NUCLEAR WASTE TECHNICAL REVIEW BOARD

1995 SPRING BOARD MEETING

THE EMERGING WASTE ISOLATION STRATEGY
THERMAL MANAGEMENT STRATEGY
ENGINEERED BARRIER SYSTEM DESIGN & RESEARCH

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PROCEEDINGS

DR. CANTLON: All right. If you'll take your seats, we'll get the session underway.

For any new arrivals, this is the second day of the Board's spring meeting. We are going to pursue today the examination of the engineered barriers, progress that DOE is making in that area. And chairing that session will be Dr. Donald Langmuir. Don?

DR. LANGMUIR: Please take your seats back there so we can start the meeting.

I'm Don Langmuir, Professor Emeritus of Geochemistry at the Colorado School of Mines. Since the Board has only six full members at this time, while it has seven panels, I find myself chairing two panels. They are the Panel of Hydrogeology and Geochemistry and the Panel on Engineered Barrier Systems, or EBS. Today I will be presiding in the latter capacity; that is, as chair of the EBS Panel.

First, I'd like to thank Carl Di Bella for the effort he has put into this meeting in organizing it and piecing it all together and helping us all with it. I have one overhead to go in my opening remarks.

This is from a presentation by DOE to the NRC Advisory Committee on Nuclear Waste about a month ago. We also saw it as a Board in Beatty, in the presentation there. It was presented in this same form by Steve Brocoum yesterday.

An earlier version of this, the one in Beatty, which we saw, showed the waste package perched on a concrete base. At that time, I mentioned something about my driveway in Denver only lasting 10 years. You'll notice that it isn't identified. The material is not specified here.

The figures shows that the top-level strategy for waste isolation has five parts. First, as you can see, the low ambient flux and saturation. Second, a robust waste package. Third, limited mobilization of radionuclides. Fourth, a robust engineered system and possible diffusion barriers. And fifth, slow migration through the geosphere.

Of the five parts, at least three of them, that is numbers two, robust waste package, and three, limited mobilization, and four, robust engineered system and perhaps diffusion barriers, are exclusive functions of the engineered barrier system, or nearly so. And one of the five parts, that is number one, low ambient flux and saturation, will significantly affect how the EBS performs its role.

Clearly, the EBS has a very important place in the current DOE waste isolation strategy. It is three-and-a-half of the five parts of that strategy. This strikes me as a much different strategy than the one in place when I joined the Board six years ago.

Because of the importance of the EBS in the overall DOE strategy, we've decided to devote a full day to discussing it in its broadest context. We'll start with the concept of repository operations. This is terribly important because the way the repository is designed to operate affects the design of the waste package and all engineered barrier system components. Similarly, design of the waste package and engineered barrier system affects the concept of repository operations. These are all areas that must be tightly integrated.

Along the same lines, we will hear about the interface of the multi-purpose canister, or MPC, with the repository, and then about waste package design. Any differences between the multi-purpose canister and the waste package should be evident by the end of these two talks, as well as the reasons for those differences.

As we stated in our just-released 11th Report, the results from the total system performance assessment exercises done in '93 illustrated the very important role that the EBS can play in repository safety over thousands of years. However, many aspects of the engineered barrier system were omitted from the TSPA-93. Today we will hear about plans for addressing engineered barrier system components in the next iteration of total system performance assessment, TSPA-95. We will also hear about ongoing studies, or plans for studies, of backfill and packing. I am personally very interested in how capillary barriers are being or will be handled in these ongoing or planned studies.

Although the use of rudimentary mechanistic corrosion models in the TSPA-93 was a welcome first step, such models clearly must be refined, using long term corrosion data, to become acceptable bases for predicting repository performance. Today we'll be hearing about corrosion modeling, how it will be handled in TSPA-95. We'll also get an update on the important corrosion research program and the effects of human materials, whether engineered or inadvertent, on the repository.

This afternoon we will look at the issues of criticality control in a repository. Although the Engineered Barrier System Panel addressed this issue more than a year ago at a public meeting in Pleasanton, this is the first time we will be addressing the issue as a full Board. It is also the first meeting of the Board since the controversial hypotheses of two Los Alamos scientists about what is called autocatalytic criticality were aired by the New York Times last month. We look forward to DOE's comments on hypotheses and hope that DOE will tell us what it plans to do about them.

Our first speaker is Kal Bhattacharyya--I'm sure I didn't do that right, Kal; we worked on it--who will discuss the concept of repository operations. Besides being a prelude to today's other talks, Kal's talk will also be a preview in a way to our Salt Lake City meeting coming in July. In the July meeting, a major topic will be repository advanced conceptual design, including issues such as retrievability, ventilation, heat transfer, and so forth.

So welcome, Kal. We look forward to your talk.

DR. BHATTACHARYYA: Good morning. This morning I'm going to address the concept of repository operation primarily for the subsurface areas. As Dr. Langmuir said, that we will have another session in Salt Lake where we can address the surface areas and everything else.

The topics I'd like to cover this morning are primarily the ones that are asked by the Board, current concept of operations and its compatibility with primarily three things: the waste isolation and thermal management strategies, specifically retrieval and the reasonably available technology issue, which is both a site suitability issue, as well as a licensing issue. I would also like to discuss the ventilation aspects in preclosure period of time, alternative concepts considered, and primarily in the maintenance of emplacement drift. And we'll go over and summarize what we have talked about.

These are a listing of a few of the key assumptions that commence our concept of operation, and I found that most of them are listed, also, in the Board's 11th Report as a concern that they have. So it was kind of coincidental.

The first one is, of course, you're talking about integrated rail transport system. This is almost a done deal I'd say at this time, and we'll discuss it a little bit further. We also talked about emplacement of large waste packages in-drift, although the variation of how we can emplace in-drift is still being considered. And this third assumption is that the waste package will not be shielded to personnel limits. It means that as we receive the waste packages for emplacement, they are not shielded to personnel limits. They are still very, very active. And we'll discuss this as we go along.

A few more, just to set the stage for our concepts for operation. Remote handling and robotics will be used to handle these largely unshielded waste packages to achieve this ALARA as required by 10 CFR 60. This is, again, the same concept of ALARA, no human entry will be allowed into the emplacement drift while the waste packages are present. This could be modified in an abnormal situation. We are talking about primarily during emplacement.

This is a 10 CFR 60 requirement, retrievability. We have extended to 100 years, and backfill options will be maintained.

So these are some of the key assumptions to govern our conservative operation, as a matter of fact, and I'm sure the Board will have some questions about them as they're raised in the 11th report.

Let me just put this on for a minute here.

This is just to keep you focused on the overall repository concept, and I'll take a couple of minutes to show you how a waste package gets here, gets down from surface facilities, down this ramp and into one of these emplacement drifts.

This is a very simplified depiction of a waste handling building, just one part of it. I'm sure we'll have some opportunity to discuss that in a later session. What it shows is that within the waste package handling building at the surface, somewhere near the north portal, the surface facilities will put the loaded waste package on a cart in this concept within the shielded area, and then will put it within a transport cask, and then and then only hand it to subsurface. That means all of this is going to be done within the waste handling building. And that transport cask is what is going to shield us from--or shield the personnel for transportation.

So that's why we figured out we're right over there at this point.

And we have seen these cartoons in the past. This is--then this depicts the area. This depicts the transport cask, which is shielded, and it is brought to this larger drift, which is going to be that particular drift, and the smaller drift is that emplacement drift.

We bring it over here, with a turntable in this case, turn it around 90 degrees, and then by some internal mechanism, push that waste package out. This is unshielded now. This is a shielding door that has to open.

Then we'll simply push that out, close this door, and turn this transport cask around, and it will go back for a second waste package to the surface.

This here, too, has been shown before. This simply shows a locomotive will come--a remote handling--a remotely operated locomotive lets us out with one of these waste package carts, and then places it at a predetermined distance for each of the waste packages into this emplacement drift, which is one of these emplacement drifts. This is a little smaller one, about five meter diameter at this time.

The primary purpose, I believe, of the talk was how our design and our concept of operation is handling or considering, as Dr. Brocoum said, emerging thermal strategy. I think what we are doing from a designer's perspective is maintaining a developing a number of metallurgy concept, which will keep us flexible, though we overuse the word flexible, to go from say a lower thermal load eventually to a higher thermal load, or change the thermal load as we go along.

And these are a few of the things that I have listed that the designers are looking at, which will allow us either to buy some time in this--making this decision, and when we do make the decision, allow us to achieve the decision. You know, a simple example would be to go from a low thermal loading to a higher thermal loading.

And I'll discuss with you each of these, with the exception of strategy for emplacement. There's no point in going through this list. Let me just go to the next one and take one at a time.

These are handouts that I'm not going to discuss. I should have pulled it out. There's nothing wrong with the chart itself, but it is not--that's the one about the oldest fuel first and so forth. That's beyond the purview of the positive designers, we'll just skip that.

Now, this chart talks about that we will--right now, currently, we are talking about emplacing 70,000 MTU within a period of 23 years. That's our current waste received rate, as a matter of fact.

What we can do--first I want to show you what that means in terms of decision time and so forth, and then tell you maybe something we can consider to buy us some flexibility, as a matter of fact. It's not--it's a simple concept, but it is not quite evident from talking to people.

This is a picture of emplacement. If you were emplacing at 80 MTU/per acre at the full receipt rate of 3400 MTU per year, that's the current receipt rate.

And this shows that in the year one, you'll have utilized only this much amount in area. Year two would be that one, three, four, five, six, seven, eight and nine. And on the 24th year, you would have completed a repository, if you are emplacing at 80 MTU/per acre, at 3400 MTU peak emplacement rate, as a matter of fact.

What I would like to do is just hold your attention for a second to around this 11th year. At this rate, at this MTU, you have only come to only about less than half of the repository on the 11th year.

And if I were to put this picture over here for a moment and show you this colorful picture here, we are looking at a repository. The important thing to remember is still it's the 3,400 MTU per year as the emplacement rate we are maintaining. We are only emplacing a 25 MTU per acre in this case, which is kind of the law limit. We are eating up the real estate very quickly, obviously.

So by the 11th year, we were only about halfway into the repository here, and we very simply used up the entire primary area, the upper and lower block.

So there is a limit to the flexibility. And I mean if you are making a decision early on, you don't have any problem. But if you say I'm going to make the decision 10 years later, you'd be--you'll have a problem. So that's the idea of bringing these charts up.

Now, let me show you what we can do to buy us some time in the flexibility, and this I will draw attention to Dr. Di Bella. He said this chart was too complicated. I may have to have here a separate discussion about it. But let me try to explain this.

What it shows is it kind of tries to depict when you're going to run into risks of the construction process. The cost, of course, of design is there, but design costs is much, much smaller than construction costs. If you make a decision late, then you might run into some construction costs.

This first line, where it says 2008 development at year end, that's where you would be at that date that we have been talking about where we are supposed to make a thermal decision, as a matter of fact. And if we are to make that decision on 2008, there is no risk involved, meaning that we have only at that time to develop the perimeter drifts, put the shafts on and those two drifts--I mean, one drift is already there, and the second shaft's made in this perimeter of drifts, and we have started developing only about three drifts. We have not really lost anything, as a matter of fact.

What this is showing is that these drifts have a 22 1/2 meter center to center, which will allow us to go and emplace the waste at 100 MTU per acre. That's approximately upper limit we are looking at.

DR. CANTLON: Could you say that distance again? What was the distance?

DR. BHATTACHARYYA: Twenty-two and a half--

DR. CANTLON: Twenty-two and a half, thank you.

DR. BHATTACHARYYA: --center to center.

Now initially, if you recall, our initial receipt rate is fairly low. We start out at 300 MTU per year, and then 600 and then 1,200 and 2,000 and so forth. So initially, your impact is not that high. You can emplace the waste as you receive them. Also, the thing to remember is by the time you have started 2010, we have developed about eight drifts. So we'll be up approximately about here. So we have a few drifts ahead of us already. And we start emplacing them. Somewhere around 2013, you will not be able to emplace it as you receive them at 25 MTU per acre, the reason being that you are--at this point, you are developing four drifts and you are emplacing only one drift and then keeping three for future expansion, as a matter of fact.

So we are saying that if you use two TBMs and you're developing all these drifts and only using one other

--for example, then you wouldn't be able to keep up with the pace of receipt rate.

So at that one point, then, you will not be able to--it would be at that time you would only be able to maintain about an 850 MTU per year rate, as a matter of fact. All you have to increase is the number of TBM operations.

This second half shows arbitrarily that at that point if you said I'm going to go to 50 MTU, then I have now increased my spacing at 45 meters from center to center, and I can now emplace 1,700 MTU per year, as a matter of fact, because now I am excavating two drifts and emplacing only one drift.

The whole point of that is if at this say 2010, 2020 decide that I'm going to go for a high thermal load, then you can then start putting waste in the three empty drifts out of every four that you have

If you at this point decide that I want to go to high thermal load, you can take all these waste packages out of here and start putting them back here, and you can start developing for say a year or so and starting putting them in.

So there are options that develop as you go along, but there is some penalty to pay, as a matter of fact. I hope that is clear to a degree.

This is a fairly simple concept. We are looking at--as I discussed a minute ago, we are looking at excavating more drifts than necessary maybe initially, and then which will allow us to--I'll show you a picture in a second.

The first thing to remember is many combinations of waste package and drift spacing can lead to same thermal loading, obviously. And there is something to also remember, that various arrangements will lead to different thermal near-term regimes. And let me show you what I mean by these two things in this next picture.

These two pictures depict the same thermal loading, 25 MTU per acre. In the upper picture, we are loading every alternate drift and keeping this waste package spacing at 31 plus meters, as a matter of fact, so that from 25, if I were to put in between--and on the same spacing here, from 25 I'll go to 50 MTU, and if I put one waste package in between each of these, then I'll go to 100 MTU, obviously.

Same thermal loading is achieved by these. I have four drifts, and I'm loading only one of them. But in this case, I'm loading them as twice--at twice density as in the top one. This one drift alone will give me 25 MTU, 50, 75 and 100, so if I fill them up, I'll have 100 MTUs. We can do that initially, as I showed you in the earlier picture.

Obviously, the thermal regime is going to be different when we have them in kind of a square pattern or we have a localized disturbance pattern. That needs to be a consideration that post-closure folks hand to us. What I'm trying to tell you is that what we can do to achieve either of these strategies.

Now, this is some--it's fairly obvious that the heat will be, you know, shedding quicker towards the edge because of the heat sink. And what this shows is that we can take the repository, for example, and then pack the edges in a denser manner and get more MTUs in and have the same end thermal effect. And let me show you a picture of that.

Supposing--these are purely notional. Supposing our spacing was 20 meters for a given thermal effect at the end of the preclosure period, but by--but we would be allowed to pack it--let me just go back here.

Here's the repository. This little box shows this area. This is the perimeter drift, that's the perimeter drift, and these are the emplacement drifts, and that's the edge of the repository. If we pack the edges at a higher density, then we will get more waste in, but overall thermal effect should remain the same because of the heat sink effect. And that's something again that can be used to put more MTUs in the repository.

This is another concept that we are keeping options open, repositioning of waste packages. Someone mentioned this as re-racking. It's the same thing.

This can be used to adjust thermal loading before closure any time, really throughout the preclosure period of time, or if you have any localized thermal perturbance, say if you have a young fuel package that's very hot initially, you can keep the package or the packages cooler. Again, let me show you how we intend to achieve that.

This is a concept of--as you know, we have seen the cart-mounted concept. This is a concept where we're looking at where the waste package is sitting on a pedestal, I believe it is called. There's a continuous base, if you will, for the waste package.

This is a gantry concept. It's hard to see on this thing. It's probably easier to see here. This is the waste package, that's the gantry, and this is the mechanism--this is the mechanism that just holds the waste package, picks it up a little bit. We have done some preliminary calculations to show that this, indeed, can pick up the waste package by this rim.

What this can do, of course, is you can see that this gantry can clear emplaced waste packages. So you follow one back to one of these earlier pictures. And if I wanted to take this waste package and move it closer, take this package and move it closer and so forth to get a denser packing at this time, say it was the closure time, then we could use this mechanism for doing that because it will clear this waste package, and it can go to any other waste packages and pick it up and move it over, as a matter of fact.

This could not be done in the cart concept, where you would have to take every one of them out and re-rack them at this point. So this looks fairly promising, as a matter of fact.

This was another question that was asked by the Board to address, compatibility with reasonably available technology. As I said, this is a suitability issue, as well as, of course, during license application time. We have to prove to the NRC that we can do these things at the current level of technology, which would be about 2001 technology.

A couple or three areas I have listed where we feel reasonably comfortable that we are in the reasonably available technology area. One is excavation by TBM and mechanical excavation of shafts, also. That's another basic requirement we have imposed on ourselves, that we are not going to drill and blast the shaft.

Transportation of waste package using rail system. Heavy loads are moved all over the world, and the remote handling of most of the rail system is fairly current technology. So that's probably within the realm of current technology.

And the third one is kind of an extension of the second one, that we could use a cart or a gantry system. Gantry systems are used everywhere in the world to move very heavy loads, and we are talking to gantry manufacturers.

They seem to be fairly comfortable in the direction they're going.

So these, although they're not certainly by any means resolved, we are in the conceptual design stage, but I think we are in a comfortable situation.

To illustrate one of these reasonably available technology, we think this is a concept we have developed with TBM manufacturers for launching a smaller TBM with a five meter TBM out of, I believe, a nine meter drift, launch drift.

What we are looking at is this is the emplacement drift which are these, and that's the TBM launch drift. That's about a nine-meter drift.

And you have seen this picture, also, in the past. This is a launch tube that is wedged or put together firmly in this thing, and then the TBM can use that as a--again, this is not built yet, but it doesn't look like it's a very exotic technology, as a matter of fact.

There's a long list of things that we have not come to a closure on on currently available technology. Emplacement drift maintainability for 100 years, we don't know what material we can use that will last for 100 years, but we have to figure out exactly what the emplacement environment is it going to be. If it is going to be dry and so forth, ventilated, maybe we can do that easily.

Retrieval equipment, we have not really started looking at it yet.

Cooling during retrieval, we have looked at the ventilation part of it, and I think we are presumably comfortable, but there's another part when we cool this hot repository or hot drifts that there could be some stability problem because of rapid cooling. We haven't looked at it.

Accident events, since we have not really developed that concept, we have not started looking at accident events. We most certainly will have to.

Backfill system, again, was discussed yesterday quite a bit. We have not figured out how to do that yet.

Remote handling, we have just started looking at it. It looks promising. And monitoring, again, we don't know the environment, what we are going to monitor yet. So this is going to be something that we have to develop within a year or so.

Another question that I was asked to address was this compatibility of current concepts with retrieval. First of all, of course, it's a requirement, 10 CFR 60 requirement. We shall keep the retrieval option open. So if your design does not meet that requirement, then the design is unacceptable. So that's just a bullet to remind us that that is a requirement. We have an extended requirement to 100 years, making it a little bit harder.

A couple of things we can look at to see how retrievability potentially should be enhanced. We have an emplacement method if you think back in the CBCDR design, that when you have 35,000 waste packages sitting in boreholes that are 50 feet deep, and you have to dig each of them out, I think that's--that boggles my mind, at least. And when you compare to that one waste package is sitting in some sort of a pedestal or a rail cart, I think intuitively it's an easier--it should be an easier operation.

We have made these drifts, meaning basically it's virtually zero grade. Basically, the drifts used to be over 5 per cent and so forth. They are dead straight, so the safety of moving on them should be easier, as a matter of fact. You can sit here and look at them a bit.

Emplacement drifts are oriented favorably. You know, the particular joint system is not solved. We have put the emplacement drift east-west. Hopefully, that's what we find out from ESF. So they should be inherently stable from the geotechnical point of view.

If we do backfill, we'll not backfill before NRC has given us permission to close it. So that means we are not going to take the backfill out and then retrieve it.

And these--these access drift ramps, et cetera, you know, these ramps, shafts, access drifts, are sufficiently set back from the repository emplacement area over here, so that they should not get hot or have any access problem. We have to remember that retrievability is kind of a planned off-normal scenario. We are given six years or so to get ready for that, another 23, 24 years to do--you know, to actually achieve that. So It's not really an emergency type of situation.

