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I N D E X

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P R O C E E D I N G S

MR. WARNER NORTH: Good morning. Welcome to the second day of this Joint Panel Meeting for the Nuclear Waste Technical Review Board. We're picking up with a couple of presentations that were scheduled for yesterday afternoon on our flexible agenda. Mr. Ryder, I believe you're on.

MR. ERIC RYDER: Thank you.

Today I'll be talking about waste characteristics & inventory, areal power density, and layout development. It seems like kind of a mouthful to put into one presentation, but I'll be approaching it from the standpoint of how we determine area requirements for the spent fuel. Now, in a simplified way, this can be represented by the following flow chart. And, you've already heard about repository layout from Tom Hunter. So, I'll go right into waste inventory & characteristics and where we get that information.

Waste characteristics & inventory is part of what is contained and what's known as the characteristics database which is managed by OCRWM. Just for completeness, I've put up all the sub-databases or the main sub-databases that you can find in this characteristic database. There's the assemblies database, the non-fuel assemblies hardware database, and the high level waste database. Now, these two don't have any bearing on today's discussion; so, I won't be discussing them in any detail. The high level waste database does have an effect on area, but because of the heat output of the defense waste, it's constrained and it's emplacement by geometric limitations. So, other than say that, I really won't be going into that either.

Now, the final two sub-databases that do have an effect on today's discussion are the radiological data base which has the characteristics of the waste contained in them and the quantities database from which we get our inventory. Going into the first one from the radiological database, the first one I'll talk about just briefly is what's known as the calculated integrated heat release, a subsection of the radiological database. And, contained in this database is information that gives the total radioactivity in curies and heat output from spent fuel as a function of time after discharge from the reactor. This information is calculated using what's known as the ORIGEN2 Code which performs two major functions, isotope generation and isotope depletion, both within the reactor core and after discharge. A note on units, everything presented in the following graphs is based on one MTU or metric ton of initial heavy metal.

This is just one example of the information that's contained in terms of thermal decay. This is for PWR-type waste and burnups ranging from 10,000 to 60,000 MWd/MTU. As you can see, at early times, you have a very sharp gradient and widely varying heat outputs, but as you get into, say, 100 years, they approach one another and single representations of the curve actually appear to be a reasonable approximation. Just a note on some of the codes we use, whenever possible, we use each one of these curves for our heat calculations, but a lot of codes are limited to a single curve definition so we use a nominal basis. And, for PWR-type waste, that's 33,000 burnup which would fall in-between here. And, as I said, at longer times, that's a good approximation, and even at shorter times, it's not too bad.

Now, in terms of radiological information, this is just to tie in with the waste package information you'll be hearing later on today. Again, it's for PWR-type wastes with the burnups I mentioned earlier, similar shaped curves and since it doesn't really have a bearing on the area requirements, I won't be talking too much about this.

Now, going to what was in the quantities database, which was the historical and projected inventories, the current version of it has historical inventories from 1968 through 1987. Now, this represents approximately 16,000 tons of fuel. The only inventory projection scenario included in the characteristics database is what's known as the "No New Orders-Extended Burnup" case. Now, we talked about this yesterday and just to remind you of some of the assumptions that go with this any reactor that's not currently under construction or really close to it -- I guess, would be the answer I would give -- is considered cancelled. Also extended burnup indicates that the reactors that are currently working will go to a higher burnup as time goes on. Now, based on this projection, it's expected that 87,000 tons or approximately 87,000 tons of waste will be available at the end of discharge year 2037. Now, comparing these two numbers, you see that 16,000 tons only represents about 20% of the final inventory. So, any changes in the projection or any change in the projection scenario could have a significant impact on area requirements and that has to be considered whenever updates come out.

Just to give you a graphical representation of both the historical and projected, this is for the historical waste through 1987, and just to tell you, the BWR waste average falls in the 20 to 25 burnup range and the PWR is right on the edge of the 25 to 30 range for the historical waste. Now, in terms of the combined historical and projected, these ranges shift up one. The BWR is in the 25 to 30 and that's why the nominal waste is considered to be -- nominal BWR is 27,500 burnup and the PWR shifts to the 30 to 35 range and I've already told you that the nominal burnup for that is 33,000.

Okay, just a quick summary. All the physical description information provided in the database is given to OakRidge by the fuel assembly manufacturer. All radiological description estimates are made using the ORIGEN2 computer code based on manufacturer supplied information again. The next scheduled update for the database is in July of 1990 and, thereafter, on an annual basis. Now, this 1990 update will bring it through 1988 discharge and then they're going to try and do a two year update the following year. Unscheduled updates as required simply indicates that if a manufacturer comes out with a new fuel assembly, that will have to be incorporated into the database.

All right. Returning to the flow diagram, the next information we need is allowable design bases APD. Now, you heard a lot of numbers thrown around with APD tacked onto them yesterday. And, just to give you an idea of how they're defined -- areal power density, yes, I'm sorry. The first is what's known as local areal power density and this makes no assumptions as to the layout in terms of standoffs required, barrier pillar widths, access, driftwoods, et cetera. It's simply the initial loading of a canister divided by a unit cell area defined as the canister spacing multiplied by the drift, center line to center line spaces.

The one you most commonly hear, however, is what's referred to as design-basis areal power density. Now, this does make an assumption as to the layout in terms of there is a standoff that's included, a non-waste emplacement areal included, as well as the barrier pillar width, the width of the access strip, et cetera; non-waste emplacement areas. And, this is the number you'll see most commonly in the literature. When they refer to 57 kW/acre, they're talking about design-basis APD. The correspondence, just as a note, between the two based on the layout you heard about from Tom Hunter is that the design-basis APD is approximately 80% of the local areal power density.

Now, a lot of people have asked where the 57kW number come from. Historical design-basis APD of 57kW/acre was established in the unit evaluation study by Johnstone. This was published in 1984. The calculation of this number was based on that waste concentration that produced emplacement drift floor temperatures not to exceed 100 degrees C. However, because of changes in the repository ventilation design, the 100 degrees C floor temperature of the emplacement drift is no longer a goal. It's not a thermal goal anymore. However, this number was carried through the SCP.

Now, you've had several discussions on how we come up with our goals. So, just as an example, the 100 degree C floor temperature was an example of the iterative process we go through in establishing our thermal goals and I really don't think I need to go into that anymore since you've had several discussions on it.

In terms of current SCP thermal goals that guide our thermal design now, there's the borehole wall temperature based on the container centerline temperature of 275 degrees; the one meter rock temperature of 200 degrees. There is a retrievablility concern for the access drift temperature where they don't want the wall temperature to exceed 50 degrees C for the first 50 years. Also, there's the adjacent strata, the interface condition where it's not to exceed 115 degrees; surface environment; and the last one which seems kind of at odds with the others of keeping the canisters as hot as long as possible.

I'll be showing some scatter plots, basically to address, based on current thermal goals is the 57 kW/acre number valid and also is it possible maybe to raise that number because of its implications upon areal? We'll be looking at scatter plots for this first goal, the borehole wall temperature goal, the one meter goal, the adjacent strata goal, and also just briefly touching on the keep it hotter longer goal, I guess is what I'd call it. One note about this temperature limit is that it is model dependent a lot of times and the results I'll be presenting are from an analytical model and we've found from model comparison that this model overpredicts the borehole wall temperature and we have to lower this limit to 235 degrees C when we use an analytical model.

The specific results from the model we'll be looking at, as I said, it uses an analytical solution which is a 3-D linear superposition of heat-generating points and cylinders. It's a very simplified six panel model geometry. We'll be looking at the conservatively high temperatures below a central canister within a central drift or a central drift and a central panel. The fuel is modeled as 60% PWR and 40% PWR which is a hybrid mix. You'll be hearing more about that later from the waste package people. It was also assumed to be 10 -- the average age of the fuel was assumed to be 10 years out of reactor and simultaneous emplacement of all drifts were assumed. Now, this also gives you a conservatively high temperature which in terms of our maximum temperature limits is good, but when we look at the final goal of containment, you have to realize that that is conservatively high.

Looking at the borehole wall temperature first, each one of these little squares here represents a different combination of canister loading, drift spacing, and canister-to-canister spacing. And, it's important to note that there is no -- there's an infinite number for each areal power density of combinations. You could come up with drift and canister spacing and canister loading. These are just some representative cases. The green line here -- and it's plotted against Local APD and just a note as to -- you know, I gave you the conversion factor, but 69 Local was approximately 57 design-basis APD; 100 corresponds to about 80. And, as you can see for the 235 limit, we don't seem to violate -- there are viable configurations until you get above the 100 Local number. At 57, there's this one that's well below the limit. And, the question yesterday, at the end of yesterday, was how low can we go before it's not economically feasible, I guess, if I could paraphrase. And, we did some back of the 3 x 5 card calculations and just roughly we think on the order of 35 kW/acre and that would correspond to temperatures -- borehole wall temperatures, peak borehole wall temperatures -- on the order of 200 degrees.

Tom?

DR. TOM HUNTER: This is Tom Hunter. Just to comment on the lower limit that Eric just mentioned, you have to pick a couple of things to make that assumption. You have to pick at least an age at which you think the waste will be when it's put into the repository, and for practical purposes, if you pick something like 30 years, that's pretty close to the current schedule for waste emplacement. So, 30 to 50 years, we thought, was reasonable. You have to also pick something about economics because you can take each of the spent fuel assemblies and put one into an individual container, if you like, and spread them out. And, you have to pick something like an extraction ratio for the repository and decide how much -- what kind of spacing you can actually get. You have to assume that you don't want to spend an enormous amount on a large number of waste packages. So, they're not terribly inexpensive. They're something on the order of $50,000 apiece and there's something like 30,000 of them which turns out to be about $1 1/2 billion, I think. So, with those kind of assumptions, you get some practical limits of what you can do and that's where the 30 number comes from. And then, you also are trying to just put 60,000 metric tons into an area of about 1700 acres. So, you can get the problem reasonable bounded and it comes out to something like that.

MR. EDWARD CORDING: That's still assuming 10 years after -- 10 year old fuel, is that correct?

DR. THOMAS HUNTER: No, it would be an average of like 30 year old fuel.

DR. CORDING: 30 year old fuel.

DR. HUNTER: And, actually, I don't know if Eric is going to go into it -- are you going to discuss something about the current average age, actual average age? You said this was 10 years.

MR. RYDER: Yeah, this was based on 10 year old waste, but if it were to be a 2003 emplacement start date, say, it would be a 24 year old fuel average. 2010, you'd go up another seven years. So, it would be in the 30's.

DR. PATRICK DOMENICO: You say the peak temperature at the borehole is 200 degrees under this?

MR. RYDER: Well, see, you'd have to run specific layouts because you'll see the banding here. This is for 40kW. There is any number of, you know, combinations and they can give you ranging from, you know, 225 down to 190 degrees. And, it's potentially possible at the 30, 35 number we're talking about that there may be one lower. You know, it's --

DR. CORDING: But, again, that chart is for the 10 year old fuel?

MR. RYDER: It's for 10 year fuel, yes.

DR. CORDING: So, it would be lower if you're looking at 30 year old --

MR. RYDER: It's not certain because you are further down on the curve and if you have -- okay. There's a point about younger and older fuel. If you have two canisters with the same initial loading, but one is, say, 10 year old fuel and one is 30 year old fuel, the maximum peak temperature of the 30 year old fuel will be higher than the 10 year old fuel because of the fact that you shifted up where you are on the curve I showed for thermal decay. If I can pull that out real quickly. What you've essentially done is taken this piece of the curve and shifted it up. So, you've got a greater energy deposition area underneath that curve. If you have, say -- well, here's 10 year old fuel, 3kW, and then you take 40 year old fuel, 3kW, you have to shift this curve up and the area is greater. So, your peak temperatures will be higher. Does that answer your question?

DR. HUNTER: I think basically it's because you just have a -- it doesn't decay away as fast if it's older. It has the same source strength. It has the same initial power output. It's older and it doesn't decay away as fast because it's older.

MR. RYDER: Yeah.

DR. NORTH: So, it would rise up to a hotter temperature over time?

DR. HUNTER: Yeah, right.

DR. NORTH: The peak borehole temperature is achieved some 10 years in the future?

DR. HUNTER: That's right.

MR. RYDER: Assuming that your emplacement density is the same also. That's another assumption.

DR. HUNTER: Eric, these do not include any ventilation in the repository, do they?

MR. RYDER: No. They're analytical models; so they assume everything is rock, first of all. You know, there are no openings and it's not capable of handling the changes in stratigraphy.

DR. NORTH: What's the effect on the peak temperature of running ventilation during the period prior to closure?

MR. RYDER: Well, peak temperatures tend to occur, according to the models I'm presenting now, within the first 20 years. So, it would have a significant effect on it. And, also there is leakage air flow and actually having the openings there would have a difference.

DR. NORTH: Have you run a case where you've looked at the combination of assumptions you might make to get the temperature down as low as possible? Supposing we were really worried about keeping the temperature in the rock low because of thermal-mechanical and chemical effects, how low could you get it by some combination of using older fuel in the 50 to -- or make it the 30 to 50 year range, spreading out the spacing, and using ventilation for a longer period of time, perhaps at least 50 years or maybe even looking at 100 year prior to closure case?

MR. RYDER: I haven't run any models with that scenario in mind.

DR. NORTH: Any feeling over there as to --

DR. HUNTER: Well, yeah, you can look at the curve he just showed and I think you're talking something in excess of probably 150 degrees for the borehole wall temperature.

DR. NORTH: Somewhere between 150 and 200?

DR. HUNTER: Probably closer to 150 because if you do the ventilation case, particularly as long as you implied, 50 years would be longer than we expect the repository to operate because the waste -- it takes 20 some years to put in. You do take significant energy out in the ventilation system or you can depending on how you do it.

MR. RYDER: Actually, I did run just some very short ones based on the limiting 30% extraction ratio and the 7 1/2 foot borehole spacing for a variety of ages, but it has, you know, initial kilowatt loading of .5 which is very low considering even with 50 years of additional aging the average kilowatt loading will be much higher than that. And, the temperatures for 60 year old fuel peak at about 150 degrees at the borehole. So, what Tom said is correct to answer it.

DR. HUNTER: You also have to assume you're dealing with about 1700 acres. If you change that assumption, then it changes, of course.

MR. RYDER: Which limits your flexibility at the site.

DR. NORTH: So, that's using essentially the whole available space?

MR. RYDER: Of the primary block, yes.

DR. DON DEERE: Is it fair to say then that the ventilation is probably a more important way, a more reliable way to lower temperature than aging?

DR. HUNTER: Well, as was just pointed out to me, one thing you also have to account for in the design is what that hot air does to your ventilation system during operation. It can cause some very serious impacts on the size and number of shafts and the air flow requirements if you essentially intend to take out a lot of feet. We currently don't intend to use it in the emplacement rooms all during operation to have continued ventilation. So, it is a possible thing, but whether it's a practical thing would depend on what the impact would be on the ventilation system.

DR. NORTH: Do you have a ballpark for what that does to cost if you were to plan it to b able to ventilate for 50 years for all the fuel?

DR. HUNTER: You mean, another back of the envelope, 3 x 5 --

DR. NORTH: Another 3 x 5 back of the envelope. I mean, are we talking a billion dollars or are we talking 100 million?

DR. HUNTER: I guess it's -- I don't know. I guess it's conceivable you could double or triple the air flow requirements which might require bigger or more shafts. For the record, I'd hate to put a number on the record, but you're probably on the order of a few percent of the repository costs, you know, with any of those things. You know, the difference between horizontal and vertical emplacement is 10%.

DR. NORTH: And, ventilation might be of that order of somewhat less?

DR. HUNTER: Of that order, yeah. But, we haven't done that analysis. We have not thought of the repository as a place to age the waste which essentially is what that is.

MR. RYDER: Plus the ventilation system would only be a skin temperature effect. It would increase the flex, but it's not in contact with the actual waste canisters. So, it's not sure if it would be a linear reduction in the peak temperatures. You know, I believe it would be a reduction, but I don't know by how much.

DR. DENNIS PRICE: I have a question on that. Do I understand that scatter right that as you get above the 57 kW/acre --

MR. RYDER: Just here in this region here?

DR. PRICE: Yes. Well, all the way up above it. That canister spacing, drift spacing, and canister loading makes less of a difference? It only really makes a big difference below that? Am I understanding that right?

MR. RYDER: Okay. You're near-field temperatures are primarily controlled by your nearest neighbors which would be the ones adjacent to them within a drift. And, in these cases, the main thing that causes the banding in these at the earlier times is that -- and, yes, I think you're correct that as you get to higher APD's which are closer spacings, you start to smear that effect. So, yes.

Any questions? Any more?

(No response.)

MR. RYDER: All right. Similarly, at the one meter rock temperature goal affect, again you don't violate the temperature goal until you get above a Local APD of about 100. The 57 number has at least one and potentially several repository layouts that would be acceptable. Okay. And, down at the 35 number we're looking at one meter temperatures ranging from 130 on up. And, potentially lower depending on the layout.

DR. NORTH: Could you give us a sense of the same 3 x 5 back of the card calculations if you wanted to try to lower that number by the same combination we were just discussing, how low might it go?

MR. RYDER: Okay.

DR. NORTH: Does ventilation make much difference with the one meter in?

MR. RYDER: It would probably have essentially the same effect. It would be a skin temperature effect. There would be some reduction, but by how much I don't know. Is that fair to say?

DR. HUNTER: Yeah. It obviously is not going to have anything like proportional effect because it only takes energy out some 20 feet away from the canister which changes essentially the boundary condition. But, I guess you are asking to estimate what this temperature would be if we had that other case which we estimated to be 150?

DR. NORTH: Right.

MR. RYDER: That would correspond to a peak temperature in this range of 130. These are essentially the same layouts that were shown in the other. They're the same scatter points. What we've found was that when you drop below or around 2 kilowatts canister loading, this becomes the controlling goal in terms of establishing your canister-to-canister spacing. And, that's why I put it in.

DR. HUNTER: You could just ratio the lower left hand number off these charts, I think, and -- now, I guess, we should go for the -- we really didn't calculate the 150 number. You realize that was truly a back of the envelope based on some guesses on the previous curve. That is something we could do without much difficulty.

DR. NORTH: It seems to me it would be an interesting sensitivity case to essentially ask the question how low could you get these temperatures if you really tried.

DR. CORDING: And, some of your goals may not be those local temperatures, but it may be the contact between the Calico Hills and the overlying welded tuff. And, I would imagine there would be more effect there than right at the borehole on air circulation.

MR. RYDER: What we found is if these are satisfied, the peak borehole and peak one meter temperatures, then the far field temperatures were satisfied. Those goals were also satisfied. These were the controlling factors in those.

It is important, however, and to address actually a good setup, the goal between the TSw2 and TSw3 interface, this is just to show you -- this is from the interactive graphic system -- the approaches from the repository floor to that interface, the contours of that, you can see this 200 foot line running right through the center and on the -- for, I guess, the majority of the primary repository block, 200 feet is a reasonable approach. You do have closer approaches along the perimeter, but remember we're looking at a central canister within a central panel. And, that would be more consistent in this region and this region. Perimeter effects would cool that temperature or the temperatures that you'd see. So, we'll be looking at just a 200 foot approach and these are again -- one nice thing about this is you see it getting much more linear as you get further away from the waste package which discusses your point.

Again 57kW/acre, you can see you're down in the 80 to 85 range of temperature moving all the way up to 80 kilowatts design-basis. You've exceeded 100, maybe 105, say, and you don't really exceed the 115 limit until you get well beyond 115 Local APD. So, this just supports my point that if you satisfy the near-field, the far-field will just fall in according to our current goals. And, just also to be complete here, the 35 number, you can see the temperature change is on the order of -- is 55 degrees down there. Now, this model doesn't include the temperature gradient or the thermal gradient. So, this might be slightly higher based on that, but still is quite low.

DR. CORDING: Just one question. You had indicated a six panel array for the model.

MR. RYDER: Um-hum.

DR. CORDING: Now, when you get further away, you're basically assuming though that it's basically a large areal source, larger than six panels. Is that correct?

MR. RYDER: Actually, this still considers the discrete heat generating sources at this distance.

DR. CORDING: It considers those, but it considers them for the entire repository? Are you --

MR. RYDER: Okay. No, it's still the six -- all right. It's a six panel model. Since we're looking below a central canister, additional panels would not see any effect at the peaks because they're far enough away from where we're looking straight down below that central canister where there are --

DR. CORDING: At some standoff distance, the other panels beyond six panels would have an effect?

DR. HUNTER: That's right. In other words -- but the width of the panel is on the order of what? The total width is --

MR. RYDER: 1400 feet.

DR. HUNTER: 1400 feet. So, we're 200 feet away here.

DR. CORDING: Okay. All right. So, you're still close, yes.

MR. RYDER: Yeah. And, finally, to look at the containment goal of keeping the canisters hotter longer, again 57kW/acre, you can see at 350 years after emplacement which would be 300 years after closure, is that correct, well above boiling, and very close to it, at 1000 years there's a range here. Going up to 80, you significantly increase your chances of keeping a central core of a panel hot. However, if you were drop down to the 35 number we've been talking about, your chances are very unlikely that you'll be able to meet that goal. And, that's really the only point here is that higher APD's give you a better chance of meeting that goal.

Just a quick summary, based on these calculations, historical design-basis APD of 57kW/acre easily satisfies all current SCP thermal goals and the additional calculations that I've displayed indicate that we might be able to raise that design-basis APD up to 80kW/acre. Additional studies are needed with more sophisticated models including mechanical and operational facts, et cetera, before we make any final decision on that. But, I will be carrying the 80 number through just in the hopes that it might happen.

All right. Returning to the flow diagram, what we need next is an APD scaling technique to equate thermal-mechanical effects. From the waste inventory I've showed you, the histograms, there's a wide range in ages and burnups for each fuel. So, what we need to do is to equate the thermal-mechanical effects for each type of waste against a base -- because it is possible based on different ages, like I told you, to emplace them at the same initial areal power density. We get widely varying thermal-mechanical effects because of age and burnup. So, we need a scaling technique. The two major ones that have been used are what are known as the equivalent energy density concept and the equivalent peak temperature rise concept. This is a more recent development. This is, you'll find, in more historical literature.

The premise that the equivalent energy density concept proceeds under is that if, over a given period of time, two different types of wastes with different ages deposit the same amount of energy into the rock, then the thermal-mechanical effects should be the same. What we've found in recent calculations, however, is that equating that to a baseline waste of 10 year old PWR nominal burnup fuel, which is what was used in the unit evaluation study, we get widely varying temperatures, peak temperatures at the center of the repository, and for that reason we feel or we felt that it was necessary to go to another scaling technique because this indicates that the thermal-mechanical effects will not be the same.

So, changing the premise of the scaling technique to that which scales the waste concentration based on equivalent peak temperature, we get curves like this. This is the base line and these are for the PWR fuels indicated. As you can see, the peak temperatures are essentially the same. There's some variation out here, but since the peak temperatures are the same, we feel the thermal-mechanical effects will be the same based on this scaling technique.

Now, what you get out of, you know, scaling the waste concentration is a curve like this which this is just for 10,000 burnup PWR fuel scaled to a design-basis APD of 57kW/acre. And, this tells you if you have fuel 16 years old, you have to emplace it at an APD of 57 or 56kW/acre, I guess, on down. You know, if you're at 29 years, it goes down. Or if you're at younger fuel, you would place it at higher APD's. And, that's all I want to say about scaling techniques.

The next thing we need to know is how we're going to receive the fuel at the repository and how we're going to put it in the ground. Now, you'll be hearing a lot about this later on today from Lyn Ballou. So, just briefly, I'll touch on the two majors which are, oldest first, FIFO, also called the OFF or off schedule, and levelized. Let me just put this up there.

The FIFO which actually that name comes from "first-in/first-out" or OFF is the design-basis of the SCP-CDR and it indicates that the oldest and relatively low burnup fuel would be emplaced first followed by progressively younger and higher burnup fuel. The resulting waste stream exhibits the characteristics of monotonically decreasing age and also a corresponding increase in average burnup as the emplacement goes on.

Levelized actually picks and chooses from the available inventory. And, what it chooses on the basis that it wants to have a more levelized yearly energy density and age and what that does -- well, I have actually some graphs for you. This shows you the average canister loading per year of emplacement beginning at 2003. As you can see, the FIFO for off schedule varies from around .5kW a up to over 2.25kW per can, whereas by picking and choosing from the inventory, you can levelize this so that you get about 1.8 kW per can, I guess. Yeah, 1.8 more constantly. And, that has some design implications that will be talking about later.

DR. HUNTER: Eric, how many tons per can is that?

MR. RYDER: This is on a basis of two tons per can or a little more than two. That's also a hybrid canister, I believe.

DR. PRICE: Is the reason that goes -- the levelized goes up right down toward the end is your options for choosing or --

MR. RYDER: Yes. Yeah, you don't have as much to choose from anymore. So, you have to take what you got.

Okay. And, this just shows the age for the two schedules. Actually, it's two levels. Again, the FIFO has a widely varying age, you know, sharply decreasing; whereas the levelized is more banded. They both have the same average and this would be a much easier to design around. Again, you'll hear more about that later.

Tom?

DR. HUNTER: You might point out that the age for 2010 is what if you would use the FIFO?

MR. RYDER: Okay. I've got 2013, the average age is 31 years. It would begin up here at an age of 41 years and drop down to about 26 years down here. The 2003, that starts at 31 years and goes down to about 16.

DR. LANGMUIR: Are there any practical reasons or logistic reasons why you can't go with a FIFO approach?

MR. RYDER: Well, FIFO approach because of the widely varying canister loadings you've got just a tremendous range of canister spacings required to emplace it at the APD necessary. Whereas, with the more levelized approach, it becomes much closer, more constant. And, I believe Lyn Ballou will be talking about this later. So, I don't really want to take away his talk.

DR. HUNTER: Yeah, I think the question of construction, if you fixed your drift width, you then have to vary the canister space, am I right?

MR. RYDER: Right.

DR. HUNTER: So, you've got eight canisters a day coming in. You've got to have the boreholes drilled ahead of time. You've got to have everything programmed exactly right to get all the right spacing. It's a little more complex.

DR. PRICE: It could be done, though.

DR. HUNTER: Oh, sure, yes. In the age of computers, anything can be done.

MR. RYDER: Sure. All right. But, there are some benefits to the levelized which will be talked about later, as I said.

All right. With those two taken care of, what we need to know, there are fixed area requirements on the SCP-CDR layout that you've seen and that has to be considered when we look at area requirements. This is from the layout that you've already seen. This just demonstrates there is for the underground support facilities, we need about 36 acres currently. The mains and the main standoffs require about 112 acres and panel truncation takes up about 34 acres. This all adds up to about 182 acres total required for fixed area requirements.

That leads us to the final sections of this which is area required for spent fuel emplacement, and in conjunction with fixed area requirements, that gives us total area requirements. Now, on this table you'll see for both the FIFO and levelized case and the two design-basis APD's we've been discussing. At the beginning of the talk, I indicated that defense high level waste essentially was a constant area requirement and it turns out to be about 93 acres currently. For the 57kW/acre, for both the FIFO and levelized, it's on the order of 1430 acres. There's really no benefit in terms of area requirements for either emplacement schedule, total area. But, in terms of the micro kind of design basis where you have to look in each section and the spacings, you know, there may be a benefit. I don't know. And, for the 80kW/acre, you can see you save about 300 acres which increases your flexibility to account for things like the Ghost Dance Fault or any areas in the primary block that we have to isolate.

I understand you've already had a presentation on this. So just to remind you that the primary block is approximately 1690 acres without the expansion zones. So, even at 57kW/acre and a 2003 emplacement start date, there is about 200 acres of flexibility.

DR. NORTH: What changes if we go to 2010 start date?

MR. RYDER: Okay. Wish I'd brought that curve. It's between 5 and 10% decrease in area requirements at 57 and also 80. Changing the APD actually has a stronger effect in aging on reducing area requirements.

DR. HUNTER: Well, we clearly won't be doing 2003 on the current schedule. So, it's more like 2010.

MR. RYDER: Actually, I keep getting set up here. I like this. A couple of the strong factors that impact area requirements are APD which I've already discussed and I've shown that the higher the allowable design-basis APD, the less area required for waste emplacement. In the case of the 2003 emplacement start date, that's a 300 acre savings.

Delayed start date, I know this is a good topic. You'll all love this. As the emplacement start date becomes later, the fuel inventory ages becomes colder and it becomes possible to emplace it in a diminishing amount of area. However, there is a point when it gets so cold and it's on the order of about .5kW that you're constrained by geometric limits, meaning the 30% extraction ratio which translates to a little over 16 meters in drift-to-drift spacing and a 7 1/2 foot canister-to-canister spacing. So, there is a point where you actually don't save anything and, in terms of the containment goal, you may not be able to meet it because you won't be able to emplace it at the required initial areal power density by the APD scaling technique.

DR. CORDING: That 30% extraction ratio is based on stability of the drifts. Is that correct?

MR. RYDER: Yes.

DR. CORDING: Of the pillars?

MR. RYDER: Um-hum.

DR. HUNTER: Basically, it's derived from that plus practical mining concerns about how you want to do the layout.

