the size of your being able to make a turn with your equipment; narrow disposal rooms for stability. This would require minimum roof support, just-in-time mining. All you'd have to really pay attention to is maintaining the mains, your egress and--incoming and outgoing. And then basically you mine and emplace in the same part of the repository.

This is a picture of what we're talking about conceptually. You'd be making a new drift here while you're retreat emplacing in this drift right here. You'd have a remotely-operated vehicle that brings in the waste container, drops it--lays it down--I shouldn't say drops it. And then we bring in run-of-mine salt and cover it and then the next one and then the next one. So pretty simplistic.

This is basically showing the same thing, but showing that the angles here allow you to turn your equipment at the entry. And then we would basically be able to come in to both sides and put run-of-mine salt at the front and the back of the room. And the interesting thing here is the ventilation air flow while you're actively working this room. This would cut off most of the air flow, although there's always some. But the idea is that you would remove moisture the whole time that you're operating.

This is the same kind of thing. And we have been consulting with companies that actually make equipment that can remotely deliver rock materials. The idea is to do a low back, maximize the stand-up time.

And this is a little animation. You put in the waste containers and then blow in the salt, put in more waste containers, blow in the salt. And I'm a little impatient, so we'll move on.

So this experience of looking just at this part of the problem, other people in the program have been saying, "Why don't you think a little bit larger?" And maybe it's useful to perform an engineering trade study. Now, I can't emphasize that enough. You don't just come up with a bright idea and say, "Let's do it." You do a serious trade study, because there's always pros and cons.

But one of the serious suggestions has been to do retreat emplacement on the whole repository rather than just on a panel basis as we do it now. So that means that you make your mains at the beginning, and they shorten with time, because you can seal them up as you come back; and all the panels can be permanently sealed as they are filled. You don’t have to do this, you know, sealing the openings afterwards.

But a problem with that is that the initial extent of the excavations is larger, which means there’s an earlier larger investment before you start emplacing waste and get the payoff. And also the maintenance of those mains will--you know, it’s just a larger problem that you’re creating right up front. And then also, if you were retreat emplacing on the whole repository, the flexibility for future expansion or major design changes may be reduced.

Now, one thing that we have found and that I found very interesting in a 15-year operating repository. And if you look at the change requests that we have sent in to EPA and to New Mexico, it has been a continual reevaluation of how we do business and asking for permission to make design and other changes, because optimization is something that you can’t do ahead of time. It’s only after you start operating that you realize, oh, this could be done differently; this could be done more efficiently; this can be done safer. So you don’t want to be cavalier and say, this is my design for all time; we’re going to emplace from the back and move to the front. But this is why you need an engineering trade study to look at all the pros and cons and really think through them.

Now, another area where WIPP has given insight to the nation is, there were generic heater tests performed. They were basically performed as surrogates for the Deaf Smith County site in Texas. At that time we were looking at taking these very large spent fuel and high-level waste containers and putting them vertically into the floor of a disposal room, and so heater tests were done in in-floor borehole disposal. We found out that this invokes processes that if you do it differently they can be mitigated.

Now, a lot of these processes are because the vertical boreholes intersected. This is bedded salt, so it would have layers of clays that are water-rich. And so wherever these vertical boreholes intersected these layers, there was water inflow. When you do a vertical borehole, also it’s a steep and very localized temperature and pressure gradients, and a pressure gradient is what moved the water. It’s not particularly the heat.

In-floor borehole for large, heavy packages is physically difficult, inefficient. It actually--just like with our wall borehole disposal device, your equipment is going to determine the height and the width of the disposal rooms. If you have something that comes in horizontally, then has to be tilted vertically and put down below, it’s going to determine the height of your room. It requires heavy, complex shielded equipment to set containers upright and lower them into holes.

So this is a picture from the heater tests, one of them, one of many that was conducted at that time, and you can see the vertical boreholes. Now, the reason that you do vertical boreholes is you put a lid on it; just like with a plug, we put a plug in the boreholes in the walls now. And you can actually have waste down below and walk over the top of it. We were looking at 18 watts per square meter, which is pretty hot. We were doing also coupons, brine, and temperature monitoring.

The peak temperatures in these tests were never reached, because they were terminated rather abruptly because of the Nuclear Waste Policy Act amendments that were passed in 1986/7. They basically said, “We have found the site. Stop working everything else.” And there was forensic examination, but it was not completed. It was limited.

So here is another picture. You have the disturbed rock zone. You’re putting this container in here, and the formation pressure drives brine towards the higher porosity into disturbed rock zone. The intact salt is not really influenced, and ventilation air flow is not helping you very much, because it’s way over here on the top. So that’s the point I was making. Whether there is temperature gradient or not, there is going to be water flow because of the pressure.

So our experience that is useful, I think, to considering salt as a medium for disposal of other types of radioactive wastes has two components. Our way that we emplace the remote-handled waste in horizontal boreholes gave us direct insight into a potentially more efficient, simpler, and intrinsically safe emplacement scheme. And we have to thank the people at Savannah River for suggesting this scheme in the first place.

Our only past experimental work has yielded insights into processes that are stimulated by high heat and pressure gradients and how these gradients can be reduced. Now, the one thing that I haven't really emphasized, which I should, is if you do this horizontal emplacement and you have ventilation going across the run-of-mine salt, you're basically, with the heat and the ventilation, removing moisture and really reducing the ability of these packages to see a lot of brine.

So I've basically said this two or three times: Put it on the floor, put run-of-mine salt over it, and you'll have a good repository. Because as long as you're deep enough, the salt will close in on itself and basically remove all of the evidence, all of the fractures, and everything else that you've created through operations and construction.

So that's basically the two items that we wanted to contribute to this discussion. I have to emphasize that when we reopen the site for visits, we hope that you will come. We hope that you will ask us to take you up to the hot cell, because originally the construction included a hot cell for repackaging high-level waste if we needed to. If there was a damaged container, we didn't want to put it back out on the road and ship it.

So we have a beautifully, totally unused hot cell that is completely operational. It is kept in pristine operational condition, and people are actually trained to use it in case something in the future actually happens. If you've got it, you've got to maintain it. So will it ever be used? No. But that's not the point. But maybe we can lease it out and make some money.

I wanted to make a comment on Bob Neill's presentation, which I enjoyed very much. EEG actually, even though it was considered by some people in DOE--and I won't name names--to be a pain in the butt with their 90 documents that called into question many things, they also contributed to the way that WIPP has been operated, they contributed to the confidence of the public, and I think it was a necessary ingredient in getting WIPP buy-in at more than just a local level. And I appreciate that.

Today we have CEMERC, the Carlsbad Environmental Monitoring and Research Center, run through the New Mexico

State University at Carlsbad, that is doing the independent monitoring that was started by EEG; and it’s continued through that organization. And I think that with the recent events at WIPP, they have stood up and basically helped keep the public confidence that they do have a second opinion on what’s going on at WIPP.

I was only kidding about everything I ever touched failing, although who knows? Thank you.

EWING: Thank you, Abe.

Questions from the Board? Sue?

BRANTLEY: Sue Brantley. Thank you for that. That was great. The slides really helped envision what you’re doing.

Can you talk about lessons learned in terms of the actual deformation of the salt? Are some of the rooms actually deforming around these waste packets? And also what about the roof fall, that sort of thing? What do you know, and what have you learned in that regard?

VAN LUIK: I am not as familiar as I could be had I known that this was going to be of great interest, but I know that we have hundreds of monitoring points, and we have a model that takes daily, weekly, monthly readings on stress and strain and movement. And that model predicts from that data, keeps predicting forward, as to what’s going to happen next, so we have a very good idea.

The older excavations, you know, when you first excavate a room, the movement is rather rapid, and then it kind of slows. But it continues; it never stops. And we do monitor that. There was a question before: Do you monitor in the sealed-off closed rooms? And the answer to that is: We monitor around those rooms but not in them. And maybe that's a suggestion that could be made. But the idea is that once the room is closed, we don't care. We don't care that it collapses and closes, because we want it to. Within a hundred years they should be--basically you're looking at intact salt all around the waste packages.

Now, the other thing in my talk that I failed to mention is that you will hear later--and you will see a picture, I think, in Kris's talk--about salt basically coating one of the heaters out of the heater test. And Stauffer's talk that you're going to hear later, I think, is going to suggest that that is evidence of basically a very small-scale heat pipe where moisture is evaporating, recondensing, bringing new salt back down, evaporating again, and coating the waste packages with salt. So you don't even have to wait--if you have a hot package and you have moisture, you don't even have to wait for the room to close for that package to be encased in salt. I think this is a very interesting insight, but it remains to be seen experimentally whether this is actually the way it works.

But we monitor the heck out of the place, and our mining engineering people have a very active monitoring program, and they think that they can predict within minutes of when something needs to be bolstered up.

EWING: Other questions from the Board? Efi?

FOUFOULA: This--is this on?

EWING: Just remind Board members to identify yourselves.

FOUFOULA: Efi Foufoula, University of Minnesota. So are this data that you collect available? Are they made available to other scientists for research?

VAN LUIK: I am not sure. I know that we do occasional reports, but the raw data is a constant feed, and I don't think that we have that available to anyone except the people internally that interpret that data. But I don't know. I would have to go back and find out.

EWING: Mary Lou?

ZOBACK: Mary Lou Zoback, Board. So just getting back to--thank you for clarifying all of this. This was a really helpful talk, as was Dr. Neill's.

The rooms that have been filled and closed do not have strain deformation monitoring equipment. So even though the models say a hundred years, there should be some corporate memory. We won't know for sure in a hundred years if that's true, because there's no monitors in there.

VAN LUIK: That's true, except that our experience with

Room 1, which was kept open a lot longer than we wanted to keep it open because of decision making, this is also when we learned that we should only open a room just before we start using it. We lost the use of two or three--Panel 1--two or three rooms in that because of the closure from both the top and the bottom. And it was as predicted, but we didn't get to put waste in it, so we had to bypass several rooms and lost them.

So all of this information is incorporated into the knowledge base that basically guides our operations.

BRANTLEY: And then, I guess, just a follow-up question. You kind of quickly went through the animation. But is the run-of-the-mill salt backfilled all the way to the ceiling of the room? It wasn't clear. It looked like--

VAN LUIK: No. It was basically just a few feet of salt on top of the waste packages as shielding, and then only at the beginning and the end would you go all the way to the roof. But it's physically very difficult to really go all the way to the roof when you're just pushing salt. In fact, we have a test ongoing in that vein right now. We can get close, but as time goes on, the salt settles. But that's when the room closure comes in, and it'll seal it up.

But the idea is that there is no shine from the rooms as people go past to go to the next room.

BRANTLEY: And so is there any ventilation then in that air gap?

VAN LUIK: There would be, yes. It would continue to remove--at a very low rate it would continue to remove moisture the whole time until the repository is finally sealed up.

BRANTLEY: Thank you.

EWING: Jerry?

FRANKEL: Jerry Frankel. I found it really interesting, the comment that you made--are you hearing it?

(Pause.)

Jerry Frankel. So you made the observation that through years of experience you've come up with better approaches, and that's very sensible. We'd like to think that we will be able to engineer a process, the best process to begin with. But, of course, that's not the way things work, right? And so being the world's only operating deep geologic repository for radioactive waste, I think there may be lessons learned there about how to approach--you know, to leave open the possibility for an evolutionary type of processing.

So I guess what wasn't clear to me is that you have this new procedure for excavating rooms and emplacing waste. Is that being done now, or is that just a suggestion for a plan for the future?

VAN LUIK: This is actively being pursued as the next change we would like to make for the repository. We haven't done it yet. We are basically going around the world explaining what we have learned from the way we do things now and how it could be done more efficiently. So we would like to get permission from both EPA and the State of New Mexico to go to this new approach as soon as we can.

FRANKEL: Right. So I guess that's the question is: With that procedure, how is it going forward now? Are those authorities open to changes in the processing? How does that happen?

VAN LUIK: They are open to changes. They have been all along. But we have to prove to them--or "prove" is not the right word. We have to convince them that there is no operational or long-term safety implications that are negative. We can't back away from the degree of safety that is required. So basically we have to do a new performance assessment that accompanies this request for a change in the way that we do business, and that's actively in process.

I think another thing: We have given this type of presentation in several international venues, talking to people who are embarking on repository projects, basically shaking them a little bit, saying, "As soon as you start operating, you will see opportunities for improving your efficiency without sacrificing safety. This has been our experience, and these are the changes." In fact, one of my talks that I've given overseas lists about nine or ten smaller changes that were made because we saw that what we were doing was not optimized and that there was a better way of doing it.

And I think it's a wake-up call, because when I was on the Yucca Mountain project, we thought we could nail all this down before we start. And now I'm beginning to see, as you mention, that reality sets in and you say, "Oh, that wasn't such a great idea after all."

I have to make a comment on Bob Neill's talking about the brine reservoir. We did hit that one brine reservoir. We've looked for others since. We are very heartened that all of that deep drilling that you see around the site, which was not there when WIPP started--it's fracking that made all this possible---none of these wells have intercepted a pressurized brine pocket. They've intercepted brine but not a pressurized brine pocket.

So I think, you know, that's interesting.

EWING: Jean?

BAHR: Jean Bahr from the Board. You mentioned that the earlier heater tests were terminated prematurely before they reached the full temperature. There have been proposals to start some new heater tests. Would those be with a different configuration, or would they be vertical? And what do you think you might learn from actually getting to the temperatures that those tests were initially designed to achieve?

VAN LUIK: We proposed to EPA that they give us permission to do heater testing at WIPP. If you go to the EPA Web site, you will see a description of the two proposals that we have. One of them was a lower heat proposal for salt defense disposal investigations, is what we called that one. The other one was for a higher heat disposal scheme, which was the SDI proposal. These are available still on the EPA Web site.

The status of these programs is under review at Headquarters, and the latest that we heard was that the funding may become available in 2015, at which point there will be a lot of discussion before that time on the actual scope and what we hope to get out of these tests. And if you want to talk about that in the future, you should invite DOE, Bill Boyle, for example, to talk about it, because this comes under their purview, per the legal mandates that NE and EM have. But the proposals are there if you want to read them under the EPA Web site. Everything that we proposed to them goes on their Web site.

EWING: Mary Lou?

ZOBACK: Mary Lou Zoback, Board. Just a question about the two proposed tests. In the new tests, are you proposing that the waste be inserted horizontally rather than vertically--or the heaters?

VAN LUIK: If you go on the Web site and look at the scheme, we would do the heaters on the floor--

ZOBACK: Just on the floor.

VAN LUIK: --with run-of-mine salt on top of them. That's the proposal.

ZOBACK: Okay, good. And just for geologic curiosity, at what depth are they fracking for the shale gas?

VAN LUIK: I believe it is between 7,000 and 11,000 feet.

ZOBACK: So substantially--

VAN LUIK: Substantially below the salt. I mean, there's no oil in the salt.

ZOBACK: Right.

VAN LUIK: One of the reasons the oil and gas are there is because of the protective cap provided by the salt.

And I must say, salt is exciting. I did my Ph.D. on the Great Salt Lake, so evaporite chemistry is my bag. I'm home. This is Evaporite Chemistry 101, 202, 303. But the thing that's exciting about this is we have also done some research on the content of brine inclusions in the salt, and the DNA signatures of bacteria from one place to six feet away show that there has been no intercommunication between those two pieces of moisture in that salt since the time that the salt was laid down 250 million years ago. This is really a strong argument that salt can hold things without ever letting them go. I don't think we'll ever have a 250-million-year standard. I hope Bob's common-sense approach prevails. We'll see.

EWING: So let me follow up on the compliance period. That was going to be my questions. Perfect segue.

So Bob made his comment about the million-year standard, and you have the advantage of experience working in the Yucca Mountain project with a million-year standard, WIPP project with the 10,000-year standard. So could you describe some of the difficulties of going from 10,000 to a million years? And what would be your view on an appropriate compliance period?

VAN LUIK: Let's see, how many months am I away from retirement?

EWING: You can take the Fifth. That's allowed.

VAN LUIK: In fact, I have been on international committees on this very issue where we decided that for countries where there is no limit on the time, the implementer has to suggest a limit, that the implementer should suggest it, it should never go more than a million years. And if you look at Swiss performance assessments, it's interesting, because they will have white backgrounds up to a million years, and then they'll go to 10 million years, 10 to the 7th years, and they will have kind of a blue background and a little asterisk saying, “This becomes very speculative.” So, in my opinion, beyond 10,000 years is already very speculative.

But I like the idea at the Yucca Mountain--the original Yucca Mountain standard said, “Go to the time of peak dose and report that in your EIS as a qualitative assessment to give us an indication that there is long-term safety.” I like that approach.

Now, they were subsequently, through legal maneuvering, forced into giving us a million-year standard; and in order to accommodate their disbelief in a calculation beyond 10,000 years, they gave us a higher limit. I don’t like that approach, because it basically says you’re discounting the future. But it was their way of representing the fact that they had a lot less confidence in numbers beyond 10,000 years.

And other countries have done the same kind of thing. Some go to 25,000 years, some go to 50, some go to 100. I kind of like 10,000 with a qualitative going beyond that. But when you do a qualitative going beyond that at WIPP, you can go out to 250,000 years when the plutonium is basically gone, and it’s still the same thing. It’s totally dependent on human intrusion assumptions. So it’s an assumption-driven result, and you might as well argue the frequency of human intrusion.

I have argued and not been heard that what we should do, if there is a new repository somewhere in bedded salt--I don't care where it is--that you do preemptive drilling and remove the oil and gas beneath it, help pay for the repository. And that will not stop future human intrusion, but right now these oil companies know for a hundred years past what has been found, and that'll be the way that it is. So basically you slow down your frequency of intrusions to a very low degree, because each dry hole or a hole with miniscule resource becomes a marker for at least 200 years. So I like that idea, but nobody listens to me.

EWING: All right. Let me turn to Staff. Questions from Staff? Yes, Bret.

LESLIE: Bret Leslie, Staff. I have three questions. I'll just ask one right now, which is: Bob mentioned something that there were no engineered barriers in the compliance determination; and having lived through Yucca Mountain, you know the NRC requirements for multiple barriers. How does engineered barriers play into a potential salt repository for high-level waste?

VAN LUIK: Interesting question. Other repository programs, including the German program, working in salt consider the sealing of the boreholes to be engineered barriers. EPA has said, no, that's not an engineered barrier. You're basically restoring the permeability of the opening that you’ve created to the way that it was before you started. Your engineered barrier--it will be the magnesium oxide that you place in the repository so that when microbes degrade all of the organic materials in the waste--and don’t forget this is a mixed-waste repository as well as a transuranic repository--it’s transuranic mixed waste--that you will have enough MgO to basically absorb all the CO2 that’s created, and you will stabilize your pH. And that’s an engineered way to stabilize your pH in case of a brine intrusion so that there is a limit on the solubility of the actinides. So MgO is our engineered barrier by regulation.

Doesn’t mean that we can be sloppy about sealing boreholes, but that is the way that the regulation meets the law. It’s also the law that you have two barriers, natural and engineered. Next question?

EWING: Other questions from Staff? Gene?

ROWE: Just a quick one. Gene Rowe, Staff. I have a question on your last bullet there. Can you define hot? How hot?

VAN LUIK: When we look at the inventory of the waste currently managed and actually existing under the purview of EM, we’re looking beyond our current mandate here. This whole effort to look at a different way of doing things was spurred on by a motive within EM. What if we are asked to dispose of our own waste forms? So the hot would include the spent fuel that's in the EM inventory that's being managed right now, and that includes some pretty hot stuff. Not very much of it. Most of it is old, cold, and useless; but there is some stuff that's still quite hot. It basically falls in the mid-range of the commercial wastes, which are very hot.

ROWE: Thank you.

EWING: Bret?

LESLIE: Bret Leslie, Staff. Could you talk a little bit more about the closure of the panels? So, for instance, in Panel 6, when you say it was closed, is it no gap at all or how long--once you say you've closed a panel, is there no access to the mains?

VAN LUIK: The first few panels were closed with a very elaborate closure system. We have permission from both EPA and the State regulators to go to a--well, this is basically still in progress, but right now we have temporary closures. We have basically curtains that keep the ventilation from going in and out.

And so what we hope to do once we get permission from the State basically is to use run-of-mine salt to seal these rooms permanently, and we will shove that salt in and basically make contact with the whole system. And then there will be settling, but at the same time we'll have the roof coming down to compress that salt.

But this is all still in the works. This is another one of those optimizations that we looked at, because the original plan was for a very robust concrete barrier that we see now, one, would not have been any more effective and, two, would have been very, very difficult and expensive to put in place.

So these are some of the changes that, as you go along, you say, well, what we said at the beginning--the point is, it’s not necessary from a safety point of view. What we were thinking might be the consequences of putting waste in these rooms, through monitoring we have shown are not the consequences. There’s miniscule amounts of volatile organic compounds coming out in the air out of these rooms, miniscule.

EWING: Nigel?

MOTE: Nigel Mote, Board. Could you say something more about magnesium oxide as an engineered barrier? If I saw apparently from the picture, there’s a bag on top of the top drum; and over time the salt will close in around all of the materials in the panel. But you said--and I haven’t heard this before--that microbe communities that are six feet apart had no communication for 250 million years. So if the salt closes around the drums in the panel like that and the first perforation of a drum is at the bottom, how does the magnesium oxide work in correcting the pH?

VAN LUIK: I wish I had the illustration that we used to show that. When the roof comes down or the floor lifts up, the first contact will be with the MgO sacks, which will break, and the granular compound, the MgO, will then fall in between the waste packages. And so it'll be distributed as the roof is collapsing and the floor is coming and the sides are coming in. And then this is only effective if there is a brine flow from a human intrusion event. When that brine comes in and begins to dissolve waste, the pH will be controlled, because the MgO in the solution will absorb all the CO2 that has been basically created by the microbial activity.

This is all very conservative. I mean, in my own mind, I can't see this ever being invoked. But at the same time, we have a requirement, we meet it.

But I hope I answered your question that it'll be distributed as the roof collapses, as the roof comes in.

MOTE: Was extensive modeling done of how the--I take it the magnesium oxide was granular.

VAN LUIK: Yes, yes, it's a pretty fine granular material.

MOTE: So is there confidence it'll be distributed in a way that does allow that sort of performance every time?

VAN LUIK: Yes. But if you're asking me if there has been detailed modeling of exactly where it falls and how it's distributed, the idea is that the entire room would be the place where the CO_2 builds up; and when the brine comes in, it would mix with the CO_2, and it would find the MgO in many places. And so it's a room effect. It's not just a local effect per package. It's an effect that spreads across the whole room.

So we think that it's a very conservative approach that we're using right now. And, in fact, we have cut back on how much MgO we put in, because we were using way too much in the past. But now we're trying to match it that it's 1.2 times the potential CO_2 build-up from whatever organic materials are in the waste, and we actually characterize that and evaluate it on a shipment-by-shipment basis.

MOTE: Okay, thanks.

EWING: Sue?

BRANTLEY: Sue Brantley, Board. Can you just talk a little bit about what the observations are in terms of the distribution of brine? Is the amount of brine the same in all the different panels and all the rooms, or does it vary from one spot to another? What can you say about that?

VAN LUIK: This is bedded salt. It's not domal salt where it's been squeezed and basically purified by the geologic processes, and so we have a certain content of brine in the salt, which is pretty constant. But wherever we have interbeds with clays, there is much more moisture there. And, like I said, when they did the vertical borehole tests for heaters, wherever they intercepted one of those lenses of clay is where they had a lot of brine in-flow from the disturbed rock zone. So if you stay away from the clay layers, you have basically a very predictable amount of moisture. If you intercept one of those clay layers, then you have a less predictable amount of moisture coming in.

But if you go--you need to go into WIPP and look at this yourself, because you will see that when you have a fresh excavation, you immediately create a very large pressure gradient that takes water from the salt in the disturbed zone that you've just created and brings it into the one-atmosphere-of-pressure regime; and you will see on the walls that there are stripes of salt where moisture has come in and evaporated and left the salt behind. You need to see that for yourself to get an appreciation of it.

And then that stops. It doesn't continue, because it's only the disturbed rock zone that contributes moisture. When you get a little bit further out, moisture has a heck of a time moving through that salt, because, like I said, the salt that we have sampled shows that there is no communication even from here to there.

BRANTLEY: And can you say a little bit more about this idea that there is no communication? I mean, you mentioned it before, but, I mean, what is it based on? I mean, you're assuming that the DNA you're finding was there for 250 million years?

VAN LUIK: Oh, yeah, there is no question about that. And, in fact, we have DNA characterization--and this is not my area, so I'm just going off what I have heard in presentations--and then looked at salt-loving bacteria and Archaea today and seen that the DNA basically matches. Now, if you have evolved to live in an environment like that and that environment never changes, I guess you'll never change. That's the way evolution works.

But the idea is that we can link the bacteria from 250 million years ago to what we see today in similar environments. Not that they are totally identical, but their DNA shows that they are very closely related to what we see today.

BRANTLEY: Wouldn't that argue that the bacteria of the DNA could be from much more recent bacteria?

VAN LUIK: No. You need to visit WIPP and get your own sample with a fluid inclusion in a crystal. That fluid inclusion hasn't gone anywhere. It's part of the original Permian Sea that basically when the seawater evaporated became enclosed by the precipitating salts around it. And we need to find you a nice little rock sample, because once it's in there, it is saturated. It's not going to move, because it doesn't dissolve the walls around it, because it's saturated. There is some interstitial water that's inside the actual crystal. There is some intercrystalline water that moves, especially when you create a huge pressure gradient by drilling through the salt.

But I think the evidence--and other people who actually do this work need to talk to you about this. But they have convinced me that there is no movement of water once that salt bed has been laid down unless you have tectonic movements that squeeze that salt and actually purify it. If you go to a domal salt, it has about ten percent of the moisture that we have at WIPP. We have a much wetter environment than the domal salts that are being used in Germany.

And when I said WIPP is the only operating repository, it’s only because Morsleben is closed, because the Germans had two operating repositories, Asse and Morsleben.

EWING: Ewing, Board. I think that’s the first time I have remembered to identify myself.

Going back to the MgO question, it’s very interesting to me the limited number of barriers, actually, in a salt repository, particularly at WIPP. So the MgO story is, from a chemical point of view, a little bit complicated, right?

VAN LUIK: Yes.

EWING: It’s not so obvious that it would work as described. So if it didn’t work as planned, what is the impact on the safety assessment?

VAN LUIK: The impact on the safety assessment--and don’t forget, the only way to get anything out of WIPP is human intrusion.

EWING: Right, right.

VAN LUIK: Human intrusion that allows brine to flow into the repository, which in itself is a low-probability event. If there was no MgO, we would have less control on the solubility. The pH could vary and fluctuate. And, in fact, there are acid brines that would be carrying more; and so you would have higher releases through the pathways that are assumed to exist in order to address the regulations.

The regulations make us assume that the brine in the aquifer or the moist section of rock above the repository is actually an aquifer that can be pumped and used and drank, which is ludicrous, but at the same that’s the way that you simplify the situation so that you can do a calculation.

If you dilute this brine, you--I must say, the National Academy of Sciences, I think, in 2002 looked at WIPP, basically took a relook at it, and made recommendations for optimizations. And one of the things that they observed was, we’re not sure that this MgO is really necessary, but EPA is the regulator. EPA says you will have this as a barrier; therefore, we have it as a barrier. Some of us are not that convinced that it’s that important, mainly because of the speculative nature of the scenario that invokes it in the first place, but the point is, you do what the regulator says, and you meet your regulation.

EWING: Right. You’ve described a probabilistic risk assessment. I understand that. But the wide range in probabilities in even the conservatisms can’t be used to explain away a barrier that’s part of the analysis, right? So the assumption is that it works as described. And I’m just curious, how important is it that it works in terms of the calculated dose from the analysis?

VAN LUIK: I don’t know the answer to that question. I think there were sensitivity studies done on that exact thing, but I’m just not familiar with that work at this point.

EWING: Okay, thank you.

Other questions? Jean?

BRANTLEY: You mentioned that there is monitoring of the areas around the rooms to look for deformation and strength. Are those data accessible from the surface, or do they have to be retrieved underground? I guess I’m wondering if anything has shown up on that monitoring network that might inform the recent incidents.

VAN LUIK: We also do seismic monitoring, and we were listening very carefully to see if anything with these incidents showed up on the seismic monitor and then told that the answer is no. We have two types of monitoring. One is manual, and the other one is automated. The automated is continuously fed to the surface. The manual is weekly and almost daily inspections and measurements on strain gauges and other things, and that information then is input to the data system. But I must confess that I am not that conversant with those particular data archives.

EWING: Okay. Abe, I want to thank you for your presentation, but also thank you for indulging us through so many questions. This has been very helpful.

If you want to speak in the public comment period at the end of the morning session, please sign up at the table at the door where you entered.

And also, I was remiss in not mentioning Wendell Wert. Wendell, will you stand? Wendell was the Chief Scientist for the WIPP Project for decades. I’ve argued with him over many points, and I would say it was always a pleasure, and he deserves a lot of credit for navigating WIPP to its opening. I wanted to be sure that people who don’t know him recognize him and I wanted to thank him for being here.

So we’ll end this session now, and resume at ten after the hour.

EWING: The next speaker this morning is Kristopher Kuhlman, who will be speaking to us about issues related to disposal of high-level waste and spent nuclear fuel in salt.

KUHLMAN: Thank you very much. It's an honor to present here to the Board and to the audience. What I am going to be presenting is essentially a kind of a fast-paced history of high-level-waste-related testing in salt.

So what have we learned over 50 years? And for some of you that are familiar with this, this will be a trip down memory lane. Hopefully, for at least a few of you, there will be some new things you'll learn here about what's already been done, because if you don't understand history, you're doomed to repeat it. So let's hope we don't reinvent the wheel too many times.