So I think having these accesses already stable and maintained and have a kind of an inherent stable type of drift, and we are in the right direction. But we are nowhere near proving that yet.

This is a subject all by itself, and I'll have to really kind of gloss over it, and I hope to talk about it in Salt Lake City in great length. We have done some good work in this area, I believe.

Use of ventilation during preclosure period of time.

First thing, obvious, I have not put that in, but I should probably done that. We need ventilation no matter what. If not for thermal management, you could not run a mine or underground repository without ventilation. You have to have two sets of ventilation for emplacement in development. So that's a given.

Then to maintain our flexibility or meet thermal strategies, we can remove heat and moisture. We can maintain this drift at a given temperature, if you wish, and then again, smooth out a hot spot if you have young packages sitting there, and let me address them quickly.

This is a schematics of the ventilation scheme. We would have that exact scheme, whether we used it for thermal management or not. That's how you have to ventilate the repository during emplacement. This will look exactly like that. The only thing it shows is this shows an airflow diagram for continuous emplacement. For cooling, this shows that we have emplaced the upper block, and most of the lower block, we are basically coming to the end of the repository. That line divides the emplacement side from the development side, and this shows that each of these emplacement drifts ease open allowing, and we have the reversal along it, allowing this given amount of air passing through to maintain a given temperatures, as a matter of fact. Same thing over here.

If we decided not to do that, we'd simply put stops all along that, and it will eventually be exactly the same way.

We have done some work on the amount of air necessary to do this. This is--I should have probably put some more labels on it. This is for 100 MTU per acre repository. It's one of these drifts that's about 1,200 meters long. And all it shows is that if I had a repository that I'm loading at 100 MTU and the given fuel is 40 gigawatt days, 26.3 year old, et cetera, et cetera, if I were to--if I'm going to try to keep this at 50 degrees Centigrade--if I wanted to keep this water temperature at 50 degrees at all times, I'd have to push about 25 cubic meters of air per second to the drift. Whereas if I put in filling, it would draw 10 per cent of the moisture, meaning increase my relative humidity by 10 percent at exit time, my added requirement to achieve that same temperature would drop by almost half, as a matter of fact.

So what it shows and has been shown before, also, in other meetings is that the moisture draw has a very dramatic effect on air--amount of air requirement, as a matter of fact. Just 10 per cent increase will reduce the 47 per cent reduction in air necessity and so forth.

The bottom one simply shows you a little bit about how much water you really have to withdraw if you are doing that for each of these. For example, if you are keeping at 50 degrees Centigrade, for this scenario 100 MTU, then you have to withdraw 1,200 liters per minute.

I think that our report says you'll have to have a 15 meter deep pool covering the entire area over the life of the repository. So that's a substantial amount of water.

What we have not done is make an assumption at this time before the tests are done about how much water we can actually withdraw from this.

This simply gives you an idea of maintaining a drift at 90 degrees, again. It's based on 100 MTU per acre. We start at the repository--there are air quantity people who feel more comfortable at 272,000 cubic feet per minute. And for maintaining this continuous ventilation, you can go up to 2.4 million cubic feet at the height of--at the end of the repository when you are ventilating this entire repository to maintain it at 90 degrees Centigrade.

You'll have to at a given point, around 2015 year to put two extra shafts. Normally, you have two shafts, one ramp and one shaft. You have to put two shafts to handle because the capacity of this initial ventilation system is about 300 cubic meters. But that's basically what we would do, is add two more shafts somewhere during the life of the repository.

Briefly going over this alternative concept operation considered to date was another topic that was asked for us to address.

I believe that the in-drift emplacement is the way to go. We have basically looked at vertical emplacement of these large waste packages, and they don't make any sense thermally, and they also don't make much sense mechanically to handle this thing.

Transportation system is another one that we feel comfortable, but we probably--we are going to go to a rail system. We have--in the past I have shown these trucks and crawler mounted transporters, but now that the entire repository is less than 3 per cent rail system--we are looking at various--we are looking at these tunnel boring machines. How they excavate. A TBM is rather inflexible. There are only two, three ways you can do this thing. But TBM is the preferred excavation--or excavator, I should say, but still we are looking at how to ease that TBM in the best way. There aren't very many things that have come to a closure, is what I'm trying to say.

This is an old subject basically I've touched upon in the past. How are you going to maintain this drift for 100 years was a question that's raised, and we really don't have an answer yet. Again, as I said, that we have tried to make these drifts inherently stable by orienting them in the right direction, we hope.

It is a very low obstruction ratio, so there is no pillar stability problem. We're looking at it during 1995, repository subsurface design--and in the question of monitoring, there was a systems study done briefly a couple of years ago. And they looked at the solubility of instruments in this atmosphere. I believe 160 degrees Centigrade was considered to be upper limit where instruments survive. We have not started working on that yet at this time.

To summarize, that basically what we are saying is that all our design and our effort and concepts of operation, therefore, are primarily to maintain options so that we don't lock into any kind of thermal strategy or waste isolation strategies. We have a lot of engineering options to meet these requirements throughout--not only now, but even throughout the life of the repository, the preclosure life of the repository.

Some of the reasonably available technology is--we feel comfortable about. Some of them we have to--we have a long way to go.

Alternatives are being evaluated for all major design features. That's a requirement by 10 CFR 60. Even if we have , for example, decided upon in-drift emplacement, we are looking at eight different ways of emplacing in-drift to make sure that we have come up with the one that gives us the most safety and waste isolation possibility.

That's basically what I have. I'll try to answer any questions you have.

DR. LANGMUIR: Thank you, Kal. We have time for several questions.

Let me ask one to start. Early, in the third overhead I think, Kal, you mentioned that no human entry would be allowed in the emplacement drifts--

DR. BHATTACHARYYA: Right.

DR. LANGMUIR: --while waste packages were present. How then are you going to monitor performance of the waste packages if people aren't going to be allowed to examine them?

DR. BHATTACHARYYA: We are assuming that the monitoring instrument will be--and you can visualize this, I have no solid--just simply a thought process--that we have radiation dose, and you can actually through some cable system and so forth, can have instruments go and come back in if you don't leave them in this heat and moisture and whatever it is, and if that's going to be that moisture. If you are ventilating it, it would probably be fairly easy.

But this has to be some sort of a cable system which allows the instruments to go, scan and come back or stay there, take the temperature, radiation, and come back. We are not going to--at least we are not visualizing people going in there and submitting them.

DR. LANGMUIR: Let me ask a follow-up to my own question. This leads me to another issue, which apparently you haven't gotten to yet, and that is the performance of wheels and mechanical parts under the high temperatures you're dealing with. This will affect the--certainly retrievability issue, which you haven't yet had a chance to examine.

DR. BHATTACHARYYA: That's right.

DR. LANGMUIR: But certainly, also, the monitoring process that will involve mechanical devices.

DR. BHATTACHARYYA: Yeah, we have--

DR. LANGMUIR: So I guess is there any experience of that sort of thing in the industry?

DR. BHATTACHARYYA: Yeah. Yeah, we have looked at, briefly, for example, the wheel bearing, as we said. I believe it's the Coors Beer industry, for example, but people use ceramic bearings, and they are very good for very high temperature.

One thing to remember is that these axles are not flexible. We're going to them rigid because they are absolutely dead straight. So we are not steering them, as a matter of fact. So there is no steering mechanism or anything like that. We can make them double flange so that they really sit tight on the rail, as a matter of fact.

And if you have this kind of a ceramic type of bearing, and we're using it only once. We are moving this whole thing only about at the most 1,200 meters and leaving it there. So there's not a lot of wear and tear, and hopefully, we never have to move them, but if we do, we are moving them back only 1,200 meters.

Depending on if we ventilate and so forth, then I don't really see any problem. We are talking about only a 90 degree or a 50 degree water release. Even at the highest temperature, I believe we are looking at 160 degrees C. So it does not look like--it doesn't look like an impossible task. We have indication that this is done and done fairly, you know--

DR. LANGMUIR: Clarence Allen?

DR. ALLEN: Clarence Allen, Board. Have you given planning thought to how you were going to tie these waste packages down to withstand earthquake accelerations of close to or more than 1g several times during the life of the repository, particularly during the time when retrievability may have to take place?

DR. BHATTACHARYYA: That's correct. Well, not a whole lot yet, but I think by easing this pedestal system, where it is sitting not on wheels, but directly on a pedestal, the chances of them moving around and rolling around is automatically reduced.

And you remember this picture that I showed on the--

DR. ALLEN: But we have seen accelerations close to and exceeding 1g several times in recent years.

DR. BHATTACHARYYA: Well, if we expect that in the first 100 years, then we'll have to--in this concept, our waste package is going to be sitting--you are right, two things could happen. One is that if we backfill, then they're probably being held fairly well, as a matter of fact.

b) By putting them directly on this pedestal, the chances of them moving around, they can fall off, of course, from the pedestal, as a matter of fact.

We are looking at the fact that some holding mechanism has to come down maybe in a robotics manner and hold these in where they are. We have not gotten there, as a matter of fact, but, you know, we are cognizant of the fact that, yes, this can move around, and we need to tie them down. But we cannot tie them down by, you know, human approach. We have to have a certain amount of robotics to lower some holding mechanism. But I don't have any idea what that looks like yet.

DR. LANGMUIR: Other Board questions? Dennis Price?

DR. PRICE: Okay. Can I ask you what your guess is on the underground transport cask with the MPC waste package in it, what the maximum weight is that you see involved there?

DR. BHATTACHARYYA: Of course, we have a very preliminary shielding requirement for the transport cask, and it shows somewhere around 40 to 60 tons. So when we add the 65 metric ton, 65,000 kilogram waste package, that's about 40 to 60 tons of shielding and transport; plus the cart, which weighs about five tons. Plus, you know, the locomotives--I mean not locomotives--the platform and the locomotive cart there. We are looking up to about 180 tons.

DR. PRICE: A hundred and--

DR. BHATTACHARYYA: 180.

DR. PRICE: Yes. If then there is a failure on this underground transport cask loaded, such that it doesn't respond to your requirements to tow it or push it or pull it because of this failure, it would seem that you have a large problem on your hands?

DR. BHATTACHARYYA: Yes. Well, one saving grace could be as long as the accident happens within the transport cask, the transport cask will allow us to approach it without equipment. The transport cask is shielded to human approach. So as long as the waste package--a lot of it is inside the waste package, and everything is fully loaded.

DR. PRICE: Or part way--part way out.

DR. BHATTACHARYYA: No, if it is part way out, we have another problem. We are looking at these air pallets that industry uses to move some incredible weights. I don't have the number in front of me, but maybe in the five, six hundred ton range easily.

So that's where industry is going to, for example, put something that is derailed, you know, into these air pallets. We are looking at the air pallets to recover from accident in that sense. So we're just beginning to look at it. But the technology looks very promising to handle these very large weights.

DR. PRICE: And what about quality control with respect to the waste package, dings and dents and other things that might occur in the process of handling?

DR. BHATTACHARYYA: Well, as I showed in the first slide, or one of the earlier slides, we believe we'll accept the waste package from the waste handling building in a clean, slight form. It's not going to be a leaky waste package, or anything like that, or have any kind of damage.

The waste package would be inside a transporter, with moving less than five kilometers per hour. Even if it's derailed, I don't have any calculation to show that, but, you know, intuitively, that the waste package probably will not get damaged to the point that it could not recover.

Hugh and his group have looked at rock faults and so forth on the waste packages and found that there's no plastic deformation on any of that so that it could be recovered, pulled back on the surface and put in the new--

DR. LANGMUIR: I'm sorry, I'm going to have to interrupt. We're right on schedule. There will be plenty of time in the discussion of the panel, I think, to pursue this further.

If I may, I'd like to take this to the next speaker, who is Richard Memory. The presentation is titled "Multi-Purpose Canister System Study, MPC Repository

Interface Issues."

MR. MEMORY: Good morning. I will talk to you about the repository interface issues associated with the MPC.

So first, I'll give you a quick overview of the MPC concept. Then I'll talk about some major MPC-MGDS interface issues or approaches, or at least highly visible interface issues, and then a chart on how we're working with the NRC to get some kind of indication of MPC compatibility with the requirements of 10 CFR 60, and then finally, a top-level schedule relating the MPC activities to the MGDS activities.

Now, what I want to talk about pulls information out of pretty much each of the presentations that you'll see today, and I'm doing that with the purpose of giving some background and feeling for what the issue is and then what our approaches are for addressing those issues.

So as part of the overview of the MPC concept, the fuel is put into a MPC at the utility, and if it needs to be stored, it's then emplaced in the storage unit. At the time of transportation, it's been moved from the storage unit. The sealed MPC is put into a transportation cask and transported to either the MRS or the repository. At the repository, the plan is to take the MPC out of the transportation cask and then put it into a disposal container, which will then become the waste package.

The MPC--I'll give you these numbers basically as the MPC conceptual design. They may very well change with the final design. But at the point of conceptual design, the MPC, the so-called large MPC, can contain either 21 PWR or 40 BWR. Its dimensions are roughly this: 4.9 meters in length, 1.5 meter diameter. The weight of the MPC empty is 18 metric tons and including the fuel, it becomes 34 metric tons for the large MPC; with the loaded weight for the small MPC is 23 tons, and it contains either 12 or 24 PWRs.

And when we integrate at the repository, the MPC with the waste package, or the disposal overpack, the dimensions--I want to point out you have a slightly different picture in your handouts, that we've given you an updated picture. This is the correct notional diagram of what an MPC being placed into a waste package would look like, or disposal container.

But nonetheless, then the large MPC waste package has a length of 5.7, approximately, meters, 1.8 meter diameter, and its total weight, as Kal mentioned before, would be 65 metric tons. The smaller package comes to a total weight of 48 metric tons, with the same length and a slightly smaller diameter.

The issues that I want to address today are the long-term criticality control, thermal loading, our approach for waste containment and the interface and repository operations.

Now, as a I say, each of these are going to be discussed later today, with the exception of thermal loading. It's already been discussed. So I'll just give you highlights, requirements associated with each of these issues and then some background, and then tell you what our approach is.

So this is just a statement of the 10 CFR 60 requirements that says we need to provide criticality control for all the systems, including isolation systems. Criticality is not allowed unless we get two unlikely, independent, concurrent changes to occur. And then finally, the k effective must be less than .95, once we account for bias and uncertainties.

That's simply a statement of the 10 CFR 60 requirements.

What I want to do in the next couple of charts, the next two charts, just indicate to you what value burnup credit has in terms of providing criticality control and what value neutron absorbers might provide in giving criticality control.

So this is a chart that I think has gotten quite a lot of mileage. What this shows is k effective versus time. This is for a 21 PWR design, and we're assuming that there are no neutron absorbers in this waste package, and the waste package is fully moderated, and that the basket geometry is constant throughout this time period.

Now, the point that I simply want to make out of this chart is that if you have zero burnup in the fuel, 3.75 initial enrichment, you'll get a 1.21, approximately, k effective, which stays constant, pretty much constant, for close to a million years.

If you take credit for say 37 gigawatt days for MTU burnup, the k effective, then, comes down to this curve here as a function of time.

So the point I'm trying to make is that there's a great deal of value to acquiring credit for the burnup that's occurred in the fuel.

This chart is to indicate the value of neutron absorbers. This is assuming a half-inch aluminum boron between each of the fuel assemblies for the basket material, and it's at five-year old fuel. We're showing k effective versus a percent of B-10 in the aluminum boron. And again, the only point that I want to make here is that with the addition of neutron absorbers, you can pull down this k effective fairly significantly.

So our approach to obtaining this criticality control is that we expect to rely on burnup credit and engineered neutron absorbers, and the analysis process that Hugh Benton is going to go into in fairly good detail is that we will perform both deterministic and probabilistic analyses, depending on the time period, in order to evaluate k effective.

There will be uncertainties for a certain time period as to what sort of credit the NRC may ultimately allow for burnup, and there are long-term basket material testing programs taking place. So there's a certain amount of uncertainty as to what material is appropriate for the basket.

So to work with those uncertainties, we have a set of contingencies developed, and that is that we'll allow for opening a portion of the MPCs as a planned off-normal event. This is not expected to be necessary for a terribly large portion of the fuel, but we need to plan for it at this point, but only as an off-normal event.

And we may open the MPCs to add moderator displacement in the form of filler material, if that's necessary. Disposable control rods are looking like a good option as well. Then there's always the option of potentially repackaging a small fraction of the MPCs.

Another contingency would be to allow for the insertion of the disposable control rods at the time of loading of the MPC at the utility. We could do some waste stream management by loading least reactive fuel into the MPCs first. And ultimately, as we get a better feel, or a better understanding of what the NRC will allow in terms of burnup credit and how the basket material may perform, we can go back and look at potentially MPC design modifications.

I'm moving to the thermal loading and how the MPC relates to thermal loading. The issue associated with the MPC and thermal loading is the power output that you get from the MPC. In other words, being a large package, has a large capacity, increases the power output of the package.

The MGDS-requirements document has identified some requirements to facilitate the design of the MPC and the repository. An assumption that the MGDS-requirements document makes is that the laded MPCs emplaced for disposal have a maximum thermal output of 13.2 kilowatts.

Given that assumption, then, the requirements document requires the MGDS to provide an emplacement environment such that a waste package with a 14.2 kilowatt power output will not result in MPC surface temperature higher than 225 degrees. So that's a requirement on the MGDS.

The requirement on the MPC is that given 14.2 kilowatt output of the package and a 225 degree surface temperature of the MPC, they're required to maintain the cladding temperature below 350 degrees.

So those are the stated requirements as they currently exist in the MGDS-requirements document.

Now, again, I just want to give a little background on some of the issues and things you can do with thermal loading. So I'm going to talk about temperature variation due to drift and package spacing; peak temperature sensitivity to the waste package power output versus repository location, meaning either the waste package surface on out to the mid drift; and then I'll show a chart on the impact of aging, how that might reduce temperatures.

I think the primary point to be made is that with a higher thermal loading, the MPC is easily compatible without taking any steps to make it compatible. As the thermal loading is reduced, depending on what the goals are, we may need to do something to make it compatible.

There's three points I want to make on this chart. First, is the, this is the time after emplacement versus temperature of the waste package surface. We have three thermal loadings, 100 MTU per acre, 83 MTU per acre, and then this family of curves is for 25 MTU per acre.

So the temperature--this is a temperature history. The temperatures of the waste package, then, varies over this time period. And you see the peak temperature for 100 MTU is up, in this case, to about 190 degrees C. By reducing the thermal loading, you get some reduction in the peak temperature. This is about 165. And then down to a lower thermal loading at 25 MTU, you can get this family of peak temperatures, and the way you do that is by increasing the waste package spacing and decreasing the drift spacing.

So this lowest curve is with a 32.2 meter waste package spacing and a 45 meter drift spacing, and that comes up to a peak temperature with this 21 PWR package at 10.22 kilowatt power output. That gets you a peak temperature-- well, this higher spacing gets you a peak temperature of around 148. And this waste package spacing here gets you a temperature of around 117 degrees.

So you can keep the same MTU and draw the waste package temperature spacings down by increasing the waste package spacing. This is one of the things that Kal was touching on in his presentation.

Now, if you go down to a smaller package, you can reduce these peak temperatures somewhat, down to about 100 degrees C for the largest spacing and still get the 25 MTU per acre.

So the three points to be made are that the density of the MTU per acre draws these near-field temperatures down. Varying the combination of waste package spacing and drift spacing can draw these peak temperatures down. And then also varying the power output from the package can draw these peak temperatures down.

The point that I wanted to make with this chart is to show the peak temperatures as a function of the location in the repository.

So we're showing two packages, a 21 PWR package and a 12 PWR package, with these sorts of power outputs. So the large package, waste package surface, has this temperature of about 117 degrees with this 32.2 meter waste package spacing. And the 12 PWR package, then, has a peak temperature of around 100 degrees.

But as you see as you move away from the waste package, the difference diminishes. You get this difference in the drift wall temperature. As you start getting into the mountain, one meter into the rock, three meters into the rock, the difference is small. And once you get out to the middle of the drift, you don't care what the power output of the package is anymore because it's looking more like a line source than a point source.

I do need to make the point that these peak temperatures are not occurring all at the same time. So this is not a plot of the temperature at any given moment at these locations. This is the peak temperature for each of these locations, and they occur at different points in time.