MR. RYDER: Now, you've already seen the top curve of this when I showed the FIFO versus the levelized emplacement and this shows your average canister loading as a function of emplacement start date over the emplacement life of the repository. As you can see, it sharply goes down with increasing age. Now, if you were, as a scenario, not to accept any fuel younger than 50 years, that would correspond to a 2043 emplacement start date which is this line here. And, I've already indicated that the .5kW line is a rule of thumb kind of cutoff point where you get the -- you're geometrically constrained and that would correspond to the first four or five years of emplacement where you may or may not, probably not, be able to meet the containment goal of keeping the canisters hotter longer.

DR. HUNTER: Eric, excuse me?

MR. RYDER: Sure?

DR. HUNGER: Is that limit just because you can't put the canisters so close together in a given drift?

MR. RYDER: Right. Based on the scaling technique for waste of this age in this region here, you simply can't put it that close according to the required initial areal power density, and if you can't do that, then, you can't guarantee that you're going to keep the temperature hotter longer which I showed on that scatter plot as you drop it. You'd be actually putting it at a lower APD than you wanted and, as such, you would be at a lower end of that scatter plot which would put you at a lower temperature.

DR. DEERE: Well, would that be only for a few years?

MR. RYDER: That would be for the panels that include this. You know, this amount of fuel. They would probably never reach because it's the nearest neighbors that affect your near-field temperatures the most. Also, another point about aged fuel is if you go to, say, an average age of 50 years, it takes longer for it to establish the 100 degrees C goal. It could take up to 12 years and that's simultaneous emplacement which is conservatively high. So, it could take even longer than that to establish a boiling envelope. And, in addition, that boiling envelope won't be as large and you won't have as much of a buffer of resaturation before the water comes back into the region of the waste canister. So, there are several implications with older waste.

DR. CORDING: In all your models, you're assuming that you are still going to obtain the boiling or greater than 100 degree temperatures around the canister. Is that correct?

MR. RYDER: It is the current thermal goal and, as such, we have to address it.

DR. CORDING: So that everything that we're talking about of adjustments are based on that. Is that --

DR. HUNTER: Yeah. As he showed in that one chart, all scenarios at 57 do provide that goal.

MR. RYDER: Within the thermal core. I mean, there are edge effects at the panel which we're looking at possibly tailoring the geometries out there or possibly the canister loadings out there to keep the edges hotter longer and Lyn will be talking about that also.

DR. LANGMUIR: This may be a little bit unusual to think of it this way, but why not use electrical heaters if the goal is to keep it hot, but not to have the radioactivity doing it, from a public perception point of view?

DR. HUNTER: If you really need it, it's a time scale problem. The repository decommissioning plan is based on essentially no civilization to support the repository. That's the premise of the EPA standard. And, so you have to assume that there's really no one with the competence to run the heaters after a while because there's no civilization to support it. Basically, it's a practical thing if there's someone who can -- I mean, it's a possible thing if someone could do it, but it would -- it would kind of violate the premise of repository which is you don't have to do anything when you've walked away from it. That's just the way the system is set up.

MR. RYDER: Any questions? You must have questions.

(No response.)

MR. RYDER: Well, that's all I have to say, then.

DR. NORTH: Any further questions?

MR. RYDER: Aging questions?

DR. NORTH: Yes?

DR. BOB SHAW: I'm Bob Shaw from EPRI. My first question is do you assume throughout that in your point source model that every point is the same intensity?

MR. RYDER: It's a canister model. Yeah, it is. It's the same average nominal waste with the same average age. Yeah.

Dr. Shaw: I'd like to point out that, first of all, I think that's going to vary.

MR. RYDER: Yes.

DR. SHAW: And, the significance of the variation of heat intensity from point to point could have a significant effect on some of your calculations.

MR. RYDER: Well, that's why we have the scaling technique to equate that. We'll emplace cooler, older waste closer and hotter, you know -- it depends. The waste concentration changes based on that.

DR. SHAW: That brings me to my other point which you've assumed almost infinite flexibility here, I think, in obtaining the fuel from the utilities so that you can mix and place appropriately.

DR. HUNTER: Yeah, well --

DR. SHAW: And, as time goes along, that becomes less and less likely because as you approach, for example, 2003 and there hasn't been any removal from the plants, you're going to have on the order of 1/3 of the plants who have been forced to have on-site dry storage. They're going to be highly reluctant to remove those particular systems and they're going to have particular choices themselves as to which fuel they would like to have removed from their fuel storage pools, et cetera. So, the flexibility, the first-in/first-out, you know, goes by the wayside already because first-in is already out and in dry canisters. And, that's not the fuel that people want to ship. And, so I think the importance of getting input from utility organizations, we know there's negotiations going on constantly with DOE on this subject, but I think including some of that variation in your calculations -- you've done a lot of variations on parameter studies, but I don't see that taken into account. I believe that you'll have some significant variability from can to can in the amount of energy just because of those practical limitations.

DR. HUNTER: That latter point about the flexibility of what you can actually get -- particularly if there's no MRS at a repository -- is going to be very significant if you try to maintain all of these criterion. Essentially, you're really -- in one case, you're just trying to take it and dispose of it; the other case, you're trying to manage it in a very controlled way.

DR. NORTH: What plans are there to address the issue that Dr. Shaw just raised? I mean, are there plans to do a series of calculations where you vary these uniform loading assumptions?

DR. HUNTER: Well, he made two points. The first point was about assuming that there are not unit -- that each package, I believe is not identical to the other package in terms of its thermal characteristics. And, the other was how do you modify the inventory so you know what your input rate is. I really think the initial point is or can be covered by fairly simple calculations and using these equivalent methods like Eric talked about. Eric might want to comment further on that.

MR. RYDER: Yeah. Currently, the model -- I mean, there's so many canisters and so much variability the models just can't handle 35,000 canisters with different decay curves, et cetera. So, we have to make some assumptions and this is one of them. We will hopefully being going to more complicated models that allow for five definitions, et cetera. Again, remembering this is an analytical model and it has limitations.

DR. HUNTER: It is, however, a model based on linear superposition which means you can add together 35,000 if you're willing to wait.

MR. RYDER: If you wanted to make 35,000 runs, you could do each and every canister, sure.

DR. HUNTER: In concept, it's not a very hard thing to do.

DR. PATRICK DOMENICO: Well, why don't you do it numerically and save all your problem?

MR. RYDER: Because numerical models take substantially longer to run. I mean, we can -- you saw all the scatter plots. That's just a small fraction of the number of runs I've made with this and, in terms of efficiency and cost and being able to get these numbers faster, understanding the limitations -- and we've got comparison models and comparison studies going on to see what those limitations really amount to quantitatively. And, we have run some numerical models also.

DR. HUNTER: That's an excellent point. You don't actually use these scoping parametric studies for design calculations. Once you get close to layouts that you think are reasonable, then you do more complex, either 2 or 3-D, models depending on what you're doing and get more -- and different scales and different regions of interest.

MR. RYDER: Sure. Well, with these models, we can spend just a large number very quickly and find out the regions where we want more interest and more sophisticated models in. Is that correct?

MR. WILLIAM COONS: I'd like to address this question to Tom Hunter and this is in response to Don Langmuir's question or statement about the heating electrically. And, when you indicated that EPA says that there's not going to be a civilization around, isn't that at variance with the fact that they're talking about human intrusion scenarios? How do you address that?

DR. HUNTER: Well, the basic premise of the standard is that you don't require active institutional controls to keep -- to maintain isolation of the repository. And, basically, it allows any assumption on civilization. Human intrusion is based on the fact that there are people there who will do things in the future and you try to predict what they're going to do. But, you don't require them to have active ways of monitoring and controlling the repository. If you look at human intrusion, say, for future drilling operations, it's assumed that the people doing the drilling have no knowledge there's a repository there. You try to do something to make them have some knowledge, but basically you can have just an advertent drilling intervention at some later point in time. It's basically based on the premise that this generation is supposed to protect future generations from having to be concerned with controlling and monitoring the repository. Maybe someone from the NRC would like to elaborate, but I believe that's the basic understanding. Seth, is that --

MR. SETH COPLAN: Seth Coplan, NRC. Probably the question would really be best addressed to EPA, but my recollection is that Tom has it about right. That at the time the standard was being formulated, the concern was that this generation should not be doing things that would cause future generations either harm or some kind of burden in terms of maintaining the upkeep of the repository.

MR. JACK PARRY: What the standard actually says is that active institutional controls over disposal systems should be maintained for as long a period as possible. But, they essentially -- you can't take credit for beyond 100 years.

DR. HUNTER: Yeah.

MR. PARRY: It doesn't say they there won't be there.

DR. HUNTER: Yeah. And, Don's question was one of maintain some active input of energy to try to do that.

DR. LANGMUIR: Just take the uncertainty out of it.

DR. HUNTER: Yeah. But, you'd be taking credit, I believe, in that case for that happening.

DR. LANGMUIR: Not necessarily.

DR. HUNTER: Not necessarily, okay. That's interpretation, yeah.

DR. LANGMUIR: Yes, that's right.

DR. NORTH: Any further discussion or questions?

(No response.)

DR. NORTH: I think we're ready to go to the next presentation, then.

DR. DEERE: All right. Just as a transition from what we have been listening to and discussing into this next, I think that the thing that has been driving the design is going to be brought out right now and that's maintaining this temperature right at the near-field.

DR. HUNTER: The other thing is that we are trying to limit it to a finite area like 1400 acres of waste emplacement. That's the other factor which also keeps you from -- and set up some flexibility.

DR. NORTH: Yeah, I think it's very important for us to gain an understanding of just how that separation occurs, to what extent you're driven by the area consideration and to what extent you're driven by the goal of maintaining the temperature on the canister.

DR. DEERE: And, at the same time, how this could be affected by different amounts of ventilation or the cost because we have no feeling for this.

DR. THOMAS BLEJWAS: Well, I'll try to bring some of the things that we've talked about together. However, I first want to talk a little bit about alternative designs, one of the things that you had expressed an interest in, and also get back to a discussion, a very brief discussion I hope, of the contingency plans.

When we talk about alternative designs, generally we're not emphasizing just parametric variations. When we talk about parameter variations like varying borehole spacing, drift spacing, standoff, et cetera, we usually are talking about those based on a given layout. And, we think we can account for different waste characteristics including age by varying the parameters within a given layout and that's what the discussion up to this point has been focusing on. So, typically, when we talk about alternatives, that's not what we're talking about.

The kind of alternatives that I think we would classify for alternative designs would be, for example, the horizontal/vertical option. And, we heard a little bit about this yesterday. I just wanted to reiterate our present position on this. We've done a study and made a preliminary recommendation that for the near term, at least, vertical will be our reference orientation and we're going to terminate all work on long horizontal emplacement. As Tom Hunter mentioned yesterday, it places a lot of uncertainty over a higher degree of uncertainty with respect to our ability to retrieve the waste, if necessary. But, we are going to maintain the flexibility in our exploratory shaft facility to possibly perform some horizontal tests. However, any horizontal emplacement will be a maximum of two or three packages per borehole is our present position. So, these horizontal tests will take that into account. We're talking about short horizontal now, not long horizontal.

Also, we've decided that it would be prudent for us to re-examine the question of orientation at the start of advanced conceptual design. By that time, we hope we'll have additional information and it may be that there are advantages to horizontal over vertical that we did not recognize in this preliminary study.

Also, there are other options that I would put in this class of horizontal/vertical that are under consideration as part of the alternative study, but I know you're going to be getting an update on that in the near future and I won't be discussing that.

DR. NORTH: Have you looked at options other than horizontal and vertical, other angles?

DR. BLEJWAS: We are presently looking at options that include a variety of layouts and that's what you're going to hear about in the next meeting. I'm not prepared to talk about those today.

DR. DEERE: That will be at the Las Vegas meeting coming up in April?

DR. BLEJWAS: Yes, the April 7 where you're going to get an update?

DR. DEERE: Fine.

DR. BLEJWAS: I wanted to use a couple of viewgraphs very briefly that I put on the screen yesterday just to remind us what some of the important temperatures may be based on what we presently know about the rock. I wanted to emphasize that a temperature like the boiling point of water, 95 degrees at the repository horizon, we get a couple of effects; we get the removal of pored water and we also get the dehydration of the hydrous minerals. So, if we wanted to expand and look at other potential goals for the repository, these are perhaps -- that's perhaps one good temperature to look at, 95 degrees, primarily because we have not seen any significant temperature effect below 95 degrees. And, we have done quite a bit of testing at temperatures below 95.

The other temperature that I think is also important is the temperature at which we begin to see some silica phase transformations in the welded devitrified rock; that would be the Topopah Spring unit. We have, you know, various -- we have some cristobalite and clinoptilolite in that region, and if we would stay below 150 degrees in that region, then we would be relatively confident that we're not going to have any silica phase transformations.

DR. DEERE: Are those from your yesterday's presentation?

DR. BLEJWAS: Yes, they are.

DR. DEERE: Yeah, okay.

DR. BLEJWAS: Okay. So, I'm getting to something that we've been talking about primarily with questions and answers over the last couple of days and when I prepared this I didn't know which questions you'd be asking. So, I may vary a little bit from what I have on this viewgraph. Why wouldn't we want to emplace only very old waste? And, here, I put up what our present design requirements are, the present approach to perhaps meeting those requirements, and then what effect we might have if we were only looking at, say, 50 year old waste as the minimum.

If we want to limit the borehole temperatures, the temperatures near boreholes, we would limit that through the areal power density through variations that Eric just described. We think that for the present limits, those are easily met; however, there's been some questions as to whether those limits are limits we want to stick to throughout the design process. If we put in older waste, indeed, the temperatures would be lower as you would expect. Also, limits on temperatures of the container in the borehole, it's about the same. However, I'd like to point out that we don't really have to get the 50 year old waste, as we were discussing earlier. In our back of the envelope calculations, you can see that we have a potential to perhaps get below 150 degrees for most of the rock around the openings under the present design concepts if we look at other options for how we do the layout, other variations of spacing. So, it's not clear in my mind that we really need to go to 50 year old waste. We can probably accept the present plans and look more at varying spacing if we did indeed decided that we wanted to primarily have temperatures below 150 degrees near the boreholes.

I'm not really going to talk much about limiting the surface temperature and rise in the uplift. Under the present limits, that's pretty easily met and for 50 year old waste then it would just be more easily met.

We want to limit the extent of saturated conditions. Under our present approach, we recognize that we have a relatively complex local flow system and that's going to require a better understanding for us to understand the effect of the heat on that local flow system. However, if we go out to, say, 50 year waste, that local flow field will probably be less altered, but it's not clear that the phenomena would change that much. We're still going to have a 100 degree boiling isotherm; we're still going to have altered the flow field around it. It's just that it's going to be different rather than a totally different phenomenon.

So, any advantage is in my mind indeterminate at this point in time. It may be better, however.

Then, we get to the one that goes against the grain and that is the one that we've zeroed in on that causes us to want higher temperatures, that is limiting the corrosiveness of the container environment. And, under our present approach, it's clear that we probably can keep most of the containers hot, and hence, we believe that then they would be dry. With 50 year old wastes, we cannot insure that hot environment, and if we were gong to insure it, we would get back up to raising the temperatures under these other constraints. So, we do have a potential for drying that is lower with this 50 year old waste.

And then, the last one, limit the temperature in the adjacent units to reduce mineral alteration. These temperatures can be met under our present constraints and the alterations are probably not important in our minds, but we recognize that we have to study them a lot more to be sure of that. However, if we looked at older waste, the temperatures would be lower and the alterations are less likely. Again, as I pointed out earlier, under our present design concept, if we want to keep the temperatures in the Calico Hills unit, the zeolitized tuffs, below 95 degrees, the temperature at which we believe effects start occurring, we can do that presently by altering the areal power density and still have enough area to do the present plans.

I'm a little surprised I didn't get more questions out of that. But, I'll quickly go on to my contingency plan. All right. What I've put here is the contingency plan as laid out in our site characterization plan and this is for the repository design and operations. And, in our site characterization plan, we recognize that we may find when we're constructing and operating the repository conditions that are outside the design-basis. And, the kinds of things that we might find is we might find perched water. We might find water recharge pathways. We might inadvertently mine into areas that are very rich in lithophysae and there's a variety of other things that we probably haven't even thought of yet.

However, when we actually get to license application designed, what we hope to have/plan to have are ranges of parameters. We hope to have parameters that are within what we will call our baseline design. For those parameters, the baseline design will apply. That will be the heart of our design. However, we will have another band of parameters that will be outside that baseline for which we will have a contingency plan and that contingency plan will describe modifications to either the construction or the operation of the repository.

Finally, you have to recognize that we may have parameters that fall outside of both of those range and they wouldn't have been approved in the licensing process. The whole purpose of the contingency plan is to have the highest degree of confidence that we can that we're going to find things in either this category or in this category and minimize the potential for finding things in this category. So, we're going to have to get a lot smarter over the next 10 years and include broader ranges of parameters as practical and include those in either our contingency plans or in the baseline design.

I should mention that for the mechanical thermal type of considerations some of the things we've been talking about now for the mining operations, for example, we would expect to use existing empirical approaches for, say, the mechanical stability. In our baseline design, we will include and account for major faults. That won't be outside our baseline design. We will have contingency measures that will trigger minor parameter -- or contingency plans, rather, for minor faults.

Now, I've mentioned several times contingency measures, what will they perhaps look like? Right now, we really think that we've only identified two potential types of implementing modifications. One would be to continue development if we found something that was outside the range that we anticipated, but we would have revisions. For example, if we're dealing with mining and we found rock that was of a class or a rating that was lower than we anticipated, we might have increased ground support or our calculations in advance may have shown us that we needed to reduce the thermal loading in that region. So, that's one possibility. Another possibility is that we will skip and isolate unfavorable areas; so that, for example, if we found an area that had a much higher saturation, had some water flowing through it that we didn't anticipate, we would skip that area. Those are the two types of things we have planned now, but we recognize the contingency plan is going to have to be much more complete by the time we get to the license application.

DR. DEERE: Question?

DR. BLEJWAS: Yes?

DR. DEERE: On your first point there of the increased ground support, what is your baseline ground support that you're considering? Is it a rock bolt, shotcrete system?

DR. BLEJWAS: Yes. It doesn't include shotcrete. Our baseline ground support system is rock bolts and wire mesh. And, part of the reason for that is in G-Tunnel in the welded tuff we found that that's more than adequate. We could probably get by with even less ground support in G-Tunnel.

DR. DEERE: We'll take a look at the shotcrete when we go into the N-Tunnel. That's --

DR. BLEJWAS: Yes. Of course, that's in the non-welded tuff.

DR. DEERE: Exactly.

DR. BLEJWAS: Most of this mining, all except the ramps and the shafts, would be in the welded tuff.

DR. PRICE: Your increased heat load goes counter to several things as you've indicated to protect the container.

DR. BLEJWAS: Yes.

DR. PRICE: I think you said the mechanical effects of the heat and so forth, the 50 year old waste advantages, some of them are lost because you have to protect the container.

DR. BLEJWAS: Yes.

DR. PRICE: As one of the alternatives or contingencies, do you have a thick-walled container kind of option available to you as one that would be more impervious to corrosiveness and would wipe out that problem?

DR. BLEJWAS: Well, I would like to defer that question to the people from Livermore that will be discussing the waste package since that's not really something that I'm very familiar with. And, they will be talking about the waste package for a large part of the rest of the day. I can have them try to answer the question now or put it off until later at your preference.

DR. NORTH: Why don't we refer that into your presentation, Les. Are there any other questions or points for the presentations we just heard?

MR. MAX BLANCHARD: Dr. North, this is Max Blanchard. During the time that we were preparing the draft SCP and the final SCP, we had a number of discussions as we were trying to evolve design goals as you might surmise. The discussions were interdisciplinary and the people that were responsible for repository and waste package design and material selection were discussing with the geologists and the geochemists what is a reasonable temperature to try to select for a goal for keeping the Calico Hills zeolites below. And, because we don't have very much information in today's presentations about that, I asked last night one of my staff, Jerry Boak, to summarize, after talking with the Los Alamos staff who were deeply involved in these discussions, briefly what were the thought processes or the high points of the thought processes and we're willing to share that with you with -- I think he has three viewgraphs, should you so please.

DR. NORTH: I think that would be an excellent thing to put in at this time.

MR. BLANCHARD: Okay. Jerry?

DR. NORTH: We can make arrangements to get copies of these viewgraphs.

MR. JERRY BOAK: Thank you. I'm going to start with this viewgraph that you've already seen. The main formations that we're interested in here are the repository horizon in the Topopah Springs member and the Calico Hills member beneath it which is the main zeolitized zone and the alteration of minerals that are confined primarily to veins in both of these. We don't see as much veining, as many fractures, as many filled fractures in the Calico Hills as we do in the overlying Topopah Springs.

The main zeolite -- and zeolites are what are generally considered to be the most fragile minerals, the ones that are going to have the strongest thermal effect as a consequence of the repository. The main fracture filling zeolites in the Topopah Springs are mordenite and heulandiate and they occur almost exclusively in the fractures. There's not much matrix zeolitization in the Topopah Springs. On the other hand, in the Calico Hills, we have matrix zeolites, predominately clinoptilolite and to a lesser extent mordenite and in some zones a whole range of other zeolites. These tend to be silica rich zeolites. They have a high silica to aluminum content. There's a great deal of them in the Calico Hills, up to 70% and perhaps more clinoptilolite in some samples. It's contained in the matrix and again, as I say, it has a high silica to aluminum ratio and these zeolites tend to be much more stable than the more aluminous zeolites that have been described by many metamorphic petrologists in terrains where the volcanic rocks are a little less silica rich. And, that's an important point.

A number of experiments have been done in which zeolite samples both from natural tuffs and specifically prepared zeolites -- some of which have been cation exchanged to extreme ranges of calcium, sodium, or potassium -- have been heated over short term as high as 300 degrees. And, when they undergo this heating, they undergo a volume decrease. The unit cell volume decreases by as much as 8.4% and that's in sodium Na-clinoptilolites, a relatively extreme composition. It's only about 8%. As far as I understand it, this doesn't make a large difference in the major channelways in the zeolites. They're quite large and they're capable of accommodating some very large cations, not quite an eight lane freeway, but they're large enough to hold in some instances two of these things side-by-side. When these samples are then cooled back down, they recover nearly all of this volume loss. There's very tiny amounts of volume decrease.

DR. LANGMUIR: Is there physical damage though? Is there fracturing, microfracturing, for example, that is not reversible that occurs with the increased temperature?

MR. BOAK: That, I'm not sure of. That, I'm not sure of. I didn't get that from Dave. And, this resaturation is done essentially on the desktop at approximately 22 degrees and the ambient humidity at Los Alamos. And, in fact, when the rehydration is done in water saturated air, the actual volumes are greater. So that if you take this sample from the desktop in Los Alamos and carry it to Washington it actually swells up as a consequence of the humidity in Washington. Those of you from Washington may recognize some analogies there. That's not an official department position.

When you heat these things for longer terms, you do get some irreversible damage to them. However, samples heated for up to five years at 100 degrees Centigrade show only this essentially fully recoverable or nearly fully recoverable volume loss, so that -- I'm not certain if those samples are still in the cooker, but at least after five years they haven't shown any evidence of clear damage. On the other hand, when you heat the samples up to 200 degrees Centigrade, you do see variable degrees of irrecoverable collapse of the zeolite structure.

What this is is a rearrangement, an internal rearrangement of the lattice structure, changing of some of the silica and aluminum bonds. However, to look at when Dave Bish looked at whether this affected the sorption ratio what he found was that even for his extensively collapsed samples, and I believe these were calcium rich samples, the structure did not differ -- the sorption ratio didn't differ substantially for strontium, barium, and Europium, and actually increased substantially for cesium. In two of his three samples, the barium sorption ratio decreased approximately 40%. These are very high sorption ratios in the 10^4 range, 10,000 to 20,000 to even 30,000, so that that kind of reduction if these minerals are going to be useful sorbers and they're not going to be blocked in some other manner, the heating is not going to be a major problem for these minerals. So that they are, in essence, as I said, since they change their water content based on the relative humidity at the time, even if they've lost a fair amount of water, they're not changing their sorption values. And, I think if we had several hundred feet of Calico Hills at a relative humidity equivalent to Dave Bish's desktop at Los Alamos and we could be sure that it would stay that low for 10,000 years, we'd have a very nice barrier. We expect it to get wetter than that and these things will then reabsorb most of the water.

MR. BLANCHARD: Thank you, Jerry.

We weren't sure at the time we prepared the briefings a few weeks ago the extent to which you were interested in pursuing this particular topic. And, so we did not include a talk by the geochemists and the mineralogists at Los Alamos on this topic. And, Jerry, last night, worked this out. It's a representation of what was there and there's considerably more there from a theoretical and a laboratory basis to provide. Should you be interested in it, we would be pleased to prepare a briefing sometime in the future on this subject in greater detail.

DR. NORTH: Yeah, I think it is a topic that we're interested in. Perhaps, Don Langmuir and Pat Domenico may want to comment more extensively about it. My sense is that what we're ultimately going to be interested in is seeing the performance assessment calculations to assure that the heating in the design plans does not introduce problems or uncertainty on potential problems. That would be a serious concern. I think there is a burden of proof on you to deal with those problems and some unease not only in our membership, but the wider community as to the extent to which that's been done. So, it definitely ought to be an open agenda category for the future, and to the extent that you can give us some presentations in the future, I think they will be most welcome. It may be that our hydrogeology people would like to have a separate meeting to address this in more detail. I think the risk and the performance analysis point of view, we're very interested in seeing the summary of the implications in this area for performance assessment.

DR. DOMENICO: In the sense we're getting off heat transport here, one last comment. Not a question, a comment. The whole European program has gone to cool waste. They have gotten rid of the heat problem. We're the only country in the world that's going to bury hot waste. Their design goals would be to keep the canisters colder longer. Somehow, we have decided to keep the canisters hotter longer and made an advantage of the heat. I think, as Warner said, there is a burden of proof that follows that, that sort of design.

DR. HUNTER: Tom Hunter. Just two comments, I think, and following some earlier discussion to clarify a couple of points. One is Ken Beall just ran a kind of quick ventilation assessment of the needs if you did the whole repository and we need to go back and look at this. But, it may be the ventilation is impractical in terms of the amount of air flow you would really need to do anything significant. We'll take a look at that and be sure, but I don't want to leave the meeting without indicating that there are some serious limitations of using ventilation as a way to cool the waste for the repository as a whole. We'll take a look at that.

Secondly, to kind of summarize the point in particularly that Jerry just made and one, Warner, I think you were really indicating that the burden of proof that we should demonstrate in our next discussion on performance assessment, we really have not identified any negative impact of temperature at this point. That is a function of the basic performance allegation, though. And, I think that's a way to view the problem. You have a basic allocation of performance which essentially includes no credit for retardation in some cases. If that's the case, then you don't pay much of a penalty for temperature excursions and mineral transformations, for example. I think we have to look at it in the context of a basic performance allocation and then how that performance allocation might change or be altered as we learn more about the site or what the confidence we really have in that is and ask is there really any disadvantage to these thermal effects, at all? We'll try to address that when we discuss the whole performance assessment question with you.

DR. NORTH: Well, to talk a little further in that dimension, it seems to me part of the problem is your lack of data, that you don't have much experience with rock at these elevated temperatures, and to the extent that there is concern about thermal-mechanical effects or about chemical effects, you need to deal with those concerns. There's scale-up questions that may be that the testing of the kind that's been done at G-Tunnel doesn't give you the kind of scale that would show up some problems that might occur with larger volumes with surface to volume effects. I'm no expert in those areas. I'm speculating. But, what you're, I think, going to have to do is satisfy the expert community that you have really thought through these issues and that as part of your data acquisition you have obtained the data needed to validate the model calculations that you were doing as part of the performance assessment.

DR. HUNTER: I think your comment also about having interaction with the geohydrology members of the panel is important because if, in fact -- in a simple analogy, if, in fact, there's one small layer of maybe 50 meters which has a travel time, with confidence that you know under all conditions will be on the order of 40,000 or 50,000 years, then the question of what the impact is in the first 10,000 years of temperature effects becomes a different question. And so, you know, the real critical data and the real important thing is to decide, you know, what is going to dominate the ultimate transport process.

DR. DOMENICO: It has not been demonstrated that your hydrologic barrier, your hydrologic system is that robust. Those calculations of 40,000 year travel times are suspect. And, I don't think that the robustness of your hydrologic system is going to be determined until you take a good look at the Calico Hills. So, I think before you can say we don't need credit for the geochemical barrier because we have a robust hydrologic barrier, I think that's premature. I don't think it's been demonstrated yet that that hydrologic barrier is as robust as people have said.

DR. HUNTER: No, my statement was not that it was. My statement was if it were, it would impact the -- yeah, I think we do need data on that. Exactly right. And, critical data is the data, for example, in the Calico Hills hydrologic system.

DR. DOMENICO: And then, there's a thing called redundency and you'd like to have as many barriers as you could possibly stack up again to ease the uncertainty effects.

MR. BLANCHARD: Pat, we share your concern and that's why we'd like very much to be able to begin site characterization in an intensive way from both the surface and the underground and are looking forward very much to the opportunity to conduct in-situ tests from the exploratory shaft.

Well, we're ready now to shift from the repository design thermal discussions into the waste package if you are and our first speaker is Lyn Ballou who will be talking about the design requirements for the waste package.

MR. LYNDEN BALLOU: I will try to be a little more brief than Tom was yesterday in elaborating the multitude of repository design requirements because, in fact, many of the requirements, in particular the issues that seem to be dominating this meeting with respect to thermal considerations, are essentially identical to those requirements or goals that Tom has already elaborated at great length.