So the title of my presentation talks about a technical basis. What exactly is a technical basis? A technical basis is kind of the embodiment of our cumulative understanding about a topic, and it's really achieved through an iterative process where you basically--you go out and you say, "I understand what's going on here physically. I understand the processes." Then from your understanding you try and develop some sort of models.

Then the key here, which is kind of the point of my talk, is you parameterize and validate all your models with data. You collect data. You say, "Do the models that we've developed have any relation to reality?" Then you make the next step and you quantify limitations and uncertainties in these models.

But it’s a very iterative process. As you get data, you understand, oh, that wasn’t a very good assumption. And so you go back and you redevelop and you collect more data. And so this process is how you derive a technical basis. And the Step 3 here is--essentially what I’m going to be talking about is the collection of data from both laboratory and mostly in situ tests, which we then use to validate our understanding and assumptions of the system. So, really, it’ll be a trip talking about all the highlights in roughly chronological order of the high-level-waste-related testing in salt.

And on each slide I try and give a little nugget of what was learned or how did this test specifically or this testing program contribute to the technical basis which we have now for salt. And then in the last slide I’ll kind of sum up and hopefully mention a little bit of maybe what remains.

You’re not expected to read all this. This is just supposed to be impressive, but, gosh, there’s been a lot of tests. The bottom axis here is time, and you can see that testing started back in the late 1950s, early ’60s, in Kansas with Project Salt Vault, moved into Avery Island, a lot of tests in southeastern New Mexico associated with WIPP. There were tests in France at the Amelie mine, and there were a lot of tests in Germany through the years in the Asse and other places. But this is just to give you an impression of--this is kind of what the rest of the talk is going to be about. It’s discussing in general these tests.

Starting back at the early history, the University of Texas was doing laboratory testing on salt creep. During the meeting of the--it met from 1955 to 1957, the National Academy of Sciences Panel, which was already alluded to a couple times. And their main recommendation, as was already stated, was for the disposal of liquid reprocessing waste directly into salt domes. And this diagram on the right here is actually a cover page out of one of their reports. And they show that there would be a nuclear power plant co-located with a reprocessing facility on top of a salt dome, and then you would just go ahead and inject it right down into the ground. This was in 1957, and they were like, okay, problem solved; let’s move on. And we all know that it’s much more complicated than that.

But this still basically describes the processes. I mean, they were understood back in 1957 from that National Academy of Sciences Panel that we have radiation effects; we have chemical solubility effects, thermal effects, permeability of the salt; we understand the time effect and stress. It’s a simplified cartoon, but it really captures a lot of the essence of the problems we still deal with.

And the University of Texas did some extensive testing where they did creep tests in the laboratory. They actually made salt cores and cut little cavities into them and then squished them on a testing apparatus to see how a miniature little repository would get squished. They did some of the first permeability testing in salt where they tested helium, brine, and kerosene flow through the salt. And they were really--this is still the reference from the late ’50s where people point to that salt crystals themselves are impermeable. They actually took a single crystal of salt and showed that nothing really moves through it. It has to move between the grains.

And they did some closure tests, and their salt for their laboratory tests came from the Grand Saline Salt Mine near Dallas, Texas, and that’s a salt dome. And that’s why they were able to do creep tests up to 400 degrees C.

And these early tests, aside from historical reasons, are interesting because a lot of the early geomechanical tests where the theory was originally developed--this report by Serata and Gloyna--Shosei Serata is a famous rock mechanics guy, and I think he might have been a post-doc when he did this. So this was kind of where a lot of these tests--they came up with analytical solutions for salt, elastic, and plastic behavior. Obviously these have all been defined, but this is where a lot of the groundwork was laid.

In affiliation with the University of Texas, Oak Ridge National Lab was doing tests in Hutchinson, Kansas, and this is in bedded salt. And they were operating still under the assumption that we would be doing liquid reprocessing waste into a salt dome, and so they were doing tests using PUREX, which is a type of reprocessing--PUREX is a process for reprocessing, so it's a type of waste. It's radioactive isotopes in, I think, nitric acid.

So they built several tests where you excavate a pit, and then you fill it with acidic radioactive waste, and you heat it with a heater. And they did tests in a small scale, medium scale, and then they did a large-scale test here--you see this black and white picture in the bottom right--where they had to build this rather complicated system. You see a cutaway drawing here of what's going on in the photo down below where they had to build a complicated lid system to capture the off-gas that was created from basically boiling liquid radioactive waste. And it was a rather complicated system, and they monitored creep closure in the room. They monitored an extensive amount of solid, which was precipitated in the cavities, and the corrosion. They put coupons in and monitored those, and they looked at gas generation. And they found that liquid disposal is really infeasible due to cavity stability and gas generation issues.

And so this really wrapped up the liquid testing in salt, and so we have since really moved on to, let's just dispose of solidified waste. But it was all due to this early testing in the late '50s.

And Project Salt Vault, which many of you might be aware of, was--the actual title of it discussed the disposal of solid high-level waste, because they realized we're not actually sure how we're going to solidify it, but let's assume it's solidified, and let's deal with that because of all the complications that arose due to liquid waste.

And Project Salt Vault was in a different mine but nearby in Lyons, Kansas, and they did a demonstration where they were actually disposing of fabricated radioactive waste and bringing it in from Idaho, bringing it down the way they thought they were going to bring it down, trucking it in on the trucks. It was actually a demonstration. And then they took the waste out and took it back. So it was trying to show that every step is possible.

A large number of tests were done there. I'm going to highlight just a few. An important one was the hot borehole test done at the very beginning where they took two boreholes, one horizontally and one vertically, and they put heaters in them and heated them with--they were 5-kilowatt heaters, which is pretty hot. And they heated this bedded salt up to 350 degrees C, and there was a huge explosion basically, and all the salt decrepitated into the borehole, and brine was released, and they were like, whoa, this is bad. And that was basically where the recommendation came from: We should never let a salt repository get above 200 degrees C, because, wow, this is bad.

So this test right here in 1962 was kind of the test that that recommendation was based upon. And so then the rest of Project Salt Vault was designed to--this test was, you could say, kind of pre-Project Salt Vault. And so these other Project Salt Vault tests were--they did these series of three tests that were seven boreholes, like this pattern over here, and they actually put radioactive sources with heaters so that between the radioactive source and the heater, it was about 10.5 kilowatts for this array.

They changed out the sources as they decayed, so there was a fair amount of--it was kind of process testing, you know, is this a feasible way to handle waste. And they collected brine inflow into these boreholes. They looked at creep closure. They looked at lots of things. Here is an example. This graph down in the lower right shows brine collection in these boreholes, and there was a fair amount of brine collected.

They also did pillar creep tests where they took a pillar between two rooms and put 22 heaters totaling 33 kilowatts around the base of it to try and make it creep faster. And they actually developed some mathematical models to describe that, and they were able to validate them.

And there was a lot learned. A lot of our technical basis really comes from Project Salt Vault or originally came from there. For example, that there is a significant brine flow can happen from the non-salt layers in bedded salt, and that decrepitation can really be an issue in bedded salt, and that brine inclusions will tend to migrate towards heaters. And these are the small brine bubbles, basically, inside the salt crystals that Abe talked about. In the laboratory they found that if they heated them on one side, they could get them to migrate towards the heater source.

And this was all without or with very limited numerical calculation capabilities. This was all done in the ’60s, so it was--you know, everything was analytical solutions, and it was what we consider now to be very simplified analysis. But, really, they collected a lot of the data and did a lot of the initial work that is the foundation for what we do now.

Fast-forward to the late 1970s at Avery Island in Louisiana. There were several tests done there. I’m going to talk about two of them. One of them was a long-term heater test done called Site C. It was a set of--you see here is a photo. It’s a central heater and then a ring of guard heaters around it into the floor. Actually ran it for five years uninterrupted. And they were doing salt permeability testing using gas flow measurements, and they were estimating the thermal conductivity of the salt in the backfill.

And you can see here is a radial cross section through the borehole. You can see here is the heater, and you can see the temperature contours at the--I think this is near the end of heating.

One thing they learned, though. They drilled boreholes at different distances away from a steady-state heating test, and they found that the salt permeability to gas decreased by a factor of 10,000 during heating. Essentially, there is a disturbed rock zone that develops when you mine the room out; but then when you heat it up, it tends to close back up, because the creep is accelerated by heat, and also the thermal expansion of the individual salt crystals tends to plug up all the holes which have opened. And so here you can see that as the borehole temperature goes up, the permeability drops quite a bit. And so you can see that the heating actually kind of heals the salt or it speeds the healing of the salt.

Some more Avery Island tests that were done. They did an extensive brine migration test where they drilled a series of boreholes, heated and unheated tests that were very similar, and one that actually involved using deuterated water. And they monitored brine inflow into these boreholes. This is domal salt here; this is not bedded salt. And you can see that even the unheated site did have brine flow, but the heated site had more brine flow. And they found that-- you can see they turned off the heater here, and then a large amount of brine flowed in actually the next day, and about equal amount of brine flowed in the two days after heating, as compared to the rest of the test.

And they actually did an interesting thing in one of the heated sites where they actually took gas permeability measurements around the heater in the days following turning off the heater. So as they're stepping down the heater power, they're doing gas permeability tests; and they're finding that as the heater power is going down, the permeability of the salt is going back up. So the healing that occurred during the thermal expansion and the creep of the salt is kind of reversed because of the tensile fracturing of the salt. So as it's cooling, the salt grains are kind of shrinking and pulling apart from each other. And this is then accompanied by an increase in moisture collection. So this bump right here corresponds to those points up there.

And this is a very, very interesting test that shows the thermal effects on salt. And typically this cool-down period, you would say, would not happen maybe in an actual repository, because the cool-down period is going to be over hundreds of years probably during the radioactive decay of the salt.

The deuterated water test was interesting. They actually introduced deuterated water into the test, and they were looking--they initially designed the test to observe the effects of brine inclusion migration. But then they found that the deuterated water actually diffused away from the borehole rather than just migrating towards the heat source, as they initially thought.

So they learned from this test that basically brine inclusion flow, the tiny bubbles in the crystals, is not really significant, that the salt behaves more like a porous medium, and that permeability increase at cooling is really what allows this brine to flow, because they have these two datasets, and it’s pretty clear that one is causing the other then.

Switching now to a little bit later but in Albuquerque, these are a few Sandia Laboratory tests that were conducted pre-WIPP. One is kind of a large-scale laboratory test. It’s called the Salt Block. And they heated a one-meter salt cylinder, so it weighed up 1700 kilograms; it’s rather large. They actually heated it and cooled it in steps. And so the red curve here shows the thermal history, and the blue curve shows the brine inflow history. And you can see they heated it with a central heater. This is a cross section through the cylinder. And they monitored temperature and brine inflow.

And the thing that they found from this, you see that every temperature change both up and down corresponded to a brine inflow. And the largest spike in brine flow right here actually occurred when they stepped the heater power down.

So they found from this test, really, that the thermal response is pretty simple. They were able to model that quite easily. But the brine flow really required a new conceptual model, because the conceptual model at the time really was incapable of recreating these short spikes that then decayed away, because the brine inclusion migration model would show a slow, gradual increase after each one of these steps rather than a spike that decays away. So this was known before, but this laboratory test gave us a really good dataset to really prove that it wasn’t some complication due to field data that we weren’t able to explain the data.

Another important lab test that was done at Sandia a little bit later called the Salt Cracker test where they heated two smaller cylinders of salt but to 200 and 300 degrees C. And they looked at brine release due to both decrepitation, which is due to the inclusions actually shattering because they get so hot, and due to cooldown and then also at steps in heater power.

So you could see here--sorry these graphs are so complicated, but I've color-coded the lines so maybe you can understand them. The red line shows temperature going up during heating and the temperature going down during cooling. And then the blue line shows brine inflow. And you can see that when the temperature reaches the decrepitation temperature, there is a huge amount of brine that flows in, and that's these brine inclusions suddenly becoming available. The brine inclusions have shattered, and they've flowed into the--now they're flowing through the salt like a porous medium.

And then the green are acoustic emissions, so they actually put a little microphone next to the salt, and they could hear it shattering. And this was then used later--this was a little test to kind of test this hypothesis, like, can we hear the salt fracturing. And they learned from this that the acoustic emissions really do reveal salt microfracturing and that brine release at cooling happens even after the salt has been decrepitated. So the brine release at cooling is a porous medium effect, and it's because the grains are shrinking and you now have increased the porosity and increased the permeability, while decrepitation is the release of intragranular porosity to intergranular porosity.

There were a few more tests that were done at an in situ site but not at WIPP in preparation for WIPP, and these were done at a potash mine in Carlsbad in the early ’80s before the first WIPP shaft was drilled, which couldn’t be drilled until the environmental impact statement was finished.

So there was some early waste package material testing, some heater and--here you see Marty Molecke looking fashionable with a heater with some coupons attached to it. Here’s a drawing of it. You can see they put that down in the borehole, backfilled with salt, poured some brine in there, heated it up, and then looked at the borehole closure, brine inflow, how the brine affected the coupons, all of these things. It was really a dry run for WIPP, and they learned a lot of the difficulties of working underground in an actual in situ environment. And this was an actual working mine, too, so there are miners going by mining potash.

Now skipping to the Waste Isolation Pilot Plant, which obviously you’ve heard quite a bit about from Abe. But what I’m going to focus on is the North Experimental Area here in red at the top where there were three primary defense high-level waste test programs that were conducted. And they were really conducted for a future Deaf Smith high-level waste salt site, because at the time the tests were being designed, WIPP had already been chosen. No high-level waste would come to it, but it’s such a good site, and it’s available already, so let’s go forward with these tests.

So there were the thermal/structural interaction tests, which were kind of the key tests that most people have heard of and seen pictures of. There were also waste package performance tests, which they looked at corrosion and backfill materials, and then a plugging and sealing program. And these three kind of major programs are what were related to the defense high-level waste. There was actually quite a bit of non-defense high-level waste programs that were more related to WIPP itself. They did a lot of TRU tests for TRU waste that involved--and brine flow tests in Room Q. They did lots of tests that were not related to defense high-level waste that are also famous, but I’m not going to talk about those here.

So starting off with the TSI test, the thermal/ structural interactions test, in Rooms A and B, Rooms A, which are these three over here in this zoom, they were design rooms. The center room was kind of chosen to be just like the design where you’d have two rows of boreholes in the floor, and it was supposed to be the designed thermal load of what the waste was expected to be, about 470-watt heaters. And Room B, which was similar to one of these rooms but isolated by itself, was a test where they put a lot more heat into a single room to, you know, what happens under kind of less ideal conditions.

So you can see that between Rooms A1, 2, and 3, if you total up all the heaters, there were about 64 kilowatts of heaters running. That’s a lot of heat. But over here in Room B there were almost 60 kilowatts of heat in a single room. So this (inaudible) was an over-test. It was about three times as hot as the other rooms.

There were also four brine migration tests. They might be a little hard to see, but they’re the green stars behind the other--and these were boreholes that had a piggyback test where they also looked at brine flowing into the boreholes while there was a heater placed in the borehole.

And there were eighteen waste package tests where they put different materials into the boreholes, and then seven of them were actually retrieved later. And I’ll show some pictures of that.

So a little more information about Rooms A, B, and D. Rooms A and B, there were thousands of monitoring points, monitoring continuously through time a lot of things, mainly temperature, differential creep, creep at different distances into the rock, oriented stress or pressure, brine inflow, room closure, heat flux, heater power. Room D was a similar room that was unheated, so we had kind of a control room.

And then here is some data. Over here on the top right shows the thermal response in the salt between some heaters in the middle of the A room. So this is the cooler rooms. And you can see that it did pretty much--the temperatures did reach what you could probably say is close to a steady state. And you can see that the temperatures here are not that hot, but they're hot, while if you look in Room B on the surface of the guard heater, you can see that this is the hot room. On the surface of the heater, things got above 200 degrees C. So it was very hot.

And here it shows you the rapid pace of mining of these rooms and then when the tests were turned on and off. But most of the tests ran for four to five years. But, yeah, they were mined out in 1984, and the tests wrapped up in 1990.

And one of the things that was learned, the roof failure, which was alluded to as well earlier, was preceded by a rapid closure increase. And I think Lupe will actually show a little bit of this in his presentation, so I won't talk more about that.

Here is another classic photo of Darrell Munson in the central A room. You can see all the instrumentation, wiring. These manhole covers are covering the individual heaters. There was a lot of instrumentation going into the walls, into the back, into the floor. Then there were also situations like here where there was a denser array of observations around a single borehole so that we see small-scale effects, large-scale effects, all going on. An incredible amount of information was collected from these tests and reported. There were large, three-inch-thick data reports covering all this date. So it’s all out there and in the public record.

Here are some defense high-level waste tests with the waste package performance. Here is a photo down one of the boreholes. You’re looking down a borehole at the top of a heater basically here. And that smaller circle is where they grab it. It’s called a pintle. It’s what they grab the heater with. And you can see the instrumentation going down the boreholes. And then they ran the test, and this is what it looked like when they pulled it out. Some of them they were able to pull out. Some of them the creep had--the salt had closed in around the borehole. And also, due to the boiling off of water in some of them where they were collecting the brine, there was salt precipitated in here. And so they had to actually over-core, you know, run a meter-size core barrel down there to pull these out. This is before and after the heating in Room B. I’m not actually sure this is the same heater, but this is kind of characteristic of what it would look like.

The brine release tests that were done in Rooms A1 and B--and being a hydrologist I find this very fascinating--but it was a very interesting dataset that was collected. They flushed dry nitrogen through the boreholes and then flowed the dry nitrogen through a desiccant canister and then weighed the desiccant canisters every day. And so that’s what this data is here. And you can see that in Room B up here where temperatures got up to 130 degrees C, we were seeing 50, 60, 70 grams of brine per day per borehole--so it’s a pretty good amount of brine--while in Room A where it was only 50 degrees C, we were seeing much less brine inflow, you know, a factor of 10 almost, less, or a factor of 5.

So Room B produced, I guess, eight times more brine, is the number, from the same geology but only a difference of three times in temperature. So this is a pretty good dataset to show you the temperature effects on brine inflow.

But, as Abe pointed out, these are vertical boreholes. They penetrated clay layers, and a large portion of this brine actually was from the clay layers flowing into the intersected boreholes. Actually, Clay F was the name of the clay that intersected these.

We learned that a vapor transport of brine in intact salt is insignificant. When I said that the brine transport theory had to be rethought, then one of the first things that were thought were, well, probably the brine is being transported as vapor through the intact salt. But it was found--they changed the partial pressure in the boreholes for collection, and it really had no effect on how they collected brine. So it's kind of proof that it's actually the liquid brine flowing to the borehole under a pressure gradient that's the main source of brine, at least in these tests.

They observed brine inflow consistent with that salt brine that I showed in the previous picture. They did a chemical analysis of that salt brine and looked at the mass, and they found that a mass balance was--the amount of brine that we collected through this system was roughly equivalent with the amount of salt that was deposited, and so we didn't lose any brine. We were able to track it through the system.

And the thermo-poro-elasticity model of McTigue was able to explain a lot of this data, so it's basically a poro-elastic model, but you actually add thermo to it, and thermo-elasticity, you know, you add poro to it. But basically you're saying that it's a combination of the rock mechanical response and then the actual thermal--the differential thermal expansion of the brine and salt that causes brine to flow into the borehole. And they were actually able to explain it pretty well. That model didn't include brine inclusions at all, and it was able to match the data.

Shifting gears totally now and switching to some ANDRA tests that were done in the Amelie potash mine in the late ’80s and early ’90s in France. They took boreholes in a--it was a bedded salt potash mine in eastern France in the border with Germany. And they drilled a series of boreholes and filled them with different types of crushed salt that were different grain size distributions to see if the reconsolidation of salt was affected by--you know, if we have the big, coarse pieces in there or if we just have almost like table salt. And they found that with different heaters it didn’t really matter.

And they also did a test--you can see these guys installing a heater here. They installed a 4-kilowatt heater in a big borehole that reached pretty hot temperatures after seven months of heating, and they monitored brine inflow and all these things and found similar results to what was seen at the bedded salt at WIPP. And they also did gas permeability tests at other places in the mine.

And from these tests they found that boreholes, if they had a heater and it had no--if they just put the heater in an empty borehole and didn’t backfill around it, it complicated the heat transfer, because now have significant radiative effects, and it’s non-linear. So they found that putting crushed salt in the borehole, it simplified the ability to simulate it. And so they said, well, crushed salt is there in the mine, let’s just do it, it’s simpler. And so that was an interesting result that they took away from their testing.

And they found that from these gas and brine permeability that sometimes when you’re looking at these very low permeability rocks, you actually have to--you have to evoke the viscoplasticity model, because just looking at--you know, poroelasticity sometimes isn’t enough. You actually have to incorporate the creep term in there, too. So they did some rather advanced tests and were able to match them with some pretty advanced models.

Now, shifting gears again to the Asse mine, Asse II, referring to the second shaft at the Asse mine in Germany, they did a series of tests--and I’m going to talk about a couple of them--but they did heater tests going back to as early as 1968, and they were doing some of these tests to determine in situ thermal properties of salt to kind of demonstrate their systems, you know, kind of like WIPP did at the Mississippi chemical potash mine. They are basically kind of dry running some of their instrumentation.

That’s actually a significant problem, you know, because stainless steel corrodes heavily in the presence of chlorine. So you have to rethink a lot of the--you know, what you think is a robust system falls apart in salt.

They also were able to demonstrate quite a few different geophysical methods to interrogate the heated salt.

All the references that I'm referring to are at the last slide, so if you're interested in tracking any of these down.

There was a heated deep borehole closure test that was done in the late '70s where they drilled a deep borehole from inside the mine and then heated it in individual places and looked at the borehole closure both in time and in space, using calipers. And Lupe will talk a little bit more about that, too, because that's now used to validate some numerical models.

They did a heated brine migration test and another HAW, high activity waste test, which I'll talk more about. And they did some crushed salt reconsolidation tests, which I'll also talk more about.

The Asse brine migration test was philosophically very similar to the tests done at Avery Island and at WIPP. But Asse is a salt dome, so it's not bedded salt. And they added additional complications where they had four identical boreholes where there was a central heater and then a ring of peripheral heaters. And two of the boreholes had cobalt radioactive sources, and two of them were sealed and two of them were open to the atmosphere. So they had kind of a matrix of different tests. And they measured closure, temperature, brine inflow; they sampled and tested the gas content of the boreholes; and they monitored acoustic emissions, as you saw in that previous Sandia Laboratory test. They actually installed geophones and looked at it in situ.

And they found--here is the brine inflow to these boreholes through time. This right here is where they turned the heaters off. Ninety percent of the brine during these tests was collected after they turned off the heaters, which was a surprise to them. But it showed that the mechanical behavior of the tests is similar to bedded salt; that was pretty well understood. The brine inflow was much less than bedded. We’re talking--this axis here is liters cumulative during the whole test. So, you know, they collected less than two liters of brine in a whole borehole that ran for several years, while at WIPP they were collecting 35 liters, I think, in one of the Room B boreholes.

And they also found that radiation had a minimal effect on brine inflow. The radiation does slightly harden the salt; and work hardens the salt, it makes it slightly more brittle, but that seems to have really no--it’s such a minor effect, and it has really no effect on brine inflow. But that’s the point of this test, to show that those effects are minimal.

The high activity waste test was interesting. It was in a drift where they drilled a series of four boreholes into the floor in these two galleries, and they had this matrix of tests they were going to run with different radioactive sources and electric heaters on the end here. And due to regulatory problems, they were never able to do the radioactive tests. But after this sitting around for a while, they decided, well, let’s at least run the electrical tests. So they were kind of able to salvage it by running the non-radioactive portion of the test. And you could see here the temperature at the borehole wall and the different radii into the salt from these tests.

But one of the more interesting things I find is that then they came in at a lower level and excavated up to one of the heaters, so the room that the heater was placed in from is up above, and now they’ve excavated up to it. And they actually--there were coupons mounted on the outside of the borehole that were exposed to heat. And then they went up and just physically took those coupons off and went and tested them. And they excavated through a lot of different sensors. There were different geophysical sensors that were placed in the salt that then got excavated through. So that was a fascinating dataset as well.

A couple of those boreholes that were never used otherwise in the tests were then used for other tests. One of them was called the set of DEBORA tests where they placed a heater in a borehole. DEBORA-1 they backfilled around the heater with crushed salt, and then they measured corrosion, temperature, pressure, borehole convergence, and then the permeability and porosity of the crushed salt by--they had glass beads down here--or aluminum beads at the bottom and top. And they injected gas into the bottom of the borehole and then measured gas flow at the top. And so they were able, as the test was running, to monitor the permeability evolution of the test.

In the DEBORA-1 test, for less than a year they heated this with a 9-kilowatt heater, and they saw the porosity of the crushed salt go from about 38 percent down to 9 percent. And they found the permeability of the crushed salt fell about two orders of magnitude during that same period--this is one year--because you have borehole closure and you have--it’s a confined space.

The DEBORA-2 test was slightly different. They took one of those boreholes, and they just filled it with crushed salt, and then they put an array of heaters around it, because they thought that maybe some of the problems here were due to the limited space. But they saw similar results in this test where they had 15 kilowatts of heaters located around the borehole, and then they monitored the permeability and porosity of the crushed salt through similar means, found similar results, you’d say.

And they found that crushed salt reconsolidates significantly in just months--both these tests were less than a year long--in boreholes. It's to be expected.

Very interesting, in a long test that was done at the Asse mine, it's called the TSDE, the thermal simulation of drift emplacement. These two drifts were mined, and then large POLLUX casks--which are casks--they were actually transport casks, so they're quite large, but they put heaters in them instead of waste--placed them in the drift and then backfilled crushed salt over them as they retreated; installed lots of sensors in the boreholes; backfilled to the roof. And so a large thermo-mechanical time series was collected. They watched convergence, they watched temperature, evolution. And then they went in and excavated out and collected samples for laboratory analysis. So there was a large post-test dataset, too. And they found that the crushed salt reconsolidated less than in the boreholes, but that's also to be expected, because a large drift is going to close--there's more to close than on a small borehole.

But there was an extensive in situ validation dataset that was derived from this, a huge amount of data. There's two large reports. One is called BAMBUS, and one was BAMBUS-II. And the original BAMBUS report largely talks about the datasets that were collected, the time series that were collected. And BAMBUS-II is largely the post-test laboratory analysis of all the results.

Here is just a snapshot of some of the BAMBUS data. So these are basically the temperatures on top of those heaters, so these are heater temperatures. And you can see that they turned the test on, the test ran for nine years, and it got very hot. And then it started to cool off even though heated power stayed constant. And that's partially due to the backfill thermal conductivity increasing with decreasing porosity. So as you have less air in the backfill, the thermal conductivity is going up. And that was expected.

And then also the salt itself, even intact salt, has a non-linear thermal conductivity. The thermal conductivity or the ability of the salt to conduct heat depends on temperature. And then you can see here that the thermal behavior was--it basically reached a steady state right near the heaters, but at the roof it didn't quite.

So, in summary here, the technical basis has been approached a few times. Salt Vault was essentially kind of the first stab at a technical basis. It was a culmination of ten years of work. There was a giant Bradshaw and McClain report that summarized it, and they basically tried to put--they tried to make a report that would basically justify putting waste in salt. And it was a pretty good report.

There were some NRC reports from the early '80s that tried to do a similar thing slightly with more updated data. The Deaf Smith site characterization plans are these--it's an enormous 10-volume report that has lots of data that was collected before--obviously Deaf Smith was never constructed. As time went on, people kept collecting data.

Gorleben safety case, which was--the ISIBEL project was 2006 to 2010. There's a lot of information in that.

I've been involved in some recent reports that have gone through, and basically this presentation is summarizing. These reports summarize a lot of the testing that's been done in salt.

And so we could say, what's left? The technical basis for heat-generating waste is not new. This is testing that's been going on since the '50s, and even back in the '50s there was a pretty good understanding of it. We're refining it, and we're coming up with more sophisticated tools to explain it, but the thermal-mechanical behavior of salt is well known. And modern numerical models will allow us to incorporate things we couldn't do before, but these are not the technical basis themselves. Complex models are really tools to help us understand and make sure that everything makes sense, but they are not the technical basis themselves.

Long-term viability of a salt repository depends on the salt itself. The bedded salt deposit provides the containment. Shaft seals then ensure that the penetrations we drill through the salt don't compromise the salt itself. And so the reconsolidation of backfill is important, because that's typically how these seals are constructed; and seal emplacement, the process of that is important.

Other repository features, they may be very interesting, but I think they are of secondary importance, like waste forms, waste packages, and brine migration through the excavation, because if the salt contains it, what goes on inside the repository is of secondary importance for the long-term safety.

So this little matrix here summarizes the rest of the presentation, and you'd see whether it was a bedded or domal test in this column, whether it used crushed or intact salt around the heaters, and then whether it was borehole or in-drift here on the far right. And you could see that all the different combinations have been tested except for maybe bedded salt using crushed backfill in-drift rather than borehole, and this is essentially what Abe has described.

So you could see that we've done a lot of tests, and this one combination really--you know, the TSDE test in Asse is very similar to what Abe described, but that was in a domal salt deposit, which is much drier.

And there's all my references, which you can't read, but they're there for reference.

Thank you very much.

EWING: Thank you.

Questions from the Board? Jean?

BAHR: So thank you very much. That was a very informative talk. At the beginning of your talk, you said that you were going to tell us at the end what remains to be done, and--

KUHLMAN: Sorry, that was kind of--

BAHR: --I think I have sort of missed that.

KUHLMAN: Sorry. Well, I'm saying that there is not that much--the technical basis does not have begats in it. We're basically to the point now where it's like this particular combination of conditions has not been validated. We understand the thermal-mechanical behavior of salt pretty well, so what's left is small, really.

I'm sorry if that was not clear, but that was kind of the point I was trying to make with this last slide here.