So another way of modifying the peak temperatures that occur in the near-field is to age the fuel somewhat. Now, this set of curves is generated on this set of conditions, and these set of conditions are slightly different from the curve that I've shown before.

But if you start with 22 year old fuel, you get a peak temperature of somewhere around 120, and then if you age the fuel to these levels, you can draw it down to where 100 year old fuel brings you up to about 70 degrees, 67 degrees, roughly. That's the value of aging. There's a cost that goes with that, but that is something that can be done.

So, and again, these options are things that Kal touched on, and I just wanted to quantify a little bit as to what their impacts are.

So if we have a high thermal loading, we really-- the accommodating MPC is not really much of a challenge. There's nothing really that needs to be done. Given that a low thermal loading may specify lower near-field temperatures, which really hasn't been determined yet, but it could be pulled down, so we don't want to see high near-field temperatures, there are options that are available to work with that. And this is basically just a laundry list of those sorts of things. And I think you were briefed on some of these things at the November Board meeting as well.

The next point of interface is the approach for waste containment, and this simply is a statement of the 10 CFR 60 requirements. It says that we need to provide substantially complete containment between 300 and 1,000 years. We need to provide controlled release not to exceed one part in 100,000 of the inventory present at 1,000 years.

And then finally, 10 CFR 60 invokes the EPA standards for radioactivity. And what we're doing now is currently working toward the total release, 10,000 year total release--cumulative release requirement that's associated with the old 40 CFR 191.

To meet those requirements, our current goal is to allow less than 1 percent of the waste packages to fail in the first 1,000 years with a mean waste package lifetime well in excess of 1,000 years.

DR. LANGMUIR: Excuse me, Rick, you've got about two minutes left.

MR. MEMORY: Okay, sorry.

Okay. Then our approach here, then, is to provide --allocate no quantitative performance to the MPC as a containment barrier, and that all the performance will be allocated in the waste package. We'll verify that there's not an adverse interaction between the MPC shell and the waste package. This approach avoids a cost risk of designing an expensive MPC now that may not be acceptable to the NRC at the time of MGDS licensing. But then we have the option of once we determine what materials are acceptable to the NRC, of possibly looking for a cost-effective design that does allocate some performance to the MPC.

In the operations area, let me just--the surface facilities' reaction to the MPC or interfaced with the MPC is to provide appropriately sized handling equipment for a larger waste form that's coming in, and then the potential waste package. And then they'll treat the MPC opening as a planned off-normal occurrence.

Subsurface operations, Kal went over these. These are driven by the large waste package, by a concept of a large waste package, which may or may not have occurred with or without an MPC.

Nevada transportation, this is driven by large transportation casks, which may or may not have occurred without an MPC, and that is that we'll develop--we have an option for developing a rail line between the nearest existing rail line and Yucca Mountain, and there is the option for heavy haul truck of these large transportation casks.

This is a chart showing the approach to getting an indication from NRC as to our compatibility to 10 CFR 60.

This is the main bullet here. We're submitting a design considerations technical report concurrently with the storage and transportation safety analysis reports next year to allow an integrated review by the NRC of the overall package. And we're pursuing an NRC letter of no objection before we began fabrication of the MPC.

And then finally, a schedule showing the top level milestones and then design milestones, some of the certification and licensing activities, criticality control interface issues, and then some high-level thermal loading interface activities.

So in light of the lack of time, I'll stop there.

DR. LANGMUIR: Thank you, Rick. We're a little beyond time. I'd like to proceed. I realize there's plenty of time during the day, so please hold your questions, make a note of them for the time in the afternoon later. During the panel period, you'll have an opportunity to bring up your questions.

Our next speaker is Hugh Benton. His title is Waste Package Design.

MR. BENTON: Good morning. I'm Hugh Benton with the M & O, and I will be discussing waste package design, but this will not be an overall view of the waste package design program, but rather we will try to address the particular topics that were considered of interest.

I'll talk about the waste package barriers and a general indication of their importance to radiological control and safety; look at the current designs, some preliminary costing data, a couple of examples of the performance analyses that we are running, some internal within the waste package heat transfer calculations, what our shielding operations are, and finally, some engineering development plans.

Now, we've looked at the various barriers starting from the outside in, the EBS barriers, and then in toward the center of the waste package. There's a particular component, the function that component serves, and then an indication of the potential contribution to safety. Upon probably starting from the outside in, is that the first two we're saying are to be evaluated, backfill and packing.

We're looking at backfill as being something that would be put in at about the 100-year point and would go over the top of the waste packages and down the side. We're also looking at what we're calling packing as being something that could be put in before emplacement of the waste packages and would function to sorb radionuclides that would come out after the waste packages start to fail.

We will be awaiting the results of TSPA-95 to give us an indication of how important either of these are, or both, to the overall performance of the system. We have not done a great deal of work on evaluating our options in either case. We do not know what material we might use yet for the packing underneath the waste package. We have done some evaluations of the thermal effects of the backfill that I'll get to later.

Looking at the barrier materials, our primary barriers, we have divided these into aggressive conditions and less aggressive conditions.

Aggressive conditions would be a potential for aqueous corrosion over a long period of time and/or the potential for microbiologically influenced corrosion. Less aggressive conditions, if neither of those conditions pertain.

If we have aggressive conditions, we are considering three barriers as a part of the waste package. So we have a third barrier of highly corrosion-resistant material primarily to control the microbiologically influenced corrosion, and it would, therefore, have a high potential contribution to the overall safety of the system. Less aggressive conditions we do not believe will need the third barrier.

The second barrier, the middle barrier, will be corrosion allowance material, will provide us a predictable corrosion rate, control of radiolysis, but in their aggressive conditions, we don't expect that barrier to last a very long time. Therefore, it does not have a high potential contribution to safety. Even under the less aggressive conditions, this barrier will not have as much contribution to safety as the inner barrier, which will be more corrosion-resistant material and will have a high potential contribution to safety in all cases.

There are other container materials that have a potential contribution to safety other than the barriers.

The multi-purpose canister shell in the conceptual design is stainless steel. We do not intend that--we do not expect that that would last for an exceptionally long period of time. It has a low potential contribution to safety.

Filler material, if it is used, would have a moderate potential contribution to safety. Putting filler material in will not be easy, so we will not do it unless it does have a significant contribution.

Fill gas, which will be in the MPC and performs these functions would have a moderate contribution to safety, and the fuel basket itself, carrying the criticality control material, we're saying has a high potential contribution to safety since it will prevent the criticality.

Now, for the spent nuclear fuel itself, the cladding, and I will show you some analysis in a few minutes about the cladding, but we believe the cladding will function very well as a potential barrier and will have a high contribution to safety. And the fuel oxide itself, because of its low solubility under low temperature conditions, will also have a good contribution to safety.

Finally, for the high-level waste, the pour canister, similar to the MPC canister made out of stainless steel, we would not count on as a significant barrier, so it would have low. And the high-level waste glass has a moderate potential contribution to safety.

We're not able to quantify these potential contributions to safety other than low, medium or high at this stage, similar to Steve Brocoum's one, two or three check marks of yesterday morning.

We have three primary designs of the waste package, a multi-purpose canister disposal coming in the two sizes for the 21 PWR or 40 BWR, or the 12 PWR and 24 BWR.

We also have an uncanistered spent nuclear fuel waste package, which includes a basket. There are two designs of the basket.

And we have the defense high-level waste disposal container, which in our conceptual design holds four of the Savannah River size canisters and is similar in diameter to the MPC with its disposal container, and it's about two-thirds the length.

The barrier materials in the conceptual design are shown here. We're using the same barrier materials for simplicity and consistency for the multi-purpose canister and for the uncanistered fuel waste packages.

Under the aggressive conditions, aqueous corrosion and/or MIC, we have a three-barrier system with Monel 400 as an alternate. We're looking at Ceramics, but we have not gotten very far with that yet. Another alternate to Ceramics may be the 70/30 Cupronickel. The middle barrier, carbon steel, A 516, an inner barrier of alloy 825, and these are the materials for the less aggressive conditions with their alternate.

We were asked about cost. These are the information that we have on a cost so far. I've highlighted its preliminary status down here. You do not get best and final costs from a fabricator or from a material supplier when you tell him he has no chance of selling anything to you for 10 or 15 years. And, therefore, we think that these costs are on the conservative side.

But here is the percentage of the costs by category, and you can see material costs predominate. More than two-thirds of the total cost is in material, 13 percent in labor. This other are things like additional quality control, profit to the company that's supplying it, administrative costs.

So since material predominates, we have the cost per kilogram of some of the major materials that are being considered for the waste package. Down to the relatively low cost, carbon steel, and up to a fairly significant cost of stainless steel boron, which is the criticality control-- supplemental criticality control material that we're using in the uncanistered fuel waste package.

If you extend those costs across the number of waste packages that we will have, most being the MPC for spent nuclear fuel and the defense high-level waste disposal containers, these are the costs in millions for the total program.

We have a relatively small number of the uncanistered fuel waste packages, since we expect most of the fuel will come in the MPC, and we are assuming that there will be about five performance confirmation waste packages.

I will quickly review examples of the types of analyses that we are currently running, and which are at the heart of our current design effort.

We've looked at the cladding to see to what extent the cladding could be a dependable barrier. Intact cladding, obviously, will prevent a release, but even if the cladding has a small pinhole, a perforation, then it will confine the fuel and will limit the access by water.

We've used a very conservative approach to these evaluations, and we're assuming that radiolysis is present and converts the atmosphere to an aggressive species, and we've assumed that the only thing that would stop whatever degradation process we're talking about is to run out of the element that's causing it.

So we've looked at degradation by oxidation, which, of course, can't start until there's a breach of the disposal container. There are two mechanisms. We could have general oxidation of the fuel, which would occur for all fuel, and we could have the fuel oxidizing from UO_2 to U_3O_8, which that process results in an expansion and an eventual splitting of the cladding. That would occur only in perforated--in fuel that had a perforated cladding so that the oxygen could get in there.

Calculated the amount of degradation, we're using our design basis fuel. We are considering the hottest rod in the center of the 21 PWR MPC, a relatively small drift diameter and the enclosed waste packaging, all conservative assumptions. The rate of degradation has been reported by Dr. Einziger of PNL in this reference, and that's what we used.

So we also chose the worst case--well, relatively worst case, thermal load, fairly high thermal load, and we assumed that there was no protection from the dispose of it there.

With all of those conservative assumptions, only 2.3 percent of the cladding thickness oxidized in the first 1,000 years. After a 1,000 years, the temperature is down to the point where the oxidation process virtually stops. So we believe that we do not have a problem with the oxidation of the cladding, of the general oxidation.

Now, looking at perforated cladding, we considered both the high and low thermal load, and if we have an intact waste package for as short a period of 200 years, then we will not get splitting because the temperature will be low enough at the end of 200 years so that the oxidation rate of the fuel will be too slow to result in splitting of the cladding.

So the significance, general oxidation of the cladding is negligible. Perforated cladding does require an inert atmosphere, but only during a period where the container is hot, and during that period, we would not expect to have aqueous corrosion because the surface would be above 100C.

Another example of the types of analyses we're doing is looking at the multi-purpose canister to see how much residual water was going to stay in it after the loading operation, since it is loaded under water, and what the effects of that residual water would be on the long-term performance of the MPC.

This is out of the procurement specifications for the MPC. It says that after loading, evacuating and sealing, you cannot have more than .25 volume percent of water.

It's evacuated, required to be evacuated to a pressure of 300 Pascals. The vapor pressure is much higher than that. So all the water that's in there will be in the vapor phase. The amount of water cannot exceed 13 grams.

So the question is, what's the effect of that, and there are three possible effects. We could have hydrogen embrittlement of the fuel cladding. We could oxidize the cladding. Or, we could oxidize any of the other components inside the MPC. We could have corrosion by nitric acid, which is produced in the radiolytic process.

Well, first looking at hydrogen embrittlement, the amount of hydrogen that's available when it goes into the cladding cannot exceed .6 ppm, but new cladding's already got up to 25 ppm. Irradiated cladding has much higher than that. So hydrogen embrittlement is negligible.

Oxidation. Well, the total amount of oxygen that is available, assuming that the MPC specifications are met, would not exceed 15 grams, and with that amount of oxygen, the amount of oxidation of the shell and the basket that you can get is 45 nanometers. The oxidation of the fuel cladding would be up to 11 nanometers. In both cases, those are negligible.

That leaves the possibility of corrosion by nitric acid, which could occur in two ways. We could have bulk condensation of nitric acid on the surfaces. The oxygen supply in the MPC results in a partial pressure of 170 Pascals. The vapor pressure is 3,000 Pascals already at room temperature. Vapor pressure at higher temperatures is much higher. Therefore, we're not going to get any bulk condensation of nitric acid.

Well, how about nitric acid in water films? Even without any radiolysis, the relative humidity will be low, no larger than 15 percent. If we get radiolysis, that will drive the relative humidity down. And to get any kind of water film corrosion, we'll need relative humidities much higher than that, of the order of 60 percent or higher. So we're confident in saying that water film corrosion cannot occur.

So the conclusion; we've looked at the amount of water that could remain in the MPC, considered these potential effects, and gave it a very conservative treatment, and considered that that amount of water can have no deleterious effect on the MPC.

We were asked to report to you on the thermal performance within the waste package. In order to do that, there is a linkage, of course, with the near and far field. Far-field temperatures depend virtually solely on the area mass loading. Near-field temperatures depend on the heat sink, which is the far-field, which also depends on the waste package spacing, the fuel age, emplacement drift diameter, and the time-dependent output, heat output, of the waste package.

And the internal temperatures within the waste package depend on its heat sink, the near-field temperature, the characteristics of spent nuclear fuel, the number of assemblies that we have in each waste package, what the materials of fabrication are, and then also the design type, since if we have a flux trap design with its spacing in there, we need to have some type of thermal shunt in order to get the same kind of conductives that we would have with the other design.

This shows the decay of spent nuclear fuel over time, starting with the design basis fuel for PWR, which gives us 850 watts per assembly, down to the design basis fuel for PWR, which gives only 265 watts per assembly.

And this shows the decay over time, so that in the first 200 years, in all cases this is decayed by a factor of five to eight.

We're analyzing the thermal performance using three separate models. The repository model output then feeds the waste package model, and then we can then use that to give us the performance of the individual assemblies.

Well, how would the 21 PWR MPC perform in the repository? This is our 350 degree peak cladding, we're saying goal, but we're treating it as a limit. We have two lines of peak cladding temperature. The conservative line is for 10 year old fuel with a fairly high burnup, which is our design basis. The best estimate is for the average fuel, which is 22 year old, with a lower burnup.

We peak the cladding temperature fairly early, about eight months after emplacement. We peak the waste package surface temperature much later, of the order of 50 years, and this is how the temperatures will perform over time at this thermal loading, a high thermal loading.

Dr. Langmuir asked yesterday about the effect of backfill. If we backfilled at 100 years, the temperature will be out here. We have estimated that the increase in the cladding temperature due to backfill at that point could be of the order of 100 degrees, but we have not done the detailed analysis. How much of a bump-up in temperature we'll get is, obviously, highly dependent on what the grade of the backfill is. But we're thinking if it's about golf ball size, then we could get about 100. We have about 150 degree there before we get back up to our 350 degree limit.

This is just a representation of the finite element model of the waste package. Our ANSYS program is tracking the temperatures through time at each intersection throughout the model.

So what does it look like? The peak temperature for cladding occurs at .7 years, and that maximum is 316 degrees. From then on the temperatures decrease, so that at 10 years, the peak temperature in here is down to 301 degrees Celsius. At 50 years, it's cooled considerably; we're down to 245 peak temperature. And at 100 years, we're down to 207.

We have run the same analyses for the smaller MPC, the 12 PWR MPC. The peak occurs a little later, at three years. The maximum temperature is only 251.

We wanted to compare the results of our analyses with some actual test data. To do that, we constructed the model of the Westinghouse 15-by-15 assembly, and again, our model is tracking temperatures at the intersection of each line.

We compared that against test data that was done at PNL, in which they had thermocouples down the guide tubes, and the result was the measured temperature was 206 degrees Celsius. Our calculated temperature under the same conditions was 214. We believe that is within a reasonable araban.

Concerning our options for shielding, there are two basic reasons for shielding. We need to limit worker exposure, and we need to protect the materials from the deleterious effects of radiolysis.

There are some specific requirements on shielding, given 10 CFR Part 60, which refers to 10 CFR Part 20 on the occupational dose. 10 CFR 60.135 discusses the radiolysis effects and says that we must consider those, and also restricts the materials that can be used in the waste package; for instance, no corrosion enhancing materials, obviously, and nothing that would be pyrophoric.

The design of the waste package incorporates sufficient shielding to protect us against radiolysis effects. For that, we are using the regular waste package barrier materials. We have no additional or extra materials in there, solely for the point of shielding.

The waste package transporter and the other facilities need to be shielded for the personnel safety, and for that, we could use a multi-layer gamma and neutron shielding, which would be significantly more efficient. So these are short of the shielding options.

The current design of a waste package gives us a dose rate at the surface of approximately 16 R per hour. The radiolysis threshold is much higher than that. So we have adequate shielding from the regular barriers that are there primarily for anti-corrosion purposes to shield the components against radiolysis.

Now, we could shield the waste package for personnel safety. If we use the same barrier materials, we would have to thicken up the outer barrier to a figure greater than 390 millimeters, but, of course, we would get some improvement, additional improvement in containment from that. Or, we could use a shielding sleeve, which we would not expect to add to the containment, which would degrade over time. It might be made out of concrete. That would have to be about the same thickness. The problem with that is the thermal difficulty that it would create in the early years.

In either case, if we shield the waste packages, it adds to the weight. And the shielding, if we use the barrier material and order of magnitude in addition to the weight, is about 100 tons, metric tons. So it increases the weight of an MPC in this disposal container by about 150 percent. It would be a significant addition.

The weights of the shielding, the more efficient shielding that could be used on the transporter are of these orders of magnitude, 42 to 54 or so metric tons, depending on what material is used.

The transporter shielding is not only more efficient, but also, you don't have to leave it in the repository. You re-use it over and over again, which is, obviously, a very significant cost saving. However, it does mean that we have to have temporary shielding if we ever have to enter a drift that has emplaced waste packages. If we have any instrumentation that's going to stay there, it would have to be radiation hardened.

Now, what do we need to do in the future on this? We will be evaluating the potential for component activation within the waste package and within the engineered barrier system. We're going to evaluate what kind of radiation-inducted corrosion products we might have under various accident scenarios. We have so far considered dose rates from single waste packages. We will be looking at dose rates that occur from a line of waste packages and a drift. We have not yet done that.

Kal and his people, our surface and subsurface people will be developing shielding needs for the remote and robotic handling systems, evaluating the shield door requirements, the emplacement shield door, calculating the operational dose rates.

The last topic is the engineering development program. What we are doing with that is to develop the methods and the processes for fabrication, remote closure and inspection. We have to make sure that what we are designing can be fabricated, can be tested, can be closed and can be inspected in service.

In the fabrication area, one primary concern is to minimize stresses in the waste package as fabricated and to make sure that our fabrication techniques that need to be used are economically acceptable and that we can reproduce the fabrication of the waste packages with a very high degree of quality and consistency.

For the closure joint, in our conceptual design we've selected the narrow-gap welding process. We have done a preliminary evaluation of other processes, but we will do that in more detail to verify that the narrow-gap process is the best.

In the top two, in this one we have three configurations of narrow-gap welds, and this one is an electron beam weld. We will be constructing models of these areas and doing the welding to verify that it will have a high degree of integrity, that it can be reproduced with high quality and to determine cost, welding time and so forth.

So we will be establishing a joint process and a process for welding, and we will be determining how we can minimize the residual stresses in the weld. After the closure weld is done, we will have no opportunity to heat treat it, so we'll need to make sure that the stresses will not cause any subsequent weld defects.

We also determine how to inspect the closure joint remotely and how to repair a defect in the closure joint if one is found.

We'll look at in-service inspection, what the remote service -- remote inspection requirements may be and what methods would be available to us. We will need, we assume, to monitor waste package performance in the repository. We are assuming that Kal's method will be the one selected, and that this monitoring will go on until final repository closure.