Let me go back to one viewgraph that Mike Voegele used yesterday morning, the infamous singing pig diagram. A couple of points that I would like to make on it or to reinforce that he did not talk much about yesterday with respect to waste package considerations, design and performance assessment considerations is that it's our feeling that the design requirements for the waste package and the ancillary components of the engineered barrier system and the close proximity to the packages are going to be very largely dominated by the postclosure performance objectives of Part 60, those having to do with containment and control of release from the EBS.

And so, a great amount of the emphasis has been put on what is really erroneously from the waste package point of view labeled postclosure. It's labeled postclosure because that is the time period during which these performance objectives apply, but in fact, when you examine the system -- and some of the things that Eric pointed out this morning are consistent with this and we may show you some more along the way -- is the point that many -- in the very near-field and within the packages themselves, the peak temperatures all occur pre-closure. The peak temperature gradients in the system occur pre-closure. And, by the time closure occurs at a minimum of 50 years following initiation of emplacement, you're well over the peak for most of these very near-field phenomena and on the down side of the cooling curve. Certainly, not true at significant distances away from the repository horizon, up or down or laterally, but within the near regimes around the package, that is true.

There are a number of requirements of a primarily thou-shalt-not-be type that are included within the design criteria of Part 60 in that most of them really focus in the preclosure period with respect to what you ought not put in waste packages with respect to free liquids, pyrophoric and explosive materials, in quantities that are potentially deleterious to the performance of the package, requirements on the fact that the waste form is to be a solid and is to be emplaced in a "sealed container" and have some manner of identification as to its -- or traceable to the documentation of its contents associated with the package.

There is a requirement within the Nuclear Waste Policy Act in the section that requires the construct of a site characterization plan and a conceptual repository design. There are such three subparagraphs within that section and one of them calls for a discussion of the waste and packaging schemes that are under consideration and the interaction of that waste and waste package with its geologic environment. The requirements of that section of the Act are, we believe, fulfilled by parts of Chapter 7 of the site characterization plan where a conceptual design-basis and a conceptual waste package design is described primarily for the purpose of identifying needed information and this viewgraph is really a little incomplete in that there is clearly site characterization information needed and that is discussed in this section and will ultimately be propagated down through the study plan and individual studies sort of hierarchy.

There's also a fairly large class that has to do with the characterization of the waste forms and that information is summarized in Chapter 7 and is the subject of a substantial ongoing program with respect to obtaining that information, recognizing that there are some serious limitations on what one can do in that regard. It's very hard to make a convincing case that you have taken a representative sampling, for instance, of some 1/4 million fuel assemblies that will ultimately exist for disposal in the repository with a fairly wide spectrum of characteristics, both physical and exposure experience, and so on. So, the efforts have been made to predict its performance for protracted periods of time well beyond the length of time that any of it has existed to date recognizing that, as Eric pointed out earlier, only about 20% of that inventory presently exists at any age.

The waste package and associated pertinences that compose the engineered barrier system as presently interpreted really are most strongly driven by the two performance objectives that Mike talked about at some length yesterday morning. There has been, as he alluded to, a continuing dialogue with the staff of the NRC with respect to what do these words mean, "substantially complete containment" for a period of 300 to 1,000 years or some other period that may be proposed by the department for consideration by the Commission. And, also, a lot of discussion with respect to the implementation of the control release performance objective, as well.

The particular parts of it that we have had the most difficulty in the performance allocation process have to do with the non-differentiation among radionuclides and the annualization of the release rate. In general -- and those of you have read the section are well aware there are some additional words in it and there are provisions for proposing different release limits within the terms of this paragraph. However, the text as written leads you to some problems for some isotopes, it appears, because of this per nuclide per year kind of constraint. And, curies is curies as far as what nuclide is. There is no differentiation on the basis of toxicity or released characteristics or anything else.

What that has done is lead us in the performance allocation process to allocating performance to all of the available components we have. We have in various ways operative at various time periods allocated performance to a container system. We've allocated performance to intact spent fuel cladding. We've allocated performance to the intrinsic properties of the irradiated fuel pellets themselves as being a mechanism for retardation or -- essentially control of release.

The other section of the regulation that most closely deals with the postclosure performance of waste packages is contained in the design criteria in Section 135 which requires that the individual components or properties of waste packages not compromise the overall package repository or site performance, and it sets a series of specific standards with respect to, as I indicated earlier, requirements for solid waste forms and sealed containers. That is the only mention within the rule of a criterion that requires sealed containers.

In addition, in the (a) part of that paragraph, there is a non-inclusive list of factors to be considered in the design of waste packages and it's sort of a shopping list of a wide variety of possible factors, phenomena, processes, and conditions and goes to the point that -- I don't know whether it was Dr. Domenico or Ed Cording was making yesterday with respect to the complexity and inter-dependent coupling of many of these phenomena which is conveniently covered in this factors' list as synergistic interactions. I certainly agree with the comment that what we have is a reasonably complex system, especially when it is further compounded by the presence of, for at least a significant period of time, the potential for radiolysis kinds of reactions to occur. We expect that those will be primarily within the region, within centimeters essentially, of the waste package. The attenuation of the rock will be such that those will not be significant at any significant distance.

But, certainly, a lot of the discussion the last couple of days has been on this subject of thermal loads and thermal effects and we want to talk some more about those at several different scales, specifically at three scales, later in the day; hopefully, later in the morning. And, we will try to give you some feel for some of the rationale that has gone into some of the work that has been reported to date.

There are a number of key interfaces of the waste package with other components of the Federal Waste Management System or even upstream of the Federal System with respect to the waste generators in the context of the utilities in that it's clear that we need to have time-dependent receipt stream characteristics for the waste streams coming into the repository. There are lots of parameters that are important there, but certainly, as Eric has indicated earlier, two of the dominant ones are the distributions of agent burnup as it tends to affect the thermal response of the repository, at least, or if there is an MRS in the system that is operated so as to become a modulator of these spent fuel characteristics, then that will be a key interface, as well.

Interfaces with the repository system, I think, most of them have been talked about maybe from the repository perspective in the last couple of days, but certainly when it comes right down to it, the major function of this surface facility is to be an assembly facility for waste packages. It has a few other peripheral functions, but that's its primary job. It's a plant to build waste packages in.

With the respect to the subsurface interactions and interfaces, Eric has dealt specifically with some of those, but there are, you know, with respect to the physical layout, dimensioning. Dimensions is a subject that we might talk a little more about for a minute. Several people, Dr. Deere and others, have commented on the number of bits and pieces of criteria or mythology or whatever that are residual in the system as a result of happenings in the past. Certainly, one of those has to do with the rationale that has been employed in the diameter sizing of waste packages. It has come directly from some decisions that were made -- I'm not sure how long ago, but back in the early 80's at least -- coming out of the at that time conceptual designs, but now turned into real structures with respect to the defense waste processing facility at Savannah River. The determination was made that those pour canisters for vitrified waste form to be produced there would be two foot diameter, Schedule 40, 304L, stainless steel pipe and that was known to be an input in the system. Lacking any good reasons to the contrary, we and Sandia agreed a long time ago that it would certainly make the operation of the repository system simpler if we didn't have a variety of diameters of packages to deal with implying different construction equipment, handling equipment, transfer casks, and on and on and on into the night with respect to different facilities. And, so have wound up with package configurations in the conceptual designs that are uniformly in the 26 to 28 inch diameter range, about the same size that would be appropriate for a disposal container overpack of a DWPF glass pour canister. Certainly, a subject though that might well want to be revisited in the context of the other things that are being talked about such as significantly aged waste prior to disposal.

There are a number of operations that will occur within the subsurface that have the potential for affecting performance and there are interactions and interfaces with the repository sub-system in that area. An example, certainly one of the things that if we wind up with a metallic container that will be an important consideration is we would want to avoid handling damage in the process of emplacement that would lead to preferential sites for localized corrosion mechanisms, pitting, stress corrosion, cracking, and that sort of thing to occur as a result of dings in handling.

And, there are a number of other components that will be in the engineered barrier system that are not strictly waste packages and were shown in some of the cartoons that Tom Hunter showed you yesterday afternoon with respect to shield plugs, partial or full length liners within emplacement holes, support structures of some sort assuming that the packages are not going to be levitated. A variety of other bits and pieces that have the potential for impacting upon the performance long-term of the package.

If we've done our job well, with the exception of the very near-field, there are not much in the way of interfaces with the site itself except via the repository. There are a number of information needs that arise from the site investigations program, some of which have already been talked about with respect to thermal and mechanical properties of the rock in the pre-emplacement environment, but more importantly, in its thermally-perturbed mode and similarly with respect to hydrologic properties. After all, with the exception of a couple of gaseous nuclides, the only available transport medium for release of radionuclides from this system is a water transport medium and this begins to come to the question of why keep it warm for long that has become such an interesting topic here for the last couple of days.

We are concerned with the vadose water composition as it has to do with the constituents and possible concentrating mechanisms particular for some ions that are known to be bad actors with respect to corrosion mechanisms, chloride ion, fluoride, and some others. Concerned with the flux and saturation characteristics as they relate to the development and ultimate collapse of a dried out region around the packages. But, these are really inputs to computations and observations of the thermally-perturbed responses.

This is the same list that you've seen before and there's a little bit I'd like to add to some of the bases and I'll do that in a minute. The one thing that I do want to come directly to is this third bullet. There's a little bit of a disconnect in the history that has gone on over the past few years and let me try to explain it very briefly.

At the time that the conceptually repository design was done, the tentative performance allocations that were in place at that time proposed to take credit in a regulatory sense for this type of a goal, requirement, which was translated as a requirement within the repository design requirements document that was used for that conceptual design development. As time went by and as we became more knowledgeable of the implications of the characteristics of the waste streams and the lack of direct control that DOE contractually has with respect to some of those details, as were alluded to by Bob Shaw a while ago in his question, it became obvious to us that unless there were some changes made within either interpretations or terms of some of those contractual agreements that there was really no mechanism in place for the department to have sufficient control over the characteristics of that stream, nor the timing of information with respect to the characteristics of whatever the stream were to be, to be consistent with the conceptual design of a staged development where you would either be asking for some control over the characteristics of the incoming stream or knowledge of those characteristics such that they could be factored, as Eric and Tom and others have mentioned, into the geometric construction of the future to be emplaced repository panels.

As a result of all of this, this goal was essentially downgraded to the status of a characteristic that we believed had the potential for enhancing the confidence with which we could make the case for the performances of waste packages and the engineered system with respect to the regulatory performance objectives. It is not a requirement. It at this juncture -- in fact, as the site characterization plan is presently written with respect to the allocation of performance, having to do with the substantially complete containment and controlled release performance objectives, there is no credit taken within those performance allocations for this factor.

DR. ELLIS VERINK: I'm not clear on what this factor is. More than 95 degrees or what?

MR. BALLOU: The ability to essentially maintain the walls of emplacement boreholes in excess of 95 degrees or in excess of the unconfined boiling point of water at the elevation repository for any specific period of time.

DR. NORTH: I'm going to ask that we try to speed up. A lot of this material is a review of material that was covered in the January meeting and, as I said at the outset, I think since a number of us were there, we want to go through it relatively quickly so we have time to discuss the new material.

MR. BALLOU: Right. Right.

The items that have led us to allocation of temperature limitations or goals with respect to or requirements with respect to temperatures, with respect to waste form temperatures, the primary one that has been alluded to earlier is the imposition of a peak temperature for cladding. As I indicated earlier, we have allocated performance in the containment period to cladding as a barrier that is present in the system and there are some experimental information that we believe is sufficient to support a limitation of about 350 C as a maximum temperature in an inert environment for cladding. We are certainly also concerned with the distribution of package internal temperatures, waste form temperatures, as they relate to the establishment of testing environments for spent fuel waste forms to elucidate information on fuel oxidation rates and the release rate characteristics of various isotopes at different states of oxidation.

Container temperatures, obviously this was talked about at considerable length in the meeting in January with respect to the modeling of various degradation modes and degradation rates, almost all of which are in one way or another temperatures dependent.

Borewall temperatures, again the matter of is there a water vapor or liquid transport medium available as a function of time. The question of borehole stability was talked about pretty much by Tom yesterday. And, we've heard a little bit about the mineralogic alterations question.

There is one possibility at the very near-field of a potentially detrimental effect there that's been observed in the laboratory under some conditions and that's essentially a fracture healing phenomenon associated with the precipitation of calcium minerals in fractures as a function of a thermal cycling. Again, we need to know something about the near-field temperatures in order to appropriately model nuclide transport, both mechanisms and rates, in making the transition from the engineered system to the total system performance assessment in terms of providing source terms for those kinds of calculations.

I'd like to introduce my boss, Les Jardine, to describe some of the status of the design concepts as they presently exist.

DR. NORTH: Okay. I'm going to suggest that we continue with Dr. Jardine's presentation before the break, but since it's almost all material that at least some of the board members have seen before, I'm going to ask that you go through it quickly.

DR. JARDINE: Okay. As introduced here, my purpose or intent -- and I'll go through this material fast -- is to make sure that we're aware of what the reference or baseline design is for the waste package. And, what you're going to find in the subsequent three speakers are what I would call -- we put together some applications of how thermal analysis methods have been used as part of a design assessment and they're sort of -- you will find that they're variations from the reference design in most cases. And, so I think it's important --

DR. NORTH: All right. So, we need a quick review.

DR. JARDINE: Okay. Quick review. Well, you're really making me be quick here. But, there's a couple of points I wanted to make. In putting together the presentation, I want to get my plug in for quality assurance or records because there is a long history that the project has been involved with in Livermore from 1982 onward and Lyn Ballou is our remaining corporate memory of persons who have been on the project since then when he came off of the Climax facility where he was the successful project manager. In addition, people like Gary Johnson had to go back -- who is new doing thermal analysis -- had to go back nearly eight years into our records at Livermore in order to make the presentations that he's going to do and that material was available, although it's sometimes called QA indeterminate, but we were able to do that. And, it's a very important thing that we need to be aware of is have the ability to explain what is in the SCP which is the theme here and we were able to do that. But, it's an important point to be aware of.

Now, these are the viewgraphs that Tom Hunter showed you and I'm not going to belabor it, but for the vertical reference is what we're going to be primarily talking about. The waste package emplaced in vertically in the bottom of the drift. Now, this again is Tom Hunter's slide that was used and the point maybe is that the defense waste glass is not in as deep a hole as the spent fuel because the containers are longer. They're nominally 10 feet below the bottom of the drift floor. And, remember, the reference configuration uses commingling and you're going to hear that in Lyn Ballou's talk coming up, the commingled consideration and variations from that reference design.

In terms of what the waste package configuration looks like in the SCP -- remember, what I'm trying to do is show you what is in the SCP and emphasize that -- this is a blowup of the waste package and, in general, it's nominally 10 feet below the surface of the drift. The waste container, as indicated here, is emplaced in the vertical position. The reference design has two things that are of importance; a partial liner which only comes down slightly below the pintle in order to facilitate retrieval or removal of that container should it be for retrieval or performance confirmation activities. In addition, the reference design has an air gap that would be on the order of an inch and a half or so between the container and the borehole wall. You will hear in some of the later talks where we did sensitivity studies where there was a packing material in there. In Gary Johnson's talk, he will show you a sensitivity study where the conclusion is what happens with packing in there in terms of thermal.

In terms of the kinds of materials or waste containers that will go down in the borehole, there really are two types of waste that have been considered in the SCP and in the designs to date and that is nominally spent fuel containers and high level waste glass containers. And, the difference primarily is in the reference design is the 66 centimeter, 26 inch diameter containers, nominally 15 feet in height, where the defense glass is shorter at nominally 10 1/2 feet. In the case of spent fuel containers that would be contained in this package, the defense containers have an overpack, the red here, and the pour canister that would come from the defense facilities is inside it. And, as Lyn pointed out, this is why diameters tended to be centered around 24 inches nominally in the history of the development of the waste package concept. This became sort of a design constraint.

Now, in terms of going back now -- and, I had the same problem that Gary Johnson had as what has gone on over the years in the waste package program. There are a whole lot of designs that have been looked at in various waste configurations. The favorite -- I don't know about favorite. One that received a lot of attention was consolidated fuel rods and these varied for different geometries and diameters were up to 6 PWR consolidated assemblies were put into cans and up to 18 BWR assemblies were put into cans. Studies and design concepts were also developed for intact fuel assemblies containing either three or four intact fuel assemblies; six, seven, or ten BWR assemblies are intact. And, a final one which was a mixture which I'll show you later of 3 PWR's and 4 BWR containers, you mix the PWR assemblies and the BWR's in a single container.

There also were a series of studies done in 1985 and last year in support of MRS system type studies and container configurations involving square packages that would come out of the MRS were also considered as, you know, having container designs that would accommodate the canned fuel from the MRS, be it the 1985 proposal to Congress or later studies that were done last year.

And, in terms of the range of things that have been looked at, in general, you could classify them as sort of thin-walled containers and another extreme that was looked at was thick-walled containers which was nominally, you know, 35 to 45 centimeters in thickness sufficient enough that they would do self-shielding concepts. This was a 1982 era container configuration.

And then, in addition, the kinds of materials that were looked at -- these kinds of things were reported in more detail in our January 18-19 meeting as indicated -- included the iron-based alloys or metals and copper based and some ceramics or alternative type waste forms.

Now, in terms of the specific geometries, this is what is in the SCP and I think the thing to point out is that they're basically a 26 inch diameter container and 15 feet high. In the case of consolidated fuel, you would put in six consolidated PWR assemblies in these sectors that are divided and then the central portion would be the hardware from the consolidation process. In terms of BWR in the SCP-CDR, you would consolidate with 18 BWR assemblies and it had the same sector geometry with the hardware doing in the center. For those cases of fuel assemblies that you did not consolidate in the SCP-CDR, this is the geometry that was used to put in three PWR's or six intact BWR assemblies. And, this is the basis of the numbers that Tom Hunter showed you in terms of through-puts.

Just to inject one slide which is not in your handouts, but this was a model that was made for a show-and-tell to represent those two configurations just to give you something to look at besides the line diagram. These are just simulations or representations of PWR's, how that would look in an actual sector.

Now, there's a second configuration in the SCP-CDR which you're going to hear about from Lyn Ballou's assumptions or calculations and some of Gary Johnson's. It uses what's called the alternative -- well, a hybrid container. It's an alternative container. It's the one that has a two inch bigger diameter at 28 inches going up two inches in size, allows you to put more fuel in, but you do mix three PWR's with four BWR's. It turns out there are more BWR assemblies than there are PWR assemblies and in order to have containers to accommodate all of that fuel that would be discharged up to the nominally 62,000 tons/MTU, you would have extra BWR's left over. In that case, you would jam in or pack in 10 intact BWR assemblies into that kind of configuration. And, you're going to see heat transfer calculations inside of the container from Gary Johnson with this configuration later.

Now, to make the transition to what's going to follow after the break, I wanted to introduce, you know, the design assessment type activities that we've been doing. And, there's a point that I'd like to make and it's -- we're educating ourselves and becoming more sensitive to it, but the fact is said here that the near-field environment -- that's the shaded area that surrounds the waste package -- is a large fraction of the underground repository. And, when you consider the waste packages which are in arrays or whatever there's a large volume, if you like, of the repository horizon is in some elevated temperature sense -- perhaps closer in some elevated stress configuration -- and the real point is that a large fraction of the underground repository horizon is perturbed due to the emplaced waste. And, what we want to focus on today first are some of these thermal analysis that might be represented here and you're going to see more of that.

What's going to follow are three talks that illustrate how you might apply thermal analytical methods to situations that support the development of a repository design or a waste package design. And, one case, the first talk, is going to be by Lyn Ballou that basically uses an analytical solution to look at the effect of levelizing the heat loads in the repository and his technique uses this superposition and it allows him to consider variable heat sources in large arrays. That's the power of that technique, one of the powers, and he's going to tell you more. However, he has some constraints that he's constrained to use, constant properties for things such as thermal-conductivity.

After Lyn shows you his technique which is a way to handle large arrays and get information on what the temperatures are around the borehole walls, we want to talk about two talks about examples of numerical methods and how those were used. In the first case, John Nitao is going to talk about how one of his missions -- the one, in essence, is to look at the effect of two phase/multi-phase flow and compare that heat transfer calculations with that phenomena to a pure conduction model. I think he'll show you that it's a 10 degree Centigrade difference and there are certain limitations with the numerical methods in terms of very expensive computer sizes, times, and things and those are very nice calculations, but they have their limitations in how you use them.

And, finally, Gary Johnson is going to show you some additional numerical methods which does accommodate things such as variable thermal-conductivity, but they're generally constrained to single arrays or single waste containers as they're emplaced and he's going to show you some calculations of the inside of the container calculations of temperatures, in addition some sensitivity studies that he has done. And, the idea of this is that what we view these as are methods that are available for us to go back and look at the designs and apply new parameters in order to support the design activities and so these are basically going to be presented as applications of how these techniques could be applied with specific examples.

And, I think, you know, I might mention one last thing and that is that the title on the agenda had something called design concepts and approach and really, you know, this was sort of a summary of the baseline. And, the waste package program is looking at a slightly different twist and direction and just recently received approval of a waste package program plan or waste package plan which outlines the direction that the development of the waste package is going to be going in the future and that has been approved very recently, last Thursday, at the project office and that has things laid in it as to how alternative containers fit in, as well as the direction it will be pursued in the future as to how we're going to design or come up with design concepts and designs for waste package.

DR. NORTH: Good. This seems like a good point to take the break.

DR. STEPHAN BROCOUM: I'd like to say one word on this waste package program plan just for the record.

DR. NORTH: Yes?

DR. BROCOUM: Last fall, we had asked the waste package people to put together a waste package program plan so we can get a better understanding of the whole waste package program and a better understanding of how we intend to proceed in the future. That plan was recently completed and we have it at headquarters and, although Lyn said it was approved, the implementation of it is not approved. But, I want to make one point. That plan suggests two parallel efforts in design of a waste package; one is the reference design, the second is an alternative materials design. That alternative material design would have its own set of requirements which may differ from the reference design. It would have its own requirements to the near-field environment constraints that may be different from the reference design. And, those two design efforts according to this plan would be carried in parallel for a period of time as long as the -- application perhaps until one or the other was chosen. And, so that -- we need to make a policy decision to implement that. But, that plan has been written. I think it will be available shortly and it would also be perhaps a good topic for a meeting at some time in the future. I just wanted to put that on the record because what most of these people think, well, it was in the SCP.

DR. NORTH: Good. Good. Well, we would very much look forward to the opportunity to see this new waste package plan, and perhaps as the speaker go forward, some comments about what is in there and how your thinking has changed from the SCP would be useful. However, we're running a little short on time at this point. I would ask that we try to hold the break to about 10 minutes and resume then at about 10:30.

(Whereupon, a brief recess was taken.)

DR. NORTH: We're about ready to start. If you don't mind, please come in and sit down. Take your seats so that we can have the first speaker begin.

MR. BALLOU: The first of these three applications type analyses that Les Jardine alluded to is one that was done, I think, pretty truly in the spirit of what Warner has been asking for repeatedly as a series of slightly better than the back of the envelope, but not much, analysis to look at the sensitivities of various parts of the system in terms of performance. And, this is a study that we did in support of a broader effort conducted by the systems integration branch of DOE headquarters in looking at some of the questions relating to the interpretations or modifications that might be needed to the 10 CFR 961 contracts in order to in one way or another improve the performance of the Federal system. And, to acknowledge the points that Bob Shaw made earlier with respect to the fact that decisions are being taken now by the utilities with respect to moving fuel into reactor dry surface storage in one concept or another.

This study was one that specifically done to compare the thermal response in the repository of an oldest fuel first of OFF or first-in/first-out or however you care to characterize it; sort of a receipt stream compared to one that essentially levelized the integrated energy deposited by the spent fuel. Our objective was to determine whether the postclosure performance of packages in an unsaturated repository could be enhanced, as I indicated earlier, by limiting the potential for liquid water contact of the waste package containers for extended periods by utilizing some decay energy management or a jargon term that has grown out of that, "heat tailoring" kind of technique.

A number that is not in the handout material, but you might jot down for reference is that the total thermal energy from decay heat that will be deposited in the rock mass in the period from emplacement to about 350 years after emplacement or 300 years after closure is a number that is of the order of 6 gigawatt years. This is roughly the equivalent of the annual thermal output of 2,000 megawatt electric reactors per year. That's a lot of heat.

There are a number of techniques that have been suggested for possible ways to engineer or tailor the use of that heat. It can be divided broadly into four general categories. The one that we're going to talk about in this study is essentially receipt tailoring where some controls are exercised on the characteristic stream as it arrives at the repository to levelize -- or for de-levelize for that matter -- if there is some other optimum mechanism for taking best advantage of these characteristics.

Eric, earlier today, talked about the second one in a little detail with respect to the possibilities that exist for geometric tailoring, modification of the emplacement panel geometries, both drift and hole spacings, to compensate for boundary effects or variations in the waste stream characteristics.

A smaller scale possibility that with some reasonable amount of inventory at the repository or at some intermediate facility that could be employed would be to modify the individual loading of packages based upon their intended position in the repository to compensate for boundary effects at a local scale.

And, a fourth that is somewhat different is essentially treating the spent fuel and defense waste in a different fashion than that that is presumed in the conceptual design that envisions commingling of the defense level waste with spent fuel.

So, the scope of this study was to identify and analyze some repository scale thermal effects of a couple or three spent fuel scenarios that might be more effective than oldest first in meeting the study objective.

Skip the next one. It's basically what we -- the only items there is just note that we did use two, actually three. I'm going to tell you about two of the cases to be analyzed. We did a little bit of a sensitivity study on a couple of options; one having to do with what portion of the inventory was to be considered and the other is this point I just made with respect to non-commingling of waste forms.

A quick summary of the assumptions that were used in the study so that we're real clear on that subject. The receipt schedule that was used is that that is contained in the 1988 draft mission plan amendment by DOE. The same discharge historical record and projections, the No New Orders-Extended Burnup case that Eric talked about earlier is the basis. We have used a simplified 20 panel repository geometry and in the base case with commingled fuel and defense waste. I'll show you a little more about that in a minute. The areal power density that was employed is approximately the same as that used in the SCP-CDR. There are a couple of variations from that, but we attempted to use the methodology described in the appendix to that document with respect to adjustment of areal power densities to reflect different ages. As Les mentioned earlier, all of the spent fuel packages in this study were presumed to be in that hybrid configuration containing 3 PWR, 4 BWR intact assemblies per package, two metric tons, and on average, but quite variable, an initial thermal output in the vicinity of about 1.8 kilowatts per package at emplacement, all assuming vertical emplacement, one package per hole. The arrays that we looked at employed the minimum spacing specified in the conceptual design, namely 7 1/2 feet between all holes. And, the thermal effects in terms of some temperature distribution plots that I will be showing you in a minute were analyzed at nominally 300 years after closure, specifically because all of these things do have a calendar tied to them. The analysis were done for the year 2353.

The acceptance schedule, you can't read probably, but this is the -- there's only one real point that I want to make in this. I think somebody mentioned it yesterday. I don't remember who. It's that there is a scale-up period in the first five years of operation in the repository. Spent fuel only is received in that period, the first 3900 metric tons, prior to the receipt of any high level waste. Then, a steady state operation down to near the end at 3,000 tons a year or 1500 packages per year in the case of this study, 400 tons per year equivalent, and by whatever mechanism you care to select, we have used the assumption that is consistent with that contained elsewhere and described yesterday that these are roughly a half a ton per package or 800 packages per year. So, 800 here, 1500 here, a total of 2300 packages received and emplaced per year in the steady state operations with the first five years treated separately.

With respect to options of the inventory, as you have already seen in one of, I think, Eric's slides, the inventory information in the OakRidge data base is broken up -- well, it is elaborated by fuel type and by discharge year and is subdivided in this axis into 12 or 13 burnup bins, so-called of 5 gigowatt days per ton. The problem we were confronted with in starting this study -- this is the 1987 inventory projection that Eric talked about earlier -- was the bottom line value slightly different than the SCP-CDR from the 1988 draft mission plan amendment. It calls for 63,000 tons of spent fuel rather than 62 and about 7,000 tons of equivalent in high level waste. So, our problem was to determine which 63,000 tons out of the inventory were we going to deal with. The bulk of the work is done with that section of the inventory that is represented by oldest go first and hatched horizontally. This essentially includes fuel discharge through about 2011 which is projected to make up 63,000 tons cumulative. We did look at another version of the selection which we termed hi-grading it. Recognizing our objective was in the direction of this keep-it-warm-for-long, a way to do that would be to use higher burnup fuel, and therefore, we took a cut the other way through the inventory matrix drawing 63,000 tons including discharges out through about 2017 or 2018. That would essentially walk back through the inventory from high burnup toward low. With a selection, the dotted line in here, indicating we took fuel from all of the burnup bins down to about 20,000 gigowatt days for BWR and down to about 30 for PWR to get enough of the inventory in the vertically hatched section to represent the 63,000 tons that was needed.