BAHR: Just a couple of other questions. You mentioned the difference in brine inflow in the case of domal salt versus the bedded salt, and does that have to do with the difference in porosity of those two--

KUHLMAN: It's a difference in the--

BAHR: --media or the different amount of interbedded heterogeneities in the bedded salt versus a purer salt and--

KUHLMAN: The domal salt just has less water in it, to begin with. If you take a meter block of salt and you say, you know, what percentage of water is in here for bedded and domal, there’ll be less. There’s just less water to begin with in the domal salt.

BAHR: Is that because of less intergranular porosity, fewer fluid inclusions?

KUHLMAN: Yes and yes. The domal started off as bedded salt many millions of years ago or thousands of years ago, and it’s been deformed so much by geologic processes that the water has kind of been worked out of it. And so it’s been kind of kneaded and purified to the point where there is less water in it. And so you just expect less brine to inflow from a drier rock.

EWING: Okay, thank you. Sue?

CLARK: Sue Clark, Board. So I just want to follow up on your last bullet there about the secondary safety case. You didn’t say anything about any previous on the source term itself. Does that mean it doesn’t exist or you just didn’t--

KUHLMAN: The source term. Sorry. Would you clarify?

CLARK: So the waste itself and its behavior in the brine, its solubility, its release from the repository.

KUHLMAN: Right. So I think as long as the repository is--you know, the salt provides an adequate seal for the repository except in the conditions like WIPP where we’re forced to say, okay, you have to throw that all away and assume someone is going to drill through and pull that out.

In those kind of situations you need to worry about the solubility and those things.

But if you have an undisturbed repository, those things really--you know, what goes on inside--you can consider the waste to be a homogenized pool of--you know, it's all been stirred up, it's all dissolved, you know, you can kind of--because the salt is such a good barrier.

EWING: But just to follow up on that, because salt is, let's say, co-located with natural resources, don't you expect to be required to consider the case where there--

KUHLMAN: In those cases, then, yeah, you'd have to consider the--I'm talking--you know, this is--we're talking hypothetically about a site that doesn't exist.

EWING: Right.

CLARK: And just to follow up to clarify, are you saying that that's a gap? There is no information or are you just--

KUHLMAN: Oh, no. Sorry, I didn't talk about it in this presentation, really. Sorry. No, that's not a gap. There's been lots of work on the brine chemistry and all these processes--Phil and Florie are going to talk more about a lot of these processes, which are very interesting and very important for the short-term behavior inside the repository.

EWING: Other questions from the Board? Sue?

BRANTLEY: Sue Brantley, Board. I guess you're assuming or asserting or something--I want you to clarify--that the brine migration just isn't a problem. So, I mean, Sue is worried about the waste dissolving into the brine, and you're not worried about that because the brine isn't going to get out even if it dissolves in there. So I'm just--I'm confused about that. Why are you assuming the brine isn't going to migrate?

KUHLMAN: Well, the brine that flows into the repository is the brine from the salt immediately around the repository.

BRANTLEY: Right.

KUHLMAN: So you've basically taken the--you mine some salt out, and you've taken it away, and you took some water with that, but then it's the brine that's in the disturbed rock zone, kind of a halo immediately surrounding the repository--there's a limited amount of brine which is going to flow into the repository, and it's going to dampen things up slightly at the very beginning, but its conditions--like at WIPP where we assume that a borehole is drilled through to a brine reservoir, which then floods the repository, where a lot of these solubility effects come into play. When you have just a tiny amount of brine, you know, the native brine from the repository, it's not enough to really cause a lot of these processes in the first place. You have to have some other source of a giant amount of brine to flood the repository, because that's just really not going to happen under the undisturbed conditions.

BRANTLEY: So what do we know about, sort of, slow brine migration through these deposits? I mean, you know a lot about let's perturb it, let's heat it, let's drill into it, brine migration, but that's like the transient. What about the--

KUHLMAN: Well, Abe discussed some of the recent tests that have been done where they've shown these small brine inclusions nearby each other have not--they're dissimilar. There is also some--

BRANTLEY: Well, he said that they had DNA, and I didn't buy that the DNA was--

KUHLMAN: There's other tests that have been done that don't have anything to do with DNA that were done that involve looking at brine inclusion chemistry or marker bed chemistry. We're talking about layers that are a few meters apart that are completely different chemistry.

BRANTLEY: And so why would that be, like, geologically? Why would that be? I mean, I've worked in halite deposits, and they precipitate from brine that has the same chemistry, you know. Why would it be so disparate?

KUHLMAN: Well, you know, the different waters, the geologic layers come in. A lot of these brine inclusions and the non-salt layers are kind of--you know, as the salt is forming, it's the sodium chloride that's coming out first, and all these rarer things are kind of getting concentrated into these last little nuggets of things that never precipitated. And so you end up getting a lot of weird isolated things that end up being different.

But, you’re right, it was the same body of water that deposited it all. But we see these isolated chemistries meters apart that have been there for millions of years, and so that’s pretty good proof that there is not active regional flow going on through the salt.

BRANTLEY: And how different are these little pockets?

KUHLMAN: I think Florie is going to talk about that some.

BRANTLEY: Okay.

KUHLMAN: But this has been well published since before WIPP was certified. They looked at the different--just simple general mineral, general physical properties of the water, you know, magnesium chloride ratios, simple things like that. And the waters are vastly different between different marker beds, different brine inclusions, different seeps.

EWING: So I’d like to ask the last question, and then we have to move on. Sorry.

So if I follow your reasoning about the role of brine, then would your recommendation be that domed salt would be better than bedded salt?

KUHLMAN: Domed salt is drier.

EWING: No brine pockets either; right?

KUHLMAN: The brine pockets are in unrelated geology under the salt, so that would be probably a somewhat site-specific thing.

FRANKEL: Just a very short question.

EWING: Okay, short.

FRANKEL: Jerry Frankel. Just a short one. I think you clarified it for me, but, to me, this intrusion upon cooling seemed alarming, and you're not alarmed by it because it's going to heat up and cool down. But is it because there's just a limited amount of brine and then those cracks are going to seal up anyway? Is that what you're saying?

KUHLMAN: The amount of brine coming in at rapid cooling comes because the salt or any rock has a very low tensile strength. And so as it's shrinking, the capacity to shrink is exceeding the tensile strength, and the salt grains are pulling apart.

FRANKEL: The problem then that won't happen with slow cooling?

KUHLMAN: No, but if it's cooling so slow that the creep is able to keep up with it, then that won't happen. At least that's what's believed to be the--so if you're cooling over years rather than days or weeks, then it's--we've never done a test over hundreds of years, but, you know, that's the belief.

EWING: All right. Well, I'm sorry, we'll have to move on, but thank you for a very comprehensive talk.

(Whereupon, a break was taken.)

EWING: The next presentation is by Florie Caporuscio--Florie?--

CAPORUSCIO: Yes.

EWING: --where we'll continue to talk about fluids in brine and salt.

CAPORUSCIO: I guess we're leading up to brine migration in salt.

EWING: Yes.

CAPORUSCIO: I want to acknowledge my co-workers, Hakim Boukhalfa and Mike Cheshire. Both work at Los Alamos with me. This is primarily investigative work that was done last year. It's very preliminary still, but appreciate the opportunity to present this.

I have a different take maybe what uncertainties may be in salt. First one, liquid/vapor migration in salt seems to still have some unresolved issues. Roedder at the survey did some tests: Was the brine moving toward the heat source or away? He determined that it was going toward the heat source. There have been other studies you saw, similar responses.

What happens at the grain boundaries? There is a lot of uncertainty there? Does it go through the boundary?

Does it get to the boundary and then migrate along the crystal structures. Once again, decrepitation, if you really heat the salt up at extreme temperatures? And then I'm going to talk about some mineralogy. When you look at these inner layer beds--clays, sulfates--do they dehydrate? How much do they dehydrate? What temperatures? And do we get to certain temperatures where you have phase transformations, and you're not going to be able to go back to the original phase?

We know that clays can dehydrate and then rehydrate. Vidal and Dubacq has a nice paper on that. These types of changes in the mineral structure can affect the ability to retain water and also their sorption/desorption capabilities for clays. And above, depending on the type of clay, between 300 and 400 degrees C, it turns into--does a phase transformation into mica; and at that point you start to lose a certain amount of stoichiometric water and volume to the crystal structure.

The latter portion of the mineralogy, looking at the sulfates, I want to key in on gypsum and anhydrite transformation where you get a fairly large water release, 21 weight percent, and a significant volume reduction if you make that phase transformation. Creep fracture, the rocks above and below, that's a question. In a similar vein, you may also produce a water channel at the interface between these beds and the salt.

So for the experimental portion, we used a type of clay that we recovered from WIPP called corrensite; we used gypsum from Naica, Mexico; and then the other sulfates, bassanite and anhydrite, we also collected from WIPP.

This is the outline. First, fluid migrations. And these are in single salt grains. That was the capacity that we were able to do last year. Single phase, two phase, did a little bit on the conclusions then. And then talk about the mineralogies, dehydration, phase change possibilities, and then, finally a path forward, hopefully, and the research end.

I'm an old geologist. I want to go get my own samples, so we went down to WIPP. Photograph on the left: my co-author on the very far left, Hakim; Brian Dozier, who facilitated our getting the samples; myself. This is in Panel 7. We were collecting large salt crystals--3, 4 centimeter crystals of salt--that we then used in our experiments. On the right I'm indicating an orange marker bed that's widely used at WIPP to locate themselves to make sure that they're in the same continuity of beds.

The first thing we wanted to look at is temperature profiles. On the left-hand the graph is for single salt crystals; on the right it's for crushed salt. Both of these were ramped up to 200 degrees centigrade. The first thing, for the salt crystals especially, the single salt crystals, in our experimental apparatus we had a low coupling between the heat source and the grain face, so we lost some heat there.

You'll notice in both cases that the temperature drops exponentially away from the heat source. You can see that in the one on the left for the single crystals, once you get a centimeter away, you've dropped over 100 degrees centigrade. So the temperature drops way rapidly from heat sources.

We wanted to look at the brine chemistry of these inclusions. One way--and it was very, very preliminary--we did a couple tests, and there's still more to go. We used LIBS, laser-induced breakdown spectroscopy, to drill down through a crystal and then intersect the brine. So the first--I don't have a pointer, but the first graph is drilling down through the salt itself; the second graph--sorry, I got it reversed. While we were drilling down--it was this analysis, which is calcium chloride, the salt--once we hit the brine pocket, the brine is enriched in magnesium.

We then went and looked at some of the compositions of the evaporites left behind. We had decrepitation at the top of a salt crystal. You heat it up, the inclusion bursts, and what's left at the top of the salt crystal. There we analyzed--and that's a magnesium chloride salt. In the SCM photo on the right, we analyzed that; that's a detrital quartz grain.

So, anyway, the thing you take away from this is that the brine inclusions do vary within a salt crystal, let alone a large mass of salt. It's dominated by magnesium/ sodium chloride brines, however.

So the next thing we wanted to do was--the heat on the left-hand side, the red band, is 200 C.

ZOBACK: What's the time?

CAPORUSCIO: Sorry. The time total was 30 days.

ZOBACK: 30 days?

CAPORUSCIO: Yeah. So we've taken thousands and thousands of video images and compressed it down for these actual photographs and then compressed it.

So 200 degrees C was the heat source. Again, the gradient is very non-linear. The first thing that happens is when the inclusion gets aggravated, you see that you get a movement to the portion of the inclusion on the cold side, and then it starts to move towards the heat source. And it's basically due to the heating of the inclusion and the beginning of a convection cell. And then it starts to move and dissolve and go toward the heat source.

SPEAKER: Could you play that again?

ZOBACK: Yeah, now that you've explained it.

(Pause.)

CAPORUSCIO: Especially watch the big grain on the far right. It shows most clearly, you end up with little channels coming toward the heat source, all those individual stringers. And we’re going to describe what we found.

So the come-away in the graph at the bottom is, migration rate is mostly affected by the temperature, of course, and the size of the inclusions.

I want to go back on that. Sorry.

These were the high-temperature tests. And, of course, they move to the heat source faster. These other two, this one and this one, those are more dependent on the size of the inclusion. The one is moving faster than the other. Turns out that the smaller inclusion moved faster.

So what happens inside these channels? Turns out--this is a channel, this is a channel--they are approximately 10 micron in diameter. So when the bubble starts to move, it doesn’t come en masse. Creates channels, a whole array. And one of the things we wanted to look at is how the composition of the deposited evaporites are as it moves toward the heat source. From the original site, we see in the upper photo and the accompanying chemistry from EDEX (phonetic) that it’s a magnesium chloride deposition. And as you go closer to the heat source, composition changes, and it becomes sodium chloride right before it hits the heat source. And that’s pretty much discussed in the bottom one.

Here we have two more videos. Let me describe a little, and then we'll go through them. These are the two phase inclusions, so liquid and gas. These were run for 30 days also. The picture, which will be a video in a moment, was run at 60 degrees, the one on the right 100 degrees. The takeaways, brine migration starts at less than 40 degrees centigrade. And this was modeled at--two-phase flow is modeled by Anthony and Cline in '72. Liquid goes toward the heat source, gas moves away from the heat source.

We did notice also in this one on the left, this is a stringer of inclusions right along a cleavage. When you run this for a very long time, the inclusions went right through the cleavage plane. Those were emplaced before, and the rock healed. I mean, water got into the cleavage, and then it healed, and that's why you have the stringer of water inclusions. Once again, the rate is going to be influenced primarily by the temperature gradient.

If we can start the one on the left?

(Pause.)

Most of the action is going on right there close to the heat source. You see the gas starting to move away slightly.

(Pause.)

And that should be it. Not very exciting. But it does show that it happens. There is movement at very low temperatures.

If we do the one on the right, please? This is a little more fun.

(Pause.)

ZOBACK: Was the right side encased in any way?

CAPORUSCIO: So to hold it in the visual field for the microscope, we have two aluminum blocks. On one side is the heat source; on the other side is--we tried to keep it at ambient temperature. So it is constrained.

I would like to run this again, and I'd like to point out a few things before we run it on the right.

So we have--this is going to generate a lot of gas, and those are going to become stringers. Over here you're going to see a large water inclusion, which has gas also, but it primarily starts moving this way, and you'll see that once again they move in discrete little 10-micron channels.

Go again, please.

(Pause.)

I think one of the nice things about taking these long videos, whether they be 30 days or even longer, is some of the inclusions overrun other inclusions. And this is where Roedder had some problems in the original work in the '80s was, they would look for an inclusion, they'd do it at the end, but sometimes they lost data because it got overrun by other inclusions.

Here is a still photo of that same one at high temperature late in the event. So when we have two phases, the brine migrates toward the heat source, but a small portion of the liquid is captured by the gas. When it moves to the right, it--this is not my area of expertise, fluid dynamics, but I'm assuming that it's using the liquid as a wetting agent as the gas moves away. Once again, these 10-micron channels, you see that clearly in the lower left photograph here. That's perpendicular to the travel section of an inclusion.

That's a really nice photo of what one of these looks like when you increase the magnification. Here are some of the evaporitic materials that are left behind. By the way, that one--I'm sorry--is an oblique cut. But you can see some of the evaporitic material left behind. We're thinking that these are along dislocation sites, someplace for them to deposit themselves. And, once again, as it travels, you sort of release the magnesium chloride first, and it becomes more sodium-rich as you go closer and closer to the heat source.

We wanted to also look at some imaging techniques to see if there were other capabilities that we could pull from these sort of tests. Low-field NMR analyses, we got a very good correlation, but we haven't done any calibration yet of what we're seeing. I'll show you these difficulties in the next slide.

And then we did a neutron tomography example. We got really good results for the imaging itself. I think we’re going to need some refinement to be able to--we have ten left? Okay. Hopefully, we’ll get to the neutron tomography video at the end of this.

So this is the low-field NMR. In the middle column where you see the red and yellow, that’s signifying that we’re seeing water in clay inclusions. I’m not going to go through all this. We have a lot of things to develop before we actually get any analytical capabilities out of this.

These are the brine migration results. We’ve labored on these for a while. I’m going to skip forward to the mineralogy if you don’t mind.

So what can we look at in these seams of other mineralogy? Clays, potential water loss, rehydration, phase changes in the sulfates. We could incur an even larger water volume loss, and that is if they phase transform into anhydrite, either gypsum or bassanite.

We have a fairly nice lab where we have Bridgeman sealed rocking autoclaves. We can go to 400 degrees C, 600-bar, within the safety envelope at Los Alamos. They do go to higher pressures and temperatures. We’re not allowed to. They’re getting to be scarce in America. We also do work in the used fuel engineered barrier system, carbon sequestration, geothermal tracers. Sometimes to get a run up, you’ve got to wait in line to get these going, but this is all under our domain.

We also have a nice little controlled XRD heated stage where we can ramp it up to 300 degrees C. Shortly we will be able to control relative humidity. And, of course, we have the optical microscope and video capabilities.

These little gold capsules, if you fill them with a charge, put them inside there, you can run multiple experiments at once, as long as you don’t want to worry about the chemistry of the fluid during the reaction. It’s a final reaction product.

So we looked at the type of minerals that are in the salts. Very simply, both the orange salt and the white salt have a common set of minerals: corrensite, a clay, quartz, magnesite, muscovite, hematite, anhydrite. Both of them are dominated by halite. In the white salt we also saw microcline, calcite, and bassanite. The microcline, I believe, is probably detrital. However, we see two of the three sulfates, bassanite and anhydrite.

The first thing we looked at were the clays in some sort of detail. For those of you that aren’t clay mineralogists--and I’m not one, so I had to put this up--corrensite is a smectite-chlorite-smectite layered structure. At the repetition of these, you have interstitial water. People talked about Clay Seam F at WIPP. This is it. Notice how nice and linear it is. There is no deformation in layered salt.

So we ran this at a bunch of temperatures. These are XRD powder diffraction patterns. And we went from 25 degrees to 250 degrees C, and there is a real perturbation between 25 and 100. There is a natural structural change. So we then dove a little deeper. Between 65 and 75 degrees, we lose the inner layer of water. And that's where you get this expulsion of water. When they talked in the last talk about something happening between 50 and 130, this is it. We're losing the water at approximately 75 degrees C. It is also reversible. If there is water around and you cook it off, the water will go back into the structure.

In gold seals--in gold capsules--sorry--we put corrensite, saturated it, ran it to 300 degrees C; it stayed stable. So as long as the water is around, it will keep hydrating, dehydrating.

Now I'm going to talk for a moment on the sulfates. There is a one-step reaction, and there is a two-step reaction. The two-step, gypsum to bassanite to anhydrite, it's continuously dewatering. Some people put the first transition at 76 centigrade and then the second one at 100 to 140. Others say that same reaction from gypsum to anhydrite is 180. Bottom line, it's a large water loss and a volume reduction of the crystal structure.

So, once again, we used the heating stage to look at these. The top one, the most pertinent information is that at 75 degrees centigrade most of the gypsum disappears, and you see the growth of bassanite. By 100 degrees centigrade, the gypsum is gone. Where you would see those peaks, they're gone now, so you now have just bassanite. We then heated it up. That same sample of bassanite from 100 degrees to--well, we went through a series, but at 275 degrees C it held steady for a number of hours. At 21 hours we start to see the formation of anhydrite, and by 70 hours we have anhydrite, and we have just remnant bassanite left.

We then took that same gypsum, trying to look at the anhydrite transformation as a single-step process. We looked at a nominally anhydrous gypsum sample and then one that had 30 weight percent water in the capsule. In both cases anhydrite existed at the end, starting from the gypsum, and there was remnant gypsum left. There was more gypsum when you had more water in the experiment.

These are the conclusions from the clay and sulfate parts. Both initial reactions take place at a very similar temperature. The corrensite dehydrates, and the gypsum-to-bassanite reaction is about 75 degrees centigrade.

The real interesting thing--and I'm just going to focus on this note here--Robertson and Bish did a whole series of sulfate experiments modeling Mars. However, the reactions were all very sluggish; they were quite dependent on the relative humidity. And I'm going to stress that further work needs to be done on the timing of these and at appropriate PT conditions for a repository.

Okay, those are the research plans. Since we're really low on time now, Rod, I'm going to let people read these at their leisure and open it up for questions. And the very last thing, I'd like to run that--well, let's run the video of the neutron tomography and open it up to questions.

EWING: So you're going to run it?

CAPORUSCIO: Yeah. This is, what do you want to call it, eye candy? Where did it go? It was hanging out on its own. That's it.

You can obviously see the inclusions when you use neutron tomography. The water shows up nicely. That was unheated. It was just--that full thing is three centimeters in longest length. There is a range of sizes of inclusions also.

EWING: So thank you very much. It's very interesting.

Questions from the Board? Jean?

BAHR: Jean Bahr for the Board. In Kris Kuhlman's presentation he noted that one of the things that they learned from the Avery Island experiments were that inclusion flow actually wasn't nearly as significant as porous media flow along intergranular porosity. So, volumetrically, how important is the water that's in the inclusions or that can be released from clay dehydration or gypsum-to-anhydrite formation relative to somewhat more mobile brine that's in the salt?

CAPORUSCIO: Well, it's obviously a mass balance issue, correct?

BAHR: Yes.

CAPORUSCIO: In dry salt we've been able to collect about .2 weight percent water if we really heat it up, dry it, and collect all the water. In the clay seam we got up to 6 weight percent water. So then it becomes a matter of where you put the waste, close to a seam of sulfates and clays or a little more distant. You saw that the temperature drops off rapidly in the salt. These are decisions that'll really have to be made based on those sort of mass balance considerations.

EWING: I'd like to follow up on that question. So it looked like the activated area by the heat on the one side of the experiment was in the tens of millimeters; is that correct?

CAPORUSCIO: That's where the--at really high temperature, Rod, like 200 C?

EWING: At 200 C.

CAPORUSCIO: Yeah, it dropped off 100 C in just a centimeter.

EWING: In just a centimeter? So if I imagined that distance as a cube of salt a meter on the side, how much water would I expect to get at the heat source roughly? I mean, is it a liter or ten liters or--

CAPORUSCIO: Probably in the tens of liters.

EWING: Tens of liters, yeah.

CAPORUSCIO: I honestly haven’t done it. We have a talk this afternoon that’s going to model some of the water movement, and maybe that’ll help you.

EWING: Okay, thank you.

Other questions from the Board? Efi?

FOUFOULA: Efi Foufoula, Board. I just wanted to ask whether you have any insight in the scaling that you had, one of the issues. Do we expect cancellation, linear type of growth or amplification of all these effects?

CAPORUSCIO: That was one I skipped at the end, the research path forward. That’s what we want to look at next are cores, that first we can start to trace these things as they move across grain boundaries. Do they go through, do they collect, do they pull, do they move along crystal boundaries? Hopefully in a little more time we can then jack at these things and confine the pressure and see what it looks like under hydrostatic conditions. We’ll see how far these go. Hopefully they’ll be interested.

EWING: Sue?

BRANTLEY: Sue Brantley, Board. Can you just put the inclusion migration experiments into context? In other words, there have been experiments like this done, you know, for quite a while. So what question are you asking that hasn't been answered in the literature before in terms of those experiments?

CAPORUSCIO: For the brine migration ones?

BRANTLEY: Yes.

CAPORUSCIO: There was a lot of uncertainty in some of those initial ones. I think we've captured a lot more data. We're still sort of sifting through to get rates along with fluid inclusion sizes and the type of water total amounts that we'll get at the heat source over various times.

BRANTLEY: So it's an attempt to get more accurate rate measurements compared to what was in the literature from before?

CAPORUSCIO: Correct.

EWING: Mary Lou?

ZOBACK: Mary Lou Zoback, Board. The question I have is just for clarification. These 10-micron channels, did I understand you to say they're basically dissolving, dissolution, going through--and they're just going through cleavage planes, grain boundaries?

CAPORUSCIO: Not grain boundaries. Cleavage planes was all we've been able to determine so far.

ZOBACK: Okay. And when they intersect a grain boundary, do they--they looked remarkably uniform, the streaks, as they were moving. And I can see a lot of moving that I would expect to be representing around grain boundaries, or is my scale off?

CAPORUSCIO: No, no. These were single crystals.

ZOBACK: Oh, okay.

CAPORUSCIO: Yeah. We were able to see things within crystal size.

ZOBACK: Within a crystal. Okay, thank you.

EWING: Jerry?

FRANKEL: So in your future plans, your research plans, you say you want to resolve the gas migration mechanism. What do you think it is? What do you think is happening to drive the gas away?

CAPORUSCIO: Well, no, I think that papers such as Anthony in 1970 did a very good job of looking at two-phase phenomena.

FRANKEL: Can you explain it to me?

CAPORUSCIO: No, I can’t. Seriously, that’s not my expertise. But basically it was a convection cell, okay, that they developed. And so the gas was moving toward the cold side; the liquid was moving toward the warmer side of this mechanism.

FRANKEL: But it’s a dissolution precipitation type of mechanism, and that's why you need water? Is that what it is?

CAPORUSCIO: Right, yeah.

ZOBACK: The water still (inaudible) the channel--

CAPORUSCIO: Yeah.

ZOBACK: --even for the gas.

CAPORUSCIO: Remember I said that water was also wetting the surface of the gas bubble?

FRANKEL: Right.

CAPORUSCIO: I presume that that's--

FRANKEL: But that same sort of dissolution precipitation mechanism is driving the water--

CAPORUSCIO: One way.

FRANKEL: --in the opposite direction but somehow facilitates gas to go the other way?

CAPORUSCIO: Yes. I don't know why.

EWING: Other questions from the Board? From Staff?

(Pause.)

All right, thank you very much, Florie.

CAPORUSCIO: Thank you, Board.

EWING: The next session is where we invite comments and questions from the public. I have the sign-up list, but we forgot to ask people whether they wanted to speak in the morning or in the afternoon. So I'll go through the list; but if you want to speak in the afternoon, that's fine, just wait, and that way we can be sure that those who are here only in the morning will have the opportunity to speak.

I'll just go down the list. Matthew Silva, morning?

SILVA: Yes.

EWING: Okay, please. And no more than five minutes.

SILVA: No problem.

EWING: Okay. Identify yourself and affiliation.

SILVA: I'm Matthew Silva, and I've worked with the Environmental Evaluation Group for fifteen years. For eleven of those years I worked as a chemical engineer for Bob Neill and the last four years as the director of the group.

I want to comment on a couple of questions that were asked earlier this morning. One was on the independent oversight. And I think it's fairly important that whatever area is selected, one of the successes of WIPP was the fact that the independent oversight was from the very beginning, starting in 1978. It was not done as an afterthought.

Another question that came up was congressional support, how important was it. It was absolutely essential. The roles and responsibilities of EEG were defined under Public Law 100-456, which was carried by Senator Jeff Bingaman largely, and in that law the director determined the scope of work. It was not determined by DOE or anyone who had been influenced by DOE. The director hired the professional staff, not someone else.

The results were all published. It was an obligation that our results had to be published. And because of this we were able to maintain the trust of not only the public, but also the elected officials, having access both to the elected officials and to the public.

Another thing which we talked about privately perhaps was political cover. We had some very brilliant political officials, elected officials, in New Mexico. Governor Bruce King was one; Congressman Joe Skeen, who was 22 years in the Congress; and Senator Jeff Bingaman, who was absolutely solid. And they recognized that you had to have an oversight group to look out not only for the public, but if they had questions, their staff could ask those questions, knowing that we were going to give them the straight scoop and that we were looking at the details.

Also for groups like this--the NAS WIPP committee, EPA, NRC, and looking at transportation--relied heavily on our review and our comments to help them to improve their product. And also we were in a situation where we worked full-time on this. Certainly one cannot expect you to work full-time on the WIPP project, but is what we devoted our efforts towards. So that was another advantage of having an oversight group from the very beginning.

And the public may have some concerns about individuals coming from other states that are trying to get rid of waste or from individuals who rely heavily on the Department of Energy to fund their research programs, whether it be faculty or otherwise. So this helps to assure the public that indeed someone else is looking at this.

There were two events that really--if I may bring this up--that led to the demise of EEG. One was the 1999 putting of the EEG's budget under the Carlsbad area office. That made it very difficult as a director to get our funding without too much hassle. I ended up spending a lot of time working on this.

And, second, in 2004 the law, 100-456, was gutted, and the oversight group became essentially a DOE contractor, and the Department of Energy would determine the scope of work. They would determine the hiring; they would determine whether or not the work could be published, and, only if it was published, it had to be after their review; and they would determine whether or not you could speak to a public official.

Anyway, I hope that covers it. One other point I noticed in the Blue Ribbon Commission report was that they recognized that it takes two to five years, it takes a bit of time, to get an oversight group started. It's not the kind of thing where you hire somebody off the street and they are immediately in the mindset. It does take time. And we do look at seasoned professionals, and there’s fortunately a good supply of professionals in radiation protection in the universities and other state agencies and in other areas other than the Department of Energy.

So, with that, that’s the sugar-coated version. If you want to have a few drinks later, I’ll be happy to discuss it.

EWING: Thank you, Matt, very much.

SILVA: Okay, thank you.

EWING: Next on the list is George Danko, but you’re the afternoon, right? Thank you.

Christopher Timm.

TIMM: I’ll do it this morning.

EWING: Okay.

TIMM: Thank you very much. Thank you all for being in Albuquerque and listening to good input, we hope, for solving the nation’s rad waste disposal problems. I’m with Pecos Management Services, and we were the follow-on, if you wish, the independent oversight contractor for WIPP for 2005 through 2010.

So our oversight was not as much on the technical side and getting it going as it was on the operations and maintenance as to how they were doing and how could they do better to, in fact, achieve what their mission was. And we had good relations with both EPA and the State and, of course, as well with DOE. And so we were able to work through all the relationships.

Did approximately sixty studies, fourteen major reports, which are available on the EPA Web site. Unfortunately, they're not on the WIPP Web site, but they are on the EPA Web site if you want to look at the different reports that we did.

But I want to speak a little bit about this whole idea of radioactive waste disposal. Both Bob and Abe really talked about how the assumptions are important as to how to get from where we are to where we're going. And assumptions really need to be based primarily on--they need to be realistic, and they need to be based first on the science, the engineering, the operational knowledge and experience. So we need to have our assumptions be realistic and be well founded on those aspects. In other words, it's things we know and we can prove rather than things we think might happen. We need to really have those assumptions be as close to what we know as we can.