We are also in the engineering development program looking at filler material in the MPC. It would primarily be for criticality control, but if we decide to use filler material, it would provide some additional good benefits;

chemical buffering for the radionuclides, maybe cathodic protection, could function as a mechanical packing to keep the assemblies in their original configuration, and assuming it's metallic, it would improve the thermal conductance.

This is the general time table for the engineering development program. The closure joint configurations, the design of those has started, and the method by which we're going to go about the inspection of the closure joints, that has started.

The fabrication techniques has been delayed until our design matures a little bit more so that we do not waste money determining how to fabricate something which we are then going to change.

In-service inspection and filler material, we also are delaying, and those will start in FY-97 and FY-96, respectively.

We are asked about the linkage of the engineering development program with corrosion research programs. We are cooperating and working with the Edison Welding Institute, and we're getting much good information from them. The material testing program that Dr. McCright will discuss will include welded samples, so we will get corrosion data on actual welded samples. We are studying the radiolysis catalyzed corrosion.

Finally, we are asked to what degree we were having cooperation with other countries. We are cooperating with other countries, although their environments are generally significantly different, so that there's not a whole lot of consistency in what their waste package looks like and ours. The Focus '91 Conference, which was on the subject of the waste package, had a lot of international representation and a lot of good exchange. There have been meetings in Prague and Kyoto where we have had representatives, and they are NWTRB meetings, and we have gotten good exchange with our foreign counterparts.

The Waste Package Workshop, which occurred in September of '93, included international representations, which was a very strong interchange. We have produced a number of papers for the International High-Level Waste Conference, and we have received the papers from the international representatives, and this has been a very good source of keeping the programs in sync.

And then finally, the BW Fuel Company, which serves as a subcontractor to TRW with primary responsibility for the waste package, has a very close relationship with one of its two parents, Cogema, that is very strong in France in this area.

Subject to your questions, sir, that's all I have.

DR. LANGMUIR: Thank you, Hugh. Let me lead off with one. I noticed that -- you stated that the cladding oxidation was unlikely to occur in less -- this is Overhead 26 by the way -- unless relative humidities exceeded 60 percent, you would not get water film corrosion of cladding or the MPC.

Ny sense is as a geologist, part-time hydrologist, that if you're looking at putting something in Yucca Mountain, after you close it at least, after you stop ventilating, particularly if you're at the low temperature scenario, the low loading scenario, you're going to be in 100 percent humidity all the time. The water's going to move back in towards the waste packages, and volatilization will put you at close to 100 percent at whatever temperature you have. So I'm wondering what that's going to do, if you agree with that, or maybe we can have some discussion to that; if not now, later in the day. My sense is you may have water film.

MR. BENTON: After breach of the waste package, so that--

DR. LANGMUIR: After breach, yeah.

MR. BENTON: Yes. Yes, sir, I would say that that's correct.

Dr. McCoy, do you have anything to add to that? Dr. McCoy has done this analysis.

DR. MCCOY: We have two separate analyses here that are perhaps being confused. One of them is with regard to the amount of water in a sealed MPC and its effect. The other is on exposure of cladding to--

DR. LANGMUIR: Dan Bullen?

DR. BULLEN: Along those same lines, did you evaluate the mechanism of creep rupture as the primary mechanism for clad failure under the early time scenarios?

MR. BENTON: Yes, we have evaluated creep rupture. I believe that's previously been reported. But, yes, we did evaluate the extent of the creep rupture problem under various thermal loading scenarios, and this would be one thing that might happen with backfill because by the time of backfill, we would have gotten down on a very low curve for-- low part of the curve for creep rupture. If we bump the temperature back up, we get back up on a higher curve again.

But in answer to your question, yes, we have evaluated that. We'd be happy to show you that.

DR. BULLEN: I'd like to see that.

DR. LANGMUIR: I have one more. Specifically, you showed overheads indicating the designs that DOE is proposing for defense waste disposal at Yucca Mountain, and it was clear from one that Savannah River had participated with you, and, well, you'd thought about the design of their materials that might fit in one of your waste packages.

One of our concerns as a panel has been whether-- the question and the apparent lack of communication between, for example, the Hanford people and the waste they were producing, defense waste, and what they might be doing with it, that it perhaps was not being interfaced sufficiently with your programs for disposal at Yucca Mountain, that the designs they were proposing for at least the next few years for their materials was not taking into consideration what DOE would like to do with those things at Yucca Mountain. And I guess I wonder what you might say to us about that.

MR. BENTON: We have had several meetings with the people of Hanford, and I think a very good interchange of information. I'm not exactly sure what Hanford is doing with their designs week by week, but we are generally having -- we are generally being given an opportunity to comment on what we think the needs are for the repository, and we have given them the information on our waste package designs and what generally we need that they need to fit into.

I think as time goes on, we are looking forward to even closer cooperation to make sure that not only will their designs fit, but they'll fit most efficiently, with the highest probability of good performance. I don't think we've quite matured the thing to that point yet.

DR. LANGMUIR: Ellis Verink?

DR. VERINK: I'm looking at Overhead No. 12. I suggest some of you ought to see if there's been a transposition of a couple of the numbers. I wouldn't expect an extruded 6063 aluminum to be the price per kilogram of 316.

MR. BENTON: Jerry Kogar, have we transposed a number here?

Dr. Verink, we'll research that and get back with you.

DR. LANGMUIR: Dan Bullen?

DR. BULLEN: Could you go back to--have you got No. 10, which is your waste package materials view graph?

In the more aggressive environment, you have an outer barrier of Monel 400. Have you done any evaluating of a potential galvanic corrosion effects associated with that, specifically looking at the fact that essentially you're putting a noble metal 825 and then a less noble metal A 516, and then a more noble metal again in Monel 400. So if you breach the 400, then you're going to preferentially corrode the inner barrier, and it could pose some significant problems for performance.

MR. BENTON: Yes, we understand that problem. It is being evaluated, and whether this is the right thing to do, or we need to go to the alternate, it will be evaluated. Certainly, the galvanic corrosion potential will be a major part of that.

DR. BULLEN: And I just had one other follow-on question, and then I'll be quiet for awhile.

You mentioned a number of threshold for radiolysis effects of 1000 R per hour. Could you tell me where you came up with that number and how you identified it, sort of not only 1000 R per hour, but also the conditions of humidity, temperature and duration that you'd have to worry about?

MR. BENTON: Dr. McCoy, can you shed any light on that? All right, I guess we were given that number. We'll get back to you.

DR. BULLEN: The only concern that I have is that it's actually an area of research that I've done, and I have data that would suggest that it should be at least an order of magnitude lower.

MR. BENTON: If it's an order of magnitude lower, we're still in the safe range, but we would appreciate getting your information.

DR. BULLEN: Well, the only question is, is that your 16 R per hour is for what burnup fuel. Surface contact dose was 16 R per hour sighted at what burnup and what age?

MR. BENTON: Our design basis fuel.

DR. BULLEN: Okay. So that's just your average. If you go up to higher burnups--

MR. BENTON: That's design basis, so that's higher than average.

DR. BULLEN: Okay. But your off-normal conditions, 60,000 megawatt days per metric ton, is going to give you more?

MR. BENTON: Yes, but we're talking now 48 burnup.

DR. BULLEN: Okay.

MR. BENTON: I don't think 60 will give us--you know, it won't double it.

DR. BULLEN: Okay. Well, I'd be interested in seeing your calculations.

MR. BENTON: We would appreciate you're looking at them.

DR. LANGMUIR: Any more Board, staff? I'm wondering if Carl Di Bella wouldn't like to ask a question? No?

Any further questions? Well, thank you.

Let's take our coffee break. It's 10:30.

(Whereupon, a break was taken.)

DR. LANGMUIR: Our next presentation is EBS processes to be implemented is TSPA-95. The speaker is Bob Andrews of INTERA.

MR. ANDREWS: Thank you very much.

What I'd like to talk about for the next 40 minutes, I think is what we have, is work in progress. It's work towards TSPA-1995. We'll focus on the objectives and approach and focus on the waste package EBS components of the TSPA-95 effort, which is ongoing as we speak.

I'm going to try to clear up any smoke that anybody might see, and if there are any smoke questions as I'm delivering this, please stop me immediately because clearly, one of the objectives of performance assessment as we interface with regulators and external review boards is to make this as transparent as we can, given the complexities that are involved.

What I would like to talk to are the major components that we intend to incorporate in TSPA-95, focus on the waste package EBS part, bring you back a little bit to what we did in '93 and why we want to improve upon that in this particular iteration, talk a little bit about new design information, which also requires a change in how we do some things, work through the objectives, both the general objectives and the more detailed objectives of the TSPA-1995, with the focus on these particular six sub-bullets, if you will. First, the drift-scale thermohydrology; second, looking at backfills and their potential impacts on overall system performance; the corrosion degradation models, both initiation and rate; the radionuclide mobilization; package EBS release models and their uncertainty; and then talk a little bit about colloids and their incorporation in TSPA-95. We'll finish with the schedule and some summary.

The next view graph in your package has already been shown about four times, so maybe I don't need to show it, but we will in this particular talk focus on one, two, three and four; one, providing the environment in which the package and EBS sit. And that environment, that near-field environment, which is clearly impacted by the thermohydrologic response of this system, is key to everything that comes after.

So, you know, what Dr. Langmuir said in the beginning, we're going to focus on two, three and four. One is clearly the boundary condition in which two, three and four sit. But we will not talk about migration through the geosphere, number five.

We in PA sort of put this into a little bubble diagram like this. Clearly, there's a lot of external features, events, processes that might impinge on the performance of the waste package EBS, and, in fact, the natural system. Those we will not touch on in this particular meeting. The unsaturated flow system clearly impacts the near-field environment. We will not hit on that directly, but more or less indirectly in this discussion. But in this area of the total system, if you will, is what I want to focus on today.

So what did we learn in TSPA-93? Well, we've had some discussions with the Board and NRC and NAS, in fact, on lessons learned, if you will from TSPA-93, and I've sort of encapsulated those in two slides because I think the Board nicely captured some of these things in their last report to

Congress, in fact.

What are the major conceptual uncertainties that we identified in those work efforts? Firstoff, right at the top of the list is our conceptual model for discreet fracture flow or fracture-matrix interaction, which we will not hit on in this particular meeting. Secondly, is the in-drift thermohydrology. Thirdly, the initiation criteria for aqueous corrosion under humid air and aqueous environments; the actual degradation models used for the package, the different materials in the package. And finally, or on this slide anyway finally, the degradation models.

We've talked to you, and I think I talked in July, and I think there was another presentation in November, trying to stress that the relative significance of each of these things and the things that are on the next page are clearly a function of the performance measure that we're considering and the time that we're considering.

And whether that performance measure is an EBS performance measure or a package performance measure or a system performance measure--and remember, we have two system performance measures on the table in front of us right now, one a release kind of performance and one a dose kind of performance measure, and we have varying times on the table. You know, 10,000 years seems to be the best guess of that time period that we're looking at, but we don't know what the

NAS is going to come back with. And clearly, the time component impacts the relative significance of our uncertainties.

Other aspects affecting this; the definition of what we mean by failure. Does a failure mean the first pit or the first localized crack that goes through the package, and then the package is gone; or do we want to put some distribution on how the package looks like as a function of time, or may look like as a function of time.

The water contact actually with the waste form. Clearly, there's no dissolution of waste if there's no water in contact with the waste. So that's a key uncertainty.

The solubilities in near-field geochemical environment; in particular, those relating to neptunium we've identified as key for some performance measures and some time periods.

The actual release model here, including EBS diffusion, the effects of a capillary barrier, if you will, and the benefits of a diffusive type release. And finally, another geosphere one at the end, the conceptual model of fracture-matrix transport.

You'll probably realize when PA people talk, we start from the outside, come back in, and then go back out again. You know, so we look at what impinges on my system and then look at if I do have a dissolution and ultimately release from the package, and then from the EBS, what happens into the geosphere.

And so what are some design information? You realize in TSPA-93, we looked at three thermal loads, one very, very high and one 14 KW per acre, one low and one intermediate, sort of the SCP design.

The current thinking is we still, as you heard yesterday, there's a flexible design, that we'll focus on the low end of the range and a sort of high end of that flexible design range.

Hugh talked about the four current conceptual waste package designs, which are slightly different from what we had in TSPA-93. You'll remember in TSPA-93, we just had an outer barrier, which was mild steel, an inner barrier, which was alloy 825, highly corrosion-resistant material. Now, the package or the current design, which, of course, they're evaluated in number of alternative materials and alternative fabrication, but the current one depends on the thermal load, and is slightly different, whether you have spent fuel or you have high-level waste.

There's two backfill options, yes or no, probably not much in between those two, and Kal talked to you this morning about two potential ventilation options, yes or no.

Within TSPA-95, we will probably not look at the ventilation issues explicitly. That will probably come later. So we're going to assume no on that question.

So as a general objective of any performance assessment and for what we're going to do this year explicitly, desires to be as representative, as realistic as we can reasonably be. That representation or realism comes from the more detailed process level understanding of the individual processes and how we incorporate those in the abstractive representations, of course, required in the total system models.

So we're trying to be, and in particular in the waste package EBS area, be more representative. I think the Board commented, and NAS has commented to us, and NRC has commented to us in our interactions with them that the assumptions made in TSPA-93, although perhaps better than what was done in TSPA-1991, are still very, very, very conservative. And why not put a little more realism, more representation into how you treat the package EBS system? So we'll talk about that in some detail over the next 30 minutes.

Always, we want to test the significance of those conservative assumptions. If they are very significant, you might want to--well, as long as we keep them conservative and they're very significant, you might want to say, well, perhaps I can gain a lot by decreasing the conservatism in that particular model or parameter or what have you. So it becomes, then, a guide, if you will, one of many from a lot of different parts of the program to the data collection and detailed process model development.

To evaluate the sensitivity of that conceptual uncertainty in terms of varying performance measures, and we have a range of performance measures that we desire to look at. We still don't know from the NAS group whether we have a cumulative release type of criteria or a dose type of criteria. And with a dose, we don't know if it's peak individual or average over a certain population. There's a lot of things floating around on the table about this.

One might even add risk. You know, why didn't you go to risk-based performance measure? Well, we could, but we probably won't in this iteration.

So our objectives are to incorporate more representative of the drift scale thermohydrology, analyze two different thermal loads and two different backfill options, yes or no. Use more reasonable estimates of the package degradation, and evaluate the impact, or potential impact, of cladding performance on the EBS and, therefore, total system release.

If you remember, with the TSPA-93, essentially we assumed cladding was gone at the time the package was gone, so the whole waste form could be in contact with whatever fluid existed at that time. The work from Einziger, as Hugh presented, might indicate you could take a lot of credit for cladding still being there for long periods of time.

The other objectives are to incorporate the uncertainty in the percent of waste package exposed and, therefore, available for release as a function of time. This assumption is saying that the first pit through might start processes going on inside the package, but I have a pit distribution as a function of time, which exposes more and more the surface area of the package as time goes on.

These other things are more or less incorporated in TSPA-93, and we'll continue to look at the uncertainty in waste form dissolution and solubilities, look at alternate definitions of the 7,000 tons of high-level waste, look at colloids, and look, finally, at the correlation between some of the system and subsystem performance measures.

So let's go on to first, the drift-scale thermohydrology, four design options. Considering only the 21 PWR case and considering the range of geosphere properties or assumptions in the drift now, and that range is uncertainty in percolation flux, uncertainty in the hydrologic properties of the TSw itself, which are, of course, uncertain or variable, and whether you have normal or enhanced vapor diffusion, an issue that's been raised by a number of the thermohydrology people.

Given that range, given that uncertainty, if you will, in some of those parameters or processes, conduct the number of not stochastic type analyses, but let's call them multiple deterministic analyses. So we'll get a family of curves, of humidity as a function of time, temperature as a function of time, water content in the drift as a function of time, and aqueous flux, if it is there, in the drift materials as a function of time.

How we're using that? I have a family of curves, which hopefully bound, if you will, the expected response in the drift. So the number one on the first curve, if you will, the near-field thermohydrologic environment. What am I using that for? I'm using humidity as a function of time to determine when do I initiate humid air corrosion, and what's the rate of humid air corrosion?

I use temperature for all the temperature-dependent parameters, of which there are many.

I use water content to define the effective diffusion, and I'm going to use aqueous flux to define the advective, if there is any, release.

We were asked to talk briefly about the potential effects in terms of post-closure performance of alternative backfill designs; in particular, whether you have backfill or whether you don't have backfill.

I think some of these things have been reported to the Board in the past. They've looked at backfill being emplaced at certain periods of time. The expected response in a qualitative way, if you emplace the backfill at 100 years, is the temperature will be slightly higher for slightly longer. The humidities are slightly lower for slightly longer. The invert, packing and backfill saturations or water contents are slightly lower for slightly longer when you have backfill. And you may, if you engineer it very well, divert any advective flux, i.e., any dripping that may occur, if it's properly engineered, i.e., have a capillary barrier.

All of the above effects are intended to be considered in this particular iteration with a diversion of the advective flux study as just a sensitivity case, yes or no.

A lot of engineering issues, as you might imagine, the association with emplacement of a real capillary barrier backfill material, some things the Board has heard Mick Apted talk about.

What are the potential consequences of those effects that I just alluded to? Well, in general, the lower humidities tend to delay the initiation of the humid air, and aqueous corrosion, therefore, is good.

The lower humidities tend to decrease the humid air corrosion rates; therefore, it's good. You know, if I can decrease rates or decrease initiation, I increase the time before the onset of a pit penetrating through the corrosion allowance or corrosion-resistant materials.

However, you have a down side, the increased temperatures for most of the rate-dependent things tend to increase the humid air corrosion rates. There's some data on this I'll show you in a second.

The aqueous corrosion rates; when I have actual aqueous processes occur, tend to be greatest about 60 degrees C. They drop off at higher temperatures and also at lower temperatures. So that might be a wash. It's hard to kind of figure until you do the analyses.

The temperatures tend to increase the pitting-corrosion rates from some of the data of the highly corrosion-resistant materials. This is something that we've talked to the Board about, and perhaps led to some of the results that we saw in TSPA-93, the assumption that the pitting corrosion rates of the highly corrosion-resistant material were highly temperature dependent.

The water contents within the drift, or within the package in this case, dramatically decrease the area of the waste form in contact with liquid water. It's going to dramatically decrease the dissolution, effective dissolution of the fuel.

Finally, the lower water contents, which occur when you have the backfill for longer periods of time, significantly decrease the effective diffusion by many orders of magnitude, which we'll show in a second.

All of these potential consequences will be directly or essentially indirectly embedded into TSPA-1995. However, I can't stand up here today and say that with or without is going to be better or worse. I think there is some competing things in here. Some of that will be dependent on some of the conceptual uncertainty and conceptual models that we'll talk about in a second regarding degradation, initiation of degradation, et cetera. So a priority, I'm not going to stand up here and say, backfill is great.

Okay. Let's talk about corrosion initiation and rate uncertainty. And I want to stress, you know, within performance assessment, everything we have here is uncertain. You know, everything has a certain variability from point to point in space or due to observations, we have a lot of uncertainty. We try to incorporate that uncertainty in as reasonable a way as we can and determine, does it make a difference or not? Post-process the results, as you're well aware, to look at whether it made a difference or not.

Let's talk about the first initiation of humid air corrosion. I think Dan McCright will present some results, and there's a lot of literature sort of data that indicates that it starts in the range of between 60 and 80 percent.

So before that time at lower relative humidities, I have zero humid air corrosion occurring on the corrosion allowance material. I have no corrosion or pitting corrosion of corrosion resistant material under any humid air environments. I have to have a liquid film present, which would be at the 90, 95, 100 percent sort of levels.

The initiation of aqueous corrosion processes is going to occur, and again, there's data to support this, in the range of from 90 to 100 percent, maybe an average of 95 percent. So we'll make that varying.

There are a lot of data on uniform corrosion rates of corrosion allowance materials under a wide range of environments from a wide range of sources. I'm going to show some of those data, and they fit to those data in the next slide. But it varies, you should be aware, with humidity and with temperature and also with time. This is the famous time to the almost one-half rule.

This is those--these are some of those data plotted. These are coming from all over the world, and these are data over more or less a 16--14-year time period. This solid line is the best guesstimate fit, if you will, to those observations, and I've shown on here the two sigma variation around those. That's for humid air.