The next plot is similar to, but has a little more information on it than one of the ones that Eric showed earlier. This is the distribution on an oldest first or first-in/first-out kind of reckoning. All the study was done prior to the November announcement of delay in the repository schedule. So, it's all built on the 2003 through 2027 time period for operation. Notice the point that Eric made earlier was the monotonically declining age starting from about -- at that reckoning 32 years down to about 16. The burnup in gigawatt days per ton average for the annualized discharges or receipts through that period pretty monoton-ically increases up to near 40 GWD/MTU at the end, but starting quite low. The combination of these two when integrated on an individual assembly basis as taken from the inventory leads to this integrated energy on an annualized basis running from in the low 30's to about almost 110kW-YR/MTU for integration to 350 years following emplacement.

By contrast, the levelized scenario that has been talked about primarily is levelization on burnup. The average age consequence of the mixture that we took in order to near-levelize the integrated energy at a value in the middle 80kW-YR/MTU with the exception, as somebody pointed out earlier this morning, there is this increasing tail at the far end of it driven primarily by the fact that you can run out of old cold fuel at that point.

The scheme that we used for selecting fuel to make up this receipt schedule was a mixture of first-in/first-out and last-in/first-out proportioning those in a way that would approach levelization of the integrated energy parameter. You can't see them very well in the handout because it's been xeroxed one too many times, I think, but there are a series of a little plus marks both along this line and along this line. What we basically have is a two band stream of age characteristics, some generally in the range of about 15 years and some in the middle 30's blended in the proportions appropriate to producing a near-level energy deposition.

By the way, if you overlay the oldest first one with that, the integrated energy in these two integrated over time is the same. It doesn't look that way because in the first five years the receipt quantities are reduced and, therefore, that tends to make this a bigger area appearing on the plot; then is, in fact, on a pretend basis.

The hi-graded scenario has exactly the effect you would expect it to have. It increases the integrated energy deposition by about 10 to 15% as a result of essentially using higher burnup fuel.

Repository we modeled, unlike the duck shaped primary repository block, we chose to regularize a little for computational convenience. We did honor many of the geometric considerations from the conceptual design. Each of these panels is a 2,000 by 1,000 foot panel with a standoff spacing between them of 250 feet and a 500 foot divider in the center. This panel labeled A is the panel in which the first five years receipts were emplaced and it is truncated in this configuration to handle the 1950, I believe it is, packages, the first 3900 tons of fuel received at the repository. All five of those years are placed in that first panel, all spent fuel.

The other panel that I'm going to show you some information on is this panel, an interior steady state kind of 3,000 tons of spent fuel and 400, an equivalent high level waste commingled or not, in a position like this. In that reckoning, this would be the discharges from the year 2000

-- or the emplacements for the year 2014 on the old schedule.

Going to the point that I think Ed Cording was making earlier with respect to were we modeling a big enough piece of the repository, here the -- when we're looking at detail at the temperature distributions within this panel, we included in the model these other panels to be sure that we had a large enough representation of the neighboring panels that there would be no effect in the time frame of interest with respect to what was happening within the panel we were examining in detail.

Specifically, the geometry of that panel is as I described earlier, a 2,000 foot by 1,000 and a little foot panel. There are 1500 spent fuel packages and 800 defense glass can packages and they are all commingled in all of the interior rows except that at the outer end of those rows, the first six packages are all spent fuel on each end and the outer two and innermost two rows are all spent fuel. The hole pitch, as I said, is 7 1/2 feet. This was modeled in the 2,000 feet as 17 rows, 125 feet apart; essentially, the same values as we used in the conceptual design.

We do this and look at the temperature distributions at 300 years after closure. This is what we wound up with. These are not really contours. They are pseudo-contours in the sense that they are the locus of points on borehole walls that are at these temperatures. They are not representative of what is happening, for example, the temperatures out in the middle of a pillar between two emplacement rows. As you'd expect, on the inner and outer ones which are all spent fuel, the temperatures are a little higher; 100 to 105 degrees at 300 years, 95 nominally boiling point of the configuration about like this in each of those two cases, a few 90's out here along the edge. All of this, in-between; between 90 and 100 degrees. Through 95 and 100, actually, or 90 and 95.

A look at the distribution temperatures within that panel for that configuration is like this. Generically, the point to be made is that there are two relatively high -- this histogram shows relatively high values. A lot of packages within a fairly narrow temperature band of 5 to 10 degrees with a little tail on the spent fuel out here; the defense glass packages a little cooler like 5 to 8 degrees on average because they are essentially being kept warm by their spent fuel neighbors.

The same calculation using levelized stream for that panel. While it looks quite different, in fact, is not. The temperatures here just happen to catch the 95 degree contour coming through rather than 94 or so that was in the other one. So, there's a significantly larger fraction of the packages in that geometry and are at or above 95, but not much above. As you can see in the distribution for the levelized case that follows, essentially the same kind of a shape, about the same kind of amplitude in the histograms for the two waste forms and the distribution very close to the same. You see here that this happens to be -- the 95 was right in here. Caught a bunch of packages in this case and was over 1 degree and certainly the calculations are nowhere near that good. So, I think that has no significance. But, remember, that that panel happens to be just about the crossover point between the oldest first characteristic for energy content and the levelized energy content. They ought to be very close to the same.

With respect to the initial panel which contains all spent fuel, the story is quite different. This is the region that was modeled again enough neighbor panels to be sure that what was going on in here was not being adverse -- we had a big enough problem for the calculations to be valid over the period of concern. This is the panel geometry there where we have -- it is essentially loaded from the outside in. These are the emplacement years. A couple of rows of 100 packages each to make up the 400 tons for each of the first three years, then four more rows, somewhat truncated for the 900 ton year, and the balance of the panel filled with the 1800 tons from the fifth year.

The point to be made here is that the distributions from oldest first at 300 years, no packages are above 97, only a few above 95 located right in this region, and many packages quite cold down in the -- you know, only 20 or 25 degrees above ambient at 300 years. That, not a surprising result given that both age and burnup of these early years in the oldest first scenario just don't have much thermal energy in them. The distribution, all spent fuel there, you see it's smeared out quite a bit compared to the quite highly peaked histograms of earlier. No packages above 97 in that calculation and some down in the 50's.

By contrast, if one takes the levelized stream and uses those characteristics for the first five years, it's a quite different picture. Peak temperature is between 135 and 140 over a large area. The 100 degree contour includes almost all the packages in the panel except for a few on the extreme edges. The histogram distribution again fairly broad, but shifted about 40 degrees higher in temperature at 300 years peaking at around 140 for a few packages, but a lot of them in the 110 to 140 range.

As indicated earlier, we did want to look at the -- as a sensitivity case -- what happens if we deploy the defense glass in a non-commingled mode. That's what we have done for this panel, a repeat of that intermediate panel that we looked at before, but with a different configuration in it. Here, we did just the reverse, kind of, of what we had done before. We put all of the glass around the perimeter and put all of the spent fuel together in the center in the central region of it. Same number of packages, but a quite different distribution and what happens is what you would expect to happen.

Temperatures are considerably higher in the center and cooler on the periphery where the glass is with its low output. A distribution not unlike that in the all spent fuel first panel that I showed you earlier or just a minute ago; 300 year temperatures in the range of 100 to 130 through all of the spent fuel region, very low values in the 50's and the glass packages in the periphery. A quite different sort of distribution, very broad, no big high amplitude, large number of packages in the histogram spread over -- spent fuel spread over more than 40 degree, 300 year temperature range. Some are sort of arranged with the defense waste, the bulk of it down in the 70 and below range. Because the characteristics of that stream are just about the same, as I indicated earlier, for either the levelized or oldest first case, you have essentially the same picture for the non-commingled case with the levelized receipt stream as I just described. The same thing with the borehole wall temperatures as the histogram, same sort of a distribution.

Some conclusions from this study, it appears that in about the first half of the operating life of the repository a levelizing on deposited energy receipt stream will produce significant higher boil temperatures for extended periods than the oldest first case for the early 10 to 12 years, roughly a half of the operating life. Conversely, the oldest first is a little better in most of the later years. We really can't have it both ways because you will have used up some of this not oldest first fuel in levelizing and, therefore, it is not available for you to use later.

One point that is a clear uncertainty in this sort of analysis is that the response of both streams, but in particular the oldest first stream, I think, rather strongly depends on realizing the extent of burnup projections. If something institutionally or otherwise happens within the utility system that should for some reason preclude the realizations of these extended burnups, all of these temperatures would drop.

Obviously, the boundary effects, as I pointed out on several occasions, are significant. They can clearly be compensated for by some of the schemes that Eric talked about this morning with respect to geometric tailoring. Those were not optimized, at all, in this study. We held the geometries dead constant throughout, so that we were really looking only at waste stream characteristic effects.

And, it's clear that there are alternative strategies, one of which we've shown you, for the emplacement on high level waste that are possible and maybe beneficial in advancing the thermal response for spent fuel packages.

Questions?

MR. BLANCHARD: Thank you, Lyn. Proceeding on with the thermal analysis that affect the waste Package, John Nitao would like to present discussion on analysis that he has done on near-fields thermal fluid effects.

DR. NITAO: I'd like to present some of the effects of the fluid flow on the thermal field, a point that was touched upon yesterday and a question that was asked yesterday.

Some of the work I will present will be on long-term calculations. In the January TRB, we were able to present some work on short-term calculations and calculations in support of field experiments. And today I want to present the long-term calculations that we have done.

The main objective of the study in reference to the thermal field, which is the primary subject of this TRB, we would like to present the effect of fluid flow on the temperature distributions on a waste package using numerical models. Quantify this effect in relationship to conduction-only analyses that have been presented thus far today and yesterday. And to find what processes are giving rise to these temperature differences. And, what we have also done at Livermore is to perform experiments, fuel experiments in order to validate these fluid flow and thermal models.

These field experiments were described in detail at the last January TRB. Just to summarize some of the possible effects of fluid flow on the temperature field the vaporization and movement of pore water and it's subsequent condensation in fractures will affect near-field temperatures. But, because of computational cost, when we do analyses of multiple waste package geometries as Lyn and the workers at Sandia have also presented and Eric Ryder has presented analyses with multiple waste package geometries, they are done using only thermal conduction-only modes because in order to include fluid flow into multiple waste package geometries, would require a tremendous amount of computational cost. However, it's possible to determine the relative importance of the fluid flow by simulating single waste package geometries which equal into an array of waste packages.

I really won't get into this since we've already covered many of these already. But the fluid movement around a waste package impacts not only the thermal distribution but many other areas.

The general approach that we are taking to our model prediction is to do these four types of activities, model development, preliminary scoping calculations, which I will present today, laboratory experiments and field testing.

The long-term simulations were done using a VTOUGH Code which is the same code that was presented at the January

TRB. It's a finite difference model, computer model, which includes thermal conduction in the rock and thermal radiation between the air gap between the waste package and the rock. It includes the latent heat of boiling of the water in the rock and also includes convective heat transfer by air, liquid and water vapor.

The study that I will present uses these parameters. This study was done when there was a different idea about the emplacement scheduling so at that time we used 8.6 year-old PWR spent fuel. We had a 5.05 meter-long waste package, a 3.4 kilowatt initial he output and a LAPD of 57 kilowatts per acre.

The simulation was for an infinite array of horizontal waste packages. Right now we are looking at some short-term and in the future long-term simulations of vertical waste packages. This is now the reference case, but at that time we were interested in horizontal partly because the fuel tests were horizontal.

This is the problem domain of our model that was sub-divided into a grid. The waste packages here were 350 meters from the ground surface, 225 meters from the water cable. This is the symmetry midplane between drifts. We have an infinite array, so this is a symmetry midplane that you take advantage of which is a no-flow boundary. And this is also a symmetry plane and also no-flow boundary here.

The rock properties, since we were interested primarily in a near-field, at that time what we did was we used the rock properties of homogeneous rock properties of TSw2. What turned out was by putting an infiltration at .1 millimeter per year, we obtained an initial saturation field which was the same as was obtained by other isothermal simulations which took into account the different layers, so we were pretty satisfied with using this approach here.

This is a prediction of the amount of drying around the waste package. The waste package is here and what we have plotted here is down the line, the center line of the waste package we plotted, the vertical center lines of the waste package we plotted the saturation, which is the fraction of water that's in the pore space. At 100 years we find that the extent of drying is roughly about ten meters above the waste package and between around 15 meters below the waste package. And here we have at 400 and 800 years after emplacement, one thing to notice is at these times the amount of drying is roughly held to a constant volume. This mound of water here is due to the fact that the water around the waste package has been vaporized and driven outward and condensed in the cooler portion. As time goes on this mound decays because of capillarity in the rock.

Here we have waste packages here and we've plotted, now this is the problem domain, the substantive domain of our problem here. And the shaded region denotes the region around the waste package that is fracture, as water in the fractures. And this is at 30 years. As time progresses this dried out zone here begins to coalesce with that of the neighboring waste package and this fracture saturation halo then becomes two regions; one above and one below. And the water in fracture starts to go downward.

This plot here is relevant to the analyses that have been done. They have been presented in the last two days. Here I compare the effect of adding fluid flow to the model. In the green dotted plot here is where we have just thermal conduction in our model. And here we have, including the fluid effects--note that--there are several things to note. One is during the heat-up period here, this is the temperature by the way at the borehole wall. During the heat-up period here, the peak temperature is roughly the same. It may be a couple of degrees centigrade difference. Before the heat-up period the temperatures are virtually indistinguishable. And, it's only during the cool-down phase where we get roughly a ten degree centigrade difference.

What we found is that during the heat-up period, the latent heat effects were relatively insignificant. What's happening here, I'll describe in later view graphs is due to a heat pipe effect, a recycling of fluid.

This is the same plot except we've extended it out further to 2,000 years. And we notice that this difference--this roughly ten degree difference extends to roughly around a thousand years in this range here. At this time, the heat pipe effect becomes negligible and the water in the rock around the waste package starts to coalesce back onto the waste package.

What we found is that the primary reason for the temperature difference is because of a heat pipe effect where we have water in the rock being boiled away and driven outward in the fractures and condensing, whereupon, it's then absorbed by the rock and pulled back and the cycle continues. This heat pipe effect, in our simulations, we don't see it until the cool down period. And in the heat up period, the rock is being boiled out too fast--much faster than the counter current flow can come back.

DR. DEERE: What kind of time period is that over? You say you don't see it at the beginning.

DR. NITAO: You see it roughly around, when this temperature difference starts to take effect, which is roughly around 60 to 100 years is roughly when it becomes significant.

This is a plot, again the profile along the vertical center line of a waste package, and the temperature distribution. And this is with only thermal conduction in the model and this is with fluid effects. Even though the temperature near the waste package is roughly only ten degrees difference, what's interesting is that the thermal gradient can be significantly flatter where the heat pipe is occurring. This is roughly around the boiling isotherm. It's above the boiling isotherm, because it takes a raises of the pressure in the rock in order to drive the steam out of the rock into the fractures. So that drives up the boiling point and that's why you see it's above the boiling point there.

And this is a picture roughly--this happens not only above and below but all around the waste package there is a heat pipe like region that that occurs.

These arrows here show the velocity of fluid. And in this case the velocity of the gas phase which is basically steam being driven outward from the waste package. In this case this is at 400 years and the dried out region has coalesced with the neighboring waste package. And you are getting basically more or less a vertical velocity. The length of these vectors are proportional to the flow velocity. And this is a liquid flow velocity which is coming back in due to capillarity in the rock. This is occurring primarily in the fractures and this is occurring primarily in the rock. So, we have this recycling effect going on.

One question is how pertinent are these calculations and one of the ways that we are evaluating our approach is to compare it with the fuel experiments with G-tunnel. And I'd like to now present a couple of view graphs which were not presented at the last TRB which we have performed simulations just recently. And for those who are not familiar with the prototype heater test at G-tunnel, the test was done last year at the G-tunnel complex and it was a one feet diameter heater which was emplaced into a horizontal borehole. And a constant heating of 1.1 kilowatt per meter heat loading was applied for 130 days. The length of the heated region was about three meters long. It was then after this heating, there was 65 days of linear rampdown. So this was a relatively short test.

This is a plot of the temperatures versus real distance away from the heater and--excuse me, no this plot of temperature versus time at different distances from the heater. This is at the lower borehole wall where we have a thermocouple, and the green plot is the model calculation and the red is the experimental measurement. The difference we see here and we believe we can account for, is that we found that there was a 40 degree, roughly a 40 degree centigrade difference between the lower portion of the heater and upper portion. And we also found that same difference when we put the heater in the laboratory out in the open and we believe that's due to convection effects inside the heater. Future tests will probably peak that up.

This prediction roughly falls partly midway between what we expect the upper borehole wall to be and the lower. At .55 meters away in the rock, we find a much better agreement. One thing to keep in mind is that because that this is a short-term test what we found is that when we did not include thermal effects into the model, we still got good agreement. What that says is that at these short time spans, the fluid flow effect is not really affecting the temperature distribution. And this is not a very good test of our model in terms of fluid flow effects, although it is consistent with our model in showing that fluid flow effects are not important.

The next view graph is a test of the fluid flow effects that we have in our model. And the actually volume, this is a liquid saturation, the fraction of water in the pores and this is radial distance from the heater center. And what we find is that the model prediction predicts the volume of dried out region very well. It's the region that's just outside the dried out region where the water is being condensed is not predicted well. The model over predicts that because our model has an assumption, the equivalent continuum assumption which assumes the equilibrium between the rock and the fractures, and what we expect is that that assumption is not valid.

What is happening, this is in the rock, what is happening to the fractures is that in an coolant continuum model, as the water in the fractures is condensed, it has to equilibrate in our model with the matrix instantaneously. In real life, what we think is happening, is that the water is condensing and then dribbling away due to gravity effects. And that's why we don't see this extent here. So we have plans for future model development in order to use some kind of dual porosity concept in order to match the data.

In summary, some of the conclusions are that the peak--in our preliminary calculations, we see that the peak waste package temperature is not strongly affected by the fluid flow effects and that thermal conduction-only models would be a good approach for calculating the peak temperature. The effect of the fluid flow on thermal field is not significant until the cool-down period and in that instance it results in cooler temperatures around the waste package by about 10 degrees centigrade, which is not a huge difference.

The difference between conduction-only models and conduction plus fluid models is caused primarily by heat pipe effect.

And we saw that the fluid effects move the location of the boiling isotherm significantly closer to the waste package than that predicted by thermal conduction-only analyses. Remember we saw the flat profile and that brought--the flat temperature profile which brought the 100 degree boiling isotherm closer to the waste package.

And, that the return of water to borehole for several hundred years after the time predicted by the thermal conduction model. I wasn't able to show that today, but what happens is that the return of water due to capillarity is relatively slow in comparison to the return of the temperature--the boiling isotherm.

And the field tests partially conform model's ability to predict temperatures and drying around the waste package, but that the heat pipe phenomena validation will require further testing.

And currently we are still analyzing the data from the G-tunnel and also doing some laboratory experiments. And we have several experiments planned in the future in the laboratory.

And simulations for vertical emplacement geometry are being conducted. We've done some short-term simulations. We are enhancing our model to do three-dimensional simulations in order to do the vertical emplacement geometry accurately.

And we are also looking at some analytical techniques and scaling techniques and we are trying to see if we could get some idea of how we can get some back of the envelope calculations, or whatever to predict the effect of fluid flow on multiple waste packages since assimilating multiple waste packages right now requires too much computer time. Thank you.

DR. DOMENICO: John, I'm just curious just to how much of the physics were incorporated here. It seems that you just need a fractured media, and water occurs in the matrix only. Initially you heat it, you get a thermal expansion, phase changes drives it out into the fractures and then transfers quite rapidly away so you can visualize these little blocks. So most of the free convection took place probably in the fractures. It seems to me that in your model it's all matrix. Is that correct?

DR. NITAO: We used a coolant continuum approach which assumes equilibrium between the fracture and matrix. You can get fracture flow only after the matrix is completely saturated.

DR. DOMENICO: I'm talking about driving it out. It would be driven into the fractures and go off as vapor. And I think that most of your free convection took place in the matrix, at least in the dried out zone.

DR. NITAO: No, the cochea model takes into account the vapor flow occurring in the fractures. What it has problems with is that in the boiling, the steam becomes instantaneously, goes into the fractures because of the equilibrium. And that's one of the problems we have. We have done some discreet fracture modeling over a short time span and we've investigated that and we find that the region of boiling is roughly the same. It seems like it's not that significant, the following of the matrix if the fracture spacing is relatively small.

As the fracture spacing gets relatively larger, then that effect--above one meter spacing, that flow resistance becomes important.

DR. DOMENICO: I see. Okay.

DR. LANGMUIR: In your early model exercise you assigned a host of assumptions to the model in order to make it work, of course. And many of these were quite different than what we've been hearing today in terms of the kind of emplacement, perhaps the loading and the spacing options. I guess the question I have, and it came up, if I can recall correctly, with about a 20 meter dried out area around the repository vertical. And that's an interesting figure for me as a geochemist.

I know it sounds like you are in the business right now of doing additional modeling work to look at that effect further. I'm just curious if you have thought about how far you might be going with that dehydrated zone with a reasonable range of choices for these parameters. Might it be going to 40 meters dry, or 30 meters dry or whatever? Have you thought about that yet?

DR. NITAO: We have done some short-term simulations which show that the region of drying is roughly proportional to the amount of water in the rock which makes sense. And also, it goes like the square root of the permeability, so we've done some sensitivity analyses with regard to that, but we haven't done any long-term sensitivity analyses. We've done some short-term analyses in support of the field test. And that's something that we need to do is to try to get more of a handle on some of these parameters, scaling parameters. These simulations--at the time of these simulations, these simulations took about four hours and we've reduced that to less than an hour. But it becomes hard to vary that many parameters so we are still working on that.

DR. CORDING: Do you have plans on modeling the actual drift itself above the canisters?

DR. NITAO: Well we had plans to model in terms of the vertical emplacement, just a two-dimensional modeling of the drift, and not initially the flow in the drift itself.

MR. BLANCHARD: Next, we will continue with the thermal analysis studies. And the last talk is to be given by Gary Johnson who will describe the thermal analyses for the interior of the waste container.

MR. JOHNSON: My presentation today provides kind of a general overview of chronicle overview of the thermal analyses studies that have been done at LLNL on a tremendously large number of spent fuel waste disposal container designs.

The objective of these studies was to use existing thermal modeling tools and techniques to evaluate the thermal performance of various container configurations and spacings and to deliver that performance in terms of time temperature histories and temperature distributions in order to compare these against design constraints.

Since the early '80's several thermal analysis at the Lawrence lab have been involved in using our own thermal transfer conduction codes to do these models on probably 75 to 100 different designs in the repository configurations. And, to develop and determine these temperature distributions. I believe that this effort because of a continuity of the tools that have been used and the form in which the modeling has been done allow us to look at this whole history of results and compare them in order to gather some specific information on how--what things we should look for in a design. And what we can expect how the various designs can be compared against the design criteria.

I would just kind of like to remind you again about the situation we are looking in. You know at the repository we have some 70,000 metric tons of spent fuel and defense high level waste packaged in some 40,000 containers and these containers can be emplaced either vertically or horizontally. The spent fuel ranges in age from roughly ten years to several decades. The burn-up ranges up to 65 GWD/MTU Uranium. And I mention this to kind of what I'm going to talk about in the next few minutes in context.

When they do container analysis, that's what we do is the analysis of a container. One amongst 40,000. And it's important that we understand--that I hope you'll understand that the results that I tell you will either represent what we consider to be some kind of representative model that's representative of either a portion of the total or some kind of limiting model.

Again, when the analysis is done and we are looking at the detail that we want to, the things that we look for are to compare against the design constraints that have been given us, you've heard this umpteen times over the past two days and just to remind you again the main ones that I'm concerned with and the container in near-field are the 350 degree peak cladding temperature limit keeping the borehole wall above the boiling point and that keeping the temperature below 200 degrees one meter into the borehole wall. Now, the one I'm going to emphasize in my talk today, is comparing our results against the maximum allowable peak cladding temperature.

The peak cladding temperature occurs at a variety of spots in a variety of times depending on the design. But, and it doesn't always occur at the center of the various packages of canisters of fuel.

Over the past seven or eight years we've used a variety of numerical tools. But, each of these tools as we move from one tool to the next tool, then compare--we took comparable models in each and ran them on each tool. And then we also took various close form solutions to make sure that each numeric code could reproduce and be verified and reproduce the appropriate close form solutions. The tools themselves handle all the geometries from 1-D to 3-D. They'll handle variable properties, phase change. They won't handle fluid flow. They can handle temperature dependent properties and we can handle time dependent heat generation.

The early work that we did was to establish what heat transfer modes were important and moving from the Trump Code to the Taco Code we took that move mainly because we found that what was of primary importance in the heat transfer between the container and the surrounding tuff was enclosure radiation between the various fuel canisters and the container wall and between the container wall and the borehole wall.

We also wanted to be able to handle 3-D geometry. We found that it was quite important to handle 3-D geometries in the tuff particularly in the long-term calculations. We looked at the series of geometries, axisymmetric geometries to the plane for both the container and the tuff and finally we decided that for accurate long-term modeling that it was important that we model the tuff in terms of its three-dimensional characteristics.

The areas in which our final choice of modeling tools and thermal models were used in the ones that I am going to talk about in particular today are in the MRS common canister configuration studies. Several of the preliminary calculations that we did leading to the SCP-CDR designs and in particular, the one that I myself did, when we looked at an alternate design in which we stored a hybrid collection of consolidated PWR and BWR fuel. In each of these cases we looked at various parameters that we felt controlled the calculations to see what the sensitivity was.

Early on in the MRS study in the common canister study, we looked at a whole variety of internal designs. There were roughly 11 designs and in many of them we looked at the possibility of storing on PWR or only BWR so it ended up being roughly 16 configurations each of them stored under two or three spacings. The primary reference spacing for almost all of them initially was 30.4 meter drift spacing and 8 meter container spacing within the drift. The fuel is ten year-old fuel. It's consolidated fuel. In the picture the orange figures are those that contain PWR fuel and the green ones contain BWR fuel. There is some small variation in the number of assemblies per container and in the diameter of the container, and basically the geometric design that you would come up with if you wanted to fit and the particular shape of fuel canister inside these containers, the diameter of the container itself is roughly around 60 centimeters.

In all the designs that we looked at and in the fuel forms and most of the spacings, we found that all but one fell below the 350 degree limit. The other thing that we realized was that this peak temperature occurs very early after it's been emplaced, all of them falling within ten years after then had been emplaced in the ground, when the container had been placed in the ground. Another thing we found was that the dominant heat transfer mode was by thermal radiation. Basically in looking at all of these designs and seeing that all of them basically aren't limited by the internal design as far as just 350 degree design limit, we pretty much decided that it was how much fuel you put in these things, how far apart they are spaced and what you assume for the thermal conductivity of the tuff, that seems to be the dominant thermal resistance that controls the peak cladding temperature.

One of the final things they did in this design--in these design analyses was to look at the effect of changing the spacing and if you take the 8 meter by 30 meter spacing and you place the packages either 3 meters closer together or 3 meters further apart, it basically changes the peak cladding temperature by about 20 degrees centigrade.

I've included on this slide in slots 17 and 18, the other two package designs that I'm going to spend significant amount of time talking about this afternoon, or this morning, and I just put them there just so you have some relative understanding about where they stand with all the other packages that have been looked at.

One design that we looked at when we were trying to come up with a PWR design for the SCP-CDR was this one. It has three packages of consolidated fuel, PWR fuel. It's very much like the one that was presented by Les. Again, when we model these things, we look at one container that's representative of the whole array of the repository, so we are looking at essentially one of an infinite array of equally loaded, equally spaced containers. The tuff model is a three-dimensional section that goes all the way up the surface of the earth and then that distance down toward the water table. In the first thousand years or so it really doesn't know that there's a difference on either end of that, so the flow is almost equal.

We only look at a quarter section of the whole module of tuff that's associated with one container. The edges of the tuff are basically the midplanes between the adjacent drifts or between the adjacent canisters. Those are adiabatic surfaces. From the 3-D transient analysis of the tuff, we generate the borehole wall temperature versus time. And this is used as the thermal sink. In the 2-D analysis of the container in modeling the heat transfer of the container to the borehole wall and its associated time/temperature history, we have conduction heat transfer through the steam air environment between the borehole wall and the container wall, thermal radiation. And I guess at what the effective convection that takes place due to natural convection cells that's set up in that annulus.

One thing that's important to remember at this point is, that thermal radiation is a highly non-linear phenomena. And as you change the amount of thermal load that is given out by a container, you can't always depend on the same delta T between the peak cladding location and the borehole wall. So you've just got to be a little careful about using the peak borehole wall temperature to somehow infer a peak cladding temperature.

Again, I want to mention that the dominate heat transfer mode by a factor of 4:1 is the thermal radiation. Internally we have modeled conduction through the gas fill and thermal radiation between the surfaces of the fuel canisters and the inside surface of the container. There because it's so difficult to determine that convection cells that's set up and the amount of heat that can be transferred, we took the conservative route of just not including that as a heat transfer mode. And thermal radiation becomes the really dominant mode, to the order of 90 or 95 percent.

Heat transfer within the model fuel bundle in each of the canisters is modeled as a continuum rather than a collection of individual components. We determine an effective conductivity that represents the heat transfer within this bundle of rods. In the past it's been determined on radiation only between rods and I'll take some time a little bit later to discuss some of the things we learned about the new estimates about what that value can be more realistically modeled as.

One of the comparisons that were made with this little PWR model was to compare how the response of a vertically emplaced container would compare with that of a horizontally emplaced container. They two cases we took at the time of the analysis was done tended to be representing the values of the local power density of the these two individual models that are again part of an infinite array. The vertical case has local power density of 57 kilowatts per acre and the horizontal case has the local power density of 50 kilowatts per acre.