The second major one is history. We've got to consider it a human element, especially dealing with radioactivity. So the assumptions going in--because any model you have, I don't care if it's a ten-year or a ten-thousand-year model, is only as good as the assumptions going in. And we, frankly, have done a poor job in getting the assumptions to really be based on fact in many instances for WIPP. They were overly conservative; in fact, oftentimes they weren't even founded on good science, yet they were stuck with them. How I don't know, but that was the case. So that's number one.

Secondly, the development of solutions must be holistic. You can't just look at the science and the engineering. You've got to look at the operational. You've got to look at the environmental health and safety both of what the situation is now--storage and independent spent fuel sites around the country and tanks and so forth--as well as what is it going to take to get those to one place like WIPP and the aspects involved with that--health and safety, operational, and so forth--as well as future. I tend to think that this idea of deep geological disposal was formulated as a future solution without really considering the impacts on a day-to-day basis for getting it there.

There have been people hurt because of this decision to, in fact, repackage and get stuff to WIPP. Does that make sense? Maybe so. Are we more worried about generations ten thousand years from now than we are about ours or a hundred years from now? I think so. It's got to be holistic. You've really got to look at the broad spectrum. And while the NEPA does that, it doesn't do it as well as it could. Personal opinion.

Finally, I really think that we should relook at this whole recommendation for deep geological disposal. Again, this Board is separate from NRC. NRC is in a situation; they say you can keep stuff above ground for at least 60 years in the hardened storage you have, maybe 300. That whole concept, it’s 60 years old now; that basically, “Let’s go deep geological” is 60 years old in terms of when it really came into play. Is it working like it should? Are we better off keeping it above ground where we can see it and fix it as we go along?

I just think it’s time to relook at the basic premise, not to be locked into an idea and go forward lockstep because that’s the way it’s always been done. We’ve got to be open to new ideas. Abe talked about those. We’ve got to be open all the way along.

Any more time?

EWING: Actually, no.

TIMM: Okay.

EWING: But thank you. Thank you very much.

TIMM: Thank you, And I’ll stick around afterwards.

EWING: Okay. Don Hancock, did you want to speak this morning?

HANCOCK: Yes. Good morning. I’m Don Hancock from Southwest Research and Information Center. We’re a 43-year-old non-profit organization in technical assistance. I have had the misfortune, I guess a lot of people would say, of, for the last 38 years, spending a significant amount of time looking at WIPP and nuclear waste disposal issues. So, as Dr. Ewing knows, I can turn on for a long time, but I know he will not let me do that. So let me try to make three points that, I guess from my standpoint, haven't been made this morning.

So first is: What is WIPP's mission? It's been mentioned WIPP is for defense transuranic waste. What hasn't been so clearly mentioned is the fact that the idea of--Bob Neill mentioned the fact that there was interest in high-level waste at WIPP from the beginning and interest in some of the community folks in the Carlsbad area from the beginning for high-level waste; but that was something that was rejected and has been rejected consistently.

So when we start talking about consent, we also need to talk about non-consent. How many times do you have to say no before the answer is no? And it's not just from a public standpoint. Congress has consistently for 35 years said WIPP is for transuranic waste; it's not for high-level waste. The 1979 law that Bob Neill referred to, the 1982 Nuclear Waste Policy Act, the 1987 Nuclear Waste Policy Amendments Act all had opportunities to say WIPP should take on--the '82 and '87 laws all had opportunities to say WIPP should take on commercial waste or high-level waste missions.

Congress said no. 1992 with the WIPP Land Withdrawal Act, Congress explicitly said no. Section 12 of the law explicitly says no high-level waste, no spent fuel can come to WIPP for any purpose, even temporary. So we have a situation of--we have 35 years of policy, law, public understanding, technical understanding, frankly, of what WIPP’s mission is.

So one of the things that’s going to be important going forward and one of the reasons I want to make the point to this Board, who does look at more than just WIPP, is that that becomes very important from my standpoint--and I think from a lot of other people’s standpoints--in terms of how to deal with this difficult problem of what to do with high-level waste and commercial spent fuel.

If we start from 1957, the National Academy report that’s been mentioned several times today, we’re a long time out, and we only have a series of failures in this country. And WIPP clearly, as I say, is not a success, not going to be involved in the high-level waste mission as well.

So that brings us then to the second issue that relates to, again, WIPP’s mission, which is to start clean and stay clean for up to 175,000 cubic meters of defense transuranic waste.

We don’t know what happened almost five weeks ago in the underground that caused a radiation release. That, in and of itself, is very alarming. If we know as much about salt, if we know as much about WIPP, the waste in WIPP, the performance of WIPP, all the aspects of WIPP, as we're supposed to know, it shouldn't take more than five weeks to figure out what's happened. But we don't know the basics. We don't know what happened. We don't know how much has been released. We don't know how much more might be released. We don't know more than so far 17 workers being contaminated. There could be more.

None of these things were supposed to happen. From modeling standpoints they weren't supposed to happen. From environmental impact statements they weren't supposed to happen. From permitting requirements they weren't supposed to happen. From public assurances they weren't supposed to happen. So while you may say, well, this is a WIPP-related thing, but it goes to the fundamental basis of: What do we know and how well can the Department of Energy and its contractors perform in carrying it out?

So those are important issues that this Board knows about, you've talked about in your reports over time. So I'm not saying something you don't know, but we now have the practical experience of a repository that was supposed to operate for 30 years, starting clean and staying clean, and performing for 10,000 years or more with no releases; and it didn't accomplish that.

So that raises some significant questions that I think need to be looked at. And among the things, WIPP is a pilot plant in its name. We now have a situation that among the things we don't know is how much contamination is there in the underground and what will it take to clean it up, to decontaminate it; what's the experience of decontaminating radiologically contaminated salt mines in the world; what kind of decontamination is going to be necessary on the surface of the facility; what kind of decontamination is going to be required offsite.

So these are very important questions that deserve a lot of technical and public attention, and I'm hopeful, but not overly optimistic based on my last 38 years of history, about how well the Department of Energy wants to do that. In fact, I would argue that now is one of the times that we most need independent review of what's going on at WIPP. We don't have the EEG anymore. We don't have independent technical. We have regulatory agencies, EPA and New Mexico Environment Department, that are very under-resourced to deal with these questions.

So one of the things that the political and the technical community need to look at now is: How do we construct independent review of what's happening with WIPP and what has to happen with WIPP going forward?

Which brings me then to the third point. I was very interested in a couple of aspects of Abe’s presentation this morning. One is, as I understood what he was saying about the understanding of the geology and the strength and roof fall possibilities, etc., the option that there was a roof fall in the WIPP underground seems to be off the table. If it’s not, if that’s what happened, it raises major technical issues about the technical understanding of that facility. If it is off the table, though, then where does that leave us with how much we know, and what else is happening with the facility and the waste coming to it?

The other thing that he talked about, which was the first time--and I’m pretty observant about things that get talked about with WIPP--the first time he presented apparently the current option for what to do with another failure of WIPP long before what’s happened in the last six weeks, which is that WIPP cannot fulfill its mission when it comes to remote-handled waste. The legal limit for remote-handled waste is slightly more than 7,000 cubic meters. WIPP has no ability to come anywhere near that.

So there has been discussions, but there hasn’t been any presentation about how to deal with that significant failure and the facility being able to accomplish its mission. So I am glad to finally start seeing what the proposal is going to be in terms of handling remote-handled waste at WIPP, but I’m disappointed that once again we’re not going through the kind of public discussion, technical discussion kind of thing that should happen if we’re going to have successful geologic repositories, not only WIPP but others, in this country. So I think that’s a very important issue.

So the last thing I’ll say just briefly, you all have raised some excellent questions about the kind of technical review. The other kind of thing that needs to happen to have credibility with the public is, there have to be--the way I tritely put it is, the worst critics of any project should be given an opportunity to show what they know or don’t know. And that’s, again, something that in this country we have primarily shied away from; that’s the kind of thing that needs to happen as well.

And I will give a plug to the Canadians, who are trying to do it a little differently in terms of that kind of thing. That’s why I even went to Canada last September to testify about their first deep geologic repository facility.

Thank you.

EWING: Okay, thank you, Don.

Again, if you want to wait until the afternoon, just let me know as we go through the list.

Michael Loya.

LOYA: My name is Michael Loya. I sit on the Citizens’ Advisory Board, and I’m making comments as the sole source, as an individual. I'm also a generational New Mexican. And I had an environmental drilling business, and I've done work out at the test site, and I've done work for the Army Corps of Engineers.

I want to make a comment that Mr. Silva said earlier about Senator Bingaman and Congressman Skeen. There is also Senator Domenici. And we pushed for casing advance drilling method when the lab switched to that back at the end of 2007, and we pushed for that so they could eliminate the drilling fluids. So the last thing that Senator Domenici did, he got the funding for the lab. He got $26 million for the lab so they could go to casing advance. That was very important, and that was a boatload of money, and he deserves kudos for that.

I also want to make another comment--and I think this is very important--knowing people from all over the state, and they want you all to know that you need to be cost-conscious. And I'm not trying to say this in a gruff way or whatever, but there's a lot of money spent, and people need to be more cost-conscious about this. You need to run this like a business. And I think that that's very important, because, you know, funding is finite, and it's getting harder and harder to fund all these projects. And I think that that's very important.

And I think by doing so and showing these people--

showing the public that you're--you know, you've got to win their confidence. And that's very important. And you need to make these presentations--and I know most of you all are--you know, you're up there with Ph.D.'s and all this. But for the regular public, you need to make them simple and just precise so they'll understand--and bullet points--what's going on. And I think that's very important.

I'll tell you something, my mother was born in Carlsbad in 1917, and there's a lot of people down south--and I'm from down south even though I live up north--that are behind WIPP.

And I'll leave you with this. There's going to be mining accidents. So, you know, you can sit there and bring in all these consultants and they can say, well, this might happen and that might happen. But you need to go in and zero in and clean that up and get it operational again and move this forward. And I think that that's very important.

And I'm glad that I got to spend this time and get our point across. And I wish there were more farmers and ranchers that are friends of mine that could have been here to make these comments, and they will come if you make that possible.

So I thank you very much. Three and a half minutes.

EWING: Thank you very much.

Dave McCoy.

McCOY: I may need that extra minute and a half.

EWING: Okay.

McCOY: Hello. My name is Dave McCoy. I’m the executive director for Citizen Action New Mexico.

We’ve been dealing with local problems here in Albuquerque at Sandia National Laboratories and also at Kirtland Air Force Base. And I look at WIPP and I see a $69 billion operation, is what I heard this morning, engineered to protect us for 10,000 years. And we have an accident, a release of americium, plutonium, other substances into the environment; workers exposed; public exposed.

And then I look at the issue that I’ve been dealing with for the last seven years, and that’s the Mixed Waste Landfill at Sandia National Laboratories. Very difficult to get the executives from Sandia National Laboratories to tell us when they’re going to excavate that dump, if ever. It had defective groundwater monitoring. They used the data from defective groundwater monitoring to make a decision to leave those wastes under a dirt cover. They’re in unlined pits and trenches, 119 barrels of plutonium waste, tens of thousands of pounds of depleted uranium. There’s beryllium, cadmium, over a hundred different toxic chemicals, heavy metals, solvents, all in a mixed waste form, leaching toward the groundwater that supplies Albuquerque’s drinking water wells;

vadose zone not monitored; information about the dirt cover and how it’s defectively constructed and monitored hidden, suppressed by the New Mexico Environment Department. When we asked for the information, Citizen Action was sued by the Environment Department so we wouldn’t get it. Information about the faulty groundwater monitoring suppressed by the EPA, the NMED, and the EPA Office of Inspector General, we finally got it after we sued.

The public shouldn’t have to go through this exercise to have transparency, but we have to go through that. And then when we find out the information, it’s not pretty. The Citizen Action is currently suing the Environment Department because they’re violating their own order, their own final order, for the mixed waste landfill. That said every five years there’s supposed to be a review. That order was in 2005. It was due in 2010. It’s now 2014, and the Environment Department wants to extend that for another five years, so we sued them.

Who do we have to do this as the public? Why can’t we rely on these regulatory entities and the laboratories to do the things that are necessary to make us safe?

I listen to this scientific information. I’m an attorney, so a lot of it was way over my head, but it was interesting. And all I can say is, from a public perspective of looking at this, you know, you’ve got reality versus models. You had all these scientific models that say it’s not going to happen, we’re not going to have the exposure. We got the exposure. Now I’m asking you to do something about the mixed waste landfill. What kind of exposure can we expect from that, you know? If you’ve got WIPP here that’s so highly engineered and you’ve got the dirt trenches and pits out there at Sandia, we’re asking Sandia’s executives to come out of their caves and speak with the public about this. When are you going to get rid of that mess out there?

Thank you.

EWING: Thank you very much.

Last on the list, Robin Falko.

SPEAKER: She’ll be here in the afternoon.

EWING: In the afternoon? Robin Falko.

Okay. So that brings us to the end of the morning session. We’ll reconvene at 1:10. So thank you all for being here today.

(The lunch recess was taken.)

AFTERNOON SESSION

EWING: Welcome to the afternoon session of the NWTRB spring meeting. We'll begin immediately with a presentation by Phil Stauffer from Los Alamos National Laboratory on coupled models.

STAUFFER: So today I'm going to be talking about a coupled thermal-hydrological-chemical process model for the high-level waste repository in salt.

So the outline here we've got--first I'm going to go over the in-drift concept to remind people of what that entails, then look at the waste composition of the defense high-level wastes by thermal load, which is very important to understand for this project; some background on salt and heat pipes; description of the simulator we're using that FEHM developed at Los Alamos; a code validation example; some simulations with only heat so that we can contrast the heat-only simulations with simulations that add water and water vapor transport; and then, finally, processes that are added to couple the chemistry into the system.

So I think we've seen this before today, the in-drift emplacement strategy. It's simple. It's a lower-cost method. The backfill is readily available from the salt mining. We put the waste packages in the drifts and fill them back up with salt. These lead to much narrower rooms than are currently being used. In a narrow room the rock mechanics is such that there is less risk of the roofs collapsing.

This slide was supposed to be first there. This is some of my team members that helped with this. Dylan Harp, simulation expert; I’ve got a Ph.D. student working on this. We have a mesh generation expert; I’m going to be showing some fancy meshes that I can’t take credit for building. We’ve seen Florie present today, and Hakim is on his team; they did some of the chemistry experiments that we’re going to be putting into the model. And then, of course, Bruce Robinson is our project coordinator, project leader.

So the distribution of heat loads that we’re looking at for this project, 90 percent less than 220 watts. So these are not the civilian wastes that are 8,000 watts per canister. These are much lower thermal loads. You can see for the high-level waste, we’ve got over 16,000 expected canisters in the less-than-50-watt range. And it’s only until you get into the DOE spent nuclear fuel that you get some percentage that may be up in the 2,000-wat range.

So looking at this, you can see that if we can figure out how to do the defense high-level waste, it’s a stepping stone towards the hotter civilian wastes. And it’s a good path to take because of that. We’ll learn things, but we won’t be jumping straight in from non-heat-generating wastes to the most heat-generating wastes, which are the civilian loads.

So some background. We’ve already seen this. Bedded salt has the favorable characteristics of the self-healing rheology, the viscoplastic flow, very low permeabilities in the intact/final states, and extremely high thermal conductivity, 5 watts per meter kelvin.

We’ve seen this image before that Kris showed, and some of the work done showed that precipitation of salt due to the boiling front was found in this annular region where they had backfilled with crushed salt. And we believe that’s evidence of the heat pipe that I had mentioned in the outline.

And a brief description of what a heat pipe is in the lower left here. We’ve got a hot waste package surrounded by a boiling region. The boiling point for a saturated salt solution is about 108 degrees Celsius. So we’ve got a region around the 130-degree heater where we’ll have boiling. As the vapor moves away--the dark blue arrows--vapor moves away from the heat source. Eventually it’ll condense when the temperature drops, and that condensate will--the condensate has no salt in it, so that will then requilibrate with the salt that’s further from the heat source, dissolving salt and flowing back towards the heater, and it’ll form a loop where it keeps boiling away and then returning. And what you’ll do is you’ll build up salt around the heat source and dissolve salt at a fringe away from the heat source. That's a quick explanation of the heat pipe.

So the simulator we're using for this work was developed over 30 years at Los Alamos. We've got a bunch of peer-reviewed articles on a range of different fields from geothermal, nuclear waste, soil vapor extraction, a whole bunch of different topics that we've used this simulator on. And it's been benchmarked against sets of problems for the Yucca Mountain project, and we've got dozens of physics and chemistry problems that every time we make code changes we run back through for the quality assurance to make sure that by adding a new feature we haven't broken an old feature. So we're always watching for that.

We've got fully coupled thermal, mechanical, chemical, multiphase gas, water, water vapor, and rock. The mechanical is infinitesimal deformation numbers, so we're not talking about being able to simulate these rooms at WIPP closing in over hundreds of years. We can do stress changes where the deformations are very small. That's one of the challenges of this salt modeling is some people have thermal-mechanical codes; some people have hydro-mechanical-thermal; some people have chemical-mechanical; and no one yet has really pulled all of these things together. So there's several teams around the U.S. and the world that are working on this, and we’re converging, but we haven’t gotten to the holy grail of being able to simulate all these things together yet.

And, as I mentioned before, we have a powerful 3-D grid generation tool. This example is from the Nevada test site with high-resolution faults going through a mesa, and this example shows that we can capture pinch-outs and complex geometries.

So a code validation sample. This is just an experiment that we set up at Los Alamos where we had a pile of crushed salt sitting in a tub with a thermal heat lamp, and we drove--we measured the temperature at the base of the bulb, and then we had a thermal couple, and we were able to, with an existing thermal conductivity as a function of porosity model, we were able to match this fairly well.

So for the thermal-only simulations, we’re looking at calculations of a potential repository in-drift with the waste packages lying vertically like this. This is map view down. We’ve got intact salt surrounding them, and then we’ve got rooms on either side so that we can draw a nearer boundary around the system like this and reduce the number of nodes we need to calculate the system. These are called reflection boundaries.

So these are some examples for thermal-only simulation with thousand-watt canisters at different spacings, anywhere from three feet between the canisters, which results in temperatures well above the 200-degree limit that was mentioned earlier for cracking the salt crystals, which we don’t want to get into the regime where that happens, which for the bedded salt is about 275 degrees.

Then this is just showing that through these kinds of simulations we can make predictions about how far apart you’d want to space the canisters in a repository that is basically dry. So this would be with no water vapor, no water. All we have is the intact salt, the crushed salt backfill on top of it, with the known thermal properties of that material. So you can see, to stay below 200 degrees, we’re looking at a spacing here of about six feet between these canisters.

BAHR: What’s the height of the canister itself?

STAUFFER: The canisters are two feet diameter, and they’re nine feet long.

ZOBACK: And they’re laying horizontally on the floor?

STAUFFER: Yeah, in this--yeah. So these are nine feet long, and they’ve got a diameter of two feet. And they’re just laid on the floor and then sprayed with run-of-mine salt backfill.

ZOBACK: If they’re nine feet long and you show twenty feet, it doesn’t seem right.

STAUFFER: Well, these might have been a little--

SPEAKER: -- two rows back there?

SPEAKER: No, they all look the same.

STAUFFER: No, this might have been with the slightly longer canisters. The ones we’re using now are nine feet.

So then this is another way of looking at it. If we take the waste and say that it’s all 220 watts, which is more than 90 percent of the load, and we space them at .9 meters apart, what are the temperatures going to look like for 55, 110, or 220? And most of the load doesn’t--we don’t even get up above boiling here. So, you know, that says that for most of the defense high-level waste, it’s really not that much different from what we’re already putting into WIPP. We’re not going to be boiling and creating big regions where there’s dry-out and vapor transport.

But there are some--there is some percentage of the canisters that are going to create those conditions, so that’s what the rest of the talk is going to be about.

So for the rest of the talk we’re looking at a set of five canisters lying in-drift on the floor. This is a 3-D picture. We’ve got access drifts in red pictured around the edges. We’ve got our experimental drift going through the center of this block. The greenish color is the run-of-mine salt backfill. The canisters are red, and you can see them here, five canisters.

So this is a comparison of results from the thermal-only on the top versus the thermal plus water plus water vapor on the bottom. This is for a heat load of 1500 watts per canister. So in this example I was trying to create lots of vapor transport, boiling, you know, see what's the system going to look like if we push it towards the maximum impact given the waste packages that we're looking at.

And so this is zoomed in on one, two, three of those heaters so that you can see the contours of temperature better. And what you'll notice in the top is that we have fairly regular drop in temperature as we move off from the center of the canister. Maximum temperature is 223 Celsius in this case. What we see in the bottom figure, we have a region where temperatures have homogenized. And that's where we're getting this heat pipe where the vapor is moving up toward the cooler regions or out toward the cooler regions, condensing, and flowing back towards the canisters. And that's classic heat pipe behavior creating an isothermal region. And this was for a simulation that was for two years. And the maximum temperatures in this case are about 195, 197 degrees.

So in addition to creating this region with a constant temperature, we've also dropped the maximum temperature at the canister. So the heat pipe actually could be a good thing for the system. There is no guarantee we're going to get it. That's part of the rest of the story that's coming up.

ZOBACK: Can you help with the scale on that? So the floor is--where would the floor be? Is it that green line then?

STAUFFER: The floor should be right through here.

ZOBACK: And does the crushed salt extend beyond the top of the model?

STAUFFER: Yeah, well, beyond the top of this picture.

ZOBACK: The picture, I mean. Yeah, okay.

STAUFFER: Yeah. In this figure you can see the canisters sit here. They're two feet high. And this pile of crushed salt we varied anywhere from four feet to eight feet. The room is going to--you know, the experimental room was proposed to be ten feet high. At first we thought, well, we'll just make the crushed salt ten feet high, but the logistics to getting it right to the (inaudible) are very hard.

ZOBACK: It's tough, right.

STAUFFER: So then we started planning the different depths.

BAHR: That model does not include the reprecipitation of the salt around those.

STAUFFER: No, we're just--this is just with thermal plus water plus water vapor. So we're boiling the salt water around here; it’s coming out across the boiling line, which the 110-degree line is somewhere between the green and the light blue, which is out about here; and so when the water vapor drops across the boiling line, the water vapor pressure drops considerably, and you start condensing out a lot of water vapor.

BAHR: But you don’t have any--there’s no feedbacks.

STAUFFER: But there’s no dissolution precipitation yet.

TURINSKY: So what is this, a two-phase Darcy flow model?

STAUFFER: Yeah.

SPEAKER: You guys, would you identify yourselves, please?

BAHR: Oh, sorry.

SPEAKER: Thank you.

STAUFFER: So then what I’ve done here is I took a temperature difference between the thermal-only and subtracted off the thermal-only plus water plus water vapors. So what we see is, indeed, in the centers right above the canisters we get--the thermal-only was 44 degrees Celsius hotter than when we allow this vapor transport mechanism. But there’s also places in the field, once you get out past the boiling line, where the condensation is actually sucking up temperature, and the temperatures are lower than they were in the thermal-only one.

So now I'm going to talk briefly about the many coupled processes and feedbacks that are required to bring in the chemistry changes, the dissolution precipitation.

So, first, changes in porosity can cause changes in permeability. Porosity can also--if you change porosity, you're also going to change the thermal conductivity and the heat capacity of a block of rock. The ability of the vapor to diffuse through the rock is impacted by the porosity. So those are all feedbacks related to porosity.

Feedbacks related to temperature include thermal conductivity; the solubility of the salt is a function of temperature; water vapor pressure, as I was describing, is a function of temperature; and brine viscosity is a function of temperature.

So here are just some examples of thermal conductivity. We added thermal conductivity as a function of porosity and temperature of the model, based on data that Kris showed from the previous experiments. We added salt solubility as a function of temperature, Sparrow 2003 that we found in the literature. Precipitation/dissolution of salt I added about a year ago. We added water vapor diffusion coefficient function that relies on pressure, temperature, and porosity and then a permeability-porosity relationship for run-of-mine salt. And these are all reported in this deliverable to DOE.

So the vapor pressure of water is also not just a function of temperature, but also a function of the amount of salt that's dissolved in it. So as you put more salt in the water, the vapor pressure goes down, so that boiling point goes up as you increase the salt content.

And so we added this function to the code, and you can see that the region of interest, because we're in WIPP where the salt is always fully--the water is always fully saturated with salt, we're in a fairly tight range here. We probably could have gotten away with a constant value, but we ended up coding up the complete function.

Some of the specific algorithms, we've got an algorithm for radiation and convection based on heat transfer calculations, and then we just use an affected thermal conductivity. I'm going to show an example of clay dehydration. And we also added a lot of diagnostics to the code, because we are now changing things dynamically that are so intensely coupled that if you don't watch, oh, yeah, you know, permeability in the crushed salt is changing because my porosity changed; my thermal conductivity should also change; my water vapor pressure, there's another list that has temperatures in.

And so you can go to these outputs that are now coming into this green and do double checks all the time and make sure that you're getting what you think you're getting.

In these models it’s really easy to put in a function; and if you don’t look at it, you know months later you could realize that you’ve screwed something up. The reason I do this is because I have screwed things up in the past.

Here’s a quick refresher on the heat pipe. We’re going to be looking at some of these, a section with the liquid, a boiling region, a condensation region, and flow-back. These heat pipes are used commonly in industry. They have them on the space shuttle to dissipate heat. They’re in electronics. They’re found all over the place. We didn’t expect to find one in a nuclear waste repository for salt. And so when we started seeing this in the models, it was like, wow, that’s kind of neat.

So this is an example of a very high resolution, a four-centimeter mesh, two-dimensional domain, that goes five meters this way, three meters this way. We’ve got square heaters--they started out as squares--and you can see by the end of the simulation we’re starting to deposit salt. This is porosity. Zero porosity is red. The initial porosity is this blue background color. So as this simulation is running, water vapor is being driven off, crossing the boiling line, dissolving salt, flowing back towards the heaters, boiling again, and creating these rinds of almost solid salt.

And this--yeah, it’s a 2-D slice. And we get increases in porosity across the boiling line and decreases in porosity below the boiling line. And that boiling line is dynamic. It moves out from the heaters. In this particular example, I think it took 50 or 60 days for that boiling line to move out to where it is. And then it reaches a pseudo-steady configuration, so it doesn't move much after it got there. It's still moving, though, because as you're packing more salt into this region, the thermal conductivity is changing, and that's going to change this so-called steady temperature (inaudible).

So next I'm going to move to the 3-D thermal-hydro-chem simulations, and this is where the mesh generation team comes in really handy. We have heaters that are a lower resolution sitting on the floor of the drift. We have air surrounding the run-of-mine salt backfill. We have a damaged rock zone and an intact salt surrounding that. The damaged rock zone can have permeabilities that are many orders of magnitude higher than the intact salt because of the mechanical changes from mining the drift.

And so some of the parameters used in these simulations, the backfill saturation natural range is from one percent to about five percent. This is not gravimetric water content. These would convert to--gravimetric water content would be about one percent of five percent saturation. As a hydrogeologist simulator, I use saturation, and that’s what’s in all the models. What the experimentalists measure is the gravimetric water content, because you just boil off the water, measure the weight of the water, the weight of the rock, you have your gravimetric water content.

Porosity we fixed at 35 percent, which is in the middle of the range measured. Clay content we explore zero to ten percent. And we fix at the higher end of 30 degrees Celsius background. The air temperature in WIPP ranges anywhere from 15 to 30 degrees. The rock, if you dig into it fresh, you’re closer to 30 degrees. So that’s why we picked that value.

So this is a complicated figure, but I’m going to walk you through it slowly. This is for five 750-watt canisters at a relatively high saturation limit. We explored a range of parameters, because we don’t know exactly where we’re going to fall in the WIPP facility. There’s variation. There’s places where there’s a lot of clay, not so much clay, higher water contents.

What we’re looking at here are porosity saturation temperature with time going from 10 days up through 460 days of simulation. And you can see that by 60 days we start to see a drop in porosity, increase in porosity across the boiling line, and the heaters are starting to get salt deposited around them. By 460 days we’ve created this envelope of low porosity around the heaters. This is for a fairly wet system. This process stops because you end up drying out all the water that's around the heaters; and, as you can see, the saturation goes to zero directly on top of where that has happened. So water is still trying to make it back there; but as soon as it hits the boiling front, it boils off and gets redeposited as vapor up in this region. And you can see the temperatures here, the boiling line is in between the yellow and the green, which is coincident with that envelope.

Porosity changes with higher heat loads. So as we go from the 250-watt, we see almost no evidence of this heat pipe. But as you go up to 750 watts is where we really start to see this impact.

BRANTLEY: Excuse me, a question. Sue Brantley, Board. So this is chemical equilibrium; there's no kinetic?

STAUFFER: There is no kinetic, no.

BRANTLEY: There is no kinetic.

STAUFFER: Not yet.

BRANTLEY: So where it precipitates is simply a temperature function.

STAUFFER: Yes.

BRANTLEY: There's no surface area term or anything like that?

STAUFFER: It immediately drops out, yeah.

BRANTLEY: Right.

STAUFFER: We have the ability to do that. We just haven't gone there yet.

BRANTLEY: And sodium chloride dissolves and precipitates so fast, you probably don't need to do it.

STAUFFER: Probably not. We have some experiments planned for the future to help us get a handle on that.

So porosity changes more with saturation. So at low saturations, if it's a dry repository like the domal salt, we wouldn't expect to see this at all, because their saturations are much lower. But if we get in a part of a bedded salt that's wetter, we might see this.