This is for uniform corrosion in natural water, so under aqueous sort of system. And when do I kick into aqueous again? Is that a 90 to 95 percent RH value?

These data are over, like I say, 15 to 16-year period. Our predictions, unfortunately, go out to thousands of years. So what I've shown on the next slide is for just uniform corrosion, now no enhanced corrosion for that for the time being of the corrosion allowance material in that humid air environment as a function of time out to 3,000 years. This is a rate--

DR. LANGMUIR: Bob, excuse me, is this soft iron or is this just iron metal?

MR. ANDREWS: This is steel, mild steel.

DR. LANGMUIR: This is mild steel, okay, which is basically just iron?

MR. ANDREWS: Yes, essentially.

DR. LANGMUIR: What else is in it? There's some carbon and silica.

MR. ANDREWS: I'd have to go actually back to the data to tell you exactly what all the different materials that were being tested were, but they're essentially iron-base.

This is the uniform corrosion--now showing cumulative depth as a function of time. For the case of aqueous corrosion processes using those data and for the case of different RH values, I picked the peak aqueous temperature here of 60 degrees C, just to give you a rough feel.

Again, it's somewhat comforting, I guess, that the aqueous corrosion--these are now extrapolating out, from that 15 years of data out to 3,000 years. I'm just showing the expected value, not the two sigma variation on either side; that the corrosion of the--uniform corrosion of the materials in water at 60 degrees C and at 95 percent humidity at that same temperature end up being about the same curve, even though it's from two very different data sets.

So what do we do? That's just uniform corrosion and a fit to uniform corrosion data. We all realize that the uniform corrosion is probably not what's going on on the mild steel, but there's some localized corrosion. Last time, you're well aware we used a value of four. This year, we're going to sample that from two to six. There's a lot of data out there that support this kind of a range. Probably the data supports going down to a range of one, i.e., no increase, but we'll be conservative and make it two.

The pitting corrosion of the highly corrosion-resistant material; so now we're talking about the inner barrier, if you will. There's no new information since TSPA-1993. Dan will talk about the status of the testing program, which is being designed and developed and conducted to develop that sort of information and understanding, but right now, there is no new information to support any different pitting corrosion model of the highly corrosion-resistant material than what was used in TSPA-93.

This, as you're well aware, at the higher temperatures, still gives a very rapid rate of pit penetration, and it has also a very wide distribution. And you can see here from the 5th to the 95th percentile is a factor of 100 difference in rate.

There's also no data on the pitting corrosion of the moderately corrosion-resistant material, which will be this outer barrier of the package. We don't know, you know, standing here right now, how much corrosion susceptible that is than the highly corrosion-resistant material, but presumably, it's some factor. And maybe that factor is five, maybe it's two, but probably you would have to sample off of that factor.

As in TSPA-1993, the cathodic protection of the inner corrosion-resistant barrier may conservatively not be included. However, it might be evaluated--some effects of the protection might be evaluated in the sensitivity case.

Although galvanic coupling, this is Dan Bullen's question of the corrosion-allowance material sitting underneath that moderately corrosion-resistant outer barrier for the aggressive environment will be increased by a factor of two to six. And there are some data to support what multiplication factor there is on the effects of galvanic coupling.

And the uncertainty/variability is going to be treated from package to package and pit to pit.

I realize I have to speed this thing up.

The idea is to calculate stochastic pit growth rate, calculate the distribution of pits, look at the initial pit penetration as defining the failure. So the first pit goes through the multi-barrier system; everything will kick in. However, there will be a cumulative pit distribution used to define the area of the package available for advective or diffusive release. And that nominal pit size is probably a little bit high. Nominal pit, if you look at literature values, are about a tenth of that, or a hundredth of that for some of the corrosion-resistant materials.

This curve is just a draft representation of what the fraction of pits penetrating only the mild steel outer barrier would be for those two models I just gave you, assuming the constant relative humidity of 80 percent, and I think it's 90 degrees C. So we picked the relatively high one, just for demonstration purposes, where all the variability is split between packages and between pits.

And you see here that the first pit--well, you probably can't see it, essentially is on this line because this is the cumulative distribution of pits penetrating the package. The first pit for the first--and this is 10 different packages. So I have 10 curves corresponding to 10 packages. This line corresponds to the first pit. So, if you will, it defines the initiation of everything that goes on inside the package.

So we see the first one here is that, you know, roughly 1,000 years. The last package is at 30,000 years, something like that. The 10th percentile pit is at a few thousand years here and 100,000 years here.

So we're looking here at package-to-package variability and pit-to-pit variability on a package.

Mobilization uncertainty. Here we have a switch that we might consider cladding degradation or we might not. I might assume the entire waste form surface is exposed at the time the inner package is gone, which is the TSPA-1993, very, very, very conservative assumption. Alternatively, we take the results from Hugh and Kevin McCoy and say let's put that in and see what kind of performance the cladding is giving us in terms of subsystem and system performance.

Given that I have a cladding surface exposed to natural environment, I also have to have water in contact with that waste form surface. That water--and I'm going to say it has to be liquid water. You might make an argument that maybe under 95 percent RH I do have liquid water. So you might say make the same cut, that when I have 95 percent RH, then I have liquid water, at least possible to be present on the waste form surface. When that occurs, then I can have dissolution processes occurring. And I have, as before, functional relationships of dissolution rates and solubilities of nuclides in that water film contact between the water the waste form.

These are the data from Walt Gray, a subset of those data, the best fit model, if you will, and again, two sigma standard deviations on either side of those data.

Another set of data this Board has seen a number of times and had some very active discussions on regarding neptunium solubilities, the data from Nitsche, et al. Just to confirm through the Board the under saturation experiments that I think were recommended in April of last year--or sorry, July of last year, have been undertaken by LANL and have confirmed these data for neptunium for a few water samples. I don't think they have all the temperature cases done, but they have the 60 degree C case done, and it more or less confirms these from under saturation.

However--there's always a however in geochemistry it seems like. However, they're not sure whether these values are stable phases or still metastable. You know, the phase they're getting is this sodium neptonyl carbonate hydroxide, something like that, and they're not sure whether that's the stable phase in this environment or not.

These are the data, again, and the uncertainty bars, not conceptual uncertainty bars I want to point out, but uncertainties on the raw measured values, a very difference clearly; the uncertainty bars on those data. And these are the fits to those data. For the top one, I have shown the mean, and again, I think this is one standard deviation that we've shown on this particular fit.

EBS release. First, very important now. If I'm going to limit--and this is a comment that's come from the Board, it came from NAS to us repeatedly, and also from NRC, and that is assuming that the first pit means the whole package is disintegrated is ludicrous. So this first bullet is meant to address that particular assumption, that the first pit does not mean the package has disappeared. There's a distribution of pits, a distribution of surface area through which then things can be either advectively or diffusively transported, but that surface area, which is very important, is not the whole package surface area. It's some very limited subset of it.

We are going to conservatively assume, however, although you might preferentially imagine, and I think the pitting people would tell us this, that pits are more likely to occur at the top of the package logically than they are at the bottom of the package. Pits grow with gravity, not against gravity. We will conservatively assume, though, not knowing anything better, that the pits are located along the advective diffusive transport path. That's a pretty conservative assumption.

The advective release through the package will only occur if there's fracture flow in the TS_{w2}. So under nominal cases, and this will be investigated in the UZ part of the--unsaturated zone part of the TSPA-93. There may be very limited localized flow. But it is also possible that there is localized flow, discreet flow, advective flow through the drift locally.

The diffusive release is calculated using--through the package, using a relationship from Conca that I'll show you in just a second.

EBX, essentially the same thing as the package. I'm going to go from package to that invert material, a meter and a half of invert material. There might be advective or diffusive release through that.

Conca relationship the Board has seen numerous times. The key thing here, we're going to have very, very, very low water contents expected for a lot of the cases. My effective diffusion coefficient through this non-connected water film in the invert materials is extremely low. In a 100 percent humidity environment, it's zero. You know, so if I keep it just 100 percent humidity environment, I have no effective diffusion through that barrier. Well, that's--you know, Mick Apted has talked to you about this a number of times. It's the best of all worlds sort of thing. It's what a lot of the international performance assessments--I don't want to say rely on, but their bentonite barrier is a diffusive barrier. No advection through their bentonite. Bentonite doesn't fracture, the assumption is.

Let's talk briefly about colloids. I realize I'm running out of time. There is some data from Inez on colloid populations under ambient conditions. We're going to assume that plutonium and americium irreversibly sorb onto those. That, therefore, increases the total mass, if you will, of available transportable plutonium and americium. And we'll look at alternate models of filtration absorption in the unsaturated zone.

What's the schedule? We are going to complete most of the process-level models and abstractions next month. We have a draft documentation by September. I think we're on the hook to present something to the Board in October. We are on a tentative agenda to present something to NRC in December. And I think we're still on plans to present TSPA-95 for external review, NAS review in January of next year. Clearly, what we do between September and January might be based on comments we receive from some of these external review boards.

So in summary, I want to say that we have identified a number of enhancement, refinements from TSPA-93 that we want to incorporate in TSPA-95, that are being incorporated in TSPA-95. It adds to the realism and representativeness of those individual models and analyses. We're going to still test the significance of those assumptions and evaluate the importance of the different components, at least those first four components, and in fact the fifth, of the waste isolation and containment strategy that was presented earlier.

I want to point out, and this is I think a question raised a little bit at the end of yesterday, that some of the conceptual underpinning, i.e., the foundation of proof of confidence of some of the conceptual process-level models that we're using are still somewhat uncertain, and these are probably the key ones, and there's one on the following page: The draft-scale thermohydrology, the pitting corrosion degradation model in particular for the corrosion-resistant material, still very uncertain and a very significant barrier to both subsystem and system performance, but as Dan will talk about next, that work is just starting.

Cladding degradation models; I think this will be, in my own personal opinion, a reasonable assurance argument, that maybe we never incorporate it into PA exactly, but we say, look, we have all of these analyses and all of these data from a reactor experience that tells us that the cladding itself is a major or could be a major barrier to overall performance.

The solubility information--solubility models and the function of near-field geochemistry is also uncertain, as are the proof of the drift scale transport models.

Now, I say that as a negative, but I say on the positive side that some of our analyses are meant to look at whether some of these uncertainties in these conceptual models are really significant with respect to performance, whether that's subsystem or system performance. And we in performance assessment will continue to identify these models, these process level models and process level understanding as being the key to overall performance, unless their significance is deemed to be inconsequential. And I have to throw in here the other one that we just haven't talked about today, and that's the UZ hydrology model.

So I'm sorry--well, I guess I'm right on time, but no questions. Perfect.

DR. LANGMUIR: Thanks, Bob, you've got three minutes. I'm going to use one of them with a question.

It looked to me as if all of the work that's been done or being thought about with regard to pitting corrosion is using inorganic rates, and what I'm hearing from a variety of sources, including what I read and write about myself, is that the bacteria can contribute a rate increase to those processes that ranges typically from 10^5 to 10^9 times, increasing those rates. That kind of blows out the window, what you put into your TSPA model if you limit yourself to the inorganic rates. I wonder if you thought about putting bugs in the equation?

MR. ANDREWS: Yeah, you're aware there's a lot of research going on that Dan will probably talk about on the MIC kind of effects on the--well, in all material degradation. We thought about putting some kind of a multiplier, some kind of an enhancement factor in these rate --essentially rate equations, and if we could find that appropriate multiplier, I think we would and could, and it would be nice to have some references that supported that.

However, you know, there's something--you know, the data that we're using from a wide range of environments, I don't know whether those environments had microbes sitting in them or not, to be honest with you. Well, yeah, those data are long gone, and, you know, those people are probably long gone. It is data from the '70s and '80s.

But--

DR. LANGMUIR: Some of us are still around.

DR. BULLEN: This is Dan Bullen. I may be able to help you out here. Essentially, this is something I'd like to talk about in the panel discussion this afternoon. We had an MIC corrosion workshop last week at Livermore, and it was very worthwhile.

We can talk about the limiting factors that might address this, and it might be something the Board would like to take up at a future date, but it includes temperature, water contact mode and the amount of nutrients available.

So that's something that I'd like to discuss. Maybe we can get Bill Clarke up to talk about that.

But now that I have the floor, I'd like to ask one question, if you don't mind, and I'll keep it to one.

What credit do you take for the inner barrier, the middle barrier, when you do your more aggressive environment type analysis in your performance assessment?

MR. ANDREWS: Whatever credit--I mean, we have in there that it's going to be a multiplier factor on the pitting corrosion rate for that middle barrier once my mild steel--or not mild steel--once my moderately corrosion-resistant material has been pitted. We have that factor, which is a factor of four to six worse than it would be had that outer barrier not been there.

DR. BULLEN: You might want to reconsider that in light of the fact that one of the problems associated with pitting corrosion is that it's area dependent.

MR. ANDREWS: Yes.

DR. BULLEN: And if you have a real big area and a real small pit, I would contend that you might be able to drill a hole through the however thick middle barrier in a very short time, and the credit that you take for that barrier could be very suspect. So you want to be very careful about how you do that.

Now conversely, a thick package will also plug up a pit if you get corrosion products in the way. So there's an inner plate that you have to worry about. But if you try to take too much credit for the multiple barrier, particularly in the galvanic corrosion effect, I would contend that depending on the area, you could be--a magnitude higher, not a factor two higher. You might want to take a look at that.

MR. ANDREWS: We'll look at that. I mean, what we'll do is for each of those barriers, look at the times. And I can't answer your question of what that delta T is for the middle barrier right now.

DR. BULLEN: Well, your middle--your minimum time may be like 10 years, which is a non-time at all.

MR. ANDREWS: Well, if that's what it is. I don't know if that's what it is. But there are data to support those multipliers that I just presented for that--

DR. BULLEN: Well, I agree, those data are probably right. I just say you better check the area of the samples that were used when they got those pits.

MR. ANDREWS: Okay. We can do that.

DR. LANGMUIR: I'd like to go on here. I can see plenty that we can talk about this afternoon continuing this topic. We're a little bit behind schedule.

Our next presenter is Dan McCright. His presentation is corrosion research and modeling update, and I can see that I might have gotten to my bug questions here. So we'll look forward to this.

MR. MCCRIGHT: In response to a large number of questions that the Board had for me, this is going to be, in keeping with the waste package design, it's going to be a rather robust presentation, but with the multiple of transparent barriers. And I shall try to contain myself within the time limit.

The outline of my presentation is first of all, to discuss very briefly the scientific investigation plan, particularly to talk about candidate materials and bounding environments. This is what governs our work for the next several years. Then to go into the status of our experimental work, the types of corrosion testing; then to go to the status of performance modeling and to talk about one in particular, one on general corrosion and oxidation, one on pitting corrosion; then a brief status of our other activities and a summary and outlook.

We work to SIPs in our kinds of activities. These are a formal description of the work to be performed. It's not only a technical planning document, but it's also a quality assurance document because each activity has to be graded for QA content.

Our most recent SIP was revised in January of this year to cover particularly the issues that come up with a multiple barrier approach.

I have our activities grouped into four important areas, degradation mode surveys, the testing and physical properties evaluation, model development and materials recommendation.

I'm going to skip the next two charts because they just give some words that I can more easily explain just in showing you what the sequence, the layout of our plan is.

First of all, we do the degradation mode surveys. These are interpretive literature surveys on what's known about the candidate materials from applications in the past and how this could be applied to Yucca Mountain. This tells us what a lot of the data needs are, where the insufficiencies are, and that sets up a lot of what we have to do in the corrosion testing activity.

And we like to say that the corrosion testing and model development go hand in hand. The testing drives the models, the models drive the testing.

From the testing we get not only input into the model development, but then the input from our testing program, plus information that's known from the past into the recommendations; first of all, to set up a selection criteria and then to evaluate materials against that, and then to come up with a selection at the end.

Again, most of our work is directed toward the license application. Again, most of this refill will be directly used in that license application. Technical site suitability will occur midway in this program, and again, we tend to support that in a more indirect way. Our approach is that we do have a design, we do have materials that will work. Again, that's barring some adverse finding between now and 1998.

Again, we work very closely with other elements of the program. Others have said this before. In Hugh's talking, he covered a number of these where we work with the design team on say the fabrication of welding effort. We were working with them on the ultimate section.

We work very closely with the near-field environment. Obviously, that sets up the geochemistry, the water, the propensity of water to contact the container, and what the quality of that water would be.

A very important part of that is that is the man-made materials effort and what the effect of the man-made materials would be on conditioning an environment as it contacts the metal barriers.

We work closely with the waste form people. They are very interested in how this barrier will degrade and how the corrosion products and the morphology of attack will effect the dissolution and the degradation of the waste form. And between these two activities, we have a number of other components that will be in the waste package; namely, the stainless steel pour canister for the high-level waste and the stainless steel NPC. And again, the effect of those, of the compatibility issues that they raise with the materials that are ultimately selected for the engineered barriers that is an important part.

We work closely with performance assessment to ultimately derive a subsystem model for them. They feed back to us with sensitivity analysis, and very important to us, a very scenario analysis of what the different kinds of environments, what they could become over long periods of time.

Again, on candidate materials, we require several candidate materials. Again, Hugh mentioned many in his talk. We need different ones for the different barriers. And then we would probably, as he mentioned, use different materials depending on what the waste package design of thermal strategy is going to be, whether we're going to be on the low end or the high end of those because that sets the temperature on the surface container, and that sets in turn the environment and what the likelihood of degradation modes would be.

Again, we had a workshop in Pleasanton last year to determine what would be attractive candidate materials for multiple barrier waste package designs, and that was attended by many people here in the room. And to look at the corrosion-resistant materials that would be used for one of the innermost barriers, we grouped them into three categories, and again, there are more materials that were shown on earlier slides because we want to test a number of alternatives to determine which of these would be the best.

They're into three families. One, again, the nickel, iron, chromium, molybdenum alloys, which one could regard as extensions of stainless steel, with much higher nickel content; again, imparts a great deal of corrosion-resistance. These are also high in chromium and contain significant amounts of molybdenum; again, impart resistance to a number of forms of corrosion.

An additional extension to this family would be to the nickel-chrome-moly alloys, where now we essentially replace all the iron with nickel, and again, these are the commercial. I've listed both the--kind of the neutral name and the commercial name.

I do want to emphasize because we're given a UNS number and ASTM number that these are commercially available materials, and we're working within the restraints of the act that calls us to do that.

One material that is really a little bit on the new side in the titanium category is this titanium grade 16 that has a small addition of a noble metal. The Canadian program is very interested in that material.

Again, in addition to the nickel-containing alloys, we are looking at titanium, and you'll see a little bit later why the interest is here. Titanium is very resistant to a lot of chemical media, and it seems to be the most resistant material, if not immune to microbiological influence corrosion.

Again, most of these materials were developed in the chemical process industry to handle very aggressive environments.

What we're doing with the outer barriers, we, again, saw two categories there dealing with the traditional corrosion allowance materials, the carbon and alloy steels. We have three candidates here. These two are effectively the same composition, but the difference there is that they have a different product form, one's wrought, one's cast. So this gives the people who are designing the fabrication effort, gives them some latitude of choice.

We're also looking at alloy steel to see if that imparts an additional resistance to some aqueous forms that the plain carbon steel wouldn't have.

We're looking at this intermediate category of materials that, again, might be used for the outermost barrier in a more aggressive environment, and again, particularly the Monel 400, which is a nickel-base alloy, containing a significant amount of copper, and its score of --a copper-nickel alloy that contains a significant amount of nickel.

Again, carbon steels are very susceptible to microbiologically influenced corrosion. Many different kinds of species attack them. These materials are also attacked, but it appears maybe it's a more restrictive species that would attack these, whereas there are many species that will attack the carbon steel family.

Also, these materials have a benefit of having some resistance to localized corrosion. When they pit, they tend to pit very shallowly, and so that's perhaps the kind of pitting that one can more easily design around.

Again, as I mentioned earlier, we work closely with the man-made materials activity, and because the man-made or introduced materials can significantly influence the water chemistry, and so these were very influential in determining bounding environments for our testing program. And again, particularly hydrocarbons that might be brought into the repository, some of their products, if they're left behind, can react with water and form carboxylic acids. These materials also usually contain small amounts of sulfur or nitrogen, and not only can the hydrocarbon act as a fuel source for microbes, but then the sulfur and the nitrogen are there as redoxible species, that they can supply electrochemical energy for the microbes.