You'll see that the difference in peak cladding temperature is roughly that the horizontal case has roughly a 20 degree high peak cladding temperature even though it's dumping out initially less heat. And I think that's an interesting thing, and I have, to tell you the truth, no idea why. Most of my experience has been with vertically emplaced designs and I have no real gut feeling as to why the difference is there, but it is there.

One of the other things we did in deciding, it's really difficult and these things are very time consuming on the computer to run even with a two-dimensional heat transfer model for the container. There's a lot of components that we model inside in order to get a very accurate estimate about what the thermal resistance between the central fuel rods and the outside surface of the container are. And, so in order to keep the computation time relatively low, we went with two-dimensional models. If we had gone to a three-dimensional model, and we did in one case, there's a couple of things you have to add to the model besides the normal ones that I have already talked about about effect conductivity and the various heat transfer modes is that actually most fuel rods have a power distribution that's not uniform. Somewhere around the 25 percent axial point and 75 percent axial point you get an increase--a much higher heat generation rate than you do at the middle of the rod. And you get this kind of V-shaped heat generation profile. And we included that in our three-dimensional model and then heat transfer along the rods is a lot better than heat transfer normal to the rods because of the modes of heat transfer that you are allowed. You can get very little conduction to the rod because it is essentially transferring heat either by radiation or conduction through the gas. And along the rods you have this really nice conducting medium to transfer heat, so we included this orthotopic properties in the modeling of the fuel packages. And we found that after all this really complicated modeling that the difference is roughly only about 15 degrees increased temperature.

There were times when people would come to us with questions other than peak cladding temperatures. And one of the questions we got was, if I raised the areal power density, how does the outside temperature change as far as the time history, the container outside temperature history. The thing that's important to remember again is that the people that come to us with these kinds of questions have long-term temperature needs. But, it's worthwhile nothing that if you use the SCP-CDR value for areal power density at 57 kilowatts per acre, you get roughly a 220 degree peak outside temperature. And if you go somewhere around 80 kilowatts per acre, the outside temperature gets up to 300 degrees centigrade.

A couple of years ago or two and a half years ago I became involved with a project and the first thing they had me do was to look at the thermal response or an alternate design. In fact this is really not only an alternate design, but it's the limiting case of this alternate design. They decided to fill this container with a hybrid PWR and BWR fuel consolidated. The peak output of that emplacement is 4.75 kilowatts. It's ten year-old fuel. Again it's spaced on the 30.4 meter by 8 meter spacing. And we looked at various effects besides the basic reference, what I call reference case. We looked at the effect of changing the spacing and trying out an improved value for the effective conductivity of the spent fuel canisters. We looked at the effective fuel again and what would happen thermally if we changed the structural material that supported the fuel bundles and essentially provided the container.

It's the same kind of thermal model, 3-D tuff model. In this case, I only took an eighth of the total tuff because I found that the flow vertically in the upper direction and the downward direction were basically mirrors of each other in the time frame I was concerned with. And it just simply allowed me to get a more accurate value of the borehole wall temperature.

We looked at various other things. The outside temperatures of the--I'm sorry. We looked at the effective various things, what happens if you replace the gas fill with lead. What's the effect of making various assumptions on the thermal emissivity of these surfaces since the radiation was the dominate phenomena.

This is basically our best shot of how this thing performs. Peak cladding temperature gets up to 336 centigrade. And it climbs to that temperature within 3.3 years or 3.2 years of first being emplaced. Even after 50 years, the peak cladding temperature is still over 300 degrees centigrade. The response of the PWR and BWR canisters are almost exactly the same. That's not always true, but in this particular case, it was. The hot spots occur--right here and right here (indicating), this BWR and this PWR, you can see that the heat from here has to get out this way, so this is basically offset towards each other.

It's really handy to be able to draw up things like isocontours because it allows you some real understanding about where the heat is flowing and where the dominant gradings are.

DR. LANGMUIR: Before you move that off, what happens if you do put lead in the void space?

MR. JOHNSON: Excuse me?

DR. LANGMUIR: You said that you had modeled it with lead in the void space as well. What's the consequence of that?

MR. JOHNSON: The temperature drops dramatically and the weight goes up dramatically. But basically that's what we found. You can really lower the peak cladding temperature. There isn't a thermal resistance in those big air gaps where you have to transfer heat by this non-linear phenomena.

The temperate difference between the inside and the outside basically from here to here (indicating), is about 40 degrees. So, that's the difference. When you put in high conductivity, you still have basically the same gradients inside the fuel bundles, but you get this quite large drop and you get heat that transfers across here and across here and out a whole lot easier.

It was one of those things that you people come up with when they say, why don't you take your existing model and pull up the radiation and pull up the conduction and fill that area full of lead and see thermally what happens.

DR. LANGMUIR: How low did it go?

MR. JOHNSON: I can't remember right now, but it probably got down to about 290 degrees centigrade, peak cladding temperatures, on that order.

Late in the work that I did, somebody came to us and said, did you know that 30.4 volume meter spacing isn't an SCP-CDR. How much do you have to change the maximum load? How much to do you have to decrease in 4.75 kilowatts at emplacement in order to get the same thermal response if the spacing is really 15 feet by 126 feet? And basically what this 30 percent change--30 percent decrease in area, all I had to do was lower the thermal load at emplacement by 15 percent. And basically what that tells me is that early on when these peak temperatures occur, the container almost doesn't know that it has neighbors. That basically the rate of heat being conducted out is more a function of the local properties around the individual container itself. So, you can't depend on linearly changing the peak cladding temperature by lowering the local power density.

Three or four years ago the people at Battelle and

PNL did some measurements on the thermal response of a consolidated fuel bundle and reported on this information. And, we tried to use that information in order to see what would happen if we used a more realistic value for the effect of conductivity of a consolidated fuel package.

The yellow case is one in which we have used the values that I used in our reference in the green and it reflects this improved conductivity. And basically what the difference is is when you model, not only radiation between fuel rods within this bundle, but the natural convection that sets up within this stack of rods, that you get an increased conductivity and it does tend to lower the peak cladding temperature by roughly 15 degrees.

So many things seem to change the peak cladding temperature by 15 degrees, that you almost think that it's some kind of conspiracy. The one thing that I find though is that where some raise the peak cladding temperature when I vary things over the parameter range that I think is realistic, there are some things that raise it by 15 degrees and some that lower it by 15 degrees and you end up having the same response.

One of the other questions that came up when doing my work was, somebody asked, suppose that I take the annulus between the container wall and the borehole wall and I fill that with bentonite. How much does the peak cladding temperature change, and so I looked at essentially two--oh, one thing before I go on, the minute you fill this annulus with essentially an opaque material, you lose your really nice heat transfer that occurs because of the thermal radiation, and since that's 80 percent of the total heat being transferred a lot of things change, or at least you think a lot of things change. And in any case, the two cases that I ran, suppose that somebody just dumps the powdered bentonite in there, the way it comes from a potter's shop in sort of a fluffy mode and you get all these little air areas between the granules. And that's what I call the loose packed system.

And the other one was I looked at a value that I thought represented bentonite in situ as firmly packed as they could get. And the difference--the loose bentonite gives you a temperature of somewhere around 400 degrees centigrade. And the tightly packed bentonite gives you a response that's almost exactly what you would get if you had nothing there at all except the air/steam environment.

DR. LANGMUIR: Does this assume a one inch spacing? I mean if you went to two or three inches outside?

MR. JOHNSON: Yeah, it's a two centimeter spacing between the wall and the container.

DR. LANGMUIR: What if you doubled it?

MR. JOHNSON: Then you'd get a really different response, because the conductivity is linear--the heat by conduction is roughly linear where you've got a large container and a very thin wall. And since the radiation is the predominant heat transfer mode, it's not going to change much if you make that a two inch thick space or a one inch think space. You are still going to get basically the same peak cladding temperature.

But, you would see if you filled this annulus with two inches of packing, that you would get a much higher temperature peak cladding wise, because it just simply doesn't conduct as well. I think it turned out a little fortuitous that they basically came out the same.

In the case of the next view graph, I apologize for the quality of the one on the left-hand side. I do real well on computers and real lousy on xerox machines. The question came up, suppose I take the same weight of 5 year-old fuel and replace the ten year-old fuel, basically raising the local power density. And I did and the difference in peak cladding temperature was roughly 65 degrees centigrade. It went up to roughly 415 degrees. The peak borehole wall temperature went up from 220 to 250, actually from about 205 to 250. The peak cladding temperature in this case occurs at eight months after emplacement. You don't even get your panel done before five year old fuel is as hot as it's going to get, but it drops really quickly. And within three years, you are back roughly to about 350 degrees.

DR. LANGMUIR: You know, we talked about ventilation. If you were to actually physically move the air and force it around the canister in that one inch or two inch space, how much could you lower that peak cladding temperature, just for the first few years if it was worth doing it?

MR. JOHNSON: That's so sensitive to the velocity at which you do it. And in really a narrow space like the one inch space you can do a lot of cooling with air. Relative to my experience and I don't spend all my time on the Yucca Mountain project, but relative to my experience, these are relatively long-term phenomena. i would expect that you could lower it tens of degrees. How many tens, I don't know. It's not an easy thing to do. You pretty much have to blow from the bottom up because there is a definite temperature gradient from the bottom to the top because of the hot air tending to want to rise. I really couldn't tell you pretty well.

I think ideally probably what you do is you just simply wouldn't use 5 year-old fuel. It seems the easy thing would be just to leave it out for five years where it's easy to do things and let nature keep it cool for you.

Early on questions came up--the original material that we looked at was 304 stainless and somebody said well suppose I use 7030 cupro nickel or suppose I use incolloy 825, is that going to change the thermal response different, because it does make a big difference as far as corrosion is concerned. And sure enough it doesn't. The top one is the incolloy 825 is the reference case that I showed you initially of my results and you see roughly just a five or ten degree difference between those. And again the reason for this small change is the fact that thermal radiation is the dominant heat transfer mode in conduction through the supports and across the wall have a very minor effect.

So basically we've had all these results. We started with basically a real view of a repository and we are saying, okay, I'm going to look at in what is at times excruciating detail the response of a single "representative package", or a "limiting package". And, I do that in terms of modeling the tuff and in terms of a three-dimensional model in which I have all of the heat of the package coming out and I use the borehole wall time/temperature history to do the thermal sink for a two-dimensional model of a container and that gives me essentially the kinds of temperatures that I want to look at in order to compare against the design constraints.

And over the seven years, all the work that we've done basically falls under these four criterion. Initially we looked at what were the most appropriate tools and modeling techniques in order to get these estimates at peak cladding temperature, borehole wall temperature and temperature one meter into the tuff. And we applied this against the designs that came up in the MRS common canister configurations and we varied things like fuel form, power output, orientation, internal design, spacing, various things in that design. And we looked at again about a dozen different designs and about 40 different configurations.

From there we went to looking at those designs we felt were more realistic and could eventually be documented in the SCP-CDR and we chose one for the BWR fuel and one for the PWR fuel, and there were some suggested alternatives. And in those cases we looked at, again the effective orientation and spacing, and how much fuel you could put in them. How many assemblies you could put in them. Not only consolidated assemblies but also intact assemblies.

And finally, the most recent work has been on an alternate scheme in which we looked at a hybrid configuration of PWR and BWR in the same package in separate fuels and again we looked at the various parameters that control the thermal response and looked at the magnitude of those in order to guide our further efforts in the times that come.

The summary of the results is basically that again that dominant heat transfer mode is by thermal radiation. The vast majority of designs that we looked at all fall below this 350 degree maximum. If you take even just reasonable care in your engineering judgement about what the size and the shape should be, the peak cladding temperature occurs within the first three to ten years. Essentially while you are still walking around in the repository. The relationship between local power density and peak cladding temperature is not linear if I decrease the repository associated with a package by 30 percent, to get the same response, I only have to decrease the thermal load of that emplacement by 15 percent.

Things like during a 3-D model or variations in various parameters seem to cause only small perturbations about the reference case plus or minus 15 degrees. Things like what you assume for the conditions in the annulus around the container or what you assume that the conductivity of tuff, those seem to be the dominant things that control the design as far as the 350 peak cladding temperature is concerned.

It's a bit like doing myself out of a job saying that the work that I have done has less of an effect than the work that John and Lyn and Eric have done. But I think that that's very much the case.

And where does all this work fit it? As I say the important thing for peak cladding temperature, borehole wall temperature and the one meter condition is more the layout, the source strength and the conductivity you assume as far as peak--and as John mentioned, if you include the effects of thermal flow again you are just talking in the five to ten degree centigrade range on its affect on peak cladding temperature.

For a container analysis we are looking at an infinite array model, conduction-only-in-tuff, you get a pretty reasonable answer for peak cladding temperature because it doesn't know much about the thermal conditions of its neighbor and the conditions aren't cooling enough far enough out for this heat pipe effect to begin to effect the peak cladding temperature.

And finally, and the one thing that ultimately is worthwhile for the work that I do I think is that besides peak cladding temperature and knowing that you are satisfying that design constraint, you have all these other temperatures that you have. You can find out what temperature you have for handling. People can do cladding creep calculations from your time/temperature histories. You can look at the waste form degradation or corrosion calculations or thermal-stress calculations in those areas where you figure that there are thermally induced stresses within the package itself.

Any other questions?

DR. VERINK: Did anyone calculate how long it takes after the temperature gets down below the boiling point for moisture to get back into the situation and that you have more moisture around the packages again?

MR. JOHNSON: Well, I believe John said that it took centuries. There is a century differential between the time that the 100 degree isotherm arrived at the borehole wall and the time that you got water there, liquid water.

DR. VERINK: Well that's a 100 years, now we are looking at 10,000.

MR. JOHNSON: No, no, we are talking--well, I don't know. I really don't know. No, I don't think so. Normally when I do calculations, it was just for the first 3,000 years because I was interested in the near-field conditions between 300 a nd 1,000 and in the container conditions between essentially zero and 20 years. But, no, I've never done it.

DR. VERINK: Would you--you say that in any event it would go to 1,000 years, though?

MR. JOHNSON: In other words, you are saying are the boreholes that I looked at always greater than the boiling point of 1,000 years?

DR. VERINK: They get down to where ever it is below the boiling point sometime. Now by the time it rehydrates, will that be 1,000 years?

MR. JOHNSON: Well, the cases I looked at, sometimes they never got below the boiling point. The hybrid case, I never saw a case below the boiling point, ever, even at 1,000 years. That has consolidated fuel and that's a limiting case.

In the MRS common canister study there were a few that got in that range, but I don't think many of them dropped below 95 either. Normally what occurs, on the basis of Lyn's study, what occurs is that these things--it doesn't happen in the middle of panels. It happens out on the corners and at the edges and things. I've done some work similar to Lyn's and that seems to be the situation. And again that's something that's not really needed to be modeled in terms of a finite element model.

DR. VERINK: We are trying to asses situation where you now have to start thinking about water or moisture corrosion questions. And when would you estimate that's going to come to pass?

MR. JOHNSON: When water significantly effects the corrosion rate?

DR. VERINK: Yes.

MR. JOHNSON: I don't know. That's not my area. I'm a thermal analyst and I really couldn't tell you. You need somebody who won't just take a hip shot and give you something that could be really different. It's--I really don't know.

Any questions?

MR. CUMMINGS: John Cummings from Sandia Labs. I think you need to take caution. You were doing a conservative calculation with respect to the cladding temperatures and that's fine. But if you then apply those results to say it's conservative or use those results for the exterior of the container or the borehole wall, that may not be conservative. So, I think that's just important especially with respect to convective heat transfer. What you did was correct to be conservative for the cladding, but it may be non-conservative in terms of the container wall and so forth.

DR. NITAO: Dr. North, can I comment on the Dr. Verink's question about the Dr. Verink's question about when the moisture comes back in.

I've done some simulations, some preliminary simulations which I presented today which shows that it takes roughly anywhere from 1,000 to 1,400 years for the moisture to come back into the borehole.

MR. BLANCHARD: Are those yet published?

DR. NITAO: Yes, there's a Livermore laboratory report that's been approved and published.

DR. NORTH: Yes, could we get a copy of the report please?

DR. NITAO: Yes.

MR. SHAW: Bob Shaw from EPRI. I have a general question maybe for anybody in the audience that it disturbs me as we go through this analysis. We are talking here about cladding temperatures that are on the order of 300 degrees centigrade. We are talking about decades that material is going to exist at this particular condition and the material presumable is dry so I get concerned about whether we ever have any experience--I can't think of any, where we have retained a metal, a material at those kind of temperatures for that length of time and the inside of the fuel cladding is going to be exposed to a variety of chemicals that have resulted from the fission process. And it seems to me we have transferred the degradation mode away from corrosion to some other kinds of things that take place.

I get concerned about creep. I get concerned about the internal reactions with the chemicals that are present in the zircalloy and I'm wondering if anybody knows of any experience we have, not even radiological, necessary experience, but experience where we have materials for decades that are sustained at such a high temperature, so that we can say we know anything about their properties that change over a long period of time?

DR. NORTH: It sounds like a very good question.

At this point I think we are about ready to break for lunch. Dr. Deere has an announcement he'd like to make at this time.

DR. DEERE: Yes, thank you Warner. I would like to make a public announcement. Mr. William Coons has given me his letter of resignation as Executive Director, which I regretfully have had to accept. Our original agreement was that he would come to help me organize our new government agency and get things underway and try to help keep me and the other Board members out of trouble. His commitment was for one year only, but I had hoped that we could keep him for one and a half to two years.

Unfortunately, he went back to his home in Florida at Christmas time. For us that was a mistake. He will return permanently to Gainesville, Florida on April 28, one year after he came with us. Since he is my next door neighbor I will still continue to see him. However, the profession and administrative staff will miss his leadership very greatly as will our entire Board and consultants and particularly I as chairman. May we give a round of applause for Bill's good work.

(Applause.)

DR. DEERE: Next, I would like to introduce Dr. William Barnard of the Congressional Office of Technology Assessment. Dr. Barnard has his B.S. in Geology and his doctorate in oceanography. He has held various positions before coming to the OTA eleven years ago.

According to the law that created the Nuclear Waste Technical Review Board, we may ask for help from OTA. Consequently, we asked for Bill's help for the past year and he has been one-half time with us as a senior professional associate. He has agreed to join us full-time for the next four to six months as interim Executive Director. And he will assist me in a search for a permanent Executive Director.

The two Bill's will continue to work side-by-side for the next five weeks and will give us a good transition.

So, Bill, I welcome you and would you please stand

and let the people know who you are.

(Applause.)

DR. BARNARD: Thank you.

DR. NORTH: Okay, let us try to resume at 1:30, please.

(Whereupon, a lunch recess was taken off record.)

A F T E R N O O N   S E S S I O N

MR. BLANCHARD: We invite you to come in and sit down. We are about ready to begin the presentations for the afternoon. This will be the last talk on the waste package thermal studies and Les Jardine will present the result of the analyses that support criticality.

DR. JARDINE: What I'm going to do is pick up and show another two examples of some design assessments that have been done in support of the waste package or the waste container more specifically in this case.

You've just heard about some of the applications of thermal analysis techniques that have been applied to different kinds of container configurations, and you saw a lot of different kinds of geometries and quantities of fuel and also there was an implicit thing in there that really is a constraint in the sense that that two foot diameter from the defense glass waste worked its way into the system, and most of the container diameters are not more than three inches plus or minus that kind of original number, well 28 inches, so I guess it's plus or minus four inches or so.

That's really a constraint. And one of the--there are two kinds of analyses I'd like to touch on. One is the issue of nuclear criticality, which is an issue and it is called for in 10 CFR 60.131 and you have to design and basically preclude that. And back in this reference here which is a 1984 document UCRL-53595, the first assessments were reported and that's what I'm going to be talking about today. And there have been some following ones. In terms of criticality back in the 1982 to 1984 period, some calculations were done for different kinds of configurations of the waste in a container. And I've highlighted here four things that were done.

You know if you have the fuel in the different geometries that you saw containing different numbers of PWR or BWR assemblies and whether it's intact or consolidated, you have some issues, is there or is there not a criticality design problem or issue. What I want to talk about are the results of this study that looked at four specific kind of configurations and one was this nominal container with the seven sectors in it. And when the fuel was basically dry, when a container was fully flooded with water, which becomes a moderator. And then also looking at condition six, when the container had partially degraded such that the consolidated fuel rods achieved a different geometry called optimum rearrangement as opposed to close contact where the individual rods would touch. And the tenth one was further in time in this analysis where the pellets essentially had reverted into a pulverized form in the bottom of a container or a borehole. That was looking at those four different geometries and this is the configuration that was analyzed back in that 1984 time period or in this report. And basically as we said--

DR. NORTH: Excuse me, could you clarify what optimum means in this context? Does this mean getting as close to criticality or over it as possible?

DR. JARDINE: No, this is the cartoon. I need to explain it. If all these fuel pins would touch each other and if there was nothing between them, in other words when you just put them in the can, all the pencils here or fuel rods would touch each other and be in contact and there would be a void up here in the top, when a modeler comes along and they want to make a optimum rearrangement, they space them at the same difference so that you have the maximum amount of moderator between the individual rods and it's the worst case.

So, that what was looked at was the situation where all of these fuel rods would be touching, like here they are, you see, and when you pack these things up, it turned out roughly one-half to two-thirds of the space was all that was occupied with the consolidated fuel rods. In the early conceptual designs there was no provisions made to fill up all the space in this diameter. Again, I think there's something implied by the two foot diameter constraint.

So, this is the way they would normally be in a container. A modeler will come along and make an optimum rearrangement so that they are spaced such that you have the maximum amount of moderator between the individual fuel rods.

And what was looked at then was the case where a container was flooded and dry. An in summary what is reported in that report, if you have a container which is the nominal configuration, and that basically means that they are just laying in there touching each other and there is a void space over separate which would be filled water or be dry. What you want to look at in a criticality calculation is the K effective parameter. And the regulation says that that thing has to be less than .95. When you exceed a .95 value, you have an issue for which you then have to look for a solution to.

In the case of the dry container, the K effective was within the design regulatory requirement. In the case where you fully flooded that container this number is still below .95. That's an acceptable answer. When you went to a situation in scenario six where the container was partially gone and what we meant was that the individual sectors somehow had degraded to the point where you could get a geometry to look like this. In other words, some of these spacers in here were gone, then this is scenario six where an optimum rearrangement happened. And in that case, where you find some K effectives that are higher than .95.

Another key parameter here is what is the uranium enrichment that is in the fuel for what you are doing the calculation. The higher the enrichment, generally, the higher is the K effective. As this went down in enrichment the K effective comes down and you would have to have nominally a residual enrichment of U^{235} or 1 percent to be below the .95.

What this is showing you, is that there are situations where if you don't have high burn up fuel where the enrichment remains high, you can have the number which exceeds the design requirements and you have to do something. And similarly, for the case where there was total or worse degradation of the fuel and you had similarly values that exceeded the design requirement of .95, then you really have to address this and the report in 1984 concluded that.

And this might be one way to summarize really what the issues are in terms of what we know about waste package or waste container that if you were going to proceed, what you will have to be able to do is take credit for fuel burn up in order to satisfy the design requirements. As the fuel burns up the uranium enrichment goes down and the plutonium builds in. That's a plus against your minus, but in addition you will have to take some credit for the fission products themselves that build in because those are poisons and you can get additional credit. And this is an issue that's being worked in the dry cask storage business for PWR fuels and is still being discussed I guess for BWR's.

So, you do have to work this issue of how much and how can you take credit for the burn up and the appropriate parts of that.

Another way that you can address the question of a K effective is to incorporate some kind of poisons into the container design such that you satisfy the requirements. Traditionally, if you think of reprocessing plants, there the poisons tend to be put in desolver tanks or things, and the other issues come up that in the traditional regulatory environment, you have to do such things as verify that the poisons indeed remain there if there is some reason to suspect they might disappear with the function of time or be leached out.

You might consider asking the question of if we put this into a container design, over the long period of time that a container would be in a repository, and it might be a regulatory concern, what do we have to do if we are driven to incorporating poisons and it's part of the container design. Is there some different--

DR. LANGMUIR: Could you tell us what the poisons are?

DR. JARDINE: Pardon?

DR. LANGMUIR: What kinds of chemical substances would the poisons be?

DR. JARDINE: One of the common ones is a boron type thing, which of course tends to be water soluble. There are different schemes for that including adding higher boron content into say stainless steel or steel alloys. There's a limit to how much you can put in before the phases come out. You can talk about putting enriched boron 10 in, which is very expensive. It's the boron 10 that you are after.

There are other also schemes that use gadolinium or other kinds of solutions. The traditional water cool chemistries deal with boron. It's sort of a favored.

But the point I'm bringing up or trying to bring up is if you do think about putting poisons into your container configurations as part of your design process, then you are going to have to have some interactions and discussions with how we are going to proceed with the regulators on this because it is a different question that they traditionally have dealt with in reprocessing plants, spent fuel storage pools, and how are you going to be able to convince them in the traditional way that indeed criticality is not an issue.

This could have an effect of driving you to either smaller geometries because that is another fix. This is assuming that the geometry stays roughly the same. You can conceive of one assembly per container is a solution. But that has all kinds of implications in terms of repository handling interface operations. Or are there other solutions. But the analyses that have been to date in 1984 and there have been some that have been done subsequently by Sandia that confirm these results for the current designs and is an issue which needs some more attention and will be looked at.

That's all I really wanted to say on the criticality and I'm willing to move onto my second design assessment. There is a second example of some of the details that have been looked at in support of canister design. And again what I'm going to do is go back to the work that was summarized in the 1984 report which was really very typical. If you want to talk about the structural considerations for the pre-closure period which is what I want to touch on, you need to have some information about handling and placement operations so you can be sure that the container and pintle and so forth will withstand the normal handling operations.

In addition there where it looks at what might be required to retrieve a container out of a jammed borehole and then also for accident analyses. You need to know something about the way the container as envisioned in this design, how would it perform. The way that was done was to look at--to make some finite element models of a container, and this is not exactly the SCP-CDR design, it is short by about three centimeters in diameter. But, basically a finite element model was made and used 2-D and 3-D type analyses and one of the things here is yield strength that you are going be interested in in terms of the steels which is around 30,000 psi.

Now what this model did was to make an element and then let's ask the question, if we pulled up on this thing and we had it simulated as jammed in a borehole, what would happen in terms of the stresses that would build up? You know, how is the container designs and would they perform. And basically, what the conclusions were that this container weighed nominally 12,000 pounds as I've shown here. And if you would pull up on this thing with a load of 92,500 pounds, then you would begin to reach the yield stress at which point this is nominally where you are beginning to have a definition of failure. And this begins to give you some idea of what kind of a load this container design could take and the stress contours as to where they were occurring. And again, what I am trying to do is show you that these analyses have been done and have been and are part of the process.

In terms of accident analyses two kinds of analyses were done with dropping or dynamic type situations. A container was dropped ont the pintle and what happens is you get this straining plastic deformation in this area and you get a slumping. If you dropped it on the end, these were nominally from 27 feet, 9 meters, you get on the bottom of the container, it went out about a centimeter and slumped down in plastic deformation.

What you concluded is that there was not a failure of these containers in the sense that it didn't exceed the criteria of 80,000 psi and 40 percent deformation which was set up by the analyst.

And I might comment that after this, tests have been down in the DWPF container designs through the Savannah River PNL organizations and they have confirmed that when you drop containers full of simulated waste glass this tends to be the worst case, but there is no breach of the container. In fact it held helium after more than one drop and the same when you drop it on the end. These things tend to at least maintain their ability to be helium leak tight. And it's additional confirmation, if you like, of these model and things. The containers are tough and one would--as the final designs would be brought up further, you would go back and look at these kinds of assessments.

So, what I've tried to do is add to our thermal analyses, two additional kinds of design assessments that have been done and these were what were used as the basis for the designs that went in Chapter 7 of the SCP.

DR. PRICE: I'd like to ask you a question about the handling operations integrated throughout the entire system. I don't think this will catch you completely by surprise. We right now have the potential for handling the spent fuel at the utility, again at the MRS and again at the repository. And, to what extent has there been an integrated look at this problem, for example maybe having a canister that would either be filled at the MRS or at the utility to eliminate some of these multiple handling problems and then put into a shipping cask and delivered to the repository?

DR. JARDINE: Well I think one of the ways that I've seen in my years in the program is some of these MRS system studies have looked at that where repository people participated with the MRS people and the transportation people are also part of it. I think it was last Fall that it was kind of a culmination of the last of these which DOE presented to the MRS Commissioners. But, being a part of that study what the repository people did which at that time I was with a subcontractor to Sandia, you know actually had configurations that the MRS was turning out and the repository people made sure that those squares would fit into the repository packages.

Casks were agreed to among the transportation people and the MRS people and the repository people as to how many square canisters were coming from the MRS or in the scenarios where the MRS was loaded with a container, that cask capacity was agreed to among the three parties or the DOE orchestrated this. But in other words, casks from the MRS coming to a repository that had a container in it and that container was sealed at the MRS, was a set of study assumptions, I guess.

And the same thing happened back in 1985 when there was a similar effort among the three sites and the MRS designer to support the MRS proposal for Congress. And at that time the organizations that was part of that would get together and would have basically a set of agreed to criteria. So that there was for those two MRS studies that supported the 1985 proposal to Congress and the one that was supported the MRS Commissioner's report by DOE, those both involved a lot of interaction and ended up with a piece of paper that became the design basis or really a study basis that was agreed to by MRS, repository, and transportation representatives.