So for clay dehydration we have experimental data from Hakim and Florie that shows the clays giving off a certain percent of their weight, and we've coded that up as a function, and we've included that in some simulations. This slide is showing a simple simulation where we have boiling at one end, we have clay at the other end, and as the thermal wave propagates through, you get these kicks in water being released. So the saturation increases as that water comes out of the clay. And this was to help QA.

The code, and then we applied this to the larger three-dimensional system. The difference between no clay and 10 percent clay, you know, we see some differences, but they're what I would call second order effects on the system.

And that’s because the water is not free to begin with; the thermal wave has to propagate through; and you have to get the temperature change before you can release that water. But this is all very new work, and we’re still trying to wrap our heads around this.

The vectors on here show the liquid flux--the vapor flux on the top and the liquid flux on the bottom. And you can see the vapors moving like a chimney through the system from the outside coming in and moving up, very similar to Ed Weeks’ work on Yucca Mountain where the whole mountain operated as a chimney heated from below. And the water is always trying to--it’s being condensed out here, and it’s always trying to flow back towards the drier high-capillary suction agents.

Anyhow, that’s basically it. I think I’ve covered most of these conclusions.

And so for the future work we’re looking at numerically validating the heat pipe. We found a paper--after we did this and saw these heat pipes, we found a paper by the Spanish where they had taken a beer-can-sized experiment and heated one end and cooled the other end, and they got a heat pipe that redistributed salt mass. And so we were very excited to see that, because that is experimental evidence. But we want to do it at the scale of a meter or two and eventually in an underground setting.

We’ve started adding isotopic tracers in the simulations to fingerprint the water, putting enriched deuterium to see where it goes. And we’d also like to--we’re starting to look at evaporation, barometric pumping, and the impacts of the ventilation through these facilities where you’re drying out at different temperatures throughout the year.

So that’s it. Thank you.

EWING: Thank you.

From the Board, questions? I have one while you think of your own.

Just so that I’m clear, what is the source of the water in this model? Is it the fluid inclusions, the water given off by the clays?

STAUFFER: Yes.

EWING: So it’s everything in the system?

STAUFFER: It’s everything, yes. Initially, we looked at the total amount of water that could be available, and we started with an assumption that, okay, let’s say that’s the upper limit, the 10 percent saturated case, and we’ll just run in that window of possibilities to parameterize the problems, say, if we had that much water problems, what would it look like, because we don’t have a good handle on how much water is going to come in from the damaged rock zone. Those little crystals that are sitting in the run-of-mine salt, as the temperature wave moves through those, those inclusions are going to move towards the edges of the crystals and possibly be released.

So, as of now, you know, that's why Florie's team is looking at that, like how fast does that happen? Can we expect that water to be available for this heat pipe mechanism? With the clays, we didn't include that water until we added the clay function.

EWING: All right. And that's my next question. With the clay function, when you say 10 percent clay, where is that clay, and how is it distributed in your model?

STAUFFER: Homogeneously it's distributed throughout the porous media. So it's as if, when the miner was going through--

EWING: Right.

STAUFFER: --they mixed it up and then threw it back on. And that particular load had 10 percent clay in it.

EWING: And do you consider that a conservative or bounding--

STAUFFER: That's pretty high, because in this clay seam, you get--in a clay seam like this, it's still got a lot of salt in it.

EWING: Right.

STAUFFER: You know, it's not a solid layer of clay that's this thick and just goes on forever.

EWING: But the geometry of these clay seams and the concentrated amount of water that might be released along that seam, doesn’t that change the thermal model quite a bit?

STAUFFER: I should have been clear about this. The clay is only in the run-of-mine backfill. We’re not putting clay underneath in a seam. It’s as if it was mined in that particular wheelbarrow load of salt that had 10 percent clay in it.

EWING: So Marker Bed 139 wouldn’t have been captured--

STAUFFER: Not in--

EWING: --or 138 or--

STAUFFER: That’s future work. We’re going to put in--you know, one meter below here there is a bed that has hydrous minerals.

EWING: Right, right.

STAUFFER: And we really want to simulate that, but we haven’t gotten to it yet.

That gets up to about 60 to 80 degrees, so it’s right in that first transition zone. And it could be interesting, because that’s a significant amount of hydrous minerals.

EWING: Right, right. And so when you say this is a coupled model with the chemistry, actually the only chemistry is the thermodynamic properties of pure salt. Is that--

STAUFFER: Yeah. And the precipitation--

EWING: Without precipitation and dissolution function.

STAUFFER: Dissolution and precipitation, yeah.

EWING: But not in any--only in the abstract sense in terms of--

STAUFFER: And the clay dehydration, that’s in the chemistry part of the code. And it’s a water source term when you cross a temperature boundary.

EWING: Okay. So I think I’ve used my time. Other questions?

BRANTLEY: Well, just as a follow-up--this is Sue Brantley, Board--you said that the water comes out of the fluid inclusions, but you don’t have anything in there that models the mechanism of fluid inclusion movement. You’re modeling this as porous flow--

STAUFFER: Exactly.

BRANTLEY: --through porous media that had the permeability--

STAUFFER: We’re saying that water is available from time zero.

BRANTLEY: Right. So there’s nothing mechanistic related to what we saw this morning.

STAUFFER: Not yet.

BRANTLEY: I know you can model anything; right now what you’re modeling.

STAUFFER: Yes, right now.

BRANTLEY: And there’s no texture or anything like that.

STAUFFER: No, no.

BRANTLEY: It’s simply porous media flow.

STAUFFER: Yeah, we’ll add complexities later.

BRANTLEY: Yes. Which is what you should be doing. I mean, I agree with that. But it can be confusing, because you said that the water comes out of the fluid inclusions. That’s because you have to have a certain mass of water in your model, so you calculate that.

STAUFFER: Yes.

BRANTLEY: But you have moving inclusions in your model.

STAUFFER: No.

EWING: Other questions, comments? Jean?

BAHR: Jean Bahr. Just one more clarification. You’re also not allowing any water to flow out of the intact rock mass into the cavities; is that correct.

STAUFFER: In some cases that is allowed to happen. The simulations I’ve shown here, the damaged rock zone is fairly low permeability, so I didn’t have that complexity added in. But you can start out with a drier pile, and you’d still get a heat pipe if enough water can flow in from the damaged rock zone. But from the experiments that have been done before, those are like 35 liters into a room, and the pile starts out with 250 liters in the 5 percent saturated case. So that amount, you know, you really have to get a lot of flow from the damaged rock zone to get that to be an important contributor.

EWING: Please, Jerry.

FRANKEL: Jerry Frankel. So just back to the very beginning, these two-foot by nine-foot cylinders, they're meant to represent the stainless steel cans that the vitrified waste is poured into?

STAUFFER: Yes.

FRANKEL: Is that right?

STAUFFER: Yes.

FRANKEL: So there's no--so what would be the effect of some outer container? Those things have to be--I mean, in this system they might not need any protective engineered barrier, but they would be transported in some sort of a container. So if you had a big steel container around it, would that affect any of it or just make the initial radius larger?

STAUFFER: It would make the initial radius larger, and then that would make the temperatures at the interface lower.

FRANKEL: Lower because of the area, larger area.

STAUFFER: Yeah. I can't imagine it would be that big, though, if it was steel or--

FRANKEL: I don't know how they're going to transport them into your--

STAUFFER: I don't know either.

FRANKEL: --into your repository.

STAUFFER: This was the configuration we started with was just the relatively thin stainless shell over a--and the material properties on the inside of this, we just used borosilicate glass as a thermal conductivity and (inaudible).

FRANKEL: That’s included in your model?

STAUFFER: Yes, uh-huh. But, no, there’s no flow allowed in here. These are--you know, we’re not trying to model anything inside the wall, just into the wall.

EWING: And to follow up on that, so the thermal conductivity for the borosilicate glass changes as a function of the fracturing of that glass.

STAUFFER: We have not included that yet, no.

EWING: So you just took it out of the handbook.

STAUFFER: Yeah.

EWING: I mean, because it’s available in the French program, the change in thermal conductivity.

STAUFFER: We could include that function. I mean, do you know how much it changes?

EWING: Quite a lot, yeah.

STAUFFER: Okay.

EWING: Other questions? Yeah.

BRANTLEY: So I’ve got to say this in real succinct questions that are going to sound flip, and I don’t mean to be flip, because I think what you’re doing is interesting and important. But, first of all, why are you doing this? What do you hope to be able to--what question do you hope to be able to answer? And then why should I believe that answer when you get it?

STAUFFER: Well, the why is because eventually there will be a test of high-level waste in salt if the program moves forward. And as part of that test, we will make predictions. And if we can make predictions that are borne out by the tests, then we will have shown that we understand how the system behaves. We might do a dry pile and a wet pile or hot pile and cool pile. But we need to have some idea of what might happen before we do the tests, where to put temperature sensors, what the gradients might look like, what saturations might--how they may evolve. And so that's the why.

And why should you believe us? Well, I wouldn't believe this right now either.

BRANTLEY: I didn't say I didn't believe it.

STAUFFER: All we have is a beer can right now where it happened in the beer can. The hydrologic properties of the run-of-mine salt are not very well constrained. It's not a simple granular material with the same size grains everywhere. But our goal is to build one of these in the lab at a scale where we'll be able to either prove or disprove the models. And that's how these things go. You make a model of the system, then you go look at a real system, and back and forth. And we’re in the very early stages here.

BRANTLEY: And have you done this for the WIPP repository?

STAUFFER: Well, we’re using a lot of data from WIPP, because it’s available. We have access to --

BRANTLEY: But, I mean, they have canisters down there with--what do you call it--run-of-the-mine salt?

STAUFFER: Run-of-mine salt.

BRANTLEY: Run-of-mine salt. Something I’ve never heard before. Could you model that, and have you done that, and would that make sense?

STAUFFER: Well, that’s what the experiment will be. We’ll take run-of-mine salt and put it in a big box and put a heater in it.

BRANTLEY: But aren’t they running the experiment in WIPP right now?

STAUFFER: No.

BRANTLEY: Well, not with the high-level waste, I know, but--

STAUFFER: Well, there’s no heat in that, so it’s not very interesting. The current canisters are sort of the background temperatures, so--I mean, yeah, I’d love to put some hot canisters down there--

BRANTLEY: But, I mean, you’d like to do your models simple and build in complexity, so here you have an experiment that’s being run that’s not very interesting because it’s simple.

STAUFFER: Well, we’ve run background temperature; we’ve run with low heat loads; so I don’t know what we’d be running. There’s no heat being generated. It’s just a canister sitting in the background temperature field. I mean, if we had barometric pumping, we could look at dry-out.

We do have--as of early February, we had some big containers with run-of-mine salt in and water, you know, just the initial water content at a very high-resolution scale, looking at how much evaporation occurs on that through time, you know, which is important. I was talking about evaporation being something I want to--how much water evaporates from the surface that’s in barometric connection with the atmosphere and the ventilation air.

So, yeah, we are looking at things we can do with existing WIPP technology. The heat is really what’s driving everything here.

EWING: To follow up on Sue’s question, one of the previous speakers gave us a nice summary of all the previous work, heater experiments in other countries, and so on. Aren’t those data somehow useful for testing your model and--

STAUFFER: Yeah. And I’m working with Kris to include some of that.

EWING: Okay, good. Other questions? Mary Lou?

ZOBACK: This may be--Mary Lou Zoback, Board--may be the same question, but just for my own edification, in the German Gorleben mine, did they have hot waste down there?

STAUFFER: Yeah, they had hot waste, but the water contents there are so low that--

ZOBACK: Oh, that’s right. This is a dome salt.

STAUFFER: We’re less than one percent here, so we wouldn’t expect to see anything. It’s why this--there’s one piece missing. It’s the hot waste in the bedded salt lying on the drift floor with the crushed salt, because if you don’t have the crushed salt, then you don’t have the porosity with the boiling front moving through it to give you this effect.

ZOBACK: Okay. Then a related question: When the Germans began their program and they thought that that site was going to work, what kind of monitoring did they put in initially?

STAUFFER: Kris?

ZOBACK: Maybe we’ll hear that in the later talk.

EWING: But, again, maybe a lesson from your modeling is, the lower the amount of water, the simpler the system is; right?

STAUFFER: Yes. Yes.

EWING: Yeah, okay.

Other questions?

FRANKEL: Do you have some--this is Jerry Frankel. Do you have some measure of the overall impact of this heat pipe effect on the macroscale on the proposed repository?

STAUFFER: You mean on the--no, we haven't--we've done these calculations for this system, which is the five heaters in one little drift. We have not gone--

FRANKEL: So even for the five heaters, is there some measure that you can--other than the local gradients, what's--

STAUFFER: Well, the maximum temperature--the impact on maximum temperature was pretty dramatic, you know, but I expect like in the three- to five-meter region away from where these drifts are that the impacts are going to be pretty small.

FRANKEL: But that means you'd be able to put them closer then.

STAUFFER: Yeah, you can put them closer together if you can rely on the heat pipe. But I would not suggest relying on this heat pipe effect. It's really something we have only seen in the simulations.

ZOBACK: Mary Lou Zoback, Board. That's much, much hotter canisters than most of the defense waste liquid.

STAUFFER: Yeah, this is at the very high end. There's only maybe 100 or 200 at that heat mode. That was back here. Those are these guys over here, so there's less than 500.

But 500 is still a lot.

ZOBACK: Yeah, I know, that is--

EWING: Let me check and see if there are questions from the Staff. Yeah, Bobby.

PABALAN: Roberto Pabalan, Board Staff. I’m just curious. Can you take your model to a much smaller scale, a scale of a single mineral scale that Florie showed earlier, and then simulate what he observed, the movement of the liquid phase towards the heater source and then the gas phase away towards the cold side of the mineral?

STAUFFER: This continuum scale model is not the appropriate tool for that. But on our team we have Qinjun Kang, who does lattice Boltzmann modeling, which is at the sub-millimeter scale, and he’s got simulations of Florie’s experiments. He’s been able to recreate the single phase, but is working on the two-phase. It’s a more dicey problem to get the vapor going the opposite direction.

But, yeah, we’re working on that.

PABALAN: Okay, thanks.

EWING: Other questions from the Board? Staff?

All right, thank you very much.

The next speaker is Guadalupe Arguello. It’ll be another coupled model, but this time thermal, hydrological, and mechanical processes.

ARGUELLO: Thank you. It’s an honor to be here before the Board and talk a little bit about some of the work that we've been doing.

I'd first like to acknowledge my co-workers, Jim Bean, John Holland, and Jonathan Rath from Sandia, as well as contributions from numerous others who are currently at or formerly retired from Sandia, as well as many contributions over the years from RESPEC. We've been doing this sort of thing for a while now. Actually, I was hired on to the WIPP project in 1985, and I've been on and off working with the WIPP project since then.

As a brief outline, I'd like to talk a little bit about Sandia's historical efforts related to salt repositories, particularly from the geomechanics perspective; talk about the next generation of high-performance computing, the efforts and the technology that we are putting into that; talk about additional work on salt for high-level waste repositories; and then show you some demonstration problems to demonstrate the capability that we currently have in our codes; and then offer a summary and some conclusions for your consideration.

So with regard to the historical perspective, again, it's fortunate that there were several speakers that already showed a schematic of WIPP. The only thing I want to point out is the area up here. It's an early experimental area, and so I'll be focusing or talking a lot about that in the subsequent figures.

So here it is. Early on Sandia was tasked with developing technology for predicting geomechanical response of rock salt, in particular, looking at thermo-mechanical, particularly with regard to creep models, how the material behaves, and then as well as looking at solution algorithms, codifying all of that into usable computer codes that we could use for the prediction of the response out at the underground.

In concert with that, there were these TSI full-scale experimental rooms that were fielded at WIPP, and one of the objectives of those rooms was to look and evaluate the predictive models and the techniques that were being developed. And I'm pointing out two rooms here. And Kris has already talked quite a bit about Room B, but a twin room was Room D. The only difference between these two rooms was that Room D was an isothermal room, so you could do direct comparisons between the isothermal case and the over test or heated case.

This shows a schematic of what the stratigraphy looks like around WIPP. So roughly this model includes 50 meters above the room and 50 meters below the room, and this is the configuration typical for Rooms D and B. Again, Room D is the isothermal case. Room B has heaters in the floor, as was shown earlier by Kris. What you will note is that this is a layered stratigraphy, so we’ve got all kinds of material in here, including argillaceous salt, clean salt. There is some anhydrite and some polyhalite. In addition to that, there are multiple clay seams or stringers running throughout the configuration here. And, in particular, for the geomechanical model, these strings are modeled as signing (phonetic) surfaces, so these can move relative to one another as the deformation of the room proceeds.

ZOBACK: Mary Lou Zoback, Board. How thick typically are these clay seams?

ARGUELLO: So the seams can vary from roughly three millimeters to feet.

ZOBACK: Okay, thank you.

ARGUELLO: So all of the data that was taken for Room B--and it was about 1,500 days’ worth of data--was reported by Darrell Munson back in ’88, and that data is out there and archived and available for use. And, in fact, we are using it for the current state-of-the-art models that we are developing. This shows an example of that technology relative to the previous legacy generation of codes, and these were some calculations put together by Darrell Munson and RESPEC. And it shows that the model does a fairly good job of doing room closure.

The second room is Room B. Again, Room B had a heated 74.4-meter test section that was uniformly heated with these heaters in the ground. There were also guard heaters on either end, and here at the entrances were insulated doors to preclude heat flow out of the room. Again, the data for that is in this over-test for the simulated defense high-level waste report, again put out by Darrell Munson in the ’88 time frame. And the figure here shows a picture of the room as it was being constructed.

As with Room D, Room B was also used to look at the predictive technology in terms of temperatures and in terms of closure again. And this closure figure shows the isothermal room relative to the heated room. One thing you will notice is, for the heated room there were significant deviations at some point in time. And what Darrell attributed this to was that you started getting microcracking right in here up in the roof, and then there was a full-scale separation of a roof slab starting to occur in this region. And, of course, the models that we had and that we currently have in the U.S. are macroscopic models that account for only primary and secondary creep. And we’ll talk a little bit more about that in a while. But they don’t account for damage or eventual rupture.

ZOBACK: So this is brittle failure.

ARGUELLO: So it is creep rupture basically. What happens is--or what they think happened is that there were separations at one of the seams at the roof, and you started getting microcracking in the salt, and eventually the entire roof separates.

So in addition to Rooms B and D, there were various other WIPP experimental configurations that were also simulated for comparisons with the measurements, and all these were documented in this IJJM report from Darrell Munson back in 1997.

Now, I should point out that development of our models kind of stopped in the early ’90s with the WIPP licensing coming on board and the transition from a pilot plant into an operating repository. A lot of the research activity was no longer carried forward.

But since the mid-1980s there has been approximately 30 years of software and hardware advances that have transpired. And Sandia has built a new generation of massively parallel multi-physics capabilities into a single computational framework to support Sandia’s engineering sciences missions through the Advance Scientific Computing Initiative. And most of the effort here is related to our weapons side, so that was the real driver there.

What we’re doing is, we have recently started and are currently trying to adapt these tools for simulating coupled geomechanics for waste repository settings. A lot of this work is funded under LDRD and then, of course, the used fuels disposition campaign. The figure down here on the lower left shows an early panel seal calculation where we actually started doing 3-D modeling. So prior to this, most of the modeling was 2-D, because the computing capability and all of the tools that had been developed were mainly 2-D. So this was some of the earliest 3-D calculations that we did.

So in the recent past and relatively recent, state-of-the-art is such that it integrates single physics codes to achieve coarse spatial and time scale simulations. And what we're doing is we are proposing SIERRA Mechanics as the future by leveraging the more than ten years of ASC development, and SIERRA Mechanics then provides the framework for coupled multi-physics simulations in a massively parallel environment; scalability from one to thousands of processors on a variety of platforms; and we're using it as a launching point for eventually getting to a fully integrated THMC coupling with adaptive solution control.

In particular, for the repository side, we are using two applications codes. One is known as ARIA that handles the thermal, hydro, and chemical. And I say handles the thermo-hydro-chemical; that's on the weapons side it currently handles a lot of that. On the repository side, as I said, we're working on that. And ADAGIO for the mechanical part of it. So this is a quasi-static code.

So both of these and all of the application codes within SIERRA sit on top of a foundational tool kit that provides parallel (inaudible) utilities and services, including fuel data management and transfers among the various applications codes.

So here is how SIERRA Mechanics does the coupling between the applications codes, at least what we're doing currently on the repository side. So ARPEGGIO is the transfer module that handles the transfers of data between the two codes. So ARIA solves the conservation of component mass (water and air) equations for two-phase porous flow plus energy equation on a deforming computational grid. ADAGIO solves the conservation of linear momentum equations for quasi-static conditions. Imbedded within ADAGIO is the constitutive model for salt that's been implemented in this library called LAME.

And then temperatures that are used in the constitutive model for the salt materials, that constitutive model again is implemented within the LAME library with ADAGIO. The displacements from ADAGIO are used to updated the ARIA geometry, and then you proceed forward if it's a thermo-mechanical calculation. If it's a thermo-mechanical-hydrological calculation, then you can pass out the other information such as porosity, thermal conductivity, and so forth. And that will then update your pore pressures, and you pass them to ADAGIO, and ADAGIO passes back the nodal displacements and so forth.

Now, as I mentioned, these are all relatively recent adaptations to SIERRA Mechanics. So one of the things that we wanted to do was to actually run and do a preliminary validation of SIERRA Mechanics against WIPP’s Rooms D and B.

And if you can hit the top figure, that’ll show you--so this is the WIPP Room B calculation. And for the first 384 days or so, nothing happens because it’s isothermal. You are getting some creep, but you can’t see it at that scale on the top. And there comes the thermal pulse from the heaters being turned on, and you’re approaching the end of the simulation.

FRANKEL: Can you clarify what we’re looking at here?

SPEAKER: What’s up and down?

FRANKEL: Yeah, where are we?

ARGUELLO: Okay. So these are the heaters in Room B in the floor. So we’re looking at a slice through the middle of the room. This is a symmetry boundary condition. So if you flip this around, there is your room right in the middle. Okay?

BAHR: So the little gray indentation--this is Jean Bahr--that’s the room?

ARGUELLO: That’s the room right there, which, when I zoom in, you’ll see it here. Okay?

ZOBACK: Mary Lou Zoback, Board. That’s Room B?

ARGUELLO: This is Room B.

ZOBACK: Even though next to it the plot says Room D?

ARGUELLO: Well, I’ve shown both. I’ve shown Room D up on top and Room B here at the bottom.

ZOBACK: And X and Y are horizontal axes?

ARGUELLO: X and Y are--well, X and Z are horizontal axes. Y is up and down.

ZOBACK: Y is the vertical axis?

ARGUELLO: Y is the vertical axis. Okay. So here is a zoom of this area. And what you will notice is that there is movement at those clay seams around the room, and you will see it as soon as the heat comes on.

(Pause.)

So here comes the heat. You start seeing the room really deform, and you start seeing some movement up here and movement of the layers in here and down here. And these are quantitative comparisons of room closure to data. So this is for the isothermal room, so there is the vertical room closure and horizontal room closure. And this is for the heated room, again vertical closure compared to data, horizontal compared to data. So this was the first preliminary validation that we did against all the tools that we had migrated into SIERRA Mechanics.

ZOBACK: I’m sorry, I’m trying to grasp this. It seems important. Mary Lou Zoback. So in the unheated room--

ARGUELLO: Right.

ZOBACK: --the data points with the lines connecting them are your--
ARGUELLO: So this is the prediction, the calculation.
ZOBACK: Those are the predictions.
ARGUELLO: These are the data that were collected from the measurements.
ZOBACK: So you can't predict the deformation without any heat, so why--I guess I'm confused why the model can't even predict the no-heat situation.
ARGUELLO: Well, it depends on what you mean, you "can't predict" it. You can't predict it exactly, and I think that's always--
ZOBACK: Well, you don't show any error bars. It looks to me like there's a--
ARGUELLO: Right, right. So--
ZOBACK: --10 percent--10 to 7 or 8 percent difference in the simplest possible case.
ARGUELLO: Sure. So this was the preliminary comparison against our code. We have since gone back and we can again do pretty well on the vertical closure for the unheated room.
ZOBACK: I'm sorry, I'm skeptical of models. And, yeah, once you have data, you can go back and add something to the model to make it fit.
ARGUELLO: Well, what I can tell you is that in this particular case in the preliminary calculation, we were

having some problems with the contact algorithm. And a lot of the deformation of the rooms is, in fact, intertwined with the movement of those surfaces (inaudible).

ZOBACK: So, again--Mary Lou Zoback, Board. And so you have every one of those clay seams in as a sliding surface?

ARGUELLO: That’s correct.

ZOBACK: (Inaudible) surface?

ARGUELLO: Not every one of them. We have the nine that are closest to the room.

ZOBACK: Okay. And on the thicker ones, is there sliding at the top and bottom or (inaudible) distributed--

ARGUELLO: No, only at the bottom.

ZOBACK: Only at the bottom.

ARGUELLO: Because typically up at the top they transition into anhydrite or something that makes full contact with the suggested salt.

ZOBACK: Okay, thank you.

ARGUELLO: Which gets back to the question. Modeling salt behavior correctly is very important to us. Salt constitutive modeling is very important, and it actually forms the basis for U.S.-German collaborations. Our constitutive model development effort, as I mentioned earlier, stopped in the mid-’90s; but the German development continued. The MD model, Multi-Mechanism Deformation model, is currently in use in our high-performance codes in SIERRA

Mechanics.

There was some initial work done on something called the MD Creep Fracture model, but it was very immature when all of the development ceased. And so we never put this one into the code, because it was immature, and it was not robust at all. So our current situation is that we have the MD model, which only models primary creep and secondary creep.

So we need to assess the international capabilities. We need to examine potential development of our model and evaluate other existing models. So the German models that continued under development have included features including damage and fracture and so forth, and so that is one of the primary reasons that we're looking at them. So we want to identify the best features and the deficiencies of these models, and so that's why we are proceeding forward with this collaboration with the Germans.

Incidentally, this is--I noticed that, Professor Zoback, you have something like this there on the front of the desk.

ZOBACK: Well, it's over there now. We're passing it around.

ARGUELLO: So this is the original core or an original test specimen that was tested. This is triaxial test sample of WIPP salt at 3 MPa confining stress. So when you apply

3 MPa confining stress and you triaxially test it, you get a significant amount of deformation.

So the participants in the current collaboration with the Germans on the behavior and healing of rock salt are the following. There are several--well, there is a scientific consultant, and there are several university institutes as well as a private institute that is working on this particular project. Sandia Labs joined in fiscal year 2010, as did the Technical University of Braunschweig.

So what this means is that we have basically have access to six German groups and their models and their test capabilities. The previous slide showed you a sample of a triaxial test. That triaxial test is actually being conducted by the Germans to fit the data to their models. So that’s WIPP salt.

So the joint project started out in 2010, and it was supposed to go through 2013 and end at 2013. And, of course, the focus was on thermo-mechanical behavior and sealing and healing of salt. And so the sorts of comparison calculations that we had on this particular collaboration was that it was, of course, a benchmark comparison. The three calculations that we were comparing were the borehole conversions at the Asse mine, the same borehole that Kris talked about earlier, and then the heater experiments conducted at the Asse mine, which were conducted later in time than this one but in the same hole, but now with heaters--and I will show some of those results in a minute--and then an in situ calculation of the so-called bulkhead experiment at the Asse.

As I said, it was supposed to end in 2013, but recently there have been some additions to the originally three proposed problems, and we have included WIPP Room D and Room B. And this was included as an extension from the German Ministry of Technology to fund the Germans to perform these benchmark calculations.

In addition to the benchmark calculations, there were additional testing of both clean and argillaceous WIPP salt that was needed for those models, and so they are undertaking a series of tests. And I think Frank will talk about more of the details of those in a minute.

BAHR: This is Jean Bahr. These drifts were excavated over a hundred years ago, 1911, 1914?

ARGUELLO: So in this one, in this particular case, remember Asse was a producing salt mine before they did anything.

BAHR: Salt mine, right.

ARGUELLO: This particular one, this third one, the drift was excavated then. And then they put in a cast iron miner in there. And so this is being used as an analog to what sorts of healing effects have occurred.

BAHR: Okay, I just wanted to make sure that wasn't a typo and that this really was a very long-term deformation experiment.

ARGUELLO: Right.

BAHR: Thanks.

ARGUELLO: So let me get into some of the demonstration problems. So this is the first Joint Project III target simulation. The so-called isothermal free convergence, so it's that borehole problem, but the isothermal portion of it. This is the borehole that Kris talked about earlier, and this is where the isothermal free convergence test was run at the lower part of that hole. A few years later they came back and ran a heated borehole calculation up in this level, so that means that you had to account for the isothermal deformation of the hole here up until the time when they installed the heaters and the subsequent deformation thereafter.

Now, for the isothermal free convergence case, what we did was we used the mesh details and boundary conditions shown up here and then Asse Speisesalz properties but with the MD model. And because the tests that were run on the cylindrical samples for the Asse Speisesalz were from all over Asse, it was permissible, according to the rules of engagement for the benchmark problem, to go ahead and adjust one of your parameters to calibrate your model for the isothermal case. And so this is what this shows is that we're calibrating the MD model to the isothermal case, and we see it there. Once you--

BAHR: What parameter was that that you modified?

ARGUELLO: Yes, it was the secondary creep parameter.

So now let's go to the heated free convergence probe test, so this is the HFCP test run at a shallower depth in that same borehole. So there were roughly 1,309 days of isothermal convergence of this hole. So what you're seeing here is a sliver of the hole. In this section right here is where the heaters were installed, and this shows the various boundary conditions for that.

In addition, this shows you the meshing that was used in our model for this. And what you see is that we had to model the first 1,309 days of isothermal convergence, and then from 1,309 to 1,328 it was heated at that specified temperature. And then from 1,328 to 1,331 days it was adiabatic. We just allowed it--the heaters were turned off, and we allowed it to come back to its natural thermal condition.