Again, the microbial metabolism that again, so much human activity can, in addition to the man-made materials that are brought in there, can cause quite a significant change in the environment.

On the other hand, the concretes and grouts can go the other way, where they form a more alkaline media, and this can actually be beneficial to some of the corrosion concerns.

Again, what we've outlined is sort of bounding environments that we're going to be using for the five-year corrosion that I'll be discussing just in a little bit. But again, we wanted to focus this down to a few number of environments that we would perform these longer tests with a full matrix of specimens. We've selected a dilute groundwater, like the J-13, which would be our base case. We would concentrate that groundwater, which would simulate a dry-out and a resaturation event. And again, that concentration factor would be on the order 20 to 100X.

Some studies that were done some years ago at Los Alamos and Livermore indicated that that's a reasonable concentration, and, of course, it will depend from one because the various solubilities of the ionic species are different of just what the concentration factor for each will be.

Then to simulate some of the effects of the man-made materials, we can chemically simulate some of the microbial metabolism products by lowering the pH. The ones that are usually a concern to metals are the ones that cause an acid reaction in the water.

On the other hand, we can test it in the high pH range, perhaps as high as 12, to simulate the conditioning by the concrete, grouts and other cementitious products.

We will also test at 60 and 90 degrees Centigrade just to balance sort of the range where we have a lot of aggressive reaction of different metals.

Okay. Now I would like to go into the status of our experimental work. And again, we have a number of corrosion testing activities that are underway or proposed for the very near future. Five-year comprehensive corrosion test; we have two electrochemical tests, one to determine critical potentials and another one to do some companion testing to this under electrochemical control. Then thermogravimetric analysis studies, some fracture mechanics crack growth studies, MIC, microbiologically influenced corrosion scoping studies. And then to do some work on the radiolytic effects of the corrosion, essentially what the radiolytic threshold is.

And again, all of what I've talked about in most of my presentation will be covering the disposal container materials, but I do have one chart on what we're proposing to do in the basket materials.

Then we start with the five-year comprehensive corrosion test, and the five-year, the significance of that is that's the time essentially between now and licensing. In many instances, we hope that some of these will continue beyond the five-year period.

Again, it's going to require a large number of specimens. At our workshop, we went around to poll the people that were present and ask them to put in numbers for these different parameters. As we said earlier, we have several candidate materials. We'd have specimen types because of the testing for the different kinds of corrosion. We'd have to have some replication.

Water chemistries, well, we narrowed it down to four. Two temperatures; exposure regions because we want to test some fully immersed in the water, some in the human vapor above, perhaps some at the water line; some metallurgical conditions, at least base metal and weld, and then, of course, you can have a number of different welding parameters here; and then evaluation intervals, that you would do this--this is what we call planned interval tests, where you expose a number of specimens at the beginning and then periodically withdraw sets of specimens and so that you don't have to wait for the full five years to get all your data. Plus, you want to be able from a planned interval withdrawal that you can get the change of corrosion rate with time.

So when one put twos and threes and fours and all the numbers that went around there, it very, very quickly became a number in the thousands.

Again, I won't dwell too much on the next two slides that tells some of the details. I'll just say here that the different configurations for the weight loss specimens we can get the weight loss for general corrosion. We can observe the attack here to discern if we got pitting corrosion attack, intergranular attack, or whatever.

We'll have some specimens that will be stressed, so again, it would be amenable to determining stress corrosion resistance of hydrogen embrittlements and on having a stress present.

We have some specimens that are deliberately creviced, the study of crevice effects. And then we'll have some galvanic experiments, specimens where we have two dissimilar metals that will be contacted with one another to determine the effects there.

And let me skip the next two slides to let you know where we are, and why don't we just say, it's really, really simple to draw this and talk about it in a room like this, but the devil, like anything else in human endeavors, is always in the details. And when we put one of these together, this has become a quite large project.

We determined that we needed to do 44 separate test cells, and again, the reasoning were water chemistries two temperatures. But also, when we're testing the different families of materials, when we're testing the single effects, we want to keep them separated from one another. When we're testing the galvanic effects, of course, we want to put them together. So that was the reason we needed a large number of cells.

We wanted also to have a large capacity so, again, we could accommodate a large number of specimens, and then knowing full well how things go, if this successful, we would likely want to add specimens and add other things to this as time goes on.

Again, we're about 50 percent on that. We're finishing up some of the details on the designs this month.

We had to--to accommodate the space, we had to refurbish a large laboratory, and again, there were a lot of utility concerns to do there.

We had a large order of procurements, some 12,000 specimens, and so we have to competitively bid that because of the dollar amount. And now we're going ahead--that's for all the test specimens but the galvanic ones. This was a little bit more specific design because as someone mentioned earlier here, we wanted to bring in the area effect, which is very, very important, and so we had to develop our own design for that one. And so we're nearing completion on that.

We hope to have all of the design of the experiment, procurements done by the end of May, and our major goal is to get this test underway this fiscal year.

Again, which goes along as a companion is the electrochemical studies, and I'd add that the five-year test is Ed Dalder and Greg Gdowski, are the principal investigators for that.

And I'm going to paraphrase Steve Brocoum's title the other day, and I'll say that this you might call the immersing electrochemical strategy because electrochemistry, of course, is what drives corrosion reaction. So it's fundamental, very critical for us to do the electrochemical work.

And again, it's particularly dealing with the corrosion-resistant materials as determining what the potential region where the passive films are stable and where they break down; in other words, determining where the critical potentials are.

And the benefit is that we can do these kinds of tests in a couple days, and these supplement the results of the five-year test because we want to do these over a much wider chemical and metallurgical range than we were able to do with the five-year test. And these are the kinds of tests that lend themselves very readily to factorial designed experiments, and so we can study the single effects and the combined effects of a large number of variables.

And again, I might point out some of these where we're trying to address, for instance, that question about inner and outer barriers, and particularly if you have an outer barrier that forms--that's not completely bonded to the inner barrier, and it sets up a crevice between the two, what the corrosion products of that outer barrier would do to the localized corrosion resistance of the inner barrier.

We're also going to do some corrosion testing under electrochemical control where we'll measure the corrosion potential with time. We'll do some testing where we're above and below these different critical potential determinations, and this is very important to the model development, as I'll talk about a little bit later.

And again, some of these we'll do for a short period of time, but then selected ones will run for a much longer period of time.

And this is the work that Ajit Roy, who's on assignment from B & W, and he's working at Livermore with us. One of the advantages of joining the M & O was that we could arrange that kind of a transfer.

Now I'd like to talk about thermogravimetric analysis. Again, this is work Greg Gdowski is the principal investigator of.

We have a unit, and one could, I think, best think of a TGA, thermogravimetric analyzer, as a very, very sensitive microbalance. And what one has is the test specimen here and compare it to a tare. And in this reaction zone, we can pass a mixture of dry air and humid air to achieve whatever level of humidity we desire.

And again, this is another one that looks so simple schematically, but the devil was in a lot of the details because we effectively had to rebuild this unit so we had a large area where we could maintain a constant temperature and a constant humidity level.

Anyway, we have surmounted that obstacle, and we're on line with this set of experiments.

Again, the kinds of things that we want to do with it, is again, it's important to determine where this temperature-humidity transition point is, where we go from a dry oxidation phenomena to an aqueous corrosion phenomena.

And again, the results of these studies will be used to select conditions where we'll do longer term testing. And again, when we get to some of the models, you'll see why that's so important.

We feel that the aqueous corrosion in human environments is going to be dependent on--it's going to vary from one metal to another where the transition point is. The gaseous species are going to be important. I think Bob Andrews showed a slide again where you showed gas like SO_2, or any one that tends to be reactive with water is going to have a big effect on where that transition point is. Not only is it going to affect the humidity results, it's going to affect the pH because of the acid hydrolysis of those kinds of gases with water.

And again, those are reasonable kinds of species that one might have in a repository that's been contaminated by some of the man-made materials and human activity.

We'll also be dealing with surface condition. We'd expect this transition to be a factor of surface roughness, whether or not the corrosion products are there. And in particular, that there are hygroscopic materials present because again, their effect on preferential absorption of water.

We'll do this over a range of temperatures. And previous studies of this kind of analysis have usually been limited to atmospheric types of corrosion, where we're dealing with ambient temperatures and humidities, and again, often with not much control because you're dealing with mother nature.

We will do this set of experiments over a temperature range where again, we will be in the very low side of oxidation where we're having it transition toward aqueous corrosion and to determine some of the effects there of temperature and the corrosiveness of individual gases.

Again, this work is emphasizing the outer barrier materials, carbon steel, and the Monels, and the 70/30 types of materials that should be very sensitive to this kind of technique.

And again, as a representative data set that we've obtained where we've tested now carbon steel, 1020 carbon steel, in a range of humidities, from very dry to very wet, and you can see here where we have almost no response, again corresponding to a very slow oxidation. But somewhere between 75 and 85 percent relative humidity, we get this enormous increase in the weight, indicating that there's been a significant reaction of the atmosphere with the metal.

This compares to some of the data we've used, and Bob Andrews talked about it, 60 percent relative humidity as being critical, and in other pieces of literature talked about 70 percent. Well, anyway, again, I think that when we're dealing with the kinds of--that we are, we have a machine surface and so forth where I think the--an 80 percent, let's say if that's the transition point, would be an expected value. I think it would go to a rougher type of surface or pre-oxidized specimen. We would see that this critical humidity would fall.

Let me switch--I'm giving you, I know, a little potpourri of everything we're doing. We're doing some work, again, under subcontract at Argonne National Laboratory, where we're studying the stress corrosion behavior using a fracture mechanics approach. And again, stress corrosion, again as I think Hugh mentioned in his talk, we're doing all we can to minimize stress, but particularly around that final closure weld, there will always be some residual stress that's left in the material regardless of the welding processes used. And so we expect a lot of stress corrosion effects, if they exist, would be in and around welds.

What we're doing here is to take what's called a compact tension specimen, and it's been pre-cracked in a fatigue operation. And the idea there is to take the stress and to magnify it so that we have a very high stress intensity in that crack region. And then we use this in an apparatus. It's very, very sensitive to crack growth. Actually, it measures the crack growth by the change in electrical resistance of the specimen.

And one tries to generate a curve like this where again, you take what would be the critical stress intensity to fracture a material without environmental considerations; in other words, just in the atmosphere, you'd get a K_{1c} value.

If you have an environment that causes stress corrosion cracking, that will lower the effective K_{1c} to what we call an index of K_{1scc}. And this is important to the design team because they will try to keep the stresses well below this K_{1scc}.

And what one does is to measure the stress intensity, measure the crack velocity I should say as the stress intensity is applied to the specimen. And to make the experiment go a little bit faster, one varies the load so that one can start to generate different points on this curve using the same specimen.

These are very rather costly test specimens, but particularly the apparatus to do this is very costly, so one is limited to the amount of parameters that one can examine at any one time.

And some results that we've obtained to date on, again, the very highly corrosion-resistant materials and tested at 90 degrees Centigrade J-13 water, we've measured very small crack growths. And these would correspond to, I believe, points in this area right here.

And now what we want to do next is to generate the rest of that curve to see if, indeed, we are down in this region, and we can generate this and see if we can determine where this is. And remember, this is a logarithmic curve, and so we get to the point where we've just really exceeded the sensitivity of the apparatus.

Again, we want to continue these kinds of tests in other environments, particularly in aggressive environments where we'll start to see a more discernable effect of stress corrosion cracking. And again, because of the costliness of this approach, we're looking at different alternative ways of getting the same kind of information to supplement these results. Again, that would be, if you're familiar with some of the stress corrosion testing terminology, that would be a --type of specimen.

Let me now go to microbiologically influenced corrosion, and again, the major thesis is based on an NRC-sponsored study that was performed by Professor Gil Gesey at Montana State University, and he had the foresight to go through all the materials that we had at that time considered as candidates, and he must have been reading our minds of where we might be going next because he summarized, I think very beautifully, what the propensity for different kinds of microbes to attack our candidate materials.

And again, as I mentioned earlier, there are just many kinds of bacteria that can attack carbon steel. These range from the aerobic to the anaerobic.

And again, one of the factors in dealing with MIC is that although you might start out with a bulk chemistry that could be either very alkaline, different kinds of microbes can work conjointly and reduce that pH from very alkaline to very acid.

They can also work the same way with the redox condition. You could start off with very aerobic, very oxidizing conditions, but near the metal surface, they can become very anaerobic. Again, many types affect carbon steel.

When we work into the 70/30 and the Monel 400s, again, we're more limited, but sulfate reducing bacteria are very severe for those. And again, that's very understandable because the sulfides that--sulfide films on these kinds of materials are much, much less resistant, offer much less corrosion protection than the oxide films that they would have replaced.

We move on to the higher nickel alloys, nickel chromium alloys, incoloy 825. There were just two studies that showed, again, some propensity for sulfate reducing bacteria to cause some damage.

When we move into the Hastelloy-C types of materials, it appears that these are immune. At least Dr. Gesey didn't document any failures of those. But it's pointed out that pure nickel is attacked. Of course, when one adds a lot of chromium molybdenum, then one does increase the resistance in acid media, which the bacteria would be creating.

And as I mentioned earlier, the engineering materials to date, it seems that titanium is so far immune. But again, a lot of materials that we thought a few years ago weren't susceptible to MIC have been found to be susceptible. So whether that trend continues or not remains to be seen.

Again, our plans for MIC evaluation and testing, as Dan Bullen mentioned, we had a very exciting microbial activity workshop last week in California, and we brought together materials people and also a lot of biological people. We plan to evaluate the Yucca Mountain site to determine what the native microbial populations are, what microbes would be associated with the man-made materials and other human intrusion into the repository during construction and operation. And then, as I said, many of these bacteria can live in consortiums where they actually--one, the metabolism products, for one, just provide food for the next set.

And again, it appears that the same kinds of films that promote aqueous corrosion--again, that's where we talk about those critical humidity levels--can also act as biofilms. So there's a real tandem approach here between just ordinary, what I'll call abiotic corrosion, and microbiologically influenced corrosion.

Then we plan to do some experiments where we'll actually have real live microbes, if you will, that will be exposed to our test specimens, and then we'll compare those results with others that we've generated in a more conventional chemical simulated laboratory.

Okay. Now I would like to go on to radiolytic effects. Again, what we propose to do there is to set up a series of experiments to determine what the critical radiolytic threshold is. Again, when we consider that we've got perhaps maybe five inches of metal barrier between the waste form and the external environment, roughly an inch of material reduces the gamma dose rate by an order of magnitude.

Now, whether that number that would actually penetrate the five inches of material is significant or not, we will have to determine.

And again, the way we plan to do this is to set up some rather sensitive experiments. The limit of discernible corrosion attack will be one of the criteria, the changes in corrosion potential because the species that are usually produced in a radiolytic reaction are oxidizing in nature and so they'll raise the corrosion potential, and then these kinds of chemical species can also be readily determined analytically. And we plan to start that either the end of this year or early next year, and again, emphasis is on the outer materials and at the onset, the carbon steels and the copper-nickel alloys.

Again, we do want to say a few words about what we're planning to do in the basket materials, since the radiolytic effects are a major issue there. We have, again, completed an SIP on the basket materials, and right now we're considering different environments that might be used to simulate the long-term chemical environments. And again, these will be--because near the fuel source, these will be very high in these different kinds of radiolysis products.

And we're planning to study a number of different candidate materials. This is the work that is being planned by Rich Van Konynenburg at our laboratory, and he's going to investigate aluminum, copper, stainless steel, that would be dosed with boron, and he's also examining the commercial grade of zirconium, 702 zirconium, which has a natural concentration of hafnium in it, around 2 to 3 percent. Hafnium is a very good neutron absorber. This is the way things come in nature; when one tries to produce zircaloy, one has to go to a very elaborate series of separating the volatile halides of hafnium and zirconium to produce zirconium zircaloy where you don't want any hafnium present.

Then he's also considering some ceramic materials, a range of ceramic materials that could be doped with gadolinium and other lanthanides. Again, then, from his shorter term experiments, he would determine what would be feasible to do in the longer term.

Okay. Now I'd like to go into the second half of my presentation and talk about what we're doing in the modeling area.

Again, our modeling work is basically governed by the degradation mode, just as our testing activities are, and I believe the kinds of models that one would develop with the different forms of corrosion would be as given here.

For oxidation, aqueous corrosion, intergranular corrosion, I think those could be handled deterministically. In other words, these could be related to parameters such as temperature, pH, chloride concentration, oxygen content and so on.

When we get into the localized corrosion, pitting corrosion and crevice corrosion, we have, again, deterministic factors, again like temperature, oxygen, pH and so on, but also there's a probabilistic term. And again, because these are so sensitive to small electrochemical perturbations, that even with rather sophisticated tools, we can never discern just exactly what the electrochemical change is from once site to another. So one way of handling that is in the probabilistic way.

When we get into the environmental accelerated forms of corrosion, not only do we have the deterministic form, we also have the probabilistic due to the electrochemical potential variation. But also with these, because they're governed by the amount of stress or strain, that's another term that has to be handled probabilistically because we can't quite measure it down to the micro dimensions that we need to.

Then a non-corrosion related degradation that can be important, particularly for some of the highly corrosion-resistant materials, would be phase and stability. One of the prices one pays for adding so much chromium molybdenum to some of the nickel-base alloys is that it does increase the propensity for certain brittle phases to form. Again, particularly in and around the welded areas. And again, that can be handled deterministically, but I believe that that also would need a probabilistic treatment.

Again, some work that Dave Stahl did and was presented at the Kyoto conference last October, was that he went through the literature and looked at the oxidation phenomena and the aqueous corrosion phenomena over a number of investigations where there was--where it was done at fairly long times; long times in this case could be months to years. And what he developed was two equations, and they're like an arrhenius type of equation where you have a pre-exponential and then an E to the minus Q over RT type of expression, but it also added a correlation factor to deal with time because it's been observed so many times, as when the initial rates of oxidation or corrosion are high, but with time the rate appears to decrease and reaches more or less a steady state value due to the protection qualities that are offered by some of the corrosion products.

And so he developed these two correlation factors, where, again, this is the depth of penetration and t is the time, and the large t, the capital t, is for temperature. And then he calculated the wastage of materials and found out, of course, that that's going to be very heavily dependent on the thermal load.

Considering the high thermal load case, as you might expect the analysis, that there would only be a few microns of penetration over a very long period of time. And again, that's one reason why from a materials point of view, the high thermal load appears to be quite beneficial.

Now, as we consider the lower thermal loads, he considered that the transition from dry to wet occurred at the 60 per cent relative humidity mark, and he used some projections of temperature and humidity that were made by Tom Buscheck at our laboratory. And then his estimates were that, again, that these would be rather favorable numbers even though you had aqueous conditions obtaining in the repository, that there would only be 20 millimeters penetration in 5,000 years, say 20 millimeters out of a container that was 100 millimeters or 10 centimeters thick.

Now, there's a big however that we'd like to put onto this, and that is, of course, that this equation right here that he used applies to near-neutral pH air saturated water, and again that this exponent of the time that appears to indicate parabolic growth of the film. And again, if we had conditions where that wasn't true, that the film spalled off and that this exponent went to a higher number and say approached linearity, then, of course, the penetration is much, much higher.

And again, another case where this equation would not be valid, not only would the time-temperature be changed, but also the exponential, the pre-exponential and probably the exponential term, would be if we had more aggressive water chemistries. Again, like microbial activity that would move the pH to a lower number.

Now, I'd like to talk about the model that Greg Henshall at our laboratory has been developing for pitting initiation growth. And again, this is primarily dealing with right now the stochastic part of it, but we're working on developing the right electrochemical experiments. Then we get not only the stochastic part, but the deterministic part, and also to give a reality base to the stochastic part, as you'll see in just a few minutes.

This is the kind of model that would apply to the corrosion-resistant materials, either nickel-base or titanium-base, and again, we're working to get the experimental input that's so necessary.

Again, the physical basis of this model is that when you have a passive film on a material, it's not everywhere stable. There are places where the film breaks down locally, and in such cases, that could be a site where there would be a birth event for a nascent pit. It's also possible because the film breakdown and the film repassivation connectics are competitive with one another, that that site would repassivate, and that would correspond to a death event of the pit.