I don't know if that--is that what you were after?

DR. PRICE: Yes, so basically you have come to some conscious decision to handle these things three times, perhaps?

DR. JARDINE: Some of the scenarios looked at that in these studies. And there were also scenarios that looked at fuel that would come directly from the reactors to the repository, particularly the western fuel, which, in all the scenarios I recall never went to an MRS. And there was also cases done where the fuel was shipped directly to the repository and bypassed an MRS. But, I'm not sure I'm answering your question.

DR. PRICE: Well as far as the canister goes, shipping the canister to the repository rather than repacking at the repository were all looked at?

DR. JARDINE: Those scenarios were looked at and in terms of what the mission was and the facility modifications were done at the repository to simplify, if you like, because there would be less to do. You would be receiving clean containers from an MRS out of a shipping cask that was agreed to by parties. Then in those system studies, the repository people developed design perturbations of the reference case SCP-CDR that was a simpler facility and the cost deltas were worked out. And they operating cost deltas were worked out.

DR. PRICE: Is there a report?

DR. JARDINE: I'm aware of two reports. One--and I believe we can provide those to you. I don't have the number--there are two published reports in the 1985 time frame and I believe the Task A through E report of DOE is--I don't know the status of that one. The one I know is the Task E Report--the Task C--maybe Steve will know.

DR. BROCOUM: I'm not sure if that report has been published or not, but I think it is completed and I'll check on its availability. We don't have any of those people here at this meeting that can really speak directly to that report.

DR. PRICE: I think we would like to request those reports.

MR. BALLOU: One point that may be a nomenclature problem here Les, is the two of you I believe are kind of talking past each other in the context of canister container. The convention that has been adopted to try to avoid this confusion is that the term container has been--well it was suggested in the NRC rule and has been adopted by the DOE as the term to be applied to the disposal container. Whereas, there may be a whole variety of different "canisters" that may get into the game either at a utility in the context of some intermediate packaging that may be needed, for instance in conjunction with transportation of failed fuel or with reactor consolidated fuel, or with the object that is created at the MRS for either consolidation or for other reasons. And the term is also applied in the defense waste community where the canister is essentially the vessel into which the molten glass is poured. So that generally canisters go into containers before the whole works goes into the repository, if there are canisters associated with any particular package configuration.

And I am not aware of any study over the last ten years anyway that looked at the question of containerization (sic) anywhere upstream of the MRS. There has been one of the cases that was evaluated in the most recent set of MRS studies, was containerization at an MRS and shipment of the disposal container essentially to the repository for emplacement.

DR. PRICE: Yeah, that's exactly what I was asking about was in effect, as I understand what you say, containerization is to eliminate at least some of the handling that is going on and that would make maybe a shipping cask--we've got a dual purpose shipping cask now that would end up with a triple purpose shipping cask in that all you do is take it out of the shipping cask and it is prepared and ready to go into the ground.

MR. BALLOU: Yeah, there are some concerns, I think and I'm certainly not the right person to address them in detail, but I know that there are some jurisdictional concerns with respect to for instance an MRS that is operated under a Part 72 license producing a package that is to then be disposed of under a Part 60 license. And I don't know whether that's a problem or not. Some of our NRC friends here might be able to shed some light on that subject. There also are concerns with respect to what if any subsequent inspection would be required at the repository, and given that the thing had been trundled across the country presumably from an MRS.

DR. PRICE: But just from an industrial engineering stand point, the elimination of handling here in this case would be desirable.

MR. BALLOU: Perhaps.

DR. JARDINE: Okay, any other questions?

MR. SHAW: I'd like to take that just a step further. This is Bob Shaw from EPRI. We have been doing some work on what we call transportable storage casks. And the reason for using those particular words is we see it primarily as a storage cask but also as a cask that then could be transported or shipped. And a couple of concerns come to mind.

The utilities right now are of course building on-site dry storage. So, one concept that was first surfaced was the idea of having these as transportable dry casks, storage casks. So that once you are ready to ship, all you have to do is put that on a flat car or a truck and send it off to MRS or the repository where ever it is.

Well the feeling has gone that that probably is not the right way to go because the utilities are not going to want to send first the material that's dry stored. They are going to want to keep that there and send the material from the pools. Then the second scenario that comes along is if you have an MRS that's truly a storage and not a processing depot, then this can work very well because you can load elements from the fuel pool directly onto this transportable storage cask, ship them to the MRS, have it stored there, and then decide from that point what you want to do. And from the industrial engineering approach, I think it does reduce the handling.

Sometimes the question arises, is when you change title also. So, that becomes part of the concern. When does the title go from the utility to the DOE and we know there are going to be questions if it's long-term storage about the inspectability of such materials. So there have to be non-destructive evaluation techniques that are developed to make sure the casks still is integral, has the strength to endure some of the tests that were just described here, you know drop tests and so on.

But, there are studies that are going on now that attempt to characterize such a transportable storage cask.

MR. BLANCHARD: Thank you.

Well now you've heard from the general topics, regulations that drive the waste package in the repository design requirements. We've talked about the repository design requirements yesterday and this morning the waste package design requirements, later this morning and this afternoon. And we thought that the topic wouldn't be complete without wrapping it up to describe the site information that is believed to be needed from the regulatory aspect as well as from the design standpoint for the repository and the waste package, and Jean Younker will present that.

DR. YOUNKER: Well it's time to move back to a slightly different topic with you as Max just explained.

First of all the way that this talk is organized is to first of all kind of give you an overview to think of what you've heard from our people in the last day and a half and then also to at the end give you some examples. We certainly didn't expect you to want a comprehensive list of all the things we think we need to know about the site in order to get to the advanced designs that we will need for a license application for the repository and waste package. So, that's not what we tried to do, but we tried to give you a little bit of a flavor for that and whet your appetite so that in case you want to hear more about it you have some topics to propose for future meetings.

What we wanted to talk you through was the idea that, I think you heard from Mike Voegele yesterday and it's kind of been the theme in some of the discussions that you heard, that in order to write a site characterization plan, it seemed to us that it was clear that you needed to have an understanding of the pre-existing site conditions and as you know the requirements for the site characterization plan required us to write a fairly lengthy Chapters 1-5 that explain the existing conditions of our understanding of the geology and the hydrology and the climate geochemistry of the site. But we also obviously, in order to figure out what kind of additional data needs we had, we needed to have some kind of a conceptual design for the repository and a waste packaging. You have heard about some of the program that has been conducted by the national laboratories for the DOE in the last eight or ten years I guess in total to get to where we are right now with the waste package conceptual designs and repository conceptual designs that were published as a support to the site characterization plan and what's called the SCP-CDR. You've heard us refer to that.

Well, much of that kind of leads you to this part of the diagram that you've seen us use in a number of different discussions with you. Particularly, I think we have focused the one on the site characterization story last summer, I guess it was, and also last May when we talked about performance assessment with some of you.

We talked about how up in this part of this 12 step diagram that you keep hearing us talk to and call the issue resolution strategy, we told you that a lot of our thought process had to do with figuring out how given pre-existing site conditions, given some conceptual designs, and given up here in the top box, as you'll recall, some requirements that the NRC had laid down for us, how it was we should then characterize the site to determine whether that site when the engineered system was placed upon it, would in fact function to meet those requirements.

So, this is kind of just to lead you through the thought process that we went through while we were working in the upper of this diagram. And what I'm going to do though is to drop down and talk about the testing program which is really the lower part of the diagram that you haven't really had too much focus on in the past meetings with us.

One point I think I wanted to make an observation that it's very clear from listening to your questions in this meeting and other meetings with you that one of the things I think that the DOE really had to do in order to write a focused site characterization plan was to make some assumptions about the requirements being the requirements. And I think some of your questions in many cases tend to ask us to kind of almost do a sensitivity on the requirements. What if they had been different. And I think they are good questions and clearly they need to be thought about, but I think that in getting to where we were at the time we wrote the SCP particularly, now we have moved beyond that. And some of those I think you've seen where some of those sensitivities are beginning at least to be thought about.

But I think in order to walk through a logical systematic process and to develop something as comprehensive and as complex as a site characterization plan, there were certain assumptions that the DOE really had to make. And one of them was that those requirements were basically the requirements we had to live with. Albeit they have some clauses and waivers and places where you could go in and negotiate, but the preferred position would be that they were written the way they should have been written and we could implement it. So, just kind of an observation on the kinds of questions I've heard you asking. Let me move on here now.

Just to kind of orient you with what you've heard in the last couple of days, clearly from the standpoint of what kind of constraints the pre-existing site features and site conditions gave us for a repository, you all know very well that the reason why the green diagram that you've seen on a number of view graphs is where it is, is because there's a major structural feature on the west side of that diagram, as we place it which is the Solitario Canyon Fault which also is a major bluff feature on the west side of the repository block.

On the east side of the block we have some relatively, heavily faulted area which you've seen referred as the Imbricate Fault structure. If you come south and kind of to the southeast, down in this area, you have a variety of faulted areas, one of them named Abandoned Wash Faults, but you also have heard a number of times referred to the Ghost Dance Fault which of course is the one major fault that we no of within the primary area.

So, just to give you the impression obviously from the beginning, the overall structure and topography controlled the orientation and the location of the overall facility and the conceptual designs.

You also heard Tom Blejwas yesterday talk about how our understanding of the larger scale site constraints is evolving. I think that's an excellent example of what I suspect as we proceed with site characterization, get some new information from both surface based drilling studies, and certainly when we get into the subsurface with the large diameter openings, I suspect that what we will see is more of what you heard from Tom yesterday, which is that he told you that we have some constraints from the stratigraphy that has to do with where the bottom of the vitrophyre is located, where the high lithophysal zone with the Topopah Spring is located and if the orientation of this repository plane was constrained to some extent by how far up in the section it could go and not get into the very high lithophysal content and how far low it could go and not intersect the vitrophyre at the bottom of the Topopah Spring.

Well you heard Tom tell you yesterday that now we have a little bit of understanding and done a little bit more analysis of the data base and it appears that we may have more flexibility in that vertical plane or the number of vertical planes such that we can change the orientation and buy ourselves a little bit more useable rock volume. And I think that's an excellent example why I assume we will see through time as we collect additional site data some of the constraints that we had at the time we developed a conceptual design that we used for the site characterization plan. Clearly, some of the constraints are going to be relaxed as we understand more about the site, especially the three-dimensional variation and rock properties at depth.

Okay and then just graphically, moving you down through and into the scientific investigations, site characterization plan part of that whole strategy. The whole reason for doing much of this analysis and plot experiments that we did and getting to defining the testing program was to figure out what it was going to take as a first cut to characterize the site, get an adequate amount of information to lay out something that we felt would be a licensed application that might meet the requirements for compliance with the NFC's regulations.

So all of this effort was really driven at the question of what will go into the license application. And obviously, what we are heading for, and who knows if they will look at anything like this, but what we are heading for is the final repository and waste package design that can be used as the basis for the license application.

Running you back through some of what you've heard, you know that Mike Voegele told you yesterday, that it looks to us looking at where we are right now, that many of our major site specific data needs have come from this one part of NRC's requirements in the 60.133 that's called "Additional Design Criteria For The Underground Facility".

When you look at the major requirements that you've heard from our design people, where we have the relationship made very clearly for us by the NRC's regulations, between the performance that the repository has to meet and the design of that system, you'll find out that these statements, the five I have here and three coming up are the ones you heard Mike Voegele present yesterday, and you also saw them repeated a couple of times in the other talks. Basically the whole underground facility requirement, let me remind you was to contribute to containment and isolation, to be flexible to accommodate site-specific conditions, to reduce potential for deleterious rock movement and limit potential to create preferential pathways, and assist the geologic setting in meeting performance objectives.

Now, clearly these are not terribly site-specific on the one hand. On the other hand, from that what we've had to do based on our understanding of the site is to figure out what that means in a site-specific context.

The fifth one, would allow compliance with performance objectives given predicted thermal-mechanical response, which is what you've heard a lot about from our people today.

Okay, the sixth one that is part of that same section--part of 10 CFR 60.134, "Additional Design Criteria for Seals" and this one has a lot of information on it. And mostly just to kind of compile for you in one place, some of the conceptual designs for seals, and if you look as an example for the underground facility, I've shown you a picture of one of the conceptual designs for a backfilled sump, just as an example for you. This is not intended of course to do anything more than perk your imagination and curiosity about what some of the other studies are that have been done that we haven't told you about yet.

Actually, this is a presentation that was given at an Institute for Nuclear Materials Management meeting last summer by Joe Fernandez, and I don't remember who the other authors were, but from Sandia, since they are responsible for the seal design program that we have. So seals have to be designed so shafts/boreholes are not pathways, and to reduce the pathways for ground-water or radionuclide travel.

And the waste package one once again takes you to trying to make from the very less site-specific requirements and the wording to what we had to do to it to figure out what that meant on a site-specific basis. Waste package interactions with environment do not compromise performance.

Clearly, lots of what you heard from John Nitao today and some of our other speakers had to do with the question of what happens in that near-field. I know several of you have asked questions, what kinds of changes can occur in the rock chemistry and in the composition of the pore fluids, based on something like this sort of model that you heard presented by John Nitao from Livermore where you get boiling very close in, condensation, capillary imbibition, pouring the water back into the pores and then some kind of a cycle set up.

Finally, the performance confirmation requirement. Another one that probably at this point the DOE hasn't spent as much time on for obvious reasons because it's something that although we are supposed to start during site characterization, isn't something that you really ramp up until at the time of license application. I think the requirement is you submit a performance confirmation plan with the license application.

Some of the kinds of things that are required there, I think Mike Voegele mentioned for you was the subsurface conditions are within the limits that you assumed they were. Or, that you monitor the waste packages in a representative environment. So this is just to get you back into the flavor now of what the requirements were that all of these design assessments and options and alternatives that we've looked at in the last day and a half were all about.

Okay, now where is the connection to performance? I know Dr. North always wants us to think about this from the standpoint of what does it mean in the performance perspective. And I think it's very clear if you look at this view graph with me for a minute that if the focus is to design an Engineered Barrier System that helps the site meet the performance objectives, if you look at the kinds of phrases that our engineers have given us and are in the regulations, such as, diagnose unfavorable areas and skip and isolate. Use acceptable thermal loads. Limit water volume and quality. Limit adverse excavation effects. Reduce potential pathways.

Those words such as unfavorable or acceptable can only be looked at from a performance assessment view point. But you wouldn't have any way of knowing what was really acceptable. I mean accepted in the most extreme setting, unless you looked at it from the performance assessment question of what is acceptable and what's unfavorable.

Slow me down if I get moving too fast. You all know I tend to barrel away.

Okay, so given the performance driven design considerations where are we heading now with the site characterization program or at least where would we like to head.

Data is needed obviously to support performance assessments and to look at the effects of any sorts of design changes or design alternatives and options. Clearly, another area where we would like to be able to collect some additional data and where we need to put some effort is in developing site models. I think this question has come up a couple of times in the last day and that is what kinds of sensitivity studies can we do on the basis of current information. Clearly you can do some back of the envelope, you know the 3 X 5 card routine that we've talking about, but my experience in the last year in kind of a review capacity for the DOE and

Performance Assessment Program, leads me to a conclusion, at least. And, this is just my personal opinion that, without some additional site data, I think that the real value of a lot more sensitivity studies with the current performance assessment models is really questionable. It just seems to me that what I heard from all the people we surveyed, and this included the headquarters Performance Assessment Contractors as well as the Project's Contractors and in talking with a number of them, I think we have a real good understanding of what the current state of the models is and what the current information basis for those models is. And they all keep saying we need additional site information. If you want us to do sensitivities that mean anything right now, we can give you any answer you want us to give you. You know, tell us what you want.

So, I think what we are hearing from the PI's and the people who are down at the lab counters working at the computer screens is, some additional data, not just say I think some well-focused sensitivities can't still give you some interesting new information.

Clearly, another reason we need additional data is go reduce uncertainty in the basis for requirements and to go back through that whole iterative process that we've defined.

Okay, Tom Blejwas had in his presentation for you, really focuses in then on this site, given the site-specific conditions that are present and what we know about it right now, there are three areas of kinds of effects that the repository might have on the site that seemed to be of concern, given the site-specific conditions and characteristics that we have. And that's of course, you've heard from Tom and you heard from John Nitao today, in the hydrologic, what's the near-field effects of having the thermal load from the repository. And in the larger picture, we haven't really talked about this, but in the far-field overall, what kind of effect does this kind of a thermal fault have on the hydrologic conditions. What kind of overall modification when you talk about a 10,000 year period, does it have to the flux that comes through that repository plane? Simply where does the water go, I suppose.

Okay, you heard the thermal-mechanical and we emphasized a lot on the thermal effects today. Obviously, in the near-field we are interested in both the thermal-mechanical mechanical, certainly from the standpoint of how stable the boreholes will be in the retrievable period as well as in the longer term if we are trying to maintain an air gap as part of our design.

In the far-field, you've heard the discussions about what kind of thermal-mechanical effects might be felt from the repository given the thermal loads.

And just to review for you in the geochemical area, and this is one where we have a little bit of additional information presented by Jerry Boak from Max's staff at Doe. Geochemical, clearly some of the things that people have brought up and were concerned about we have test planned either in the laboratory or in the exploratory shaft facility when we get a chance to do the testing there, to look at changes in pore water chemistry, look at the questions of dissolution and re-precipitation in the near-field. And certainly one of the ones that was discussed today is the question of a far-field heating effects on any of the sorptive minerals that are present.

Okay, just as break point, what I'm going to do now is go into examples of few selected in situ tests that we have planned and we have people here who can probably provide additional information to answer your questions if you have any beyond what I can answer. I'm here to give you basically kind of an overview and preview of the kinds of tests that we have planned to look at these specific effects that we've just focused on. There's a much broader range of tests planned but we thought you might be interested in the ones, since these are the ones that you've focused on in the last day and a half.

DR. NORTH: Jean, let me interrupt you at this point, because it seems a natural place to do it and give you an impression. I really like your last few diagrams as a summary of major issues. My frustration is that I see your diagram and I wish that I had an analysis to back it up that I felt was current with everything that you've assembled to date including the major effort on the site characterization plan. And my sense is that you don't really have that. You have some pieces that date back to 1986.

In terms for example, what are the implications on the issue of ground-water travel time just to take off from your last figure? How serious might the problem be on the zeolites and smectites, etc., the dissolution re-precipitation area issues in the near-field, a question of drainage that we were discussing on the first day and the like? My sense is you have a lot of insight and I suspect there are many people in the program that feel they have these issues really well under control, you understand it, that there's no problem, that some additional data is going to be helpful, but basically you are running out of good sensitivity cases to run because you've been doing all this work in the past.

But it's not getting over here to my side of the table. And I'm not sure it's getting back there to our audience.

DR. YOUNKER: Good point.

DR. NORTH: And that strikes me as a major need in the near future. And in particular there are these issues that have been raised about potential disqualifying features of the site. I suspect toward the end of the afternoon we may hear some more on this subject, but it seems to me to the extent that you've got a story ready to go on those things, that story really needs to get out and be told.

Now, I have the sense that on this thermal issue, we have had a very enlightening two day discussion of a lot of technical material. And it's well summarized in your pictures. But, I want to go beyond that and see the analysis where we calculate out against some of these various criteria and see what do the numbers look like. Once we've put all of these insights and all of this science together, have we got a big problem, a potential problem, or a situation where we are virtually certain there's no problem?

DR. YOUNKER: I think that's an excellent comment. Let me make a comment. My observation is that we did what would be a first set of those kinds of analysis. Some with very much back of the envelope and a very small envelope even for the environmental assessment because it was driven by a regulatory need.

DR. NORTH: And the date of that was?

DR. YOUNKER: '86, right. So during '84 and '85 was when those were done. And then there have been some specific areas where I think Livermore, or Los Alamos or Sandia have looked at some of the effects and I think there are reports that kind of address the concern, but probably not as a directed as what you are looking for. Like you would if you were going to say, is it really problem? Come down with a judgement of is it really a problem from the standpoint of performance.

So, I think there hasn't been--it's my observation there really hasn't been a driver to do that and that's kind of focused study that reaching a conclusion that you only do when you are driven by some specific need that you have to make that conclusion. It's some kind of gate that you have to go through. And those gates are in the program and defined. You see them on the schedules, but there hasn't been one since the environment assessment.

MR. BLANCHARD: I, myself agree with your view, Dr. North, and that I think these view graphs on pages 11 through 13 didn't occur here by accident.

DR. NORTH: Good.

MR. BLANCHARD: They are a thrust of the melding of the engineering and the science in the project, and of course from a thorough process as we tried to prepare the SCP, we tried hard to think about what are we doing to the site, and what are the constraints on the site and from the site to the engineered structures and the engineered barrier. And so it's fortunate that this particular meeting is occurring now, because I think you all have a very good background from the previous meetings on all the basic subsystems and centering on the topic of thermal analysis gets us to the heart of the near-field of the waste package, which encompasses all of the design requirements, the performance requirements and the impacts on the site. And, all I can say is we believe we are trying to focus on that in our program in our annual budget reviews to our contractors. And we are trying to methodically update these analyses so that we can have the information available to share with you in design trade-off studies or bounding calculations so that we can see how one impacts another as we make design changes or conceptual changes in the approach.

DR. BROCOUM: I'd like to add just one thing and that is for example, when we started this year the prioritization, looking at the order which we were planning to do the testing and see if we can better order it or reorder it to get to these questions you asked of evidence of non-suitability.

Part of that study or in parallel is how do we evaluate suitability specifically for the site, and part of the intent of that was to do it in an iterative fashion over through time. In other words, we don't just do it once at the end of site characterizations, but we periodically look at these things and see if we have any problems as we are going down the road. And that's what we are working towards, but we are not there, I guess would be a fair statement.

DR. LANGMUIR: Well, isn't the bottom line then that until you get down on the site and have the shaft in, you can't validate the models. You really can't come to an ultimate conclusion as to the significance of the various pieces of this whole puzzle and decide what performance is critical and what isn't and what might be disqualified. You are limited in that regard.

We've seen a lot of models today which were based upon limited field information for validation. And they will remain that way until you get down a shaft and can look at Yucca Mountain. There is a limit to how far you can simplify the system at this point in time.

DR. BROCOUM: That's correct. And one can debate whether you can do something more with surface based testing or if you need both surface based and in situ testing, but yes, we do need more information from on the site.

DR. NORTH: What I want to make sure is that you are not discouraged from going as far as you can with the data now available. And further more, recognize even when you get underground, the amount of additional data you were going to get is relatively limited. You are not going to be able to explore on very fine grid mesh, the rock characteristics over this rather large area. What is it, 1,600 acres.

Now we are going to get some samples and in fact with the present plans you are not even going to explore a very large section of the area. The Board has been pushing you in the direction that maybe we ought to have more exploration like an east-west drift. So, it seems to me that these are crucial issues, how much data do you really need to be reasonable sure.

And having an analysis that goes as far as you can go with today's data is a very good way to get started on that problem.

DR. CORDING: Just one point also, I think the--you know, the scientists are frustrated because they haven't been able to collect the information they would like to have and that's something that of course is intended for the future. But I think in looking at it from the perspective of the number of panel here and supporting the Board, it seems to me that we are very concerned about having as much flexibility as possible, or what the limits are to the what you can do in terms of things such as cooling the waste or temperature. And that if you are narrowed down on some of those items, your focus on the exploration and the data collection also tends to get narrowed down and you may be not looking at all of the possibilities.

And I think particularly at this point, where we are seeing that there's a lot more to be obtained and a lot more to be collected, and a lot of information, not knowing what the impact of that will be on whether the site is suitable or not, it seems that we really need to leave our options open as much as we can and explore the broadest range of possible alternatives. And that's part of what you have to look at when you go out and set up the tests.

And some of the models still--there's a lot more work that can be done on some of these models. I mean some of them are also very preliminary. More data will help. But integrating for example the matrix and joint effects, those are some of the sorts of things that are still in the process of being done and need to be--further work can be done on that even without getting additional data at this point. But to get some of those developments on the model will certainly be helpful as well.

So, I think that the collection of data has really got to have a focus and I guess it's in the opposite of that, it can't be too narrow because we may miss some potential here for getting a satisfactory site.

DR. YOUNKER: Your last comment is exactly I think my view of what I've seen since I've been watching the program now for about ten years. And the whole question of how to focus it so that you get a reasonable scope given the limited dollars, but not get too narrow so that you don't remain loose enough to get in the alternatives should you find something that you weren't really planning on. And it's been a real difficult road.

DR. DEERE: Could I interrupt and ask you to go back to number 2.

DR. YOUNKER: What's it look like?

DR. DEERE: A large scale site constraints.

DR. YOUNKER: Oh, yes. Certainly. Here it is. There you go.

DR. DEERE: Right. Over the word fault on Solitario Canyon Fault is that a creek, a fault or contour line?

DR. YOUNKER: I think that's a splay that comes off from the Solitario Canyon that is mapped up there.

DR. DEERE: Good place for the east-west cross drift, isn't it?

DR. YOUNKER: I don't know if there's anybody here who knows the data base real well. Mike or Tom that's correct? I'm getting nods back there that that's a splay off the Solitario Canyon.

DR. BROCOUM: We can check that out once in the field.

DR. YOUNKER: Yeah, we can certainly check that out. I'm quite certain that I remember being asked that question before and looking it up and finding that there is evidence to that in the Scott and Bonk Map.

DR. DEERE: So it is something that's important.

DR. YOUNKER: Yes.

DR. NORTH: Okay, let's let you go back to 15, I believe it is.

DR. YOUNKER: I guess I'll start with 14 to get us back on track, that basically we obviously are looking at the testing program to provide us data to reduce uncertainty in the assessments of the effects on the site, but clearly we also need additional information to move toward the final designs for the repository and the waste package. And as you all have commented, I think I would be remiss if I didn't tell you that our PI's clearly feel and some of the people that have talked with you feel that additional laboratory testing that's going on right now, is part of the ongoing site characterization program, even though it's on samples that were collected pre the whole quality assurance program being implemented. There's still a lot of good work being done and a lot of progress is being made in understanding. So that it isn't--sometimes I think we are viewed as not making progress. And you know there is still a lot of work ongoing at this point in time that is advancing the understanding and developing the models further.

I have some examples for you now in terms of the types of testing that's planned. And we just picked a couple. And in thinking about what--I didn't go back through the records to see what you had heard about from our plans for the exploratory shaft for in situ tests. There's a number of them planned in the old design at least. If we have two vertical shafts, you know one of the shafts will be a test shaft and there will be a number of tests during construction of that shaft as well as then in the in situ testing area. And I presume no matter how we come out of the alternatives analysis, we will certainly still conduct a number of tests similar to this suite of tests that we have on the deck right now.

One of them I wanted to tell you about, we picked a couple that really get at and focus on the three areas of effects that we've just talked about. So, let me tell you a little bit about the Engineered Barrier System test which is designed by Livermore.

This field test is planned to improve our understanding in the hydrology and the geochemistry area in using an in situ test that allows you to over a time period of several years look at migration of fluids around that waste package and get at some of the effects we've just been talking about.

Mainly the goals are defined as determining the thermal response and movement of moisture in the waste package environment and to validate and calibrate fluid flow and transport models. And as several of you suggested, I think one of the big goals of the in situ testing program will be to validate some of the models that we will use and continue to use.

The emphasis in this Engineered Barrier System test, unlike some of the other tests that we have planned is on the cooling period. So that they are interested and concerned with how the fluids come back into that near-field area so that their plan for getting the temperature up in the rock around the canister heater or simulated heater that they would use, isn't as important as it is cooling it back down and watching what the fluids do.

Let me give you a list of the parameters here. And let me emphasize too, of course, that test plans for the exploratory shaft facility have evolved over the past eight to ten years. In fact some of my first assignments in the program was to work on developing the early plans for this particular test. I feel kind of peculiar standing here telling about it almost ten years later.

The parameters to be measured in the current design of the test, and the test is still in a conceptual phase, would be basically the saturation distribution in the near-field. The relative permeabilities, matric potentials, fluid flow pathways and rates, characteristics of fractures, getting the thermal loading--not as big of an emphasis here but of course it's important because you want to tie it to what's happening with the fluids. They also will do some instrumentation from the mechanical and thermal properties. And then there is some emphasis in the test and I suspect given the direction that the program has evolved, the questions that have been asked even as recently as in the last day and a half, wouldn't surprise me that the emphasis on this bullet, pre-and post-test water chemistry and perhaps the mineralogical changes in the test plan might be beefed up. And I'm just suggesting that this isn't something that I know is necessarily planned, although I see Len Ballou nodding his head, so he's probably agreeing.

I would think that some of Pat Domenico's questions about what happens in the very near-field to the small fractures, rather they seal or heal, whether there is changes in the composition of the fluids as a result of that is going to be a very important question. And I think probably this test is best one we have to get at that sort of issue. It's just a schematic. And the people who are working on this I'm sure have some newer ideas since this one is one that has been--this is a schematic that's been around for awhile.

Just some information about the way the overall test is planned, this one shows you a horizontal configuration. And you've heard people say that we are moving now toward the vertical so that this one would be redesigned for the vertical. And the original plans included both horizontal and vertical to maintain flexibility. I think that the thinking now would be that perhaps one of these would become a performance confirmation type of test. So you are going to want to maintain it for a long time at temperatures that are as representative as the conditions can be so that it can go on into the licensing phases as one of your performance confirmation tests.