Let's see, if you can run the upper problem, so this shows you that simulation. And I'm only showing you the non-isothermal portion of it. We've got the isothermal in there, but I didn't want you sit there and wait for 1,309 days for the heater to come on. This is a close-up of that, and here you can see the borehole. And what you see is that, indeed, there is more convergence in that heated area, as you would expect.

Again, here is a quantitative comparison of how we did and how we did relative to the German groups for the thermo-mechanical case. So this is the data that was taken by the ECN. It was a Netherlands--I can’t remember what the name of the group is. And these are the various computations. And this is Sandia right here, that purplish violet line.

So, as you can see, there is some scatter with all the predictions. You can’t expect it to hit right on.

ZOBACK: Can you just--Mary Lou Zoback--the dimensions, how deep is the borehole? I have no idea what the scale is (inaudible) looking at. I can’t read it.

ARGUELLO: Sorry. So the modeled portion was 20 meters. The central 3 meters were heated. And this was at an elevation, let’s see, of about a thousand meters underground.

ZOBACK: Oh, okay.

ARGUELLO: Because the hole was drilled from a chamber within the Asse mine.

ZOBACK: Right, right, I got that. Okay. And the diameter of the borehole?

ARGUELLO: The diameter of the borehole is roughly a foot, .315 meters.

ZOBACK: Okay. So the displacement’s convergence--
ARGUELLO: Right.
ZOBACK: --even though it looks like it’s expanding the way it--
ARGUELLO: Right, right, the way it shows it, yes.
ZOBACK: Thank you.
ARGUELLO: Yeah, so it’s convergence (inaudible) borehole.
ZOBACK: Four centimeters.
ARGUELLO: Right.
ZOBACK: In a one-foot-diameter hole.
ARGUELLO: That’s right.
ZOBACK: Okay, thank you.
ARGUELLO: One final demonstration problem I wanted to show you is a coupled thermal-mechanical simulation of a generic high-level waste repository. So this was basically a scoping calculation that we had done earlier, so I wanted to preamble it as such, that it was simply a scoping calculation that we did, trying to get some idea of how things would behave down there.

So the sample geometry is based on a configuration based on a 2008 Savannah River study using vitrified borosilicate high-level waste glass canisters with an output of 8.4 kilowatts. Now, we have since learned that that’s pretty high for typical waste that has been aged. But, again, that’s what we used because this was simply a scoping calculation.

The technical challenges in this problem was high thermal gradients, temperature dependent material properties, large deformation salt creep behavior, contact modeling with heat conduction and load transfer, and long duration simulation to room closure. So we wanted to allow the room to close completely; and by “close completely” we mean whenever the roof and the floor touch, we’re going to call that closed. Now, that’s not completely closed, but it’s a measure.

So this is what the repository plan view looked like, so it’s a bunch of alcoves drilled into the side of these rooms or access drifts. And if we cut one of those through the middle, then this is what the configuration looks like. The high-level waste canister here is here in the far back corner of the alcove, and then there is a drummage canister here that we just included in the calculation.

The whole thing is covered with crushed salt here. And, again, these are some of the details of the configuration, symmetry planes here and here, symmetry planes here in the back. This is the access drift coming through here this way. So we’re looking at one of the alcoves. Here is the alcove itself, the waste canister, and then the crushed salt on top of the waste canister. We used experimental pressure-volume strain curves for crushed salt at 200 degrees C just so that we--again, this was a scoping calculation, so we wanted to get something reasonable. This is the decay curve, normalized power curve, for the high-level waste canisters.

And so the thermal analysis--and these were two separate grids that were used, one for the thermal, one for the mechanical. The thermal analysis had basically 905,000 nodes, 865,000 elements. The structural or mechanical portion of the analysis had 295,000 nodes and about 280,000 elements.

And we actually used a couple of constitutive models for this. One is the full MD model, which is more expensive than something we call Power-Law Creep, which includes only secondary creep. We wanted to--again, we had just put a lot of this stuff in, so we wanted to get some idea of what the differences were in performance for the code, and so this was a way that we could do it. And then what we did was we modeled PLC by itself but by setting some of the MD parameters to zero so that it would mimic the PLC. So that’s kind of a preamble to what we’re talking about here.

If you can play this figure, and I want you to pay attention to this, because it’s going to--we’re going to call it closed when this comes in contact with this.

(Pause.)

Notice that there is still a gap here at the access drift/alcove intersection, which would be non-intuitive. You would think that at the intersection is where it would close first, but the problem is, you see in the rib there it's pooching out into the intersection and preventing it from closing. And you wouldn't be able to see that without the 3-D model.

If you could play the next one?

So this shows you the response of the crushed salt backfill as it compacts, and what you see are gradients of porosity developing in the crushed salt. And these are consistent with experimental measurements that have been observed in the BAMBUS-II experiments. So we have a feeling that we're doing things correctly here.

So I get now to summary and conclusions. So we've made some significant strides in adapting SIERRA Mechanics for repository applications. Basic multi-physics capability has been demonstrated, but significant work remains to make it more general and accessible in a production-type environment. So we've got a code; we've adapted it to do repository types of problems; but I think we need to harden that into making it more robust and so forth before we claim we have a production capability.

We've done some preliminary validation SIERRA

Mechanics, as I’ve shown. More is needed, particularly for the non-salt part of it. So we’re thinking of this as a tool for any salt geology, but the most validation that we have done to date is in salt, because that’s what has been our forte in the past.

We continue to work on providing state-of-the-art leading-edge constitutive models for use in repository applications.

International collaborations are very important and are allowing us to leverage against many ongoing efforts, particularly with the Germans, but we’re also in contact on the other side with, say, the French and the Spaniards on the other geologies.

Testing and modeling of WIPP salt performed by the German research groups is of enormous value to generic salt repository science.

So, with that, I’ll close and I’ll take whatever questions you might have.

EWING: Okay, thank you very much.

Questions from the Board? Jean?

BAHR: Jean Bahr. You showed comparisons of your model simulation results with a number of German codes. Are the differences in the results a function of different physics that are in your models, different grid resolution or time step issues, different parameterizations of the properties and functions of how properties change as a function of temperature and pressure and those kinds of things?

ARGUELLO: Yes, all of the above. So, for example, the Germans use FLAC; they use ADINA; and there's one or two other codes that they use. We use SIERRA Mechanics. They had different--each of those six groups has a different constitutive model, so there are different constitutive models. And so there is those differences. Each of those models is then parameterized from the data that you get from these samples, and the parameterization of those models is going to vary according to the different model.

And so, yes it's a combination of all of the above.

BAHR: So if you have different constitutive models or sort of the underlying physics is different, how do you determine which is the correct underlying physics? Because each of the models has parameters and knobs that you can twist to get a better fit, so it's possible that you could get each of the models to fit quite well, and the goodness of fit is therefore not a deterministic--is not going to discriminate between which one is the--

ARGUELLO: Sure.

BAHR: --best in terms of the physics.

ARGUELLO: So the underlying physics in all of the models is the thermo-mechanics and the solution of the thermo-mechanical equation. So that is consistent across the board. Now, the way that you model each of those processes is slightly different because of different formulations of what they're including in the models. Some are time-based creep models; others are what people like to call first-principles mechanistic models. And so there's those differences. And, quite honestly, I would tend to believe a more mechanistic model that you can trace back to something like a mechanism deformation map or something like that.

So the MD model is based on mechanism deformation maps; so is Humple's model; so is the IFG model. So those are three models that I would say are trying to do something other than CIRFE.

BAHR: Are there experiments that one could do that would help you decide which of those models is the best representation of the underlying mechanisms?

ARGUELLO: Yes. So all of these are macroscale models, and so the tests that you're seeing done are either the lab tests that are done--and so one of the first things that the Germans do, for example, is to compare against lab testing, against a multitude of lab tests. So there's a whole suite of lab tests that are run, and they compare against those; likewise with us.

And so is there a test that you can do to differentiate among all of them? Not a straightforward one that I know of.

EWING: Efi?

FOUFOULA: Efi Foufoula, Board. So the difference between your presentation and the previous one and the long title was one word difference, mechanical here versus chemical. So my question is--and I would appreciate some insight--how much do you coordinate? I mean, we heard new innovations in the grid generation scheme that they start this year and so forth, and there is a lot of common elements in the platform of the modeling. Do you envision that eventually will be a coupled thermal-hydrologic-chemical-mechanical model?

ARGUELLO: Ideally, that would be the best option. But the realities are that the structures of the codes are vastly different. I'm not familiar with what the structure is there with Los Alamos, but I do know that with Sandia we have tried to incorporate the most current state-of-the-art things that will allow us to do massively parallel. So you could go and do a full-scale room and not have to idealize it as something smaller because of the architecture of the software.

FOUFOULA: No, I fully understand that, and it's beyond trivial to make two codes that have different architecture. But my question addresses should we eventually look for a common architecture and a modern framework for both components, or they should be taking their own paths. That's a longer--it's a philosophical question probably.

ARGUELLO: Yes. And it’s probably beyond me, because I’m not really a code developer. So I know kind of how the code is developed at Sandia. Not being a co-developer, I’m not familiar with how FEHM is developed or whether it has parallel capabilities and all of that. And so it may not be ideally--yes, we would like to do that, but it may not be as simple as that.

EWING: Mary Lou?

ZOBACK: Mary Lou Zoback, Board. This is really good work, and I sounded a little skeptical before, but I think it’s good to stay a little skeptical of models always. But I want to applaud you for the close collaboration with the Germans, because they’ve got strong interests, really good lab capabilities, and access to date in the field. So I think this is all great.

But getting to this idea--so I always forget that Sandia and Los Alamos aren’t--to me, they are all New Mexico, but I think they’re two separate labs, and you--I mean, you’re both computing temperature. Have you guys tried to run the same geometry, same heaters, stay below the boiling point so you don’t get into vapor and all that stuff; and do you get the same temperature fields?

ARGUELLO: No, we haven’t done a common problem that I’m aware of on the repository side. I know that in other areas we have.

ZOBACK: Okay. It always helps to see the collaboration between the labs. We appreciate that.

ARGUELLO: So in other areas we can talk about, we’ve done that.

ZOBACK: Okay, good. Thank you.

EWING: So I have a little bit of a follow-up question to previous questions. So this is very impressive, and, like others, I would compliment you for the work and the work at Sandia. And you’ve shown that you can apply these models to a waste repository situation, and you mentioned applying the model to other rock types.

But how focused are you on the problem of disposing of high-level waste and spent fuel in salt? And what’s behind that question, what is interesting to me is that the models involve taking a stainless steel container and putting it in contact with brine, and yet that seems to be something for someone else to do.

So do you think about the disposal problems, or is the work focused on “my model can do a part of the problem for waste disposal in salt”?

ARGUELLO: Up to now, because there isn’t a site that has been selected, we kind of have to do generic sorts of studies.

EWING: Right, generic, but we know that bedded salt has brine as small inclusions or as large pressurized brine pockets. So if there's a fluid present, it will be a brine most likely.

ARGUELLO: Right.

EWING: And just the juxtaposition of brine and stainless steel strikes me as a question crying for a program. And when you have it coupled to chemical processes, that to me seems like a pretty relevant and important chemical process that will change everything else in your canister. So that's a rambling comment. But I think we have to finally in the future as we go forward trying to dispose of waste, that somehow has to be the end goal of these studies, my personal opinion.

ARGUELLO: Absolutely, yeah.

EWING: Other comments? Mary Lou?

ZOBACK: I meant to ask this before, too. Can you describe as much as you--well, in a limited time period--what do you know about the rock fall that was in one of the experimental drifts, and it happened how soon after the drift was opened? And I didn't appreciate until your talk--I hadn't heard from anyone the fact that there's these layers of clay that are acting as shear surfaces.

So once it fell down, you could go up and look where it fell from. Was it out of the clay? Was the top boundary a clay layer? I mean, what did you learn from that occurrence?

ARGUELLO: So the only one that I am at all familiar with was the one in Room B, but--

ZOBACK: Room B. Okay

ARGUELLO: But Room B was an accelerated test, and so as soon as--even before the top fell, they closed off the workings.

ZOBACK: You mean they shut it off so you couldn’t go back in there?

ARGUELLO: Right. Right.

ZOBACK: But it happened after it was heated?

ARGUELLO: Yes. So the heating accelerated--

ZOBACK: And had the heater been shut off when it fell down, or was it still heating?

ARGUELLO: I don’t know if the heater had been shut off.

ZOBACK: Is there a paper we could read about this?

ARGUELLO: I don’t know. That was in Room B, Peter?

ZOBACK: I thought someone promised us we were going to hear about this later today, one of the early speakers. It seems relevant considering what’s happened in the past month.

ARGUELLO: Yes, sure. So this was, you know, in the ’80s. I presume that there was a report written with regard to that, but I don’t--

ZOBACK: Could someone from Sandia make it a task to get us a copy of the report on the--

ARGUELLO: Yeah, okay.

EWING: Okay, thank you. Other questions? Jean? Oh, I’m sorry, Jerry’s been waiting.

FRANKEL: Actually, my question is related to--Jerry Frankel. My question is related to Mary Lou’s question about the roof. You’re using primary and secondary creep in your model, and you show nicely this is just, like, collapsing down together. But you talked about other models that have damage and fracture components in the back. Your vertical closure, you talked about creep rupture.

So if you have a chamber that you mine out and leave it or heat it and leave it, will there be failure, or is it just going to--

ARGUELLO: No, no, no. You will have failure.

FRANKEL: You’ll have failure?

ARGUELLO: You will have failure.

FRANKEL: Failure by what mechanism then?

ARGUELLO: Well, it depends. If it’s a bedded salt repository, you’re probably going to have the sides slabbing, the roof dropping. But, you know, hopefully it’s not within the first five years or whatever.

FRANKEL: And those are gravity effects. So in your vertical borehole--

SPEAKER: It’s stress concentration.

ARGUELLO: Yeah, heat, stress concentration, separation of the clays.

FRANKEL: But in a vertical borehole, would you expect then--

ARGUELLO: No, you wouldn’t--

FRANKEL: They would just--you would expect it just to seal up?

ARGUELLO: You would expect it to seal up. You might have some surface crumbling, but--

FRANKEL: And all the rock bolts that are holding the ceiling up, it’s just going to creep right around those rock bolts? Would that affect your models at all?

ARGUELLO: Okay, so the way this works is, you get relaxation around the openings. Here’s your opening. And that wave of high stress moves further and further away from the opening, because what the opening is trying to do, it’s trying to get back into a hydrostatic condition. Once it gets fully closed, then there’s no more shear, so it won’t creep and in that setting.

So what happens is that the stress wave, if you will, goes further and further out. At some point you will go beyond where the rock bolts are. Now, if the stress hasn’t diminished to the point that that material can support the weight that is being held together by those rock bolts, then you will have a separation.

FRANKEL: You could have a big drop.

ARGUELLO: Right. And that is the mechanism for encapsulating this. So you have a big drop; eventually everything starts closing together again; and you get eventually to a hydrostatic condition in salt.

FRANKEL: As long as you don't break anything while you're dropping.

EWING: With deference to Jean, I am going to call the questions to an end to keep us on schedule to save time for public comment at the end. But, Jean, of course, after we start the break, you can ask your question.

BAHR: Okay, sure.

EWING: So we'll have a break now, and we'll begin at 3:05 when we reconvene.

(Whereupon, a break was taken.)

EWING: The next speaker is David Sevougian, and he'll be presenting Performance Assessment Modeling of a Generic Salt Disposal System.

SEVOUGIAN: And Rod just stole my first line, which was to read the title. Perhaps a more appropriate title would be high-activity waste, since our first application of this is to the disposal of spent nuclear fuel in a generic bedded salt repository. And also much of what I will discuss is applicable to either crystalline or argillite concepts.

I'd like to acknowledge a number of the other participants in this activity, including our DOE colleagues, several of our Sandia colleagues, including Geoff Freeze, Payton Gardner, and Glenn Hammond. I'd like to acknowledge Peter Lichtner and our colleagues from Los Alamos and Lawrence Berkeley.

My first slide is the outline, and this is just a brief summary of what I'm going to talk about today. I'm going to first talk about the objectives of the activity, some of the methodology for the model and code development, including a discussion of a reference case for bedded salt. I'm going to talk about development of the PA code in a high-performance computing environment. Then I'll give an example demonstration based on the reference case using the modeling system. And my last slide will be a brief discussion of some integration with a source term process model that models spent fuel degradation.

So as far as the objectives of this work, I have outlined three. Really, the main goal here is to develop a flexible PA capability that readily evolves throughout the program life cycle, and we have a long life cycle ahead of us, including site selection, characterization, licensing, construction. During all those phases of repository development, we'd like this PA model to be able to evaluate a variety of sites for disposal of spent fuel and high-level waste. Right now these are generic sites, generic reference cases.

Another important activity, an objective of performance assessment modeling, is to support prioritization of research, development, and demonstration activities, at first generic, which is where we’re at now, later site-specific. And another key aspect of performance assessment modeling is to support the safety case during all phases.

What I show on the bottom here is--I don’t expect you to read this. If you have a handout, you might be able to read it. This is a diagram, a flow chart, of the iterative performance assessment methodology that we’ve been following at Sandia for 30-some years. You’ve seen applications related to WIPP. We used the same methodology on Yucca Mountain. And in some later slides I’m going to focus in on some of these steps with another flow diagram. And I’m trying to show here how the performance assessment feeds the evolution of the safety case, so its iterative performance assessment is a key aspect of the safety case, not the only aspect.

BRANTLEY: Actually, you can’t read the handout either.

SEVOUGIAN: Well, it’s actually--

BRANTLEY: I can read it on the PDF.

SEVOUGIAN: Actually, I don’t want you to turn to it, but there is a backup slide with a bigger picture of it. So if they printed the backup slides, and I can send it to you anyhow if you’d like.

This slide, Slide 5, is really the crux of my talk, so let me just spend a little time on that. The main thing about the development methodology that we are working on is to have a direct representation of multi-physics coupled phenomena within the PA simulations, within the model, the code, the simulations themselves, the idea being that this minimizes the use of conservative assumptions, simplifications, abstractions, and allows us to include a realistic representation of spatial heterogeneity of uncertainty of the features, events, and processes.

And what I'm showing here just in case people are unfamiliar with this is the main parts about a probabilistic performance assessment. We start with many input parameters, because there's many processes, many domains. So we try to accurately characterize the uncertainty, both the aleatory and epistemic uncertainties; we have some kind of integration or sampling routine; and then produce multiple representations of the performance of the repository, here shown as dose versus time. And the main point is to include coupled multi-physics over a large domain and actually many sub-domains over a probabilistic simulation. The use of high-performance computing architecture will facilitate that.

One thing we always ask ourselves when we're modeling performance assessment is: To what degree do we include these phenomena in the performance assessment model versus the supporting process models that you've heard described today, like in the last two talks, Lupe’s talk and Phil’s talk?

So the idea is that we use our process-level understanding that’s been developed over many decades to determine what fidelity of the model components, geometry, mechanisms do we need in the performance assessment code. And in this regard, of course, performance assessment is a much longer time-scale than some of the processes, so it’s a function of not only the importance of the underlying process to the overall performance of the system, but also a function of time-scales.

And I’ve just taken a couple of snapshots of Lupe’s movies that show the evolution of the backfill when heat-generating waste is emplaced. And over a period of 200 years, the porosity decreases significantly; but 200 years is a short time-scale over a span of a million years. The point I’m trying to make is that with a high-fidelity multi-physics-capable performance assessment code, we can then analyze whether or not these processes need to be in the performance assessment and in what fashion they need to be included.

I’m going to switch gears a little bit here. I’ve got my other flow diagram I’ll go over in a minute. This is the PA methodology flow diagram blown up, and I’d like to talk first about the reference case that we’ve developed for salt. The reference case is a surrogate for site- and design-specific information that's not available right now. It documents the information and assumptions that are needed to evaluate a generic disposal system, and it helps ensure consistency across the various modeling disciplines, including performance assessment, process modeling uncertainty, and sensitivity analyses of the results.

So the major steps I've shown are the reference case, which again is a surrogate for the design and the site; and then the FEPs process, which I'll go through in a couple of slides; and then these developed guidelines for constructing the model and the code. And then once that's completed, then you have the disposal system evaluation.

So on the reference case the major components are the inventory that we are using, various features of the engineered barrier system and the natural barrier system, the concept of operations, the biosphere, and the regulations, which I'm not going to talk about the latter today.

The first part I'd like to talk about is the natural barrier system in our reference case. And we've looked at a number--I don't know if you can read this. These are the major salt basins in the U.S. We've looked at those and tried to develop reference or representative properties for these basins to use in the reference case, including stratigraphy, formation properties such as porosity and permeability, information about the brine chemistry. And then another important feature in a salt repository you've heard about is the excavation disturbed zone. We've taken properties for that from international studies from the Germans and also from WIPP.

As far as the interbeds, again, you've seen a lot of discussion about clay interbeds. We have properties on the clay and dolomite interbeds, their location relative to the repository, and we're also using a representative aquifer as a connection with the biosphere.

In the reference case we have--a little strange at this angle--this is the reference case engineered barrier system and concept of operations. So we're starting with a repository that is about 1,600 meters square. It goes 5 kilometers on each side. And I'll talk a little bit more about the details in this drawing, but basically we're assuming that the salt bed is about 700 meters--sorry--the repository is about 700 meters below the surface. We've assumed 70,000 metric tons with a burn-up of 60 gigawatt days per metric ton. The drift spacing, the waste package loading, and effectively the number of waste packages is a function of the thermal limit we're assuming for salt. And I think an earlier talk mentioned that. That was 200 degrees C is generally what's assumed. That results in, for our reference case, 12 PWR assemblies with 7.5 kilowatts per waste package.

And as I was talking about the repository itself, there are 84 pairs of 800-meter drifts with a drift spacing of 20 meters in between and 10 meters between the waste packages. We're using a crushed salt backfill engineered barrier in the drifts, and we have sealed shafts.

For the demonstration problem, we're only using a quarter symmetry, and this basically shows an anhydrite interbed just above the repository and intact host rock and then the aquifer here.

Okay, I'm going to move now to the FEPs process. I see I don't have a--the last conference I was at, I had a big timer that was--it was a countdown timer to tell me how much time I had.

EWING: I'll wave.

SEVOUGIAN: Oh, you wave too late, because I'm only halfway through.

ZOBACK: Mary Lou Zoback, Board. Could you define the acronyms?

SEVOUGIAN: Yes. Which one?

ZOBACK: FEPs, for example. I know what it means--

SEVOUGIAN: I'm going to get to that--

ZOBACK: --but not everyone in the audience knows what it means.

SEVOUGIAN: Right. I'm going to get to that in just a couple slides.

ZOBACK: Thank you.

SEVOUGIAN: In fact, I'm going to get to it right here on the title of this slide: Features, Events, and Processes.

ZOBACK: Thank you.

SEVOUGIAN: So this is the part I'm going to talk about now, the FEPs process. FEPs analysis is--in fact, it's essentially codified in the U.S. regulations in 10 CFR Part 63. One of the requirements for performance assessment is to identify the features, events, and processes that are important. They are potentially important to performance. So it supports both the safety assessment and performance assessment in the safety case. It helps with the development of the system models. It helps with prioritization of research. As I just said, it's needed for completeness of the licensing case. It's used in all major programs, international programs, the German program in Gorleben, the U.S. programs.

So, basically, this diagram here shows the major features in a bedded salt repository, including the backfilled excavation drift, the disturbed rock zone, the intact host rock and interbed and aquifer, the biosphere. And then on the right are processes that would be occurring within these features. So we like to say that processes and events--and I'm not going to talk much about events--act upon features of the repository.

Some of the processes would be waste form degradation, waste package degradation, disturbed rock zone evolution, salt creep; in the far field, advection, diffusion, sorption; in the biosphere, these are just generic processes, but water consumption for example.

Now, the two main parts about FEPs analysis are, first, identification of the FEPs. So the first thing you do is identify a comprehensive list of FEPs that capture the entire range of phenomena that might potentially be relevant to long-term performance. And here I like to use the example that Geoff Freeze used at the Waste Management Conference. He chose one from the Canadian program, which was: What is the potential effect of using contaminated water in a curling rink or ice hockey rink? So very comprehensive.

Later on, it's probably not important to performance, so then you have the FEPs screening process, which is to determine the set of important FEPs, those that might potentially affect or contribute to long-term performance. In general, there's three criteria for determining that. One is low probability. There's usually a probability screening level in the regulations that say if it falls below that, then you don't need to include it. The other is low consequence. If it's not going to move the needle, then it need not be included in a performance assessment. And then some are regulated.

Once you've decided which ones are important to include, then it's important to review and analyze them in various ways with process models, etc., to determine the fidelity and dimensionality of including them in the performance assessment. And I've just listed one here from a FEPs database or table that we have in the Used Fuel Disposition Program. We currently have 208. This one, the first thing you have is a number, just an identifier, then a name or description. This one happens to say, "Electrochemical Effects in the EBS." Some additional information processes that might be associated with this particular FEP; this one says, "Enhanced metal corrosion." And then some kind of screening decision. For this particular one we did a preliminary screening for the bedded salt and thought it was likely excluded, but we have to be reevaluated once the design is decided upon.

The next step I'm moving to is the model and code construction step, and I'm going to concentrate on code construction once we have the reference case and the FEPs screening. So I've already mentioned that high-performance computing environment facilitates the use of three-dimensional multi-physics over multiple realizations of a performance assessment. It also facilitates future advances in computational methods and hardware.

So we've developed these, essentially, requirements or guidelines that we're working towards in the development of our system. One of the more important ones is, we want the software to be open source so it's sharable by multi-lab experts, and stakeholders would have access to it. It increases transparency. It should be flexible and extensible so that it's easy to add either simple or advanced component models. It should be scalable. I've just shown a picture of scalability here. This is computer wall-clock time versus number of cores. Ideally it would be linear. We want to be able to leverage with our software the existing computational abilities related to meshing, visualization, and solvers. And, very importantly, it needs to be amenable to configuration management and quality assurance.

So where does that lead us? Right now here's where we're at. We have two key pieces to the performance assessment code. They're shown in green. The top one is the stochastic simulation part, and this one is the domain simulation software.

For stochastic simulation we're using DAKOTA, which is an open source software available. It's housed at Sandia, does uncertainty quantification, stratified sampling, sensitivity analyses, other things like optimization. So it's kind of the driver code that then calls for multiple realizations of the uncertain input parameters and then feeds them to the domain simulation software, which we're using PFLOTRAN, which integrates the simulations and does the simulations in the domains.

Why are we using that? It fulfills many of the requirements on the last side, which is, it's open software; it uses sophisticated version control; it's modular extensible, highly scalable in a high-performance computing environment.

So the main components, the main uses of it, are over the three major domains, the three major features I showed in a previous slide, the far field for far field flow and transport through the host rock and through the aquifer above, for modeling processes near the waste packages in the engineered barrier system.

The ones that are grayed out are ones that we're not looking at at the moment, which is not to say we won't in the future. Just for the first demonstration, we're not looking at those.

Right now we have waste form degradation and radionuclide mobilization in the far field processes. We're also using PFLOTRAN for the biosphere at the moment. Later on when a more sophisticated biosphere is decided upon for a particular site, we can use another component model there.

So let me talk about PFLOTRAN a little bit. The original developer was Peter Lichtner. I'd like to thank

Peter. He was kind enough to come down. He’s in the audience. Peter, thank you. And the lead developer is Glenn Hammond, who is sitting next to Peter.

And it does multi-physics, multi-phase flow and heat, multi-component reactive transport, biogeochemistry. It is massively parallel, can do highly refined 3-D discretization, probabilistic runs, and it’s open source, as I already said. It’s domain scientist friendly, because it’s written in Fortran, modular Fortran, modern Fortran.

As far as the open source part, this is just some logos of the people that are using it, a number of national labs and universities. And I just showed a snapshot of the source repository. It’s on bitbucket.org, so anybody here can go and pull down the source, look at it, and look at the Wiki pages on the description of it.

And then the next slide is just a little bit more about it. For flow it does multiphase gas and liquid flow, has various constitutive models. It does advection, dispersion, diffusion. It can do multiple interacting continua, has thermal conduction and convection. And then it’s very sophisticated with its reactive transport.

Now I’m going to move to the disposal system evaluation, the demonstration case, and you’ve seen this picture before. Again, this is our demonstration reference problem, and we’re using DAKOTA for the Latin Hypercube sampling of the input parameters. For the domain processes we have 3-D flow and transport in our problem. The main transport mechanism in salt is diffusion, so we have diffusion in the disturbed rock zone and the bedded salt. We do have advection in the aquifer here once it diffuses to the aquifer. We have a realistic source term in the EBS, but for now we're only using five radionuclides for the demonstration problem. We're using a conservative tracer iodine and the neptunium series decay chain so we can investigate the precipitation/dissolution with a decay chain.

For the waste form model we are representing spent fuel degradation, essentially UO2, with a kinetic rate of reaction. And essentially it degrades almost completely in about 10,000 years in this salt environment. And then the model has solubility limits for each element, and so they will precipitate if they exceed the solubility limit.

This is a picture--this is the top half of the previous picture. So what we did for the demonstration problem is we took a slice, actually, out of the reference case domain. We took a 3-D slice, so this 3-D slice goes through one drift, one emplacement drift, in the repository all the way out to a pumping well at 5 kilometers, so I've shown a picture here. Of course, it has vertical exaggeration. And there's a typo here. This should say 5809 here and here. So it's a 3-D slice. The width of it is from a half pillar to a half pillar, 20 meters wide, which is the drift space. It encompasses one drift just showing the detail of 8 of the 80 waste packages that are in this part of the domain. And that's what we simulated, 80 waste packages.

And, again, we're using our DAKOTA and PFLOTRAN. The results I'm going to show are one--I'm going to show one deterministic simulation that uses representative values or mostly mean values from the sample distributions of uncertain parameters such as porosity, permeability, sorption coefficients, etc.