And very much like one would analyze individual dislocations coming to a metal surface, these events, whether they're canceling or they would be repeated, comes in this kind of analysis. If you take this and over various time steps, if this birth event is repeated, in other words if this keeps growing a little deeper and a little deeper, you eventually get to the time when we have the unstable pit now becoming a stable pit.

Now, on the other hand, if it's one--if one birth event is then followed by a death event, then that pit no longer is active, and then we don't have a stable pit growing at that particular site.

Again, he set up his model with--let me just keep this on here--with sort of four parameters, a birth parameter, a death parameter, a critical age and then a growth probability.

Again, where he's compared this is in the literature; Shibata in Japan did an experiment where he exposed three or four stainless steel, and this is essentially a C water composition, and then he applied a potential, and this would be the kind of potential where you would be above the critical pitting potential, so you would readily initiate pits on a metal surface.

And you can see the time steps here are very, very short, that in a matter of seconds, out of 72 specimens, all but seven of them showed pitting. And then he measured--he measured the number of pits and got a distribution like this.

What Greg did was to do a Monte Carlo simulation, where he generated random numbers, and he arbitrarily set what some of his four parameters would be, and then he determined that he could generate the same kind of--rather same kind of general distribution as was done experimentally.

The goal of his work is to generate what we call a damage function, where you take all these parameters that we're dealing with in our rule of chemistry and material science of temperature and oxygen content, fluoride content, electrochemical potential and so forth, and you're trying to translate this into the language that the performance assessment people best understand, and that would be a distribution function of the damage and how this distribution function would change with time, as he's shown schematically as it would go, and eventually the pit depth would exceed the container wall thickness.

And again, he's based this again on a previous experimental approach that was done in Canada with aluminum exposed to tap water. And again, one sees at very early times, one generates a large number, but very shallow pits. But as time elapses, the distribution changes, so that one then starts to get this hump, if you will, moving out to a smaller number of pits, but deeper pits.

And what then Greg tried to do was to simulate this kind of distribution that was produced experimentally and to try to do this on the computer because again, these are very, very time consuming events to look at the number of pits and to measure the depths and so on, and to try to consider doing this for a large number of environments and a large number of materials would be just astronomical.

And again, what he did in this computer approach was again, with again some of the assumptions of what would be realistic parameters for those different--or terms of what the distribution would be with time. And again, one sees one goes from the very beginning, a large number, but very shallow pits. And then with time, it becomes a distribution now of a lower number, but much deeper pits.

Again, what we have to do again is to put some physical and chemical reality on this, and so into the next set of computer experiments he did, was to actually assume that--he used three parameters, potential, temperature and chloride concentration, and how those might change with time and what their effect would be on pitting.

And so he has--let us assume, for instance, that the potential went into the what we call the noble direction, electrochemically. This would be the tendency, for instance, of the metal if it was going to pit, where now the corrosion potential has exceeded one of these critical conditions, critical pitting conditions.

And then the chloride concentration, we're going to let it increase. Again, we doctored this so we'd have some results that would be meaningful as far as generating a pitting model. To do modeling, you have to have something occur. And again, the one thing we do know is that the temperature will steadily decrease in the repository.

Again, these four terms have a rather complex interaction with one another. Again, the birth probability, again, you see is very high. We initiate a lot of little pits at the beginning and not so many later on. But then as conditions, because these are now the few that are generating--some are growing and most of them are dying--but here now as the environmental conditions become much more aggressive, then you see that the birth probability increases markedly. And conversely, the death parameter has a steady decline because again, many die at the beginning, but then the few that survive and go on cause a decrease in the death parameter.

The overall pitting growth probability, then, steadily increases because, again, primarily driven by this increase in--of the environment, and also his critical age. Again, it increase initially, as you start with the large number, but relatively small pits, that only a few then grow. But then as conditions become more aggressive with time, then that critical age decreases.

DR. LANGMUIR: Two minutes left, Dan.

MR. MCCRIGHT: Okay. Well, let me just say that we need to add a dose of reality to, again, determine whether these kinds of changes would, indeed, occur, and what the effect to give a more substantive basis to his calculations of what those parameters would be as a function of a range of environmental and metallurgical presentations.

Now, let me just move into the last phase. We are doing some work in other activities. We are completing degradation mode surveys on titanium. We're doing one on welding microstructures, and we're planning to do one on galvanic effects.

We've also recently completed the engineering materials characterization report, which was a project superstone, and again, it summarizes, puts together under one set of covers a large body of information on our candidate materials and what we know thus far. And we do plan to do updates on that as test results become available.

We are also working, again very heavily, with the design team and other people in the program to come up with a defensible materials selection. We'll likely draw on a lot of the same work that we did for the conceptual design materials, but again, there were seven major categories and 34 individuals selection criteria, but I think the weighting package will probably be different; again, between the inner and outer barrier and between the designs for different thermal strategies.

And then finally in summary, again, I went through the metal barrier SIP and outlined the work that we have planned for the next five years. Talked about candidate materials and bounding test environments, experimental work underway in several areas. We have developed a model in just two areas, but as the intensity of the experimental program picks up, we expect that the modeling program will pick up as a result of that.

We are continuing our efforts on completing the DMS, and we also made some progress on the methodology for materials selection.

And let me just leave you with my outlook for the next several years. Again, of course, I as an optimist, that I expect that we will have a very high level of experimental activity for the next several years. Again, that's a very important basis for the materials recommendation, and then for our performance models.

We expect that the modeling activity is going to become much greater as we work together with the experimental program to derive realistic models. We will be interacting ever so much more with the other elements in the program.

Again, we spent a lot of effort in putting together this metal barrier SIP, and again, we made the effort in planning the work, so now is the time to work our plan.

DR. LANGMUIR: Thank you, Dan.

I think we will go on. We're a little over time now, and hold your questions, please. Oh, I'm reminded that Clarence Allen would like to make a comment on the previous presentation.

DR. ALLEN: This is Clarence. I would like to make a statement formally for the record. In my question to Dr. Bhattacharyya this morning, I asked him whether the tie-downs for the waste canisters were designed or were looked at on the basis of recent records of accelerations, earthquake accelerations approaching and sometime exceeding 1g. Those observations are, of course, correct. But in asking the question, I did not mean to assume that accelerations greater than 1g would necessarily occur at this locality, in these rocks at this depth, nor that such numbers should be used necessarily in the design.

What I was asking was whether large ground motions have been assumed and the stability tests for the--as yet for the waste canisters, and the answer was evidently no.

DR. LANGMUIR: Thank you, Clarence.

Our last presentation before lunch is effects of engineered materials on repository performance. The speaker is James Houseworth.

MR. HOUSEWORTH: Well, there's currently a wide variety of materials that are being used for ESF construction, and we expect a similar suite of materials will be needed for repository construction, if a repository is built. And the natural question that comes out of this use of materials, from a performance standpoint, is what effects these materials may have on the ultimate performance of a repository.

To address this issue, I intend to highlight the following points. First of all, not all the materials introduced are necessarily of interest from a performance standpoint. So I wanted to highlight which materials that we're particularly concerned with, then go over some of the organizational interactions on the project concerning engineered materials, the flow of information from the scientific organizations to performance assessment and design.

We also have an established process now for interactive communication between performance assessment and the ESF design, which I'll go over the processes involved there, then get down a little more into the details of the performance assessment aspects of the work, the steps that we've used in our analysis of site impacts and particular engineered materials, what's the hierarchy of our analyses and addressing these questions, and then go over an example, which highlights some of those processes in terms of the discharge of organic material in diesel exhaust.

Well, like I said, not all the materials that are currently being used are necessarily the consequence in terms of performance. And so there's been a classification of materials that's used to help distinguish some of the controls required for the different materials. The first two, the temporary and permanent categories of materials, are used to help establish if the material may impact or fulfill some nuclear safety function in the repository, including preclosure radiological safety functions.

Ironically, permanent materials don't necessarily remain in the repository, so in performance assessment, we've established another set of categories, which are what we've called non-committed and committed materials. And in terms of the committed materials, which are materials remained in the post-closure repository, those are basically the materials we're interested for post-closure performance assessment.

There's only a limited subset of all these materials that are used that are committed, and principally, the first three items are ones that are either planned or are being used now in the ESF, the construction water, exhaust from diesel equipment, ground support. Additional items, which may have more impact for repository, being the invert and rails, backfill and seals.

If we only consider the currently planned items that may be committed, the list narrows to primarily diesel usage and ground support items. And under diesel, of course, the only materials that we might expect to become committed are the materials that intentionally discharge in the exhaust. And this gives us a list of some of the different items. And under organics, we have hydrocarbon vapor and diesel particulate matter, and also some inorganic gases, emissions of nitrogen oxides and sulfur dioxide.

Some additional operational losses and spills may occur due to losses of diesel fuel, oil, coolants and the like.

The bulk of the committed mass of materials is probably going to be under the ground support, and currently we're using almost entirely inorganic materials shown here, which are the steel for rock bolts, steel sets, wire mesh. Cementitious grout, that is the calcium alumina silicate grout materials and similar materials for shotcrete for--applied under poor ground conditions.

There also have been proposed a number of organic materials. However, there's been almost no use of these materials except for a certain limited application of wood for blocking of steel sets. But, of course, there may be some situations where some of these could be needed. So they're certainly an important issue.

The way the project currently--the current basic way information flows in the project is through assessment of materials in the scientific program, which encompasses these different sub-elements, which then feed information to performance assessment and on into design. There's also been some work in terms of feedback from performance assessment and design to help establish what materials we do need to have looked at from the man-made materials group.

And in addition, performance assessment has been in communication informally and also in terms of some TSPA analyses, such as what Bob presented earlier; for example, thermal loading issues.

The part of this diagram, though, that's probably the most mature and the processes have been basically set in stone practically through the QA program is this performance assessment, determination of importance through ESF design. And I'd like to go over through the details in that process of an example of how PA communicates with design.

Basically, performance assessment looks at the possible issues that could come up to affect the repository performance, and those elements are fed into a determination of importance analysis, which also looks at a number of other issues in addition to performance issues, such as test interference issues and perhaps radiological safety issues. And all those issues are incorporated into a determination of importance analysis that is then given to design for them to incorporate as design controls.

The steps in the process from a performance assessment standpoint is that ESF design first provides to the systems group where the determination of importance work is done, a preliminary design package including description of field activities associated with the design. Then they classify these design items to determine what portion or what parts of this design are required to be included on the Q list for quality assurance controls.

Then the systems group determines the need or not for a performance assessment evaluation of that design, typically if it's a new activity or something substantial were involved. And then we look at those items and activities to establish what controls might be required from a performance standpoint.

These are documented in the determination of importance evaluation under the quality assurance program.

The controls, then, are fed back to design through this document and implemented in the final design package specifications and drawings.

A total system performance assessment is the way we would like to address all the issues in terms of ultimate consequences. However, TSPA model right now is not sufficiently detailed to address a lot of the specific questions that we're asked to analyze in terms of design.

And so instead of trying to attempt to approach this from a system performance perspective strictly, we've developed a set of what might be called bounding analysis, hierarchy type of analyses, which we attempt to bound essentially the effects of these materials or activities on performance.

And the first step in the process is to identify, characterize materials that may impact performance. It's certainly possible that some of the materials we used would just have not had any impact; for example, if we were just replacing silica underground, or something.

Then we would go to the step two, which is to bound the perturbations to the ambient conditions. This is in terms of the effects on things like saturation, pH, geochemical conditions, and then to compare those perturbations to the ambient conditions with the known expected range in ambient conditions, or estimated expected range in ambient conditions.

And if that is--if we can't bound the problem at that point, we can go down through these steps to a more detailed analysis where we look at effects on performance-related parameters, such as corrosion rate and diffusion, diffusion parameters and the like and look at it in terms of these--compare those changes in terms of the performance-related parameter uncertainties.

Then we can ultimately, if we would need to, and we haven't gotten to this step yet, but look at effects of subsystem or total system performance and compare with the expected uncertainty in the total system performance.

As an example of this process, I'm going to present work we've done recently for excavation of the exploratory studies facility north ramp, and discussed the potential impacts of discharge of organic material in the exhaust.

So under the first step, we look for potential problems may exist due to the discharge and retention of these materials underground and to--part of the assumption, the conservative assumption to consider this, we assume the dissolution of all retained organic material and migration through the waste package.

So if that does occur, what potential effects might we have? We have potentially if enough organic material were in place to some effects of locally reducing conditions, which could perhaps reduce the waste package corrosion and radionuclide solubility limits. However, there's also a possibility that we may stimulate microbial activity, generate locally acidic conditions, which would have a detrimental effect in terms of corrosion and solubility limits.

There's also potential for organic ligand generation, and the ligand got shifted in column two, but its increase of aqueous organic ligand concentrations, which may result, again, in increases in the radionuclide solubility limits and waste package corrosion.

Under organic colloids, there's a potential for generation of those, with emplacement of organic underground, and again, there's some potential for affecting mobile radionuclide solubilities and transport.

So clearly, there's some issues that we have to address. So we'd go down to step two, which was to look at how much of this emplaced organic may actually affect or perturb the ambient conditions in the existing environment.

And given the fact that we want to be able to provide the design some estimates for the discharge quality in terms of the diesel exhaust, we need to get some details from them, including the diesel utilization profile, planned diesel utilization profile along the ramp, the estimated exhaust volume per unit at time of operation. And to do some bounding transport calculations, we have to have some picture of the relative positions of the north ramp potential waste emplacement locations.

Carry-on on this calculation of the perturbations to ambient conditions, we need to make some assumptions to address the source of organic.

First of all, we assume all the diesel particulate matter is deposited on tunnel surfaces; that is, everything emitted in the diesel exhaust is deposited. That's based on the assumption that the perturbial that's in the tunnel atmosphere would provide contact with the walls. And lacking specific knowledge on the deposition, mechanics and efficiency, we just assume that all would deposit. That deposition is assumed to occur at the point of emission, which is basically just a simplifying assumption to establish the distribution of the deposits along the ramp.

We also assume that the hydrocarbon vapor emitted in the exhaust is ventilated, and that is based primarily on the fact that the vapors have relatively or very little aqueous solubilities and would be at low vapor pressures and would not result in condensation phenomena occurring.

And finally, again, a conservative bounding assumption is that with complete dissolution of the retained organic material.

So with that source term, we're able to then go on to a transport calculation where we attempt to estimate the perturbation resulting at the nearest waste package from this deposition of organic material in the ramp. And this calculation is based on the following assumptions and models: First of all, we assume that the fractured rock medium behaves as an equivalent porous medium, which is primarily a simplifying assumption, not a bounding assumption.

Then we go on to use a 3-D advection-diffusion transport model for the upper part of the ramp, which lies relatively far from waste emplacement. And in the lower part of the ramp, where the tunnel is within 37 meters of waste emplacement, we model as a 1-D advection-diffusion transport process.

Then given these, the source term, these transport models would compute the peak dissolved organic carbon concentration at the nearest waste package.

With that, we need to also have the estimated variability in order to compare those results with what we might want to control the change in dissolved organic carbon, and we have to first come up with an estimate of the dissolved organic carbon variability in the system because we do not have ambient measurements of that in the unsaturated zone.

So the estimate was generated based on measurements from the saturated zone and other groundwater systems, and then based on other compositional measurements in the unsaturated zone, we estimated that the variability would be at least 10 per cent.

So that sets the variability of the organic in the unsaturated zone, and that is what is compared, then, with the peak DOC that would arrive at the nearest waste package.

The bottom line of this was that for excavation of the north ramp, the diesel exhaust was not found in terms of the organic discharge to create a perturbation above that ambient variation. However, for continued use of diesel throughout the full loop of the ESF, some emission control technology would be required based on this kind of an analysis.

So some future improvements to this type of analysis, first of all, there is a diesel testing program that's slated to be begun in the next few weeks in ESF north ramp to check assumptions on retention, and following that, there are some plans to also do some swipe tests for direct measurements of deposition in the ramp.

Additional work being carried out in the man-made materials group that can then be fed into our work are analog measurements of diesel exhaust products and microbiota in the Rainier Mesa, some theoretical modeling of the behavior of organic matter from diesel exhaust in a thermally perturbed rock-water system, and an experimental study, as well, of the water-diesel, fuel-fibercrete system at 200 degrees C.

DR. LANGMUIR: Thank you. We have about one minute, and as chairman, I'm going to take my prerogative and ask a question first.

I was delighted to see that you finally got to looking at an analog here. It seems to me that there's an awful lot of tunnels out there where diesel machinery has been used in the past in volcanic rocks, and whether or not these products, which could be important to positive performance, organic complex, in particular, reducing conditions, are an issue or not, you can go out to a lot of these sites and examine the materials there years later and see what's developed, what the products of weathering might be of the organics, where the carboxylic acids are occurring, that sort of stuff.

I would hope that this got high emphasis, this analog side of the program. Otherwise, much of what I'm hearing from you is an awful lot of guessing of what might happen in models without much--with no data. You don't want to answer, just going to nod your head?

MR. HOUSEWORTH: Oh, as far as the priority?

DR. LANGMUIR: Yes.

MR. HOUSEWORTH: Well, that work is actually--yeah, it's already underway, and there's a report that's been issued from Livermore on that subject.

DR. LANGMUIR: On the analogs?

MR. HOUSEWORTH: Well, for the Rainier Mesa tunnel.

DR. LANGMUIR: Okay.

MR. HOUSEWORTH: I'm not certain if there's--I think there may be some further work going on there, but they've already started that.

DR. LANGMUIR: Thank you. We're on schedule. I'd like to hold questions again, beyond mine, that's selfish of me. We'll reconvene after lunch at 1:50 for the afternoon session.

(Whereupon, a luncheon break was taken.)

AFTERNOON SESSION

DR. LANGMUIR: Please take your seats. Our first speaker of the afternoon is Stephen Hanauer. His presentation will be in repository long-term criticality.

MR. HANAUER: With the Board's concurrence, we've reversed the order, and Hugh Benton will go first.

DR. LANGMUIR: Okay. I'm corrected. Stephen tells me that Hugh Benton will be first with his presentation on the same title, in repository long-term criticality.

MR. BENTON: I will be discussing the waste package disposal criticality control program.

I think at first it's important to understand the scope of the criticality problem. The vast majority of the spent nuclear fuel that will arrive at the repository and be emplaced does not have the potential to sustain, to achieve or sustain a nuclear chain reaction.

Our calculations to date would indicate that more than 90 per cent of the spent nuclear fuel in the waste packages will not have the potential to go critical, and that is even without any supplemental neutron absorbers. And the remaining 10 percent can be controlled by the use of supplemental neutron absorbers.

Now, this is assuming that the waste package remains in intact conditions, that the geometry of the assemblies remains intact. We are accounting for the reduced reactivity of the spent nuclear fuel; in other words, we are recognizing that it is, in fact, burned, and we are assuming the capability of full moderation, the presence of sufficient water to moderate completely.

This is the same curve that was shown this morning by Rick Memory, and shows the line that we would achieve if we have no burnup credit, as opposed to the more realistic line with burnup credit of 37 gigawatt days per metric ton.

I will be referring to these three phases, the operations phase, the substantially complete containment phase, and the isolation phase, which are being used as our methodology for the criticality control analysis.

So what is our basic technical approach? First of all, we are designing a waste package that will take care of the criticality control problem through the substantially complete containment period and for the vast majority of the waste packages into the isolation phase. We're doing this through the multiple containment barriers that we have discussed, through the use of materials that have known long-term performance.

Then we're using the repository engineered barrier and the natural barrier features to limit the potential for future criticalities, after the substantially complete containment period. We expect to take some recognition of the dry, unsaturated site and the low potential for water movement.

For analysis, we have divided the time period into the three phases that I have indicated on the right-hand graph. And we're evaluating each phase using a method which is appropriate for that phase. Deterministic for the operations phase, where conditions are well known, a combination of deterministic and probabilistic for the substantially complete containment phase, with more probabilistic toward the end of the phase, and then probabilistic analyses for the isolation phase.

In addition to that, we're performing bounding deterministic evaluations for the entire time period of disposal, and beyond, we are using trending evaluations.

So our approach, of course, a full understanding of the regulations, the long-term conditions of the site and the waste package in the site, evaluate the available methods for long-term performance. We are developing the criticality control strategies based on the upper three, and then applying the three-phase approach to our evaluations.