The initial testing phase would be, just to give you the numbers we are currently planning, would be 5 kilowatts, 100 degrees centigrade--you would get 100 degrees centigrade one meter into the rock in about three months. This is kind of the conceptual design for the test. And then you'd go through a six to nine month cooling period. So this thing would be about--this is a horizontal emplacement and this is about 12 meters long with the heater in the internal six meters so that you get it away from any kind of a flex out here and the drift of changes in humidity and temperature out here. So you just--you have the heat source only in the inside half of the emplacement.

DR. LANGMUIR: Jean, what group is planning to do this work?

DR. YOUNKER: This one is a Livermore test.

Okay, and then just to hit the major points, we get at the effective on the water chemistry of the near field thermal perturbation and of course define the spatial distribution of the fluid, is the most important aspect that we've talked about today.

But in the design of the tests I suspect there can be some additional emphasis on what the chemistry changes are too.

Okay, the other tests that we chose for examples for you are the tests that are Sandia designed tests. And the ones we are going to tell you about are a heater experiment which is in the current exploratory shaft, it's in the upper break out room and I'll talk about that when I get to the next view graph.

I'll talk about the heater block test for you which is in the main facility, heated room experiment and then the sequential drift mining experiment.

Okay, the instrumented heater experiment, the important item on this for you to be aware of is that in this particular test, we did plan to do a canister scale heater in both the upper breakout which is I think at 550, something like that and then at the main test level of the exploratory shaft. I don't think this is a new package, but just so you can visualize this with me, the upper break out, we have elevations on here instead of depths. But I think this one is like 550 and this one is 1020. Close enough.

So the idea is that we would do a full scale heater test both at this shallower depth and at the repository depth. And the reason for that is that you recall on some of the cross-sections that Tom Blejwas showed you and what I talked about earlier, if you get into that shallower break out level you can get into the high lithophysal content Topopah Spring member. And so what we consider this heater experiment in the shallow break out room will be one that you can look at kind of the bounding case, or at least the one where you have as high a lithophysal void content as what you are probably going to have to deal with over the repository area. So it gives you kind of one in member in terms of the way the rock material will behave in the heater experiment.

Now, this particular test also is set up so that we can go into an overdrive mode and this is the only one that we have where we are going to be able to take it to the point where we actually, this one goes up in steps and there are several different ideas on how we might do this depending on how long we have. But basically, taking it up to as high as 12 kilowatts such that we actually produce borehole spalling. So that in this case we can get some handle on the bounding case of just what it takes to actual cause some crumbling into the borehole.

Okay, another experiment that we chose to tell you a little bit about, the heated block experiment, okay, this one is one of the important experiments that helps us go from the lab scale to a bench scale or a larger lab scale to a fairly large block of eight cubic meters of welded-tuff here and to get at under controlled stress and temperature conditions, boundary conditions, to make the kinds of measurements you need to make to measure three-dimensional deformation for your modeling purposes. Measure relationships among fracture permeability. Some of this at least will be modeled after the heated block test that was conduced by the Sandia folks in G-tunnel that you saw a picture of in Tom Blejwas's presentation yesterday.

We'll look at moisture movement in this one as well through some--I don't they are shown here but there will be some holes for neutron hubs to be inserted so that they can look at changes in moisture content. The heater is of course in this one, not outside of the actual block.

And look at cross-hole measurement techniques. They have some plans to use some ultrasonics in this one as they did in the G-tunnel experiments.

This one would be one of the key steps in being able to extrapolate from your--Dr. Cording was talking about this earlier that going from lab to field scale, especially if you have to deal with jointed rocks, it's very useful to have something at this scale because you can start to see the joint effects and see how that changes some of your module as you try to figure out what the lab measurements mean in the field. So this is a scale.

The next two actually give you another scale that's planned, and I'm sure for you folks that understand these tests, this is old hat. Basically, the planning in this case for the heated room test would be to measure--let's see we go into--the heated drift is here (indicating). We have two observation drifts on either side. And these are actually--

these are boreholes, something like 30 meters long that have heaters at the end of them. So we are going to seal off this area right here and put a lot of heat into that part of this portion of rock material. With thermal barriers in here then you can really get the temperature up here. And the measurements that are intended to make, in terms of rock mass deformation and temperature of course.

This gives you yet another scale. Now starting to get the scale that you were mentioning earlier, wondering if we had any test planned. We have the tests planned and I presume they will do a lot of pre-test modeling on this and then check it against what they observed.

The people I was going to ask to comment on this are clear in the back of the room. So if you have any questions we'll force them to walk up here.

Okay, one more and these really should have gone in the other order, but this one is just to look at the sequential drift mining experiment, where you can basically in this case, I think you can look at--I think we are going to have two instrumentation drifts and then get this all instrumented and bring this one in. And basically you will do a lot of pre-test ultrasonics. You'll have a good idea of what this area should look like and then you'll go in and actually mine it out, map it and determine how good your predictions were on a very large scale. This should give us a good handle on how we are able to remotely predict the rock properties. This one will be excellent for validating some of our mechanical models and this one also will contribute some to the understanding of the kinds of changes you get in permeability and the relaxed zone around an excavation.

I know some of you guys know a lot more about these kinds of tests than I do so if you have questions, we do have people here who would be happy to respond.

DR. CORDING: Those borehole permeability measurements, is that some sort of a pressure injection test?

DR. YOUNKER: Straddle packer system is what they have planned.

Anybody else? Well, this will hopefully--for those of you who are very much attuned to these kinds of tests, this should at least give you a flavor for the kinds of tests plans that have been prepared and evolved over the past seven or eight years of the program.

So, just to bring you around to a focus now. Basically, obviously what we are heading for is a sufficient understanding of the site data so that we can develop the final design and be able to make the case that it contributes to the performance of natural barriers and of course, to understand how those natural barriers work well enough in concert with that design that we can make credible performance predictions for the site, which is where this is all heading.

DR. NORTH: Comments or questions?

MR. GUPTA: Dinesh Gupta, NRC. Could you put that slide on heated room test?

DR. YOUNKER: Okay.

MR. GUPTA: We at NRC particularly are interested in this full-scaled heated room test results. Unfortunately, it's our opinion that to get meaningful data it will take a long time to get any meaningful information from these tests. These tests may have to be run for almost three years before you get any meaningful results.

I was wondering have you done any prototype testing on C&G terminal or something else that can be started early on and get some data before you wait for going underground to do these tests?

DR. YOUNKER: Yes, I think Tom Blejwas can probably can come up and address that. I think on comment would be though, Dinesh, that we can--one of the things we can do is on some of these clearly identify them as performance confirmation tests and run them on into the performance confirmation period. If we do need additional time, then the question of prototyping, Tom can address, or whatever.

DR. BLEJWAS: Yes. Tom Blejwas, from Sandia. We have plans to try to do a prototype test where we would heat a large section of the back or roof of a drift or the side of a drift and we were planning on conducting that experiment in a facility like G-tunnel. We are presently looking to conduct that in another facility that we are investigating.

And the idea there was again to get some intermediate scale where we could get some results in the period of a year or two and as Dinesh pointed out, the heated room test would then be something that would be conducted for a much longer period of time. So the answer is yes.

DR. NORTH: We are now to the point where we have a presentation on paratrac coming up. And, we have a break scheduled in about ten minutes, and then following the presentation on paratrac, we are going to hear from the State of Nevada. I have warned them to expect being on at about 4:00 and the paratrac presentation is going to take the order of half an hour or less?

MR. BLANCHARD: Yes, probably 20 minutes.

DR. NORTH: Okay, my thinking is that maybe this would be a good time for me to make a few concluding remarks on our main issue for this meeting, the thermal question and invite Dr. Allen to make any remarks that he might care to make, and then we might take our break for a few minutes and then we'll resume with you.

I'd like to commend all the speakers. I think we have had a very enlightening two day meetings. And we've accomplished a great deal of what I had hoped we would accomplish on this issue. What I think is terrific is that we have, I think, seen a lot of very valuable information that provides us many new insights on the thermal loading issues and its complexity. I will state again my desire to see it go further into the performance assessment calculations so that we can get more a quantitative sense of what all this means, as opposed to the qualitative sense and the pictures that, yes, these are in the important issues.

Where I come out in my own thinking, I'll speak strictly for myself on this, is that I conclude that the thermal issue adds to the uncertainties and maybe to the problems of assuring performance for the natural barriers and for the total system. On the other hand, the thermal load getting above the 95 degree Centigrade point helps on the engineered barrier. On the other hand, I come back having attended the panel meeting in January to the fact that we are dealing with a canister that's one to three centimeters thick where the corrosion at that level may be a problem and, therefore, being able to keep the water away with that kind of a design is important. But, I'm delighted to hear about the new plan and the fact that you're looking at alternative designs and that even things as far-afield as ceramics are still on the list.

So, it seems to me very useful to be able to circle back again and say for the total system performance, what makes sense? If we conclude that some of these thermal loading issues are enough of a liability in the sense of uncertainty, if not known problem, does it make sense to reconsider what is being done on the waste canister and have the flexibility, at least, of going to a different design where perhaps we are less dependent on maintaining 95 Centigrade plus for a period of at least 300 years. So, I hope as we go further, we will further be able to explore those questions. They seem to me like very important ones for the boards that John does and certainly for the risk and for performance analysis panel and also for the Department of Energy.

Clarence, would you like to add anything?

DR. CLARENCE ALLEN: Well, I, too, have been very much impressed with what I've heard, although this was certainly not my field of expertise. I guess, one of my impressions is that the degree of uncertainty in much of this area is even greater than I had thought. I think some very important questions have been asked. I'm particularly intrigued by Dr. Shaw's questions just before lunch. I think we've got a lot of work to do in this area.

DR. NORTH: Any other members of the board wish to add comments at this time?

(No response.)

DR. NORTH: Okay. Why don't we take --

DR. DEERE: Well, I don't want to disappoint everybody.

DR. NORTH: Good, Don.

DR. DEERE: In giving emphasis that we still have a lot of unknowns about the site with respect to the weak features that can give difficulties with respect to permeabilities and with respect to potential collapses, et cetera. So, I think that the underground exploration for me is still very, very high. I would do it on any other kind of job that I'm going in the underground, for the underground power houses, the pressure tunnels, which are very important, but certainly not as important as this facility. And, I think we would not be doing the state of the art not to get down there fast and get the kind of exploration that the state of the art of our industry requires. You are all realizing that and I hope that the priority is very high.

DR. NORTH: And, let's resume at about 10 minutes after 3:00, 15 minutes from now.

(Whereupon, a brief recess was taken.)

MR. BLANCHARD: I'd like to introduce Tim Barbour who works for SAI who will be describing ParaTrac.

MR. TIMOTHY BARBOUR: Thank you, Max.

This presentation is a followup to a request from your last meeting on some additional information on ParaTrac. ParaTrac, as some of you may be aware, is being used in support of some of the surface based test prioritization task work that we're doing on the project right now. So, I want to give you a little bit of information on the system and suggest that if you want additional information, we can certainly give that to you at some other time.

During the past couple of days, you've heard an awful lot about a variety of project terms and organizing elements that are used in the regulatory/technical arena. You've heard about issues and sub-systems and functional requirements and performance objectives. You've heard about design requirements and the parameters and site tests in the course of these two days.

You also heard Mike Voegele yesterday talk about the relationships between regulations and performance and design requirements and you've heard Sandia and Los Alamos talk about the design requirement information in the sense of site data needs and you just recently heard Jean talk about technical information needs in terms of site testing.

All of this has really tried to show the translation from a regulatory based arena into a set of site tests that could be performed on the basis of those regulations and priorities. And, what I'd like to do today is present to you an information management system. It's a simple computer based system that ties all of these elements together so that we can have an organizational framework to read and evaluate, both in the terms of our planning and our data accumulation as it applies to the site.

The system basically will define the explicit links between many of the items that you've heard about and what I'd like to do very briefly is describe what information is in ParaTrac and what the uses are and how it's been used and what are some of the other interfaces with other information systems in the Yucca Mountain Project.

The system basically contains an immense amount of regulatory and technical information thus derived from the SCP, particular Chapter 8 of the SCP. It contains information on systems in the sense of the mine geologic disposal system. It has information on issues and performance goals, design requirements, characterization parameters, tests, scenarios, alternate conceptual models, and on and on and on. These elements that are listed here and others are just simple lists of information. You can list those, and if you list them and then you can link those lists, you can develop relational data bases. It will allow you to follow yourself through the system in a sense of information linkages from systems to tests, as an example. And, I'll get into some specific examples here in a minute.

The system is parameter based meaning that we use parameters in the sense of the physical and chemical quantities, thermal properties, and other things that we're talking about here. As the unit that we relate between one element of the project and another, parameters are used in the sense of performance measures, the parameters of site characterization measurables, parameters of design and performance needs. So, it's a parameter based system. The system is also SCP derived meaning that all the information that is included in the system -- and these three volumes contain some of the information, but basically it takes the SCP and organizes it into sort of systematic tables and allows for easy reference and cross checking between a variety of these elements. It is SCP derived. It is also included and expanded on upon the basis of information put together by the project participants in their detailed study plans about site characterization. And, it's consistent with the technical planning basis that the project has in the form of control documents. So, it's got -- it's well founded on the basis of information in the project.

As I indicated, the overall system includes information about the mine geologic disposal sub-system elements, their functions, and their processes. It includes information about the performance of those systems in terms of performance measures and goals and confidences. These are all things that you may recall from the previous discussions over the last couple of days that are coming out of the performance allocation process as fundamental to the SCP. It contains information about design and performance information needs and what we need to know about the site. It includes information the site characterization parameters, the site tests, the technical procedures, and the specific locations where these tests will be performed. It includes information about alternate conceptual models of the site. And, it includes information that is more recently on site scenarios in the sense of the system changes and the impacts therein.

The reason for listing all these is basically to conclude by saying at the bottom here that all the above are related to ParaTrac. These are explicit links between these lists that are in simple table form from one to the other that provides the project with a very powerful tool in the sense of interface control and it provides the project also with the ability to perform many valuable sorts that you might have to do that you can do very quickly using your computer. For example, you might want to know all the tests that are going to be conducted in the Calico Hills unit. That's an easy sort for a system like this. Or what are all the parameters that we're going to determine in the Topopah Springs unit? We might be able to do that very conveniently.

DR. NORTH: Could you give us a sense of how this is implemented? Are you using a relational data base language or --

DR. BARBOUR: It's a relational data base. In the sense of the hardware/software, it's just on a PC network right now, a Norvell system with D-Base III. Which you have to realize there it is a relational data base. Being relational, it can work with systems like Ingres and other main frame sort of relational data base. So, it is a pure relational data base in that sense. Does that answer your question?

DR. NORTH: Can you give us some idea of the size of the amount of data that's in it?

DR. BARBOUR: The data that's here is probably about 40 megabytes worth of data, if you're familiar with computer mass storage. That's not a lot. When you start relating all these lists, then it becomes, you know, additional data, but the fundamental data is about 40 megabytes worth of data.

DR. NORTH: Do you happen to know as a matter of reference what the SCP would be translated into megabytes?

DR. BARBOUR: I don't know, but SCP was six volumes and this is three. What we've done, actually what the project has done, is basically we've taken all the objectives and the activity information, all the technical information about parameters and performance objectives, tests, and technical procedures, anything that could be -- even work scope, if you like, from each of those activity descriptions and pulled it out and put it into a data base so that you can sort it and retrieve it quickly. From my view, it helps immensely in being able to sort the information you want and look at the information you want certainly much more conveniently than by sorting through 2,000 pages of Chapter 8 of the SCP.

Uses of the system up to this point have included support to the performance allocation process. In effect, it automates that process. The performance allocation process, as Jean and Mike and others have talked to you about, basically looks at the requirements, the design performance needs, and tries to iterate on the need for site characterization. That's what performance allocation is all about, what we need to know versus what we know now. We can produce tables that show those relationships using ParaTrac. The system also has been used to provide -- affirm out of relationships, both regulatory and technical, in the sense of the study plan rationale. The system really was originally built with that in mind, to support the study plan development. One of the requirements of the study plans, which as you're all aware is an extension of the SCP in the sense of the details of the site studies, was to be able to justify the site program that the principal investigators were proposing on the basis of the regulatory needs. So, it's been invaluable in that regard. It also has been used to show the technical logic in how this information feeds ultimately to a design performance model.

In terms of site test integration, the system allows the ability to look at testing within the site program to evaluate their compatibility, their completeness, their sufficiency, and also to look for redundancies. So, you can use it in that regard.

More recently, it's been used in association with the surface based test prioritization work. This is used in conjunction with a quantitative decision methodology that's still in development, but the idea there is through relationships that ParaTrac can be used to show the relations between one element and another. Now, if you can begin the proprieties on some of these things, then you can use it to --

DR. NORTH: We're going to hear about that at future meetings, right?

DR. BARBOUR: Surface based test prioritization, yes.

DR. DEERE: It's scheduled June or July, I think, the time frame, yeah. I'm not sure of the exact date.

DR BARBOUR: As an example of how the system might be used, this is just a simple flow diagram. It doesn't include everything that's in ParaTrac, but it includes many of the things that you've heard about in the past couple days, the systems, the performance measures. You've heard about issues and design performance parameters. You've heard about the need for site testing. I'm sorry some of this is -- this viewgraph is not all that clear, but the point here is that here are these well-defined logical relationships as defined in the SCP. If you list each of these items and then relate them one by one, you can get relationships between, say, a scenario and the types of tests you ought to be performing if you have the explicit relationships between each of these boxes.

The blue highlighted areas basically is the logic that we propose to use in support of the surface based test prioritization. Again, if you're familiar with what that task is about it's concerned with focusing or prioritizing a site testing program on an early evaluation of the potentially unsuitable conditions at the site. Those start out with defining the performance -- the potentially adverse conditions which then we would relate to a set of site specific scenarios. Those scenarios could subsequently be written to related issues and design performance parameter needs which through something that's artificial in the sense of the data base, the information system, the parameter category allows you to tie over into the site and measure some testing that ought to be done to support that to take a better scenario. Now, that's a logical relationship that would allow you to get something over here that very indirectly can give you something very specifically here. You begin to rank these and -- the ranking or the priorities of some of these boxes, then that can propagate itself through the system. So, if you have a scenario that's of high priority to you, through these relationships you can evaluate the needs for the site testing in the sense of that priority.

By way of example, I've handed out here as a separate package and I didn't want to go through this, but it's a set of tables basically that are partially completed. It shows the linkage between what I've just described between potentially adverse conditions and in this particular case, reporting milestones, what was of interest is if we have a scenario, what's the test. But then, in addition to that, what's the reporting schedule on data that are going to come from those tests. These series of tables basically provide a linkage that's not meant to be read just from what you see here, but just to demonstrate that tables can be put together on the basis of these linkage to assist project planning in the sense of scheduling the work that you intend to do to support the particular -- in this case, the scenario. This case, by the way, is one that's based on the potentially adverse condition of movement of radionuclides in the gaseous phase in the unsaturated zone. And, ultimately down to the last page, you'll get to information about parameters and tests and reports and it will give you information about gaseous level in the unsaturated zone. Now, this is one example of some tables you can produce by this sort of linkage. Obviously, you could pick any one, any scenario you wanted, and go through the same exercise.

One of the other things I should mention is that the system is not just a bunch of paper in the sense of producing hard copy reports. It's developed into an interactive computer system where you can query it and you can sit there and ask and focus your answer to get the type of information you're looking to get rather than just dumping a whole bunch of tables for you to sort through.

So, I guess the point I just wanted to make here is that you can look at your site testing program on the basis of a couple of things in these relationships. You could start out with a scenario to find the ones that are important to you to come up to site testing. You could also start on the basis of system element like the Calico Hills and say, well, I want to focus and put my emphasis on the testing in that regard to identifying the key system that you're interested in and also come down to the test that might -- that are related to that particular system.

The last two slides, I'll just go through it relatively quickly just to indicate that the system is not independent of the rest of the world. It does have some key interfaces with other data bases, if you like, that the project is maintaining and building. One of those is a technical support documentation data base that is concerned with a regulatory technologic, but mostly in the sense of how the data is accumulated and reported, such that we can get a better handle on the site information schedule. In other words, when the information that we need is going to be generated in a time frame that would allow us to support DOE position papers and ultimately the safety analysis report and the license applications. It's a document hierarchy, basically; safety analysis report down to position papers to technical reports, and ultimately there's a tie there to ParaTrac and the parameter information contained therein.

You've heard about the RIB in the last couple of days and the associated SEPDB which is a preliminary site engineering properties data base that feeds ultimately to RIB. Those two systems are really concerned with the parameter values as the means to stand deviations of parameters like porosity and density, hydraulic conductivity, thermal conductivity, et cetera. It's really what we know now. So, there's a tie also between the parameters in ParaTrac and the values that are maintained over here in the sense of the data values and maintaining these two data bases.

As well, there's a tie to the systems hierarchy and the requirements therein, particularly in the sense of the natural barriers. The natural barrier sub-systems are relatable to the specific locations that testing is going to be performed in and maintained in ParaTrac. So, there's another interface to that system.

And, in conclusion here, additional informations that potentially can be incorporated -- not incorporated really into ParaTrac, but interfaces with other systems that exist in the program, one of those things could be the design requirements as they evolve and what the effect is of those design requirements on the site testing program and vice-versa. There's also a potential interface between the technical/regulatory information in ParaTrac and the more management informations contained in what's referred to as the planning and control system which is cost schedule sort of information. So, this is an opportunity, I think, for us to integrate or influence the budget cost cycle on the basis of regulatory/technical needs using these information bases and the linkages between them.

I've already mentioned the site test prioritization that's mostly in the sense of the surface based test prioritization task at the moment, and more in the distant, there's a potential interface of the system with field operations in that each of the tests that are included, the site tests that are captured by the data base, would have pre-requisites in the sense of doing that work in the field and also support requirements in the sense of subcontractor support. That's in the distance, I think, a bit more.

That's basically all I have on it. I have three volumes here that you're welcome to look at. If people would like to see those, I'd be happy to share those with you. I'll give you the examples. I'd be willing to share and produce other examples, and I think if you're interested, there's a possibility for a demonstration to see how this system actually works with hands-on. That's all I have to say. Thank you.

Any questions?

(No response.)

DR. BARBOUR: Okay.

DR. NORTH: Thank you. The next and last item on our agenda is a presentation by the State of Nevada on site assessment methodology and Carl Johnson from the State of Nevada will introduce that.

MR. CARL JOHNSON: Thank you. For those few people who don't know me, my name is Carl Johnson. I am the administrator of technical programs for the Nevada Agency for Nuclear Projects. Our office is responsible for the state's oversight of the repository program.

Actually, the main reason I'm up here is to just provide an introduction. The presentation is going to be made by Steven Frishman. He is a member of our technical staff in our office. For those who don't know Steve, he has been around this program since the late 70's. He's the former director of the Texas oversight office. He's had a lot of experience, especially dealing with early discussions on the regulations that are applicable to this repository program.

We have been concerned in Nevada with the site suitability assessment since the early days of the high level waste program. We presented extensive comments in the environmental assessment on site suitability. We again presented comments on the site characterization plans related to site suitability. I made a presentation to the NRC almost a year ago on some of our concerns related to site suitability. And, we had some discussions at the meeting last month of the board that related to the surface based prioritization program and our concerns there.

I think at this point what we would like to talk to the board about today is the basis for our concerns related to site suitability assessment. With that, I'd like to turn it over to Steve and have him make the presentation.

MR. STEVE FRISHMAN: As Carl said, I want to today get back to some of the real basics and one way of characterizing that is let's look at the horse that's in front of the cart that we've been discussing for the last day and a half.

There are some major considerations in design of this whole program that, from our perspective from believing that we understand the Act and the regulations, are being actually left out of the program. That's just the front page. I'm also not going to do remedial reading this afternoon. There's a lot of text involved here. I'll go through it and what I'm trying to do is point out some key words and their meanings because it's become apparent to me through time and even during this meeting that there are a lot of words that are heavily burdened in the program. And, unless you understand the definitions that the department has applied to those words, then we're always talking past each other; such simple words as "suitability" and what the Department's definition of that is. Other words, such as "issues", you had a little exchange yesterday over issues and the reason that it was not a very satisfactory exchange was it turned out that you were speaking different languages. So, what I'd like to do is go through this and try to develop through, first of all, a fundamental question and our answer to that question. Then, go back and develop the logic for and the basis for our answer.

First of all, we'll play the old game. We've seen this, I guess, maybe a total of ten times in the last couple of days. All I'm doing right now is putting this up here to say what's wrong with this picture? And, when I'm all done, we'll put it back up again and maybe at least some of us will have some insight into what's wrong with this picture. You all recognize it, I'm sure, as the issue resolution strategy.

First of all, we can phrase a question pretty easily having to do with whether the site is qualified or disqualified and whether, in fact, the program is designed to be able to determine that. This question is a relatively simply one. Can the Secretary under the existing planned program make an early determination that the site is disqualified for geotechnical reasons and, therefore, not suitable? Disqualification is a word that is developed in the Nuclear Waste Policy Act and goes on through the guidelines.

Let me show you our answer. I think the top line is the significant line. The answer is, no, the program is not designed for that to happen. And, in fact, there are pieces of the program that preclude that happening. Not only is the program not designed to do it, the Secretary's restructured program, if you look at the statements very carefully, also is not designed to do it, although some people have interpreted from the language of that November 30 report that, in fact, that's what's being planned to be done. And, we'll go through that in a few minutes, too.

If we look instead of 10 CFR 60, as we have been for the last couple of days, and look at the requirements of the Nuclear Waste Policy Act and look at the requirements of the guidelines that were developed under that Act, I think there are some things that, unless you've lived with it for a while or had to think about it a lot, you tend to miss because the program is going off in a licensing direction and from all of our analysis is ignoring the fundamental statutory requirement of what the Secretary is really supposed to be doing during the site characterization process.

First of all, the section in the Act says if the Secretary at any time determines a site is unsuitable, he will terminate the work. The Act also requires the general guidelines of 112(a). Those guidelines require that there be specified factors which qualify or disqualify a site from repository development. Now, the guidelines are organized in an interesting way and I've certainly served my sentence playing with them and Mussler did a few other people did. But, a very complex organization of the guidelines if you really try to under-stand what they're saying.

The Act itself, really if you stop and think about the admonition to the Secretary that if he finds the site unsuitable at any time, if you look at that admonition, you can draw from it the fact that the Congress was intending to be prudent. The Congress didn't want money spent and time spent on sites that did not meet some very early and fundamental tests and there are some of those built into the guidelines and we'll see an example of that in a minute.

Now, the organization of the guidelines is something that befuddles people. You try to write it down and there's no simple way to describe it. The last time I think anybody really tried to understand them, at all, was in the drafting of the environmental assessments for the sites leading up to the May '86 naming of three candidate sites. But, in very simple terms, the guidelines are organized into system guidelines and technical guidelines. The system guidelines just simply adopt by reference the licensing regulations including the EPA rule, 40 CFR 191.

The technical guidelines speak to the topics that you find stated in Section 112 of the Act that must be considered; also, some of the citing topics and most of the citing topics that you find in 10 CFR 60, plus some additional ones. Now, under the technical guidelines, there are qualifying conditions and disqualifying conditions. Remember, the Act required that there be and that the guidelines specify factors which qualify and disqualify.

The way the guidelines are constructed, the qualifying conditions are total system performance based. They lead towards the same types of objectives when you're in the geotechnical area as 10 CFR 60 and, in fact, they go directly to that by in places naming sections of 10 CFR 60.

The standard for disqualifying is very different. The standard for disqualifying has to do with specific site conditions that you can determine individually without ever looking at the overall performance of the site, a very different standard. And, the intent, I believe, was so that time was not wasted and money was not wasted on sites that couldn't pass some relatively fundamental tests and tests that didn't require an overall look at total system performance before you understood some of the basics of just the geology and hydrology of the site.

Most of what we're talking about hangs on this distinction between the standard of the qualifying condition and the disqualifying condition. Just as an example, this is the statement of the postclosure system guideline that's found in 10 CFR 960. The important part is you see reference to 40 CFR 191, reference to 10 CFR 60. And, the system guideline, therefore, is a total system performance based guideline, an example of a qualifying condition for postclosure geohydrology. You see, once again, it speaks to 10 CFR 60 and a very specific section of 10 CFR 60. So, again, we're looking at a performance base qualification condition.

Now, look at the disqualifier. Disqualifier is very specific and that disqualifier can be analyzed without system performance analysis. It can be analyzed without any dependence, at all, on the caveat condition of 10 CFR 60. It is the most rigorous statement of the equivalent condition or constraint in 10 CFR 60. Quite simply, a site shall be disqualified if pre-waste emplacement travel time from the disturbed zone to the accessible environment is expected to be less than 1,000 years along any pathway of likely and significant radionuclide travel. You can work that one over right now without having to do system performance. You can collect the data. You don't have to even worry about the fact that unsaturated zone models for modeling the Yucca Mountain situation don't exist and there's a great deal of wrestling going on right now with trying to develop what kinds of models they should be and then the monster question of how do you ever validate it if you do develop that model? You can look at the site and read the site, maybe even with existing data -- our office believes that there are sufficient data -- but maybe for purposes of those who don't believe it, maybe make this determination very quickly without having to do with any of the sophisticated modeling, set up a relatively simply testing program looking for this fastest path. And, there's a fair amount of evidence out there already from examples of observed cross talk between wells that were drilled years ago. It can be looked at without venturing off into the very, very expensive model development and the one that is time consuming and may never, in fact, be successful. So, we're back again with the issue of the standard. It's quite different for disqualification rather than qualification.