And then I'm going to show a 100-realization problem where we sampled the nine uncertain parameters we have in the demonstration problem. We ran it on Red Sky, which is Sandia's essentially workhorse high-performance computing cluster. It's not quite PetaFlop, but it's 505 TeraFlops peak, so it's reasonably fast. And PFLOTRAN has the capability of nested parallelism so it can run many concurrent realizations of these 100 at a time. And then within each realization it does domain decomposition, so it quorums out the domain, you know, the far field, near field, EBS spatial domain amongst the processors.

This picture was just to show some of the detail on the waste packages, so here was the 5,000-meter slice. And this is showing 29 of the waste packages. You can't really see them. What you're seeing is the full drift width. The waste packages are actually at the center of this kind of cross here. And this is neptunium dissolved concentration at 1,000 years, so it hasn’t really started to move much.

Now what I’m going to show are two simulations from the deterministic run, so this is with effectively the mean values for the parameters. I’m going to show neptunium dissolved concentration first. This is only 1,000 meters of the 5,000-meter domain, so it’s going to start at time zero and then go to a million years.

(Pause.)

So it moves up a little ways. It moves a little higher up the shaft. The shaft is on the right side of the domain. It has slightly higher permeability than the intact host rock. And for some reason it stopped at 700,000. I didn’t stop it on purpose.

(Pause.)

Well, maybe it won’t go on this computer. It isn’t really showing anything different.

(Pause.)

That is amazing.

EWING: Just a question for clarification. Where did the fluid come from?

SEVOUGIAN: We are assuming that--I’d have to look up the porosities, but the salt has very low porosity. We’re assuming it’s fully saturated. So basically we’re assuming instantaneous waste package degradation at time zero, so at that point the domain is fully saturated with water, and it’s diffusing. So the fluid is in place, as we expect it to be.

The next one--maybe it’ll go farther than 700,000 years--is the daughter of neptunium-237, uranium-233. I’m showing the precipitated concentration, so it is one that reaches its--now, see, this one only goes--this one precipitates at different levels in some of the different beds because of changes in material properties like porosity.

So if we go to the next one, this was the deterministic realization. If we go to the next slide, this was the multi-realization analysis. So, as I mentioned, we sampled 9 parameters, ran 100-realizations. I am showing here the results of neptunium dissolved concentration at a point 400 meters, about halfway into the domain, in the anhydrite interbed. And this is zero to a million years effectively or .1 to a million years.

This is the range of output concentrations. If we take a slice at 100,000 years and take those 100 points and then do a correlation analysis with the input parameters and plot the Spearman rank correlation coefficient, or effectively how much does the spread in the output depend on the spread in the input, we see that the disturbed rock zone, DRZ, porosity and the neptunium sorption coefficient have the most effect. And then I just plotted a scatter plot of neptunium concentration versus the porosity in the disturbed rock zone, and you can see the trend here.

EWING: Sorry to interrupt, but if we let time pass, we’ll be away from the slide. So how much uncertainty is there in the neptunium solubility limit? I mean, at Yucca Mountain this was a big issue.

SEVOUGIAN: Yeah, I didn’t think of that one. I brought the neptunium Kd distribution, but I’ve forgotten what the spread was in the solubility.

EWING: It’s huge.

SEVOUGIAN: Yeah, this is not oxidizing conditions. This is reduced conditions. I’m not sure that it’s quite as big a spread as it was at Yucca.

EWING: But that’s included or not?

SEVOUGIAN: The solubility is. We do have--one of the parameters we sample is the--no? We just used deterministic values for the solubility? Somebody’s telling me we just used one value, wasn’t sampled.

EWING: But then correlating it with--

SEVOUGIAN: No, no, I didn’t correlate--no, I have neptunium sorption coefficient, not solubility. So, yeah, I--

EWING: All right. I’m sorry to interrupt.

SPEAKER: Yeah, obviously it wasn’t sampled--

SEVOUGIAN: Okay, I’ll move on to the next slide, which is--it's really the last slide. And I just wanted to show--I entitled it "Example of Flexible Architecture." Basically, right now we're using a kind of a simplified degradation model that's implemented right directly in PFLOTRAN as a kinetic rate. We have ongoing work on a spent fuel waste form degradation model that's comprised mainly of two components, a radiolysis model, which is on the time scale of seconds, and then a mixed potential model, which looks at oxidation reduction reactions at the UO2 surface, along with diffusion through the boundary layer, to determine--and also homogeneous and heterogeneous reactions--to determine a spent fuel degradation rate.

So basically the coupling between it and the PA model is that the PA model will send the solution chemistry from the near field to this model, which will then determine a degradation rate to send back to the PA model, which will then mobilize the radionuclides. So that's ongoing work.

My next slide is just a summary of what I've said. We've developed a capability that is able to evaluate either generic or site-specific locations with a high-fidelity representation of coupled processes in three dimensions based on high-performance computing architecture, adaptable to future advances. We've informed it with our knowledge of salt. It's able to represent uncertainty and heterogeneity. We hope to be able to use it to prioritize research activities, and we feel it will enhance confidence and transparency in the safety case.

I've shown you a demonstration problem.

And then ongoing work this year includes further code refinement as necessary. We're also working in collaboration with WIPP on some analyses with PFLOTRAN; and also further development of the reference case, simulations and testing for salt as well as granite and argillite is being worked on this year. And then I just mentioned the integration with the spent fuel degradation model.

So thank you for your attention.

EWING: Okay, thank you.

So questions from the Board? Jerry?

FRANKEL: Jerry Frankel from the Board. I think that your spent fuel degradation model, you should consider that the instantaneous dissolution of the stainless steel canister will result in a local chemistry with negative pH. So UO2 degradation dissolution rate, I think, will be strongly affected by that.

SEVOUGIAN: I didn't mention, in the reference case we're assuming a carbon steel waste package, not stainless steel. Carbon steel is more appropriate for salt. We have iron--they do have in their model--they have an iron species, so they had iron redox couples in their spent fuel degradation model. So definitely be taken account of. One of the most important parameters is the H2 concentration in the water.

FRANKEL: The H2 concentration?

SEVOUGIAN: Yes.

FRANKEL: Not the H-plus.

SEVOUGIAN: Well, H-plus, yeah. But understand the H2 affects the degradation rate strongly. It affects whether it’s a reducing or oxidizing environment.

FRANKEL: Right. But you don’t need oxidizing environment. The iron will hydrolyze also and generate--

SEVOUGIAN: Generate gas.

FRANKEL: --a gas acidic environment.

SEVOUGIAN: Okay.

EWING: And just a quick follow-up. Is your hydrogen generation then connected to your radiolysis model?

SEVOUGIAN: The radiolysis model determines--you’re getting into not my area of expertise, but the alpha radiolysis model deposits radiolytic products near the surface. The main one is H202. And I forgot, what was the--

EWING: Well, if you’re generating hydrogen from the corrosion, that’s part of the equation.

SEVOUGIAN: Right, that’s part of the--I could put the reactions back up, but H2 is in a number of the reactions.

EWING: Okay, please, Paul.

TURINSKY: Paul Turinsky, Board. With the uncertainty analysis, how are you doing the model (inaudible) uncertainties? I mean, parameters are a part of the story, but I would think when you're doing predictions out for these times, there's missing physics. So how do you account for that?

SEVOUGIAN: You know, you always--alternative conceptual models is a key part of any performance assessment. In fact, you know, it's in 10 CFR Part 63 that you need to include alternative conceptual models. So if there is a model that equally represents the data we have, we will include it in the performance assessment. And if there is no other way, 50-50 is the weighting. Or you can do separate analyses, one with this model and one with that one. But you definitely would include it.

EWING: Jean?

BAHR: Jean Bahr from the Board. Can you clarify for us what's different about PFLOTRAN compared to the FEHM model in terms of physics that's incorporated, resolution? Yours is a somewhat farther-field model than what we saw there. We've heard about two models that are both THC models.

SEVOUGIAN: Well, there was a number of reasons to pick PFLOTRAN. One was that it was open-source software. That was very important to us. And I don't think the other one is open-source. The other important part was we wanted to pick one that had already been optimized on a high-performance computing architecture. PFLOTRAN has been run in a number of different applications. I think I have a backup slide on that, and it's--

BAHR: Does it include all of the physical process that are included in FEHM?

SEVOUGIAN: I think so. It includes thermal. It includes multi-phase flow. Both of those are in FEHM. It includes reactive transport probably to a higher degree than in FEHM. There is a version of it that's including mechanical effects, linear elastic effects.

BAHR: Has there been an intercomparison with benchmark problems among those codes?

SEVOUGIAN: There could be. That wasn't really our goal, because we're using the PFLOTRAN for performance assessment. The FEHM is right now being used to look at more detail on the processes.

BAHR: I guess I'm trying to understand why two different models are being used.

SEVOUGIAN: Why two different models are being used?

BAHR: Are you at a different lab?

SEVOUGIAN: Yes.

EWING: Good question.

SEVOUGIAN: Again, we're using that one for process modeling. We're using this for performance assessment.

EWING: Other questions? Mary Lou?

ZOBACK: Mary Lou Zoback. I do have a question. And I applaud the use of open-source. I think that’s great. But the question I have is, you’ve created the model that runs on high-performance computing, so let’s say in the future another EEG is set up and the independent advisory group such as you had here in New Mexico. I’m sorry, I forgot which of the two labs you’re--would you then make that computing resource available to--I mean, there’s no point having open-source code--

SEVOUGIAN: That’s right.

ZOBACK: --unless people can run it.

SEVOUGIAN: That was our goal in having open-source. We want the stakeholder to be able to use it.

ZOBACK: Fantastic. And you’ll have training classes to help them understand it?

SEVOUGIAN: You have to ask DOE, but I would be an advocate for that.

ZOBACK: Great. I think that’s a fantastic route to go. Thank you.

EWING: Sue?

BRANTLEY: Sue Brantley, Board. When people run reactive transport codes, your chemical reactive transport codes, and try to simulate geological systems that are 10,000 years old, 200,000 years old, a million years, one of the bugaboos is figuring out what the surface area is, surface area of the dissolving phase. And I've actually had a student to work with, Peter Lichtner, and we simulated a soil out to a million years. And we ended up having to tune the surface area parameters because we had nothing to hang it on, I mean, basically nothing. We could start with observable for the soil that had developed for short time frames, but the older soils, you know, it was a tuned parameter.

So how do you deal with surface area in something like this where you're trying to actually make a million-year simulation?

SEVOUGIAN: I'm not an experimentalist. I mean, I know that you can measure surface area with, I don't know, nitrogen gas or something.

BRANTLEY: Well, you can measure it today, but if it changes over time--

SEVOUGIAN: Oh, as devolving surface area?

BRANTLEY: Well, and also the surface area that's interacting with fluid which is under-saturated. That is something that has to be calculated by the code essentially.

SEVOUGIAN: So it's dissolving or changing, becoming more surface area maybe? I'm not up on the literature. I mean, I know in the chemical engineering field, they have these catalyst reactors, they look at--

BRANTLEY: But they don't usually go out a million years, right?

SEVOUGIAN: Well, okay, I’m not sure of the distinction--sorry--for a million years.

EWING: Efi?

FOUFOULA: Efi Foufoula, Board. So, if I understand, your rank correlation analysis shows that the porosity is one of the critical factors affecting the model--with uncertainty.

SEVOUGIAN: This is just for the--the preliminary thing where I showed the output--

FOUFOULA: Yeah.

SEVOUGIAN: --which was very close to the repository in the disturbed rock zone.

FOUFOULA: So this analysis basically will tell you what are the most critical parameters contributing to the uncertainty in the model.

SEVOUGIAN: Right. This is one of the main goals when you analyze the results is so that you can then go back and, if it’s a key parameter, spend more dollars on reducing the uncertainty.

FOUFOULA: And I ask you, this is really pure uncertainty; it’s not heterogeneity in the porosity.

SEVOUGIAN: This is--yes, right. It affects the diffusivity. That’s why it’s important here. So this represents the uncertainty range that we’re using in this example problem.

FOUFOULA: Okay, it’s just an example. Okay.

EWING: Rod Ewing, Board. I have a few questions, and I’ll start with the most general. So it’s become, I would say, fashionable in the U.S. to do generic performance assessments of different repository rock types. And the question always is: Is this a useful exercise? What can we learn? So you’ve presented a generic analysis for salt, but I think most people would agree--and certainly the case at WIPP--that it’s the human intrusion scenario that matters in terms of the release of radioactivity, and that’s not included in what you’ve done. Is that correct?

SEVOUGIAN: Yeah, I forgot to point that bullet out on one of the slides. We’re using just the undisturbed case for the initial testing of the--

EWING: Right.

SEVOUGIAN: Because human intrusion is very--often it’s just specified by regulations. It’s not to say we can’t model it. If we decide on a specific scenario that seems appropriate, then--and we also have in our plan to start modeling that next fiscal year.

EWING: But wouldn’t it already be interesting, particularly if we’re supposed to use generic performance assessment of different geology rock types, as we look at the different geologies, the probabilities of mineral exploration, tunneling, whatever activity, would be different. And with salt it would be particularly high, thinking of people looking for oil and gas in the future.

So wouldn't it be interesting to compare the different geologies in terms of, say, a single scenario that is human intrusion and see which geologies are least susceptible in terms of the integrity of the repository and which are most susceptible?

Another question would be that if you have a drilling rate, WIPP has a compliance period of 10,000 years, I've always wondered, well, if the compliance period was a million years, what's the probability of, in some distant time, oil and gas exploration penetrating the WIPP horizon? And that becomes particularly, I think, a relevant question when you look at all the holes around the four-by-four excluded area for the WIPP site.

Are these issues that you'll address with your generic performance assessments?

SEVOUGIAN: Well, I like your idea of--and that's our plan to have a specific human intrusion scenario that could be compared across the concepts. It's a very good idea.

EWING: Well, it should be different for each rock type.

SEVOUGIAN: Well, but it may be that it's just one intrusion, penetrates one package, whatever is the commonality, then use that and look at the different concepts. It's a good idea.

EWING: Not the way--I don’t want to send you in that direction--

SEVOUGIAN: You’re saying I’m not saying what you’re saying?

EWING: I guess with these generic performance assessments, they may be generic, but still they have to be relevant to the different geology types.

SEVOUGIAN: Right.

EWING: And so just picking one drilling rate and applying it to all of the generic PAs wouldn’t be very insightful.

SEVOUGIAN: Well, until we have a specific site, we can look at generic drilling rates. It must be pretty low in a granite rock, but--

EWING: Exactly.

SEVOUGIAN: --probably pretty low in clay.

EWING: Maybe.

SEVOUGIAN: Yeah.

EWING: And then I’m just curious. You had your five nuclides. Why not plutonium?

SEVOUGIAN: There was no reason not to do it. We just--neptunium happened to be one of the major radionuclides in some of the performance assessments at Yucca Mountain and internationally for a long-term million-year--

EWING: Internationally it’s not because of the reducing conditions, so you seldom see actinides on the short list of high-impact radionuclides.

SEVOUGIAN: Okay.

EWING: And plutonium, the reason I raise that with the four oxidation states and a complicated chemistry, that would be a real test of how that would be handled in a generic performance assessment.

SEVOUGIAN: Again, this is our initial demonstration. We just wanted to include one decay chain at precipitation/ dissolution reactive chemistry. Obviously plutonium is critical to any performance assessment.

EWING: Right. Okay. I'm sorry to take so much time.

Other questions? If you don't have other questions, I can keep going down my list.

I think we're near the end. So, Staff, any questions?

All right. So thank you very much.

EWING: The last presentation for the day is by Frank Hansen, describing the U.S. and German collaboration.

HANSEN: Well, thank you for your patience. I actually volunteered to go last, because someone volunteered me. But I know that it's difficult for you, because there are so many questions. So I want to move fairly swiftly through my formal presentation so that we can amplify the question period. I, myself, was sitting back there champing at the bit to ask some questions and answer some.

But I have to stay reasonably on the ranch, and to do that I want to first start by thanking the Department of Energy, because both EM and NE sponsor the U.S./German international collaboration. And as you will witness as we move through this, they get very high return on investment, and we'll see that.

So I see someone deleted my co-authors on the first slide, and I apologize for that, but I need to acknowledge my co-authors, because I borrowed some of these slides from the German colleagues. Enrique Biurrun is from DBE Tech, and DBE Tech is a sole-source engineering firm that does all of the repositories in Germany: Morsleben, Asse, the Konrad facility, which is in iron ore--the first two are in salt--and so on. DBE Tech is a very good company. And the other co-author is the ministry--he works for the ministry that sponsors the research within Germany, and his name is Walter Steininger.

So I want to acknowledge those people, because they are fundamentally important to our collaboration.

And I also borrowed these slides from DBE Tech. DBE has demonstrated--they have actually demonstrated many of the facets of salt disposal, so it's not like we're beginning over. Direct disposal, the reference repository concept, these cartoons over here show they have placed cans horizontally, they have placed cans vertically, they have shown feasibility studies, they have put heavy packages down a shaft a thousand times successfully. Some of their big tools, placement tools, are shown here. They have developed the methodologies of modeling. They did a safety analysis for Gorleben.

And this down here is just put in there because of recent U.S./German collaborations. And I say recent, I don't really mean recent. Recently it has been very fruitful. But I've been working personally with the Germans since in the former times; I worked with West Germany back in the '70s. So the collaboration has been long-term, but it hasn't always been consistent. Sometimes the road narrows; sometimes they have a moratorium--they had a ten-year moratorium--and there are other factors. But we've been working actively, Wendell and others, with the Germans for 35 or 40 years.

And, of course, we have a lot of expertise here in the United States thanks--in salt--largely to WIPP, it's successful operation since 1999, current conditions notwithstanding. Germany has a lot of salt facilities--you may know, of course, of Asse--both famous and infamous. Morsleben from the former East Germany is full of nuclear material and other places. They have toxic waste in Herfa-Neurode, which is salt that's chemotoxic waste and so on. So they have a lot of experience in salt. And salt mining is worldwide well-proven. We know quite a bit about salt. It's the most important mineral for humankind.

And there is a lot of data here. We have the Library of Congress full of work that has been done, volumes and volumes, more than you could ever digest. And so this is only a sampling platter here. We'll get through these rather high-level platitudes on collaboration. And then, as I understand, you want to dive deep on some things, so I've got the sampling platter of some things that we can dive deep on, too.

So this is a German slide here, because you'll notice the choice of words. They had a moratorium, and they were taken out of the repository business, and Gorleben was in hibernation, in their words, for about ten years. So when they came out of the moratorium, they came to us to, to Andrew Orrell and me, and said, "Let's restart our U.S./ German collaboration," which we did in 2010. And it's been wonderful. It's really fantastic. And it benefits in a lot of ways, because they wrote the preliminary safety case for Gorleben. So they have done a high-level waste performance assessment. And, of course, we did WIPP, and we have considerable experience in the civilian program as well.

And so we decided at the beginning, we can't do everything all at once. And this particular collaboration focuses on salt repository research, design, and operation.

So it’s not everything. We do more of some things than others, and a list of some of the things we do is here.

The safety case, of course, is number one to a lot of people. Salt repository design and concepts, we talk a lot about performance assessment. Well, performance assessment requires a few components before you get started, like inventory of waste, concept of disposal, and geologic media. So we talk a lot about the concepts and the design. Of course, there’s groundwater modeling, radionuclide transport.

Geotechnical barriers, I’ll spend a little time on this. It’s near and dear to me. And I also brought some--so we don’t die of PowerPoint death, I brought some actual hands-on sample that I’m passing around for several of the things that I’ll be talking about here. And you’ll notice on a lot of these slides I have a reference, and this is our Web page. You can go on that Web page. You can find bios of all the participants--not all the participants. My colleague, Christy Lee (phonetic), said when she re-launches this site, it will have bios of all the participants. But we publish this. This is widely available, and you can simply go on line and click on their proceedings, and you can see all of the great science that we’re talking about.

And this is also taken from my German colleagues. We gave a paper, this one right down here, Steininger, Hansen, Biurrun, and Bollingerfehr, because politically they want to show that the collaborations are bringing home some return on investments as well. So we gave this paper and said, “Look, here are some of the great things that we’re doing, benchmarking constitutive models. This is just fantastic.”

And Lupe covered that very well. Good job, Lupe.

But just think about that. We’re running up our thermo-mechanical models with six German entities. And I can tell you from personal experience that they are the best salt mechanicists in the world, except for Lupe and the people at (inaudible).

Also, we published fairly widely, American Rock Mechanics Association, the Mechanical Behavior of Salt Symposia, and so our Waste Management, less technical. We also collaborate with the European Union on such things as the MoDeRn project, which is: How do you monitor geologic repositories?

Of course, we have done a lot of work on the safety case. And we could talk about this a little bit more, but I want to concentrate on salt, because I believe that we’re here to talk about salt, right?

And just to get through some of these high platitudes, one of the things that we did recently--and it largely was garnered because of our collaboration with the

Germans--the ministries wanted to sign an MOU, a memoranda of understanding, with the American entities. And because it was federal, the EM and NE signed a MOU with the German ministry. And I think that's a very nice flagship to operate under.

Most recently, because you know a beautiful child has many fathers, the NEA was very receptive now to sponsoring the Salt Club. Now, the Salt Club is not new. I remember writing a prospectus for the Salt Club with Leif Eriksson about 20 years ago, but we didn't get traction for a lot of reasons. There was a slippage in Germany; the Netherlands was reticent; Poland wasn't--but now that we have this great collaboration going with the Germans, the NEA said, yeah, the Salt Club sounds like a good idea today. And it is a good idea.

Under the auspices of the Salt Club, the natural analogues workshop--and I'm going to come back to natural analogues, because I think we talked a little bit about stakeholder outreach and how do you communicate with the lay people--well, that's one. And it's powerful, and I think it's important that we get there again.

Geoff Freeze and several others have developed this Features, Events, and Process catalogue; really, really good work. In fact, Tuesday morning we had a Videocon with the Salt Club, with our partners, and they are very mature on this Features, Events, and Process catalogue for the salt repository.

And yours truly is writing with four German colleagues--I'll get back to that reference in a minute--on salt reconsolidation. This is a very important and interesting piece of work. And then, of course, Kris Kuhlman has done an outstanding job on this salt knowledge archive, and his presentation today was just fantastic. And I have to tell you, I knew almost every person that he mentioned in that talk, so that shows you how long I've been working in this business.

Now I wanted to--this is what really, I think, is extremely interesting. This is the classic creep curve for materials. And so I wanted to just show whatever we have done. Lupe talked a lot about the constitutive model for salt, and he talked about the mechanistic basis of that model. And it's very, very important. And it comes from first principles, which I will show you here. And, of course, this is just a strain-time curve. It doesn't really matter greatly. But the interesting thing about salt--and you've seen some of this tertiary stuff. I'm not talking about tertiary today. We could. If you want to ask questions about it, we certainly can.

But today this is a paper I have for the American Rock Mechanics Association meeting coming up, and it documents the isochoric deformation of the salt. That means, do you know how rooms close? The rock fall has almost nothing to do with the room closure. The ribs come in, the roof does come in, the floor heaves, but the reason it does that, of course, is because of the plastic deformation out here in the country rock. And that's what Lupe expressed. That's why rock bolts don't hold the roof up indefinitely, because what's bringing the roof in, what's bringing the entire room closed, is the isochoric deformation of the salt out there, which also is the very reason we use salt for a repository, because it's impermeable. And it's isochoric; it never increases that permeability out in the country rock. But we will talk about where it does in the disturbed rock zone.

So getting back to this, I'm going to give this talk on this evolution of the substructure. How does this happen? What's important? And the plastic deformation occurs because of these mechanisms. And the fundamental mechanism by which salt deforms plastically is the crystal imperfection, is the dislocation. And we all probably know that. But it's also augmented by glide, because it has--in the perfect cubic system it has glide planes--and I'll show you some very interesting things--and cross slip, climb, which is a recovery process, and recrystallization and annealing.

And these are the mechanisms that--I'm waiting for somebody to say wow, because this is a wow photograph. This documentation here of these slides shows you the microstructure of the salt as it deforms as a function of stress, stress difference, and temperature; and this is the documentation of the substructure that you see. And what we have here, these are--you see how they're cubic? These are emergent sites of those crystal imperfections, those dislocations. Those are emergent sites. And that's just free dislocation. But free dislocations move easily by slip along these 110 planes.

But before I get to that mechanism, I want to show you this photomicrograph, because this is a grain of salt that we plucked out of a deformed sample, and you can see how it created its own draped fold. And those are those 110s we were talking about. And this shows you the amount of ductility that can be brought on by just slip, just glide.

But slip/glide by itself--and these are interchangeable words--cannot affect the steady-state creep. You have to be able to recover. You have to be able to--among the recovery processes--you know, a cube has six 110 planes, so it actually is almost perfectly plastic just as it starts. And I mentioned to someone earlier that most of the deformation of salt, even in uniaxial compression, is plastic. Damage doesn't contribute very much to the strain.

But the way you recover creep is by cross slip, and cross slip is a thermally-activated process; so if you're gliding on this 110 and another partner gets stuck, it just can step over the--it has to have a little thermal activation, but--and what we see here is an etched cleavage chip that show the orthogonal intersections of two 110 planes.

And the reference for this work is here.

Okay, well, cross slip by itself could almost affect steady-state creep. In fact, we've seen it in the laboratory. If you go back to the classic curve, it goes way, way, way out there and strain ten percent or more. But ultimately it starts to recover, and here is a cross slip with a climb component where climb is just the movement of the dislocation into the subgrain array, which reduces the strain energy.

And here is a substructure involved with a polygonized sample. Now, this is a highly deformed sample. These are the subgrains. Those are related to paleostress in geology. And those are the free dislocations (inaudible) dislocation density is not so high.

And, lastly, of course, if things go far enough, you get grain boundary migration and recrystallization.

Now the Germans. I offered up that, because we're trying to benchmark the constitutive modeling capabilities--the hardware, the software, and the models--I offered up that we had candidate models here in the United States at Room B and D that Lupe went into detail on. We--Lupe and I--offered that up to them and they said, you know, that’s a great idea. In Peine, Germany, I took the disk of all the tests that we had run on WIPP, and I gave it to them, and they went through the analysis and said, you know, that’s really good, but we have these other features. And we talked a little bit about that earlier. These other features that they like, they have damage, they have the damaged surface, the boundary of that, and they were looking at that in more detail, and you’re already aware of that.

But what I show you here is, these laboratories, the Technical University at Clausthal and the Institute for Geomechanics in Leipzig, are testing 140 or more samples for the U.S. for free. We’re not paying for this whatsoever, and we’re getting some of the best geomechanics in rock salt that’s available in the world. And this is just the matrix of the tests, and these test matrix are described by confining pressure, strain rate, temperatures--you can see the range--and then there are creep tests, too, under these similar conditions. And these are all designed by the Germans to probe certain features of the physics of the deformation of the rock.

Now, we sent them 4,000 pounds of core, and this is a picture of that core, thanks to our colleagues in Carlsbad.

This is 12 inches in diameter. When it arrived in Germany, they said, “We’ve never seen core in that good of condition before.” So that’s a tribute to our colleagues in Carlsbad. In addition to that, of course, we sent about 35 gallons of run-of-mine to the BGR, which is another entity. That’s the USGS in Germany, Geosciences Research Center.

So what else is fun? This shows some preliminary data, and it’s preliminary because we have not gotten together and waded into this material. I wanted to show you some examples of what kind of material properties are coming out. And those of us that have deformed salt over time, salt is a very well-behaved material. And this just shows you some of the data. This is just one dataset. And this is attributed to Salzer and these folks. And we’ll meet with them again in May, and we’ll dive deep into this. I’m not prepared to dive deep into the analysis, because we haven’t done it yet.

And this shows you a triaxial strength test. And basically there is this boundary between isochoric deformation and damage. And one of the things that the Germans really like to do is explore that damage surface coming from each direction. Now, this is an example of some of the science that we didn’t get done before we kind of stopped, so this gives you some idea of the advancement of the bases that we get from the German collaboration with salt.

A long time ago, 25 years or more, the reason I use this old plot is just so I remember to say this is old. The original work here was 1993 or so. It’s summarized in this particular report here, which is now more than 10 years old. But the deformation of salt--we had all these tests that we had been running since the ’70s, and one day Joe Ratigan plotted them up and he said, “Hey, look at this.” He said, “If you plot up all these tests”--and we of course have hundreds; this is just the discovery period--he said, “you can draw a line between the damaging salt and the isochoric deformation, and the line is separated. You can do it on the stress and variance space. And if you do it on the stress and variance space, then you can use all manner of stress paths and all manner of load angles for this type of an analysis.” And, of course, we characterize this boundary in stress and variance (inaudible) and the square root of the second invariant and the stress deviator tensor.

So that’s how we describe the DRZ, disturbed rock zone. We describe it by the boundary that separates that I1J2 space that I just showed you. So that’s how you do it.

Interestingly, you can go to the underground and you can measure it. You can look down the borehole; you can take out core; you can probe it with velocity, sonic velocity; and all of those measures have been put together.

And I think I summarized it in that previous reference, the Hansen DRZ paper.

Why is this important? This is important, because if you have an underground research lab--let's say we were thinking about one at WIPP--what's important is that all of these features start when you make the excavation. Before that, salt is sitting down there very happy with all the stresses equal. When you make the excavation, a room just like this, the country rock out here is still at 2,150-psi at WIPP, or 15 MPa, as you prefer. And before you disturb it, it's impermeable, and it's been impermeable for a quarter of a billion years.

So when we establish an underground research lab, one of the fundamental things that we should do is we should measure the evolution from the undisturbed to the disturbed case. And we can do that if we're smart, but we've got to be smart. You've got to think about it before the fact, which is what I put on this slide. If you have a room that you intend to excavate for experimental purposes, it can be any experimental purpose, and it can be at any site. It doesn't have to be at WIPP. And we are smart enough, I believe, to place flow gauges and deformation gauges in the proximity where you would expect the change. And we can estimate that; we can calculate that. Then when you excavate the room, you can validate that, should be able to.

So that's one concept I just wanted you to take away. Some of my colleagues--in fact, it was Cliff Howard and Kris Kuhlman, surprisingly--there he is again--we wrote this up on how you would do that. But it's an opportunity that you should take advantage of, that we should take advantage of, if we move forward with any sort of underground experimental lab.

Moving on, I mentioned the reconsolidation of salt. This is really important stuff, because if you open up a repository in salt, you have to show that you can button it up, too. And it's that buttoning-up thing that has to do with the reconsolidation of salt. I led the team that did the shaft seal design, and that shaft deal design had several components, but chief among them, of course, was our friend bentonite. But we also developed that salt-saturated concrete that's attached to the WIPP salt that is sitting in front of you there, Paul. That's a salt-based specialty (inaudible) mass concrete that was developed before we submitted the certification application.

And the third component--there's salt-based concrete, there's bentonite, but the third component is the reconsolidation of the native material. It's perfect. It's perfect, because it's already compatible mechanically, physically, chemically. But I want to note, here is another contribution from the U.S./German collaboration, and this is a state-of-the-art paper on the reconsolidation of salt.

Now, why would you write this? Well, because at our third conference with our German colleagues we had a survey with the audience, and we asked about their level of confidence in this. And, believe it or not, much to my chagrin, they were not confident in these results. And I thought, wow, how could that be?

And so I decided with our colleagues, Till Popp from IFG, Klaus Wieczorek from GRS, and Dieter Stuhrenberg from the BGR--those are research entities in Germany--we are collaborating on writing the paper of the reconsolidation of salt. It’s very important, because we know where we’ve been, and we know where we are, and we want to identify where we need to go. All permeability--all porosity and permeability are not created equal, by the way. The damage imparted in an experiment or by Mother Nature in terms of the damaged rock zone is not the same as the reconsolidation and the reduction of the porosity and the permeability of the reconsolidating salt. And that’s just what this fandancy diagram over here shows.

In the laboratory, when you experimentally deform a sample that does actually have fracture in it, the permeability jumps up radically with the volumetric strain if you have damage. And that’s because the fracture process is oriented preferentially to the maximum principal stress, of course, if it damages. And that porosity, as you might witness here, is mighty darn small. And this is the--that’s aperture permeability from the damage imparted to an intact specimen. And on this same graph is basically the porosity/ permeability function for reconsolidating salt. So that’s the point of that.

Now, the strength of analogues can’t be overemphasized. And this is important work by the Salt Club and by us and our German colleagues. Analogues are very important because we can show them permeability, we can show them graphs, we can show them stuff, people that don’t speak salt, and they can’t understand it. But if you can show them for example, a Celtic miner that was encapsulated in a salt mine 3,000 years ago and he still has his whiskers intact and everything, they say, “Oh, I can see how salt encapsulates the material placed within it.”

These are anecdotal examples of the complete encapsulation of a material put within a salt mine. And here is just some work going on in the old salt (inaudible) at Durrnberg. Here is a shot of a room that’s closed in. You can see the plastic deformation there from the Asse mine. Here is some complete healing of the grain boundaries of reconsolidated salt. And, of course, this is another shot of the reconsolidation of granular salt.

What we have here is dynamically compacted run-of-mine salt. This is after we--we ran this big test. It had 40 cubic meters of run-of-mine salt. And we tamped it in this great big container, and then we drilled it and measured the permeability of that. And then we took that core, and we took it into the laboratory. Perfect. And we squeezed it up, and this is the before, and that's the after, 10 percent porosity, 3 percent porosity, 10^{-14} permeability meters squared, no permeability.

So here are some of the questions--and, actually, this work was done by Hansen and Knowles. Knowles is a famous Sandia scientist and my wife. So this just articulates a few of the questions remaining, and they are only to remove the uncertainty and to answer some of those salient features.

In closing, I would like to call your attention to the references that are shown here. This Web site will be very entertaining, and you can find a lot of things there. The Germans, at the end of the VSG--that's their preliminary safety case, Gorleben--said these are their primary questions at the end of their report. And so I list them here, safety case, number one; plugging and sealing, and that would be concrete and reconsolidated salt, for example; salt mechanics modeling; repository design--and this is a slide I used a couple of years ago maybe, because it could be any URL. Hopefully WIPP will have one that we can use for generic salt research, but it could be any salt repository--and the other issues in geochemistry, microbes, and hydrogeology. A little bit outside of my field.

So then what do we do in our program here that the used fuel has been sponsoring?

Oops, this is not the same--let me use this to close out because this is the one I thought I had there. So this is the list from the German R&D perspective and the VSG. And they are interested in their particular Gorleben site because it was glacially covered, so uplifted subrosion and glacial channels. Well, we aren't particularly worried about that. But if we look at the work we are doing, compaction of salt, check; mass transport and two-phase flow, check; retrievability--retrievability to them encompasses geotechnical barriers and excavation damage zone--we're doing that; numerical modeling, geotechnical barrier integrity, we're doing that; conceptual improvements to the safety demonstration, we're doing that.

Thank you.

EWING: Thank you.

Questions from the Board? Efi?

FOUFOULA: So you mentioned you collaborated with the Framework 7 in your project. Do you know, in the new follow-up of Framework 7, the Horizon 2020, is much nuclear there--is it any funding and how much funding for nuclear-related (inaudible)?

HANSEN: Good question. She’s talking about the next generation of our European collaboration. The technical platform, of course--let me explain for the ones that may not know--they have the IGD-TP. It’s a disposal decision technical platform. And all the European nations are partners to that. And within that they have, of course, all the rock types, you know, because Sweden is a partner, France is a partner, Germany is a partner, all these partners in Europe, and they have all these rock types. And their vision, as you noted, is that in 2020 we shall have an operating repository in Europe.

But underlying that is basically the breadth of the science. And because they are looking at all the different rock types, they are essentially in the same place we are in the United States, because we’re now back looking at all rock types. So under that technical platform there are many opportunities for collaboration between our repository sciences and theirs.

Within the EU, of course, they sieve that down. I know of only one that I’m personally working on, and that is, you know, the acronym MoDeRn; it’s a long, butchered acronym that means repository monitoring. They have now a proposal in the EU for the next generation of that MoDeRn program. It’s called post-MoDeRn.

EWING: Other questions? Okay, Mary Lou.

ZOBACK: Mary Lou Zoback. Thanks, Frank. That was a nice summary. But I'm kind of braindead now. Can you remind us of the status--there's Gorleben and Asse. They're two separate places?

HANSEN: Yup.

ZOBACK: Are they both shut down? And which has waste, which one needs to be cleaned up, and just the status? And are they both domes? Is one bedded? I'm just--I've lost it.

HANSEN: Yeah, I don't blame you. I'll give you the broad brush.

ZOBACK: That would be good to start with always.

HANSEN: The broad brush is that a few years ago Germany had East Germany and West Germany. And West Germany in 1979, they did this site evaluation, and they picked Gorleben. Gorleben happens to be in the north right by the Elba River, right there, so the repository itself probably would sneak over into East Germany.

Gorleben is an underground facility that has now been mothballed, and it probably will never again be used. I've been there a few times back when they were doing R&D in the underground, and Andrew and I were there recently. They have a lot of facilities on the superstructure. They have a place where they can reconsolidate waste, believe it or not. Fantastic German engineering. They have a storage facility that, I think, has one waste package in there. When they moved waste to Gorleben a few years ago, it took every policeman in West Germany to move it down the railroad tracks. They lay it on the tracks, they cut the tracks in half, and so on. Thirty thousand policemen were deployed.

So Gorleben is the identified repository in salt for the former West Germany. When they reunified--and, of course, these things all took a different texture.

So the Asse is a former potash and salt mine that started operating in 1900, give or take, 1900. And it was converted in the ’60s to a research facility. And the Asse mine actually has several rooms at the 800-meter depth that are full of nuclear waste, and this is quite a contentious issue for the German government. But they do have the waste in the underground in those rooms.

And just to follow up on that, they had a survey of what are we going to do with that, and the BFS, which is their safety group, they said, Well, the only real assurance that we can have, and they came down with a dictum that says, We have to take it out. Now, you just pause and think about that. They have to take it out, and then what? But they haven’t really solved that. But we visited Asse here recently with the U.S./German collaboration, and that’s the party line. Asse is in the former West German area.

Now, when they reunified they got by definition

Morsleben. Morsleben was, of course, run by--

ZOBACK: This is the third one?

HANSEN: Morsleben is the third one. Morsleben is full of waste put in there and covered over with potash and some other things. It was largely waste that was under control of the Russians. And it is under active closure. And it's just across the border in former East Germany. And now they have passed a law similar to the Blue Ribbon Commission law, or whatever created the Blue Ribbon Commission, that says they will now reassess everything. So now everything is back in play, including other rock types besides salt.

But they also have other repositories. One is the Konrad mine. Konrad is a former iron ore mine. The repository proper is located in clay or--it's a very, very dry--they're converting it. And that's for intermediate and low-level waste. They also have chemotoxic and other repositories for such materials in salt mines like Herfa-Neurode, which has been in operation for 50 years or so.

So those are the ones that come to mind. I did ask my German colleagues, I said, "Well, now that you've reunified, does that change your selection of Gorleben?" And you get decidedly different answers, depending on which side of the country you asked.

EWING: Okay, thank you.

Other questions? Yes, Jerry.

FRANKEL: As a metallurgist, I always thought that plasticity in ionic crystals was different because of the electrostatic repulsion between anions and cations. So is it the particular structure of the 110 plane that allows dislocations to--

HANSEN: Yeah, because you're doing like for like. So if you take the cubic structure and you draw the diagonals, there are six of them, and those 110 planes--it's the easy glide plane. What's beautiful about that is, no matter what orientation the grains--many, many of these grains--no matter what orientation, there is always some 110s ready to rock and roll.

FRANKEL: Thank you.

EWING: Questions from the Staff? Yes, Dan.

METLAY: Dan Metlay. I was just curious about a comment you made that Europe is in the same place we are. Could you kind of expand on that?

HANSEN: Yes. And those are my words, because I believe they're true. Europe has a European commission, European Union Group. You think of them like the states or whatever. But their policy is that each nation has to handle their own waste, so they have many different geologic settings in which to have a repository. So if you look at the United States, we're looking at all different media now. And so in that respect, they are identifying different media for a repository; we're identify different media for a repository. They have a technology platform that's exploring the various underpinnings of science that go along with that; we're doing the same thing over here. So in that regard they're similar.

Plus, we have pushed the restart button, and some of them also have pushed the restart button, like Germany, for example. Some of them like France, of course, have moved along. They've made the commitment. Sweden and Finland have made the commitment; they're moving along.

But, yeah, that's--okay, it wasn't a perfect analogy.

EWING: Right. Sue.

BRANTLEY: Sue Brantley, Board. We were just talking about Asse, and our understanding is that brine is coming into Asse, and that's why they want to go back in and get the waste out. Was that predicted? Is that understood? Because why is brine getting in there if it wasn't expected?

HANSEN: I should have done away with all these viewgraphs and just put a placard up here and drawn on it. But the Asse mine is a--

EWING: Stay by the mic, please.

HANSEN: The Asse is a diapir, and they mine the flanks of a diapir for product, salt, and I think it's mostly potash in that particular mine. And these flanks then are like skirts, and so the ore zones come down the side of the salt diapir. Salt diapirs are the center. And it has a long access, too, by the way. Most of those diapirs in northern Germany are not like we have in the Gulf Coast.

So they mine these flanks. And when you mine for product, you want to maximize the extraction ratio, because that's where your profit. And, of course, it wasn't converted into a research facility until it had been operating for about 70 years. So they have a lot of void space, all right?

And I wrote a paper on this several years ago about why Asse is not the same as WIPP, because they tried to retrofit an extensive mine, and that's probably not good practice for a repository where you should start with a design function, then the operation, and so on. Plus, the extraction ratio of WIPP is miniscule compared to a real mine.

So what happened was eventually, because they extracted a great deal of material, then, of course, this disturbed rock zone raised its head, and it connected to the water-bearing areas that are on the flanks of every salt dome in the world. And the in-flow has been 12 cubic meters--I forget--it's been constant for a very long time.

METLAY: A day.

HANSEN: A day.

METLAY: Twelve cubic meters a day.

HANSEN: Yeah. And there are--I don’t know the exact number of the volume that’s available. But it’s an issue. It’s certainly an optical issue. It looks bad. We were there--you can’t really see it, you know, you can’t really see it coming in. But they’ve been refilling Asse for 15 or 20 years, refilling it with crushed salt.

BRANTLEY: But this brine was totally unpredicted then, because they wouldn’t have put the waste down there, would they, if they had thought that the brine was soon to come?

HANSEN: That’s a better question. If you had that foresight, you would not have done that, yeah.

EWING: Well, I toured it in the early ’80s and was assured that this was a good site because of the absence of fluids.

HANSEN: Well, the fluids didn’t come from internal; they come from external. And, of course, there is fundamental discussion of--someone said, well, if the salt dome is drier than bedded salt, why is it not better? It’s a good question. And the answer is, well, that’s not the only source of brine. And if you look at salt domes around the world, they are surrounded by flanks that are full of water and oil.

EWING: Right. And so whether that exploration takes place before you put in waste in or after, one has to anticipate some interest in economic deposits and the consequences.

So any more questions? Because I want to be sure to leave time for the public comments, but we have time for one or more.

Okay, thank you very much.

So let's close the session with comments from the public. And Robin Falko. Yes, please. You can come up to the front if you want. And five minutes, please, for everyone.

FALKO: Good afternoon. My questions are--they're comments and questions, and they're more relevant to what is going on right now. I was not aware that that would not be addressed. So, of course, I have concerns, as do so many people all around this country, about what the current state of affairs is at the WIPP site and what is being projected for the resolution of the problems there. If that has to be sealed off, what are the other options? And my other concern is the recent event that took place near White Sands by Carrizozo that has information that has come out, but nothing has been done as a follow-up for the public.

Are you answering questions, or am I just making a statement?

EWING: A statement, please. I'll explain why when you finish.

FALKO: Okay.

EWING: Or I should say, we're not in a position as a Board to answer questions about these most recent incidents. We tried to have a DOE representative here to answer such questions but weren't successful. But your statements and your concerns, you know, please express them. We welcome them.

FALKO: Well, those are my concerns about the lack of information, the degradation of so many nuclear sites around this country. We're looking at problems with Hanford; we're looking at problems with other sites in California; we have New Mexico now; we have Port St. Lucie in Florida; there was a recent event, I believe, in Kentucky a few months ago; a few weeks ago Evanston, Indiana. I mean, there are a lot of areas of concern. So my statement is about what will be done to address this.

I know these are very expensive projects when it comes to cleaning them up, shutting them down. But we can see, as you've been mentioning about Germany, that the Germans have stopped using nuclear energy. They're phasing this out. There are other countries that are considering this as well.

So my concern is, when does the United States come up to speed with being more representative of the needs and concerns of the people of this country? And people want to have safe environments and not have to worry about the next area that’s leaking and spreading contamination.

So I’m disappointed that there are no answers, but thank you for the time.

EWING: All right, thank you. So you’re down twice, so we have more time.

Judy Treichel.

TREICHEL: My name is Judy Treichel. I’m the executive director of the Nevada Nuclear Waste Task Force. And I know that we have had discussions, many of us in this room, for years and years. And it always comes up, well, you don’t like Yucca Mountain; what would you do? And obviously I’m not the one to answer that question.

But people say we’ve got to have nuclear waste disposal. And my thought is: We don’t really need a disposal site as much as we need not to make an irreversible mistake. And once you’ve committed to something that’s irreversible, you’ve got a far greater challenge than you have when you’re just doing studies and looking around.

And I’d like to know, after listening to the presentations today where we’ve heard so much good things about salt as a repository for any level of waste, what was the research or what were the tests that should have been done at WIPP to predict what happened five weeks ago last week, whatever? Not the truck tire; I expected the trucks would have a problem. But what could have been done in the research field that would have shown this incident?

And there were hundreds of reports, tests, documents produced; and I don't know that any of them came up with this possibility. And I think part of the problem are when the researchers do decide on what the FEPs are and do probabilistic risk assessment and start to weed out or screen out or decide what is not worth considering, and many of those things that aren't worth considering are probably what lead to the real problems that show up later.

I've got a file at home that I've had for many years, and mostly it's just--well, now it's stuff off the internet, but it started out with newspaper clippings, and it's called "Things That Can't Happen." And it's actually pretty thick. And that's why probably you've got YouTube and all kinds of stuff, because a whole lot of things happen that are very strange and weren't ever supposed to happen. But when they do happen and they involve any sort of nuclear waste, it's a lot bigger problem than many of the other things that weren't supposed to be able to happen.

So my recommendation would be that, yes, you have to have a consensual site. I don't know that there was ever a test or an examination done on what would happen with something like Yucca Mountain if the public just kept saying no and if we had decent lawyers and were able to keep fighting the thing. But it's now, I think, going to die.

But when you find a site where you do get some sort of public consent, I think you need to involve them right off the bat with the discovery, the selection of what the FEPs are, with the probabilistic risk assessments, with all of that sort of thing, rather than just pushing them in there or inviting them in after you've done all of that stuff and you have models to show. Because the actual public that walks up and down the street kind of has a good horse sense about things that can go wrong, things that they've had go wrong; and I think they need to buy in all along during that time.

So thank you.

EWING: Okay, thank you, Judy.

Susan Rodriguez?

SPEAKER: She's not here.

EWING: Not here? Okay. Abby Johnson?

JOHNSON: Hi. My name is Abby Johnson. I'm the nuclear waste advisor for Eureka County, Nevada. We're one of the ten affected units of local government under the Nuclear Waste Policy Act. I've been involved professionally or personally or both on the nuclear waste issue since 1983.

I've been to a lot of Nuclear Waste Technical Review Board meetings, and at some of those DOE and other Yucca Mountain repository advocates have held up WIPP as the repository role model both for Yucca Mountain and for DOE's ability to perform, so I think it's entirely appropriate to talk about WIPP when it’s not performing.

We heard a lot today about WIPP, about experiments in salt, but very little about what had gone awry at WIPP recently. It is really unfortunate and telling that no one from DOE accepted the Board’s invitation to come and update the Board about what is known and what is still to be learned about releases at WIPP. I think that speaks for itself.

When I was driving to the airport yesterday, I was listening to NPR, and I heard part of this quirky story about a scientist at NASA who is now retired--maybe some of you heard this too--who wants to retrieve a satellite that was sent out there to find stuff maybe 30 years ago. I didn’t get that number, but we could do the math about how much the boundaries of that would be. And he was talking about all the challenges, and there’s a time element--I guess it’s going to be close to earth in May or something--you know, the software is outdated; it doesn’t exist anymore. The communication equipment apparently was sent to wherever they send all the stuff when they don’t need it anymore. And most of his team is retired. He didn’t say dead; he just said retired. And, you know, NASA is essentially a single-purpose agency. I mean, what do they do? They put things in space. And that was about 30 years ago.

So today we’ve heard time frames. I think I heard 250 million years; I heard a million years; I heard 700,000 years, 10,000 years, 200 years, 100 years, 50 years, 30 years, the magic 15 years at WIPP, and 5 years. It’s like spinning a time machine wheel with all these different times, time frames.

And one of the things that--I heard some of the questions and that there was a little, really? You guys don’t talk to each other even though you’re in the same state? I want to really encourage the Board to keep raising those questions and those kinds of concerns, because this whole time element is a reminder that the technical challenges, research, and progress are integrally and essentially connected to the institutional and cultural issues, including technology advances and obsolescence.

So the more I’ve observed this program, the more I’ve understood that it’s not the science; it’s the management, the institutional issues, the systems approach that needs as much attention as the charts and the graphs. And you can tell I’m not much of a scientist to begin with.

Finally, we, Eureka County, want to thank the Board and Staff for being a consistent ongoing forum for these important topics, for creating a public record, which is so important for now and for the future, and for your commitment to public participation. It’s not often that people who know stuff listen to people who know less stuff and we have a dialogue about it. Those days don’t happen too often anymore. Thank you.

EWING: Thank you.

And I think George Danko. George?

DANKO: Thank you, Mr. Chairman. Thank you, Board, for the opportunity to address this meeting. I came from--the name is George Danko. I'm coming from the University of Nevada-Reno, Mackay School of Earth Science and Engineering. So I will be a little bit more concerned coming from Nevada. The state of the Yucca Mountain project still the one which was fully designed and submitted for permission. And it might never happen, but what happened has been the development of many useful models for analyzing the performance of the repository, the design of the repository. And then I saw today the homework of the New Mexican's institutions on numerical modeling. What I was missing some was the massive amount of work and models developed for Yucca Mountain. So I haven't seen FLOC (phonetic), and I haven't seen the model of TOUGH or NOFT (phonetic) or TOUGHREACT or TOUGH-FLAC. Maybe these models may come back from the cooperation with the Germans, because they used these models maybe in their salt repository, I'm guessing. I've seen many of those European institutes using the TOUGH family of models. So it's interesting to see how local is this, and maybe looking to cooperation inside the United States and the other institutions would be beneficial to increase the confidence of the models' work and start from a platform, which, actually, the system models, etc., reached for the Yucca Mountain project.

And one minor side comment on this is my area of modeling ventilation coupled with thermal-hydraulic effects. And then this is a unique model, which could be actually used (inaudible) in an area which has been operating as a repository being ventilated. Now, pre-closure ventilation affects the post-closure performance in a way that it provides the initial condition for that. And then that was a lesson we learned from Yucca Mountain. We came up with a fully qualified ventilation thermal-hydraulic model for Yucca Mountain, ready to be used, and then something to consider for this confidence-building and starting working if it is the goal to emplace defense high-level waste or spent nuclear fuel in salt deposits.

Thank you very much.

EWING: Okay, thank you.

I've exhausted my list. John, I think--John Heaton.

HEATON: Thank you for being here. My name is John Heaton, and I am chairman of the WIPP Task Force in Carlsbad. And it's a task force put together by the mayor, and I'm a volunteer, as are the other 45 people that come on--we were typically meeting a couple times a week. Needless to say, we’re meeting every week nowadays.

But I wanted to express to you that the fear in the community has been alleviated tremendously. Of course, when it first occurred, I think that you all would recognize that a certain amount of fear spreads through the community; and that fear was mitigated primarily because of the Carlsbad Environmental Monitoring Center. That was an organization and a facility that was put together by the community prior to WIPP opening. And we had intended that it would do flora, fauna, soil, water, air monitoring prior to the opening of WIPP and then whole-body counting for people in the community to do epidemiologic studies and to know what the background was. We’re probably the only DOE facility that knows and knew in advance what the background was in the community.

The release that occurred on the 14th, the measurement right off the site, which is a sixth of a mile away from the release right at the fence, was .64 becquerels. If you stood in that position where the air monitor was for 15 hours, you would receive that amount of radiation, a dose equivalent--a bitewing x-ray. And I think that between the monitoring center and the people from the contractor, DOE, explaining in layman’s terms what these releases really meant in terms of real-life experiences--dental x-rays, pan x-rays of the mouth, chest x-rays, flights across country--and comparing them and then also having confirmation come back from the CDC for those 17 people that had an exposure, and their fecal samples were positive, their urine samples were all negative, but getting confirmation back from the CDC that these were negligible exposures and that they represented no more than a single chest x-ray over the period of 50 years, and I think those kinds of explanations to the public make a huge difference.

In fact, I envy you a little bit from a technical perspective, because you can come to conclusions, and you're speaking to knowledgeable people about these very esoteric subjects to the public.

And so our job has become: How do we get transparency? How do we get information out to the public in a way that's meaningful? And the mayor and I met with Mr. Klaus and Mr. Heisinge (phonetic). And Mr. Klaus, who is second down from Secretary Moniz, has agreed to cut through the red tape, and we now have a daily report that comes out from WIPP, and you can find it on the WIPP Web page. For those of you that don't know where that is, it's www.WIPP.energy.gov--easy access--and it has all the radiation numbers, all of those numbers that have been collected from both the independent environmental monitoring center, which is run under the auspices of the New Mexico State University. We argued and argued about how do we get independence in our reporting, and we believe that going to a university was our best alternative in getting tenured professors that can responsibly report data without political interference.

And so that was the objective, and that’s we have, and I think that has brought a lot of confidence to the community. And I can’t tell you how difficult the challenge is to manage what’s going out in the press and in the blogosphere and what’s happening. On Saturday we had calls, “How’s the evacuation of Carlsbad going?” from towns that were 160 miles away. And we said, “What are you talking about?” Well, they had read something on some blog somewhere, and they thought that all that was happening.

And this morning the release in the Carlsbad paper, “Second release occurred at WIPP.” It was not a second release by anybody’s interpretation. Probably the plenum that feeds the exhaust system after it goes through the HEPA filters probably had some collection of particles inside the plenum, which were released. But, you know, it was a single release, and then there was no more. So you almost have to assume that that’s how it occurred.

But until they go down in the mine and actually determine what happened, how it happened, and fix it, then they can go through the cleaning process. But I can tell you that the community of Carlsbad is now--the fear essentially is gone. There’s still a few folks--believe me, I mean, that will always occur, but the fear is gone. And the community, from my perspective, from the mayor’s perspective, is that we’re supportive of getting the WIPP facility opened up again, cleaned up, opened up, and the plans put in place, and they will become more transparent as they find out what the issues really are.

But I wanted to make that clear to you. And the ventilation system, I don’t know how much you know about that, but if you look at the picture of WIPP that’s been up here several times, the underground, the ventilation system goes from the north--the top, if you will--down to the south. It’s always flowing at the back of the workers. We don’t believe that the north end, which is where all the experimental activity occurs, has any possibility of having contamination there. And there’s been a probe put down that demonstrated that there is no radiation picked up at the bottom of the shaft. They put a camera down. They also put an air quality monitor. That was also negative.

So the system is set to go down and go find out what happened, isolate it, correct it, whatever they have to do, and then figure out how they’re going to deal with the ventilation. But we believe that there is no reason that the experimentation that’s attributed to the north end should not go forward, and we appreciate this meeting occurring, because we think that--I still think that salt is unquestionably the best medium. The geology is fantastic. Every time I look at it, I get more impressed with it.

So, at any rate, just to clear up a couple of things, you know, we heard earlier Congress said no, no, no. Congress has not said no, no, no. Congress was responding to the Nuclear Waste Policy Act amendments of 1987, which said, If you have any exploration going on for a repository, it can’t happen; we can’t fund it.” So you automatically have to put Section 12 in there, which says no high-level waste can be moved to WIPP.

I mean, it’s never been tested, never been asked about. The technical analysis has not been in place to go forward with it, and that obviously has to be the first step, the things that you’re doing. WIPP’s mission of starting clean, ending clean--every time industrial business opens or a repository, a mine, they do a very complete safety analysis. I’m not telling people in this room anything, but they do a complete safety analysis.

It was always anticipated that there would be some release at WIPP, even though we dearly prayed that it would never happen, but it was always anticipated, and it was part of the safety analysis, and it was part of how the ventilation system was set up. And the ventilation system worked as expected. There was a small puff because of differential pressures on the damper that closes the air from the atmosphere over to HEPA filtering; but, other than that, it's working as expected. And now that damper is completely sealed, and so everything goes through the HEPA filtration, which is at 99.97 percent. So it's a very robust filtration system.

So we never thought that something like this would occur. We hoped it would never occur, but it was always planned. And I think that's the important issue to talk about. We had always thought there would be some disastrous truck accident. I mean, we have gone to the equivalent of going to the moon and back 28 times. That's a lot of driving with a payload. And we are the envy of every trucking system probably in the world, but clearly in the United States. And that's a story all unto itself, but the point being is that we expected that before the other. The probabilities were all that there would be a trucking accident before an accident in the mine itself.

And I'm not going to talk about the fire. The report from the fire you can read on the Web. It's an unvarnished report; and, actually, in mind, it's a scathing report of the change in culture about safety that's occurred. And it also, I think, is a scathing report on the lack of performance assessment training and good management. And we hope that it only existed on the mining side, which is where they remove the salt, and we hope that that same report won't be as scathing when we get over to the radiologic side of the mine and the waste emplacement. We hope that there are two different cultures even though it's concerning, very concerning, to all of us.

So, at any rate, I just wanted to point that out that the mine has worked, the system has worked as anticipated, and there is no reason that it can't be either new ventilation drifts, a new ventilation shaft, work under HEPA filtration all the time. I mean, there are all sorts of alternatives and clean-up and move on. And as you saw earlier, two more panels are expected to be mined; and if Panel 7 has to be closed completely, there is no reason that you can't mine out another panel. There is no limitation on the volume of panels that we can have at WIPP. There is only a limitation on the actual volume of waste at 176,000 cubic meters. So, as you can see from that, we've got a huge amount of real estate for any kind of repository activity on that site.

So, with that, I'm going to--I've gone past my five minutes, Mr. Chairman. I'm sorry.

EWING: That's all right.

HEATON: But, at any rate, I just wanted people to know that there is information available on a daily basis, going to that Web site. And also there is a--we're going to have a weekly town hall meeting. We've had three of them now, four of them, and we’ll have one every week on Thursday at 5:30. And it’s webcast so you can listen, you can ask questions. So we want to be as absolutely transparent as we possibly can. It’s critical to any issue to resolve it publicly.

Thank you very much.

EWING: Thank you.

Let me ask, is there anyone else who would like to make a statement?

All right. Then I would like to thank all of the speakers today and also the audience for staying through the entire day. I think it’s been productive, and certainly the Board appreciate all the information and perspectives that we’ve received.

So thank you very much.

(Whereupon, the meeting was adjourned.)

C E R T I F I C A T E

I certify that the foregoing is a correct transcript of the Nuclear Waste Technical Review Board's Spring Board Meeting held on March 19, 2014, in Albuquerque, NM, taken from the electronic recording of proceedings in the above-entitled matter.

April 2, 2014

Federal Reporting Service, Inc.
17454 East Asbury Place
Aurora, Colorado 80013
(303) 751-2777

1