Now, the bottom line in the guidelines for the use of the guidelines is what is referred to as higher level findings and this is just simply the statement or the alternative statements for the higher level finding that must be made.

Now, I want to talk -- once we've got the written words down here, I want to talk a little bit about how the Department has implemented what we've just looked at now. So, just another whole section on implementation. The thing that must be remembered is that under the Act the Secretary's job during site characterization includes, first and most important, whether he can recommend the site to the president for a license application or not. Everything that's been talked about here in the last two days is speaking to the license application. The Secretary's first job is to determine suitability for any reason developed under the program including the disqualification factors of the required guidelines.

So, now, we need to look at the way the Department has developed its program over the years and I think we'll find that at least my answer to the original question is within bounds. The guidelines contain everything that the Secretary needs to know, essentially, to make his recommendation. And, there's also a requirement within the site characterization plan that the plans state the criteria which will be used to determine whether to recommend the site or not and the site characterization plan correctly indicates that those are the guidelines.

Now, we have to go back through where I saw the program begin to drift a little and begin to drift away from the concept of disqualification being a different standard. The 1985 mission plan is where we first saw exposed the concept of an issues hierarchy. And, it was developing in such a way where under information needs you could still go directly to disqualifying conditions if you were of such a mind. Also, that mission plan defines issue and this is a very important definition. It still exists today and it's just that not everybody knows what it means. Issue is defined as "unresolved questions related to performance of a repository". We're back to where we're defining performance into the structure of the program and beginning to leave the guidelines requirements about disqualification analysis off to the side.

Now interestingly, in time between the issuance of the 1985 mission plan and in 1986, a change, but much better defined issues hierarchy and a document that you've heard mentioned, the OGR/B-10. But, what happened in-between was the environmental assessments came out and one of the things the environmental assessments did was made what was required to be made under Section 114 of the Waste Policy Act, the Secretary's preliminary determination of the suitability of all of the sites that were nominated and recommended.

So, between the issues hierarchy of the mission plan in '85, when you still had a chance to use information to go directly to disqualifiers, and the 1986 issues hierarchy, we had a preliminary determination that the sites were suitable. There was an analysis of information about each of the sites relative to the guidelines, the determination was made that no disqualifying conditions were present. We don't agree with that. In fact, we're still litigating that.

The issues hierarchy in 1986 narrowed itself in on the definition of issue to the point where it appears that you can no longer use the guidelines. The issues hierarchy became what we know it to be today now -- and unless it has changed again and I don't know about that. It's still based on key issues, but it made a change in the issues below key issues. It made a change by dividing them into performance issues, design issues, and characterization issues, all apparently still carrying the meaning of performance on the word "issue". So, what I see here and functionally we'll see it as I unfold a few pieces of the program outside of the regulations, functionally that is the way it's been operating ever since and the disqualifying conditions of the guidelines have been set aside.

The way the program is arranged right now and you can find it explicitly stated in the site characterization plan in a sequence of milestone decisions to be made. The guidelines issue of higher level findings -- meaning is the site qualified or disqualified -- is not examined by the program until after a total system performance and, in fact, it's the next to last step in the entire list of milestones to be met before the secretary makes a recommendation to the president of a site. So, the disqualification issue has been entirely put aside.

The Secretary's November 1989 restructured plan may have led you to believe that there was a change in the program that would take it back to an early analysis of disqualifying conditions. You've heard stated here three or four times in the last two days because I'm very tuned in to the words that the Secretary's prioritization, you know, or the prioritization memo that was referred to that Steve mentioned where you're looking -- or the Department is looking at the prioritization of the tests that they plan to see how they fit within their program and to see if they maybe need to reorder it, what you can discover by looking at that is that it is not responsive to the disqualifier. It is responsive still to a total system performance approach. And, now we can look at his words and we'll see a little bit about what they mean.

First of all, have the question aimed at evaluating whether the site has any features that would indicate that it is not suitable as a potential repository site. Now, I didn't make a viewgraph of this, but the definition of suitability as used here and as used in this prioritization memo is totally system performance based. Site suitability is defined on the basis of evaluations of site and system performance against the performance objectives specified in NRC 10 CFR 60 taking into account blah, blah, blah. Suitability, whenever you hear it from that side of the table, is performance based. It is not individual site characteristics based. So, that's one part of the Secretary's statement that made you think he was going to do one thing, but he's not.

The other part is where it speaks to the suggestions that were attributed to the state of Nevada and some others about early looks at the site. Well, in fact, this does not reflect what we said should have been done. This reflects, once again, the semantics and the definitions of the Department's language relative to suitability. In this case, notice that it says "scientific investigation activities focus on potentially adverse conditions and that efforts be made to evaluate key suitability issues early in the process". The potentially adverse conditions are meant to be the potentially adverse -- they are meant to be the adverse conditions of 10 CFR 60. The key suitability issues are the issues of the issues hierarchy using the definition of the word "issues" which is totally performance based. I think it's pretty clear if you understand the words.

Now, here's just a synopsis of the findings out of those two sentences. Key issues are the system performance based issues and the adverse conditions are those of 10 CFR 60 which contains no disqualifiers explicitly and also is totally system performance based on in itself.

Now, if we have a problem with that interpretation, it's much more explicitly stated in the memo that we've just talked about on prioritization. The surface base testing program will serve to support a decision to proceed with underground work, but will not constitute any establishment of pre-requisites for such underground work. I hope you're convinced now. This is what they said and what they meant.

Senator Bryan in some questions to John Bartlett relating to his confirmation as director of OCRWM asked a similar question having to do with will the program look early at the characteristics of the site in order to determine whether, in fact, the site is disqualified or not. John Bartlett's answer is a correct answer. "A decision methodology for evaluating site suitability currently does not exist." And then, "use of a go/no-go decision points is a possible approach". And, the only reason that that use of go/no-go is there as a possible approach is the question specifically asks if there was such a decision process of go/no-go. The answer is clear. No, there is not.

Now, to sort of wrap it back up again, knowing now the difference in standard between qualifying and disqualifying, I think it is absolutely fair and correct to say that there is far from enough information available about Yucca Mountain to qualify the site. We don't know anything near what's necessary to even begin writing a license application. But, the program has totally set aside the different standard of disqualification and we've shown that it was set aside so far that the issue of disqualification will never come up until a total site performance assessment has been made and it's likely that will never be made until at least the Department is convinced that the site is licensable. So, therefore, de factor, it is not disqualified.

Now, we hear very often that we need to get to work on the site. We don't have good site data. We don't have enough site data. Senator Johnston was a little concerned about where the money goes, and in response to a question recently, the Secretary answered and this is the basis of his answer. There's about $1 billion spent on Yucca Mountain. Work includes about 220 boreholes, 95 trenches, lots and lots of work. Work that, yes, is not usable until it is requalified in a license application, but there's nothing that makes it unusable in looking at the site relative to the absolute standard of disqualification under a half a dozen different disqualifiers. So, there's plenty of data out there that can be looked at and can be looked at conservatively.

We did hear a comment the other day in a meeting from a representative of the Department that I found a little bit astounding and that's that he said it's really not productive to back and revisit our old data and samples and so on. It's much more productive to be out collecting new data. At this stage of the game, I find that to be a statement that hopefully won't be made again. There's plenty of information out there. There was some discussion of that today. If you're looking in the context of whether the fundamental characteristics of this site even suggests that you ought to keep looking at it in the way it's being looked at now -- the way it's being looked at now is what are all the issues we have to go through to get it licensed as opposed to does it ever meet even the fundamental criteria of whether you ought to be looking at it or not?

So, we conclude that a decision methodology, contrary to what John Bartlett said, does exist and it is the guidelines. And, the existing data for purposes of those who disagree with us may, in fact, be sufficient to look at the site under the disqualification factors and determine that it's probably or may not be worth pursuing this site because you will have insurmountable problems, and the more you grind on those programs, the more you'll know, but the more you'll also know that you'll probably never get it licensed.

So, now, we can go back to what's wrong with the picture and it's not very profound what's wrong with the picture. It's just that it's missing half of the program that's required under the statute. That being the analysis of the site under the guidelines for purposes of disqualification as opposed to qualification. And then, suddenly, what's further wrong with it is the burdened word "issue" which you would not quite understand unless you went through this type of analysis to figure out that "issue" itself speaks only to total site performance.

I'll answer any questions. I know this is a -- regulatory analysis is always fun. But, I've tried to keep it interesting enough to where there's a large point out there that I think, given the board's charge under the statute, it's probably important to at least think about it because the Secretary has a major responsibility regarding the recommendation of this site that goes way beyond the site's ability to comply just with 10 CFR 60 or the assertion that it does comply with 10 CFR 60. So, we present this in the spirit of your looking carefully at the technical validity of the DOE program and I think there's a whole piece within the area of technical validity that the Department has, in fact, literally carved out of the program as required by the Act, set aside, and doesn't intend to come back to.

Thank you.

DR. NORTH: I guess I've got a couple. There was the environmental assessment done back in 1986 at which these questions of potential disqualifying conditions were directly addressed by the Department of Energy. Is that correct in your judgment?

MR. FRISHMAN: Yes, they went through an analysis of each of five sites relative to each of the technical guidelines that didn't require site characterization.

DR. NORTH: So, in that time frame, they actually did carry out the kind of analysis that you are now saying they're not doing?

MR. FRISHMAN: They carried it out one time with a pretty limited evaluation from our perspective and I think you can find the extent to which we believe it's limited in our comments on the draft EA. We also are litigating that, at least in part, from the standpoint that we believe there were and there are data that would disqualify the site were you to analyze it and continue to analyze it, the data itself.

DR. NORTH: But, is your concern with data from the 1986 period when this analysis was done or data that exists now which may not have been available at that time?

MR. FRISHMAN: There is a lot of data that were really not available at that time that further convince us that there are disqualifiers that have been met. And, there's a lot of data in this program that has not come out anywhere. It's still sitting in files, not published, some of it not analyzed. Since 1986, we have seen some as we have been able to ask for it and receive it and analyze it and we've become more and more convinced in a few areas that there are data now. But, I think if we're going to go through that discussion, we maybe ought to set up a time to do that because we have some pretty definitive analyses.

DR. NORTH: Given the amount of time that I have spent pleading for let us get on with performance assessment beyond that which was done in 1986, I hear you saying something very similar, but focusing it on the disqualifying conditions.

MR. FRISHMAN: Right. I'm saying revisit the information that is there and I'm also not saying that it is incorrect to be looking at system performance. I'm just saying that there's another part of the program that has been ignored that may save everybody an awful lot of expense and time if, in fact, it looks as if the site will never pass muster on total system performance. And, you can discover that in a much simpler way. And, I see as being laudable Congressional intent when they instruct the Secretary to terminate work at the site if at any time he determines that it doesn't pass muster. It isn't just an answer at the end when the three billionth dollar is spent; this should be a constant evaluation just as the iteration will be or is intended to be on performance assessment. It's another iteration that should be going on all of the time.

DR. DEERE: I think, many of us would agree.

MR. FRISHMAN: Well, we wanted to bring this to your attention only because we know that you have not had to live with the intricacies of the Act and the guidelines to the extent that we have. We also felt that we could make a pretty simple explanation of what we think is wrong with that picture.

DR. NORTH: Would any of the Department of Energy representatives like to comment?

MR. BLANCHARD: Thank you.

Steve, you make some interesting and thought provoking observations. I'd like to thank you for those. Of course --

MR. FRISHMAN: Max has been thanking me for years.

MR. BLANCHARD: Of course, from much the same information, I think that we would come to a different conclusion about the current program addressing the disqualifiers and whether or not it appropriately has plan provisions in it to do so. I am a little confused because of the brevity of the presentation going into the details of the guidelines, but I'd just like to ask whether or not you consider a couple of things addressing disqualifiers in a plan. For instance, we felt that when we wrote the SCP following the issues hierarchy, we intentionally put three issues into the SCP for the sole purpose of implementing 10 CFR 960.

MR. FRISHMAN: Right, and I'm familiar with this.

MR. BLANCHARD: And, I'd call your attention to Issues 1.9 for postclosure, Issues 2.5 for preclosure safety, and Issue 4.1 for technical feasibility. And, I think at the time we did that we were encompassing both the system requirements, as well as the individual guidelines, and I think also we were including qualifying and disqualifying conditions. And, so our impression is that we do have an adequate plan addressed in the SCP to do that and perhaps subsequent opportunities will allow us to discuss that in greater detail.

MR. FRISHMAN: I'd like to respond to that because I have analyzed that and I'm aware of those key issues or those issues that are performance issues or they're in the category of performance issues. And, yes, they are there. They speak to can higher level findings be made under 10 CFR 960. And, that's fine. But then, if you look at the way that's implemented in the sequence of milestones which appears actually in two places, one in graphic form and one in a list, if you look at the way it's implemented, it does not become fully implemented until after total system performance is done. In fact, there's one scheme that describes how Issue 1.9 will be analyzed. And, if you come up with a no answer at a point, you not only go back and look to see if you can find some more data that might bring you to a yes, but you look at other issues to see if they're related to that one and maybe you can get around one of the guidelines' issues by trading off against another one. And, disqualifiers are not tradable. One disqualifier is all you need. So, in the issues hierarchy, yes, you acknowledge 960. The implementation in the SCP is where I believe the Department has gone off the track so far that it is not in compliance with the intent of the guidelines nor with the intent of the Act and the detective work that it takes to find the implementation based on Issue 1.9.

MR. BLANCHARD: You make an interesting point about the disqualifiers not calling for -- in 10 CFR 960 not calling for performance assessment analyses. Of course, that's in the eyes of the beholder with respect to the interpretation. The disqualifiers I happen to be looking at in my copy of 10 CFR 960, which is pretty tattered, at least all of the ones on this particular page have a phrase in them such that "lost isolation is likely to occur or expected to lead to an inadvertent loss of waste isolation". And, as we interpret those words, we felt we could not make a determination without going to a radionuclide release calculation in order to meet loss of waste isolation criteria and, therefore, we came to the conclusion that any application of each individual disqualifying condition in order to demonstrate it in front of our peers, we would have to have, as well as characteristics about the site, also we would have to rely on calculations of radionuclide releases to the excess of 1.9.

MR. FRISHMAN: And, I'd like to respond to that, too. First of all, it was no accident that I chose one that did not say loss of waste isolation and it happens to be a critical one in the area of groundwater travel time that does not speak to loss of waste isolations. Another one that I believe does speak to loss of waste isolation, but is an independent variable in the system is human intrusion. You don't need total system performance to make up your mind whether it's possible somebody is going to inadvertently drill into that site or not. So, I am aware that there is the use of the words "loss of waste isolation" in some of the disqualifiers and that could, even by the definitions in the guidelines, maybe be interpreted as having something to do with performance. But, there are at least two critical ones that do no rely on performance and one of them explicitly does not rely on performance.

MR. BLANCHARD: I'm sure that each of us has the opportunity to interpret the written language however we feel is appropriate for our own standard.

MR. FRISHMAN: Well, I'm looking for a way to implement that "if at any time" statement in the Act.

MR. BLANCHARD: The one that you used at the beginning of your presentation had a phrase "any pathway of likely and significant radionuclide travel". Again, although it didn't call for waste isolation, per se, directly, we again felt that the meaning of that and the manner in which we choose to try to demonstrate in front of our peers how that was met would again rely on total system calculations of radionuclide releases. And, so it's probably not surprising to you that we relied on total system calculation to meet that.

MR. FRISHMAN: And, pathway, I think is a significant word there. Also, if you go to 10 CFR 60, you see that it says "fastest" as opposed to "any". And, I think you can determine pathways for the water which will be the same as the pathways for radionuclides and the only thing that could help you would be if some of those pathways somehow got blocked by retardation. But, what you're looking for is the pre-waste emplacement of travel time and the pathways. And, the models won't identify the pathways for you. It takes actual data collection to go in and try to determine what the fastest pathways are. The models will only tell you the average rate and direction.

MR. BLANCHARD: That's an interesting observation. Of course, one would like to have empirical evidence on all likely and significant pathways. As we've laid out the site characterization plan, we've tried to be as comprehensive as we could reasonably be in due time with due consideration for finances for conducting site characterization. Albeit we only have certain ways to acquire enough information to empirically determine some pathways, the expectation was that we'd have to go to stochastic models in order to make meaningful calculations and distributions of groundwater travel time at different locations and space supported by -- those models then being supported by empirical information.

MR. FRISHMAN: Well, you have some interesting empirical information already by some of the accidental cross talk between some wells in vitro. It ought to be enough to suggest there's something more than just your simple matrix flow model going. And, I would think you could direct a program at data collection to try to get at that that did not require any sophisticated models, whatsoever, a pretty standard hydrologic analysis. And, you know, we can argue the semantics all along, but I think there are some places that are not arguable and there's an Act that needs to be implemented and the intent of the guidelines was to implement the Act and there's a piece of the Act that doesn't appear to be being implemented right now; while there's another piece of it that is being implemented and it just happens to be the one that you prefer to implement and we prefer to have the other one implemented. But, I see nothing wrong with implementing both. It's just when you only implement one that I see a problem.

MR. BLANCHARD: I have nothing more to say. I certainly appreciate your comments and your observations.

MR. FRISHMAN: The other thing just in simple bottom line language, it's fixable.

DR. BROCOUM: We have at the table with us Bob Mussler who is from the Office of General Counsel at DOE who may want to add a little bit to this debate.

DR. NORTH: Your comments will be welcome on the issues of interpretation of the --

MR. FRISHMAN: If Bob doesn't cross examine me like he usually does.

MR. MUSSLER: No, I don't plan on adding -- I'm not going to add anything to the debate. I just wanted to note for the record a piece of information which I don't know if the board is aware of, but the point that Steve is bringing up is in litigation right now in the 9th Circuit Court of Appeals. The State has sued the Secretary on the question of not having disqualified the site. Basically, the State has determined that this issue should be resolved in the Courts and that's where it is right now. And, I just wanted to make sure you were aware that that very specific issue is there.

MR. FRISHMAN: And, from our perspective, that doesn't include trying to fix it.

DR. NORTH: Well, I don't think we as a board can comment, at all, with regard to the legal issues and the litigation. That's clearly out of bounds for our territory.

MR. MUSSLER: Not while we're here.

DR. NORTH: I'm intrigued by your comments and I think what I would like to encourage you is to make your case somewhat more specific. I think I heard you say that based on the 1986 environmental assessment that you are not quarreling there that DOE missed something in not recognizing a disqualifying condition based on the data that was available then. But, you're saying that data is becoming available that might have that effect and, moreover, that there is work that DOE might be doing to search out and obtain further data that might clarify whether one of these disqualifying --

MR. FRISHMAN: I'm saying something much more fundamental than that that is bigger than the issue that we most disagree on which is whether the site should be disqualified or not. What I'm saying is something much bigger than that and that's the program is designed in such a way that that analysis is not being carried out and that is a -- it's an analysis that when the Secretary's report came out on November 30, I read a number of news accounts that was apparent that the reporter thought the Secretary planned to do something that he does not plan to do. And, I think I even recall some of your own comments where you may -- or at least in listening to you, it sounded as if you thought maybe there was something being done that I think we can demonstrate is not being done.

DR. NORTH: Well, I have certainly expressed myself previously that if there is a disqualifying condition at Yucca Mountain, it would seem to me in everybody's interest to find it quickly. And, I'm not sure DOE disagrees with that.

MR. FRISHMAN: Except they don't have a program to do it.

DR. NORTH: Well, I'll let them speak for themselves on that. But, what I would like to hear from you is some more specific ideas for development of new data or interpretation of existing data that you think ought to be done to look at the disqualifying conditions and you perceive they're not doing it.

MR. FRISHMAN: Right, and we have --

DR. NORTH: I mean, we can have a general discussion about the character of the program where I think we get into some very interesting semantics about words like "issues". And then, we can talk about some specific data like the interpretation of communication between wells and the like where perhaps there's something that needs to be done that could be done relatively easily and quickly and maybe this board ought to take an interest in it.

MR. FRISHMAN: Well, we've presented an analysis in at least one area already in our site characterization plan comments having to do with groundwater travel time. We can take the Department's data and show that groundwater travel times likely do not meet the 1,000 year standard. And, we have reputable hydrologists working for us.

DR. JARDINE: But, I think, Warner, that we would be happy and pleased at some future time to present that information and our analysis to you.

DR. NORTH: I will invite your thoughts on human intrusion in a similar vein.

MR. FRISHMAN: We have analyzed that, as well.

DR. BROCOUM: I have a few comments to make here.

First of all, the guidelines are general guidelines and, in fact, is their title. As we see it, the actual implementation specifically to Yucca Mountain site is the issue or the problem before us. We'd want to implement them in such a way that they -- in evaluating the -- they are meaningful to the actual performance, if you like, of a site, not because of some arbitrary words, if you like, in the guidelines. So, what we're trying to work towards in our assessment of suitability is a methodology that is truly meaningful in a -- sense as the overall performance of the site. And, that's what we're struggling to do with the Department. And, I just want to make that very clear.

The way I hear the State presenting it is I think they're saying forget about the performance assessments, just get a characteristic. What I've heard the last two days from the board is do some performance assessments. What do these things mean to the overall performance of site? And, I think that is the approach we are trying to take.

Also, we do intend to evaluate site suitability. It is the responsibility of the implementing agency, DOE, to come up with the method of doing this and we are working on it. And, finally, the guidelines were very clear to be used to identify the potential acceptable site which was done, to nominate and recommend sites to be characterized which was done, the EA's, and finally to recommend sites for repository development. That recommendation, that specific recommendation on our current schedule is in April of the year 2001.

MR. FRISHMAN: I'd only like to repeat one point that I made and that's that if -- I believe that I said I don't think that it's inappropriate to be looking at performance assessment at the same time, but I believe you must follow both tracks and, in fact, the data that you're going to have to collect for performance assessment is the very same data which you need as a method to keep analyzing that data for responsiveness to the disqualifiers, as well as keep shoving it in the box where eventually the big wheel goes round and round for performance assessment. See if we can possibly come to an answer before the three billionth dollar is spent if, in fact, the fundamental data and characteristics of the site aren't going to work anyway. And, I do have to admit and I know that many of you will be surprised to hear it, I was fascinated by some of the work that I heard that has been going on and what I heard over the last couple of days. And, the heat issue is a very important one and I think your approaches to looking at it, at least based on my technical knowledge, appear to be logical and sound.

DR. NORTH: I guess I'm not completely convinced that you're diametrically opposed. I go back into my book and I look at Slide 12 in Mike Voegele's presentation which talks about performance objectives and lists the thousand year pre-waste emplacement travel time rather clearly as one of the objectives relating to a particular barrier. And that, I understand, to be the disqualifying condition.

MR. FRISHMAN: Well, he was speaking there in the context of 10 CFR 60 which permits that number to be some other number based on assertions on performance that the NRC is willing to accept. The disqualifier is without the caveat and Mike and his paper. Everything that you heard in the last day and a half or two days has been directed towards compliance with 10 CFR 60, not 960. And, I think if I was wrong, we'd hear that right now.

DR. NORTH: Are there comments you would like to make as to the Department of Energy feels about the degree of bindingness of 960 versus 60? I think from the board's perspective, we'd be interested in seeing more information on the travel time issue. Just exactly what the rule is for what's acceptable and what's not is a legal issue that I'm not sure is really within our charter, but we'd love to have it illuminated.

MR. FRISHMAN: Well, my point is that the guidelines pursuant to the Act are intended to serve both as a relatively quick and easy screening device, as well as one that sets up a license application if such an application is appropriate. And, the quick and easy screening is the part that has been set aside and has been rather deliberately set aside throughout. We were not satisfied at the time of the guidelines' promulgation that the disqualifiers were adequate. We were not -- I, for one, in the program that I was running was maybe the harshest critic of the organization of the guidelines because I felt that it was not serving the purpose. Or that the guidelines, as they were developed, were not serving the purpose. But, that's behind us. But, we're also litigating that, too. It just takes a long time.

MR. NORTH: From the point of view of the performance analysis, I have strongly advocated as I believe that others on the board have, as well -- you know, you can read our report on Thursday when it comes out -- that it is important to get going on some of these performance assessment issues and the question of the travel time compared to 1,000 years and the questions of human intrusion would seem to be good candidates to be high on the list. So, I would be very discouraged if I thought the DOE's program were they're going to wait until the license application before all that goes together. I think to the contrary, one ought to start exploring those issues right now and get on with it. But, on the other hand, just exactly what is the rule which triggers disqualification is an issue that I would prefer not to comment on. I'll leave it to the lawyers.

DR. BROCOUM: No, and that's right. You're a technical review board. My only point earlier is the guidelines generally don't say much what happens between the time that you nominate the site to be characterized and you make the recommendation to the president. They were nominated in '86, they were applied in 1986, they are silent until, based on the current schedule, your 2001. That doesn't mean we're not going to look at factors that are important to site suitability. We made that commitment and we've told you the

Secretary has made that commitment in the way that is meaningful to the site, for that specific site. That's the only point I'm trying to make.

DR. NORTH: So, we looked at these issues back in 1986. We didn't find a disqualification or, rather, you didn't and maybe there's new information and we've suggested that the state of Nevada might want to tell us in more detail what they think that information is, their interpretation of it, or information that could be readily obtained quickly.

MR. FRISHMAN: And, we have material prepared on that that we can present to you at any time you'd like because we've already delivered it to the Department of Energy.

MR. BROCOUM: Which comments are you talking about?

MR. FRISHMAN: About site characterization plan comments and also the attachment to the Governor's letter to the Secretary of November 14, '89.

MR. BROCOUM: Does the board have that letter?

MR. FRISHMAN: We have an answer to it.

DR. NORTH: Do we have any further comments or questions on this issue?

(Pause.)

DR. DEERE: Well, I think you know that one of the board's strong recommendations in the very early days and you heard it repeated several times including today and yesterday is that we think we have to see the real geological weaknesses in an underground environment before we think site suitability can be determined to be adequate. And, it doesn't mean that there might not be something else beforehand, but it appears to us that until we cross several of the structures and investigate in greater detail, greater areal and spatial coverage of the area, you're not going to be able to take into account the features. Now, once the features are determined, you can see what they look like. Then, you have to see whether or not these are something that are going to be able to be dealt with or not. So, we are very anxious to see the underground work get started and we have always been a pusher of that. It doesn't mean that we feel that site suitability will be determined because you go underground. We think that it could be raising features that have characteristics that are going to be very difficult to handle and we feel the program should find out about those just as soon as possible and study their effect on the program. And, it would be by a combination of judgment and performance assessment.

MR. FRISHMAN: Right, and we would not disagree with that if it were clear today that there were not characteristics of the site that would disqualify it. If it were -- yeah, if the site were from our perspective a -- and, I know this is language that anybody can take to mean anything they want. If it were a much better geologic site and did not have characteristics that bring to mind, among people who have not even been following this program, characteristics that make them wonder why this site is being looked at because in its unstable, tectonically active, so on condition plus the mineral district that it's in, if we weren't burdened with those issues that might, in fact, make the site never licensable or disqualify it under the guidelines, then we agree; get after it. But, the fact is there are some big screening type questions out there that have never been satisfactorily examined or answered by the Department. And, the point that I'm making today is they don't have a program to do that.

DR. BROCOUM: I guess, we would say we disagree that we don't have a program to look at site suitability, say. We have this whole site characterization plan. We are doing our prioritization which in power with it we are evaluating the methodology to come up with specific methodology specific at the site. So, basically, we disagree.

I just want to say one more thing about a comment that Dr. Deere made on getting underground. At the same meeting that was quoted in the State's presentation on March 1, Senator Johnston asked if we could accelerate the schedule for getting underground which is currently to start to the actual shaft or ramp construction in 11-92. And, the reply is that we would look at trying to accelerate six to nine months. We have done some preliminary work, and based on our preliminary work, we think we may be able to accelerate it about eight months. So, instead of starting 11-92, we would be starting actual shaft or ramp construction, not just site prep, on or about February of '92, which means starting site prep around June '91 which would mean that we need to have permits to get on the site prior to that.

DR. DEERE: Well, I appreciate those comments. But, part of the driving force between our wanting to get underground, if there is a real disqualifier, we have felt that it may not be known until you get underground to see the conditions. And, we would like this to be as early as possible because I think everybody doesn't want to pursue something that has a real defect. It's not only to get underground to continue with the work. It's to get underground -- if there's something to disqualify, let's find out fast; if not, let's continue with the work. And, I understand your disagreement because you feel there are other things available and could be analyzed now that would be disqualifying.

MR. FRISHMAN: Correct.

DR. DEERE: Yes. I think we understand very well your position.

DR. NORTH: Yeah, and we'd welcome the opportunity to see more details of your position of how this information available since 1986 really changes the situation from DOE's analysis in the environmental assessment of 1986.

MR. FRISHMAN: We'll work with your staff on a schedule for that.

DR. NORTH: Do we have any further comments or business before we declare ourselves adjourned?

(No response.)

DR. NORTH: I think at this point then, we want to thank everybody for their participation and declare ourselves adjourned.

(Whereupon, at 4:30 p.m., the meeting was adjourned.)