And we will be reporting this in two major documents, the disposal criticality analysis technical report, and it will be referring in that report to the technical information that will be developed from such experiments as critical experiments, and then the disposal criticality analysis topical report that will be prepared for the NRC.

Rick Memory this morning went over the regulations, so I will not belabor that, but only to point out that the regulations does recognize that if you did have two unlikely independent events occur, that a criticality might be possible, and the regulations permit that.

Now, conditions are changing over time. The isotopic concentration is certainly changing in the spent fuel through the decay of the isotopes. The waste form will be subject to degradation over time. The waste package condistions are being subjected to material degradations, and with eventually the geometry of the assemblies in the waste package will change. There will be some slumping or some failure of the basket assembly, which will allow the assemblies to drop.

There are also changing repository conditions. The temperature and humidity will be changing, which affects the performance of the waste package, and there is some presumably slight potential for water movement, which could provide both moderator and transport mechanism.

There are three basic methods for criticality control. We can limit the fissile material. The fissile material has already been limited through the fact that the fuel is burned, is spent. We can limit the number of assemblies in a package, or we could apply some kind of loading restrictions on what particular burnup and enrichment assemblies we put together in any particular package. We can limit the availability of neutrons to cause fissions. The neutron absorbers are present in the fuel through the burnup process. We need to recognize that. We can add supplemental neutron absorbers, and we can also isolate the neutrons from the assemblies through such design features as flux traps.

The we can limit the moderator, and this could be done through the addition of filler material, which would displace the moderator. During this substantially complete containment phase, we have limited the moderator because we have a sealed system and substantially complete containment.

We could also limit the moderator through rod consolidation, although that is not currently part of the program.

Now, going with that background into what we are specifically doing in our design, we are recognizing burnup credit through the use of the principal burnup credit isotopes. These are 30 of the key isotopes that will have the principal effects on the potential for a criticality.

We're also recognizing the build-in of the neutron absorbers, both those from actinides and fission products.

And we are recognizing the long-term changes in the criticality potential, as reflected in this line, which this runs out to half a million years, and you can see the k effective is initially dropping, and then there's about a .04 delta k increase from about 100 years to about 20,000 years.

For supplemental neutron absorbers, we're considering those in the control panels, where they are in the MPC. An alternative could be control rods, which could be acommodated in most of the PWR assemblies, and in our design, we're accounting for the long-term decrease in the amount of neutron absorber material by adding extra.

For moderator displacement, we're considering a filler that could be loaded at the repository, and if we use filler, we will consider the long-term performance of the filler material, also.

Now, the three phases. During the operations phase, which covers the preclosure period out to about 100 years, the personnel safety from some potential criticality accident is a paramount issue, and we used deterministic analysis.

For the post-closure phase, substantially complete containment, which covers at least the first 1,000 years, the waste package will provide for nearly all of the waste package's exclusion of moderator.

We're using a deterministic analysis in the early years, and then probabilistic analysis of the situations that could affect criticality in the later years.

In all cases, we are considering both external events that could occur, as well as the events that could occur within the waste package itself.

For the post-containment isolation phase, which extends after the substantially complete containment phase, the primary issue is the controlled release from the engineered barrier system of radionuclides. This covers for the first 10,000 years, which is by the currently remanded regulation, and so this number is subject to EPA rule, but beyond the 10,000 years, we are examining for trends. And we're using a probabilistic approach, coupled with bounding deterministic calculations, and again, we are considering both thoes events that occur within the waste package or within the immediate area of the waste package, as well as external events.

We've mentoned deterministic and probabilistic analysis methods. For deterministic, we use them when we know the conditions and they can be controlled. Our accident conditions are being evaluated deterministically. We are developing the design basis accidents through a probabilistic method, considering all the accident, determining probabilistically which of those accidents are credible, and then the most credible accident becomes the design basis accident.

For the area toward--in the initial phase of the isolation phase and the latter phases of the substantially complete containment phase where we're using both deterministic and probabilistic, this is the area which most conditions are known, but not all. Some of the conditions can be controlled, some not. There are clearly some uncertainties. And then for the phase beyond that, the probabilistic analysis, conditions cannot be known exactly, but we can determine the probabilities of the various conditions. At least we can estimate them by a fairly rigorous methodology.

And that methodology is, of course, the probabilistic risk analysis in which we identify the initiating events. Examples might be water getting into the repository and into the waste package, either from perched water, climate change, rise in water table. Then we would identify the events that would occur as a consequence of that, such as an eventual breach of the containment barriers and a loss of the neutron absorber materials, and the presence in the waste package of sufficient moderator to create a criticality.

We combine these events into fault and event trees.

We determine the probabilities of the events, compute their expected frequency, the types of criticality events that could occur based on these fault trees. Perform a consequence analysis for each of the credible criticality events. Then combine the probabilities and the consequences to assess the risk.

For a criticality event to occur in the repository, if it's inside the waste package, we must have a breach of the waste package barriers. We must have enough water present, and that's about three-quarters of the waste package full, to get k effective to one. And it requires the separation of the neutron absorbers, both those neutron absorbers that are part of the fuel, as well as any supplemental neutron absorbers, separation of that material from the fuel assmeblies.

For a criticality event to occur external to the waste package, the waste package barriers must be sufficiently degraded so that fissile material can escape. There must be sufficent water to separate the absorbers from the fissile material. And there must be a mechanism to reassemble the fissile material into a critical configuration.

As scheduled, the disposal criticality analysis technical report that I mentioned will be prepared for submission to the NRC in Fiscal Year 1996. We are gathering key data from such things as the critical experiments. We expect nearly all of that testing to be completed by the end of Fiscal Year '97. There will be a disposal criticality analysis topical report, which will include the results of this data gathering prepared for submission to the NRC in

Fiscal Year '98. That will be concurrent, about the same time as the technical site suitability, so that the conditions in the site can be reflected in this report.

The long-term material test performance program, which Dan McCright talked about this morning, will be available--the results of that will be available by the year 2000, and so it will be prepared for a potential license application, if the site is acceptable, in 2001.

In conclusion, we have a technical approach, which covers both the internal waste package criticality, as well as the external. Our analysis approach is based on the three-phase approach, with a combination of deterministic and probabilistic methods.

And finally, our design approach is based on the regulations, on the application of long-term control methods, and we are applying the three-phase analysis approach rigorously in our design. And the schedule is as I've stated.

Dr. Hanauer?

DR. HANAUER: What I'm going to talk about is the subset of the criticality program.

As you know from reading the technical literature, like the Sunday Times and the Review Journal, there's been a good deal of discussion in the last month or two about the possibility of nuclear explosions in a repository. I put explosions in quotes throughout this discussion. I don't want to start a semantic argument with people who really understand explosions about whether this is the things that Mr. Bowman and others talk about are true nuclear explosions or whether the somewhat different time scale means I should find some other euphemism to describe them. I prefer just to use the plain word.

What I'm going to talk about is works in progress. One has to expect additional publications, some of which may actually be scientific and technical publications in suitable technical journals. I expect some real work to be done, and so I expect there will be more to say about this later.

Here's an outline of what I'm going to talk about. First, I'm going to talk about the substance of what we're talking about, what Dr. Bowman and Dr. Venneri have pointed out and the things other people have written about it, what we plan to do about it, how it applies to the Yucca Mountain repository, if there eventually is a Yucca Mountain repository, and I also plan to answer the very reasonable question, why in view of all this is it okay to keep working and spending the taxpayers' money, and characterization of the Yucca Mountain site.

The paper by Bowman and Venneri, a little later on I will have a list of references for you that you can take home in your deck of slides, is the combination of several drafts and papers back and forth within Los Alamos presenting points of view. And what Dr. Bowman, who was a senior laboratory fellow at Los Alamos, and Dr. Venneri, who is a retired scientist from Los Alamos, have calculated is that if a homogenous mixture of plutonium-239 and silicon dioxide and water is accumulated, there can be an approach to criticality, and that some possible configurations have positive coefficients of reactivity. That is to say that if there is some kind of criticality and the system expands, that the reactivity increases rather than the more usual decrease in such a situation, and this positive feedback increases reactivity very rapidly and causes a nuclear explosion.

And Bowman's and Venneri's paper estimates a yield in the usual units of kilotons of TNT, which gets everybody's attention.

Now, they also mention that the plutonium that's actually proposed to be disposed is not just plutonium-239, but it's real plutonium, and that the rock in which it is suggested it might be disposed is not pure SiO2, but is real rock, and they make an allowance, without any calculation to it, that the critical mass might, indeed, be larger.

Now, what they're talking about is the disposal of the surplus weapons material, and what they suggest is that the weapons grade plutonium, quite a lot of it, be dispersed in a borosilicate glass log, which has some of the same properties of the high-level waste, which it is proposed to put into Yucca Mountain, but contains of the order of 50 times as much plutonium as the defense waste, which has only a little plutonium in it; that this glass log, after the containment phase and the degradation of the waste package, will be degraded, and it will be dispersed, and either the boron in the borosilicate glass, which is a poison for the neutron chain reaction, will be selectively leached out, leaving the plutonium behind, or that alternatively, that the boron will be--that the plutonium will be selectively leached out and leave the boron behind. But in either case, the neutron posion and the fission material will become separated, thus allowing for criticality.

So what happens is that by geological and geohydrological means, this dispersion will take place, and a critical collection of plutonium rock and water will be formed, either without boron or with not very much boron, and so then it will be reassembled into this critical configuration.

Now, in a very short passage, he suggests that maybe commercial spent fuel would be susceptible to the same disease, but, in fact, does not pursue that, and so we have no specific discussions of our Yucca Mountain situation in Bowman and Venneri, or, indeed, in any other of the references that I'm going to discuss.

Now, here is a list. The Bowman and Venneri paper is the first one on it. The second one is a Los Alamos report put together by a review team assembled by the Los Alamos management, whose conclusions I will come to in a moment. The third paper on the list is by three Savannah River site physicists, who commented at some length on one of the drafts of the Bowman and Venneri paper, which they had received earlier, and which I will also summarize later.

There have been no real technical publications on this subject yet. I don't know whether there will be, whether the people--the various people plan to submit them and whether they will, in fact, be published in referee publications. However, these reports are available. These Los Alamos and Savannah River reports are available.

There are also 15-odd year old reports from Pacific Northwest Lab and Oak Ridge National Lab, which contain more general discussions of mixtures of fissile material, rock and water, which discuss the possibilities, the ranges, the critical masses and the critical concentrations, which would be required for such mixtures to become critical. I don't think there's any doubt that there are mixtures of fissile material, rock and water, which are critical; that is to say which can sustain a neutron chain reaction.

Now, what do we plan to do about it? Well, we have a criticality program, as Hugh Benton has just described. We are going to be serious about the possibility of nuclear explosions, even though as you will see, my personal view is that this is very unlikely and this is based on some work that's been done, and I presume will be justified by some work to be done, but we're going to be serious about it. We are going to include explosion scenarios in these events that Hugh was describing. Not just the Bowman and Venneri scenario, but we're going to spread the net and do a serious and systematic study of such scenarios and make sure they are included. We'll do whatever detailed work is required and analyze any scenarios that have non-negligible risks, and most important of all, we will include credible risks that we find in our decision making.

And so we do not plan to sweep this thing under a rock, but rather to make sure that it is correctly included in our program.

Now, the next thing to look at is this Los Alamos review of Bowman and Venneri's work. The group was headed by Dr. Canavan, and I have violated my usual principal of keeping view graphs short in order to display verbatim the conclusions of this review group.

They concluded that the Bowman and Venneri report does not describe a credible sequence of geologic events, that the probability of each of the necessary steps is vanishingly small, and the probability of occurence of all three steps in his scenario is essentially zero. And furthermore, says Canavan & Company, even if these steps should occur, any energy release would be too small and slow to produce any significant consequences, either in the repository or on the surface.

So what they said was in slightly more extensive reporting is this: Firstoff, they point out that real materials are less reactive, and they also confine themselves to weapons material, which is, of course, very different from what we propose to put in a repository. They say that the positive feedback would not occur, and give the technical reasons for it. And if they're correct, then the autocatalytic aspect of the Bowman and Venneri scenario would not occur.

And finally they said, as I read a few moments ago, that even if all this happened, a real explosion would not occur. The energy release would be small and slow.

But contrast, Dr. Parks and his colleagues at Savannah River conclude that Bowman and Venneri are correct. They looked at a slightly different scenario. Their view of what a borosilicate glass log might look like and how much plutonium they might put it in it is a little different from Bowman and Venneri, and they point out that the defense high-level waste, the kind of glass logs we propose to put in

Yucca Mountain, are simply completely unaffected, to use their words, becuse there isn't any significant amount of the fission material in it.

I think everybody has now agreed that the critical mass calculations and critical size calculations of Bowman and Venneri, the ones similar to the ones that were done 15 years ago at Pacific Northwest, are correct. The Livermore people have checked them, the Savannah River people have checked them, and the other Los Alamos groups.

The Savannah River people, Parks and his coworkers, say that the energy yield equations were not checked, but appear reasonable, and they talk about the probability, but, in fact, they don't end up with any numbers. They end up with the conclusion I have given here, that the probability per unit time must be quite small, whatever that is, but criticality must be prevented essentially forever.

That's what they say. Whether they mean explosions or whether they mean all criticality is not clear from their paper.

Now, how does this apply to Yucca Mountain? Well, there's no plan to weapons plutonium in Yucca Mountain, a subject I'll come back to in a moment. What we plan to put in Yucca Mountain is commercial spent nuclear fuel, which has a little bit of fissile uranium-235 left in it, a kilogram or so per fuel element, fuel assmebly, and a whole lot of uranium-238, which is a parasitic absorber, which inhibits the kinds of chain reactions which are proposed, which take place with thermal neutrons, and which will not separate checmically from the uranium-235, since it is identical chemically to uranium-235, and there is a small amount of plutonium-239 and many other actinides in commercial spent fuel on the order of a couple of kilograms per fuel element.

The high-level waste I've already talked about.

Now, if anybody else comes along with any other waste which is proposed for Yucca Mountain, which is not impossible, then it has to fit Yucca Mountain. It has to meet all kinds of waste form requirements, including the criticality requirements, in order to be acceptable for this licensed repository.

The other thing that's really different about Yucca Mountain is that we don't put waste in holes, in confining holes, in boreholes, as was the proposal some years ago, and was--is apparently being considered by Bowman and Venneri. We've got great big drifts, and even if they collapse, the rubble is not a confining element as required by the Bowman and Venneri scenario. So that is an important difference between the Bowman and Venneri scenario and the facts in Yucca Mountain.

Now, you might reasonably ask if we can have nuclear explosions in Yucca Mountain. My own opinion is we can't, but we're going to be serious about it. Why is it okay to spend a lot of the taxpayers' money to continue our site characterization project? And the answer is based on my opinion, which is also, as you heard yesterday morning, Dan Dreyfus' opinion. The scientific community hasn't decided what they think about Bowman and Venneri's scenarios for disposal of weapons plutonium in boreholes. But as far as Yucca Mountain is concerned, we haven't seen any significant explosion risk, and, therefore, we think that the likelihood of a credible risk is sufficiently low, that it's okay to spend some taxpayers' money in the meantime, but we do intend to be, as I say, serious about the risks, and we'll do anything necessary to protect the public and to protect the environment if there turns out to be a significant risk.

Thank you.

DR. LANGMUIR: Thank you, Stephen. We're well over the time at this point. I'd like to hold questions on this very important talk to our panel discussions, when both speakers will be present and we can address it at that time.

As you will see from your agenda, we have currently an opportunity for the public to ask questions and make comments before we convene our panel discussion. So with that, I'd like to encourage anybody in the audience who feels that they'd like to make a comment or ask a question to come to a microphone, identify themselves and do so. But please, if you would, try to limit yourself to about five minutes or less so that we can proceed expeditiously through the rest of the day.

MR. MCGOWAN: Thank you, Mr. Chairman. My name is Tom McGowan.

DR. LANGMUIR: Yes, Tom.

MR. MCGOWAN: I'm a member of the public. I will be succinct and considerably less than five minutes. But two topics of clarity and discussion. I'll take the last one first.

I note with interest that the agenda itemized, addressed the discussion of the LANL Bowman and Venneri draft, the prefinal report. I emphasize prefinal report on potential for underground autocatalytic criticality. It is unencumbered by the immediate presence and real time participant contribution or response by Drs. Bowman, Venneri, Brown, the Los Alamos 30-member review group and/or the SRS-based affirmative reviewers, Doctors Park, Hyder, Williamson and Benjamin.

While I'm supremely confident in the experience, expertise and dedication of Dr. Stephen Hanauer, I am equally cognizant of the merits and forthright open discussion of an however controversial issue of significant national interest and potential consequence, which did not occur, but was surplanted by a limited incremental unilateral summary within a segment of today's meeting, pursuant to diffusion within a broader, but limited horizon of pertinent study and serious discussion, similar to what is occasioned in the repository study, incidentally.

Consequently, the public is not availed of what I would define as a genuine consensus of opinion on this significant topic, but instead is relegated to a matter of choice between two clearly respectively limited interests, either one or neither of which may be valid in the final result.

Thank you for that intelligence. My initial question is this: Notwithstanding carefully considered responsible public criticism, I wish to express my sincere appreciation for the dedicated efforts of DOE and YMPO staff in the mandated discharge of their respective mission function responsibilities, who is overall historically not present at the magnitude of extent of variable complexities defies ready classification. Rather than terminate DOE, as had been suggested, I deem it vastly more so preferable to deal with a known problem than to envoke a new set of unknowns that variably non-ensure its superior quality effectiveness.

Therefore, let those who are self-deemed, but not independently verifiable, as without sin, cast the first and final stones and identify his successor entity of assured superior quality, which challenge may obtain as formidable, including current attendees.

DOE has my full support, admiration and respect, as well as my undistracted attention and enduring opposition to any and all instances of self-evident quality deficiency adverse to the genuine best public interest. Congratulations on being human, as we all here, and thereas ergo imperfect.

The variably perceived as lost lamb or a faithful friend, DOE is encouraged to continue to do its duty, but only on the paper and in the spirit of genuine community attain to utomost quality effectiveness beyond comparison or reasonable reproach. I think you can. I believe you can. If I do, you can, period.

Thank you.

DR. LANGMUIR: Other questions or comments from the public? Yes?

MR. SHER: Yes, I'm Rudolph Sher. I heard Dr. Bowman give a colloquium at Berkeley about a week and a half ago.

DR. LANGMUIR: Identify yourself, please.

MR. SHER: Rudolph Sher. I'm a member of the public.

I heard Dr. Bowman give a colloquium at Berkeley a week or two ago, and I thought he said that for spent fuel canisters, you could still have a separation of plutonium from the uranium because of the different solubilities. Do you have any comment on that, Steve?

DR. LANGMUIR: Steve Hanauer?

DR. HANAUER: Yes, I think you probably can have separation of plutonium from uranium. The solubilities are substantially different, and when you get into a severely degraded situation and the waste form is--the waste if mobilizied, the difference solubilities may indeed separate what plutonium remains from the uranium.

If it takes long enough, the issue is moot. The plutonium has a half life of 24,000 years and decays into uranium 235, which is fissionable, but which has, of course, the same chemistry as all the other uranium.

So if it happens in the very late era, then there isn't any plutonium, at least not any plutonium-239 left. If it happens earlier, then what plutonium there is can be separated.

DR. LANGMUIR: Any more questions or comments from the public? Yes?

MS. DEVLIN: I'm Sally Devlin. I'm a stakeholder from Pahrump, and I just had the pleasure of reading the Congressional report and there is a question I must ask, which doesn't pertain to the second part of this meeting.

The question is, I notice that DOD is going to put high-level waste in Yucca Mountain, and this is the first time I have ever seen that in the report. And my question is how much, what are they going to put in, who's going to pay for it, how much of all these brilliant brains that you're working on is going to go towards DOD and the plutonium, and I'm taping all the demolition surveys from University of New Mexico, and they're going to come up with a lot more plutonium and other hot stuff, and where are they going to put it?

And since we know who is paying for Yucca Mountain, who is going to pay for this DOD? And I think there are a lot of questions and how much should be asked.

DR. LANGMUIR: Is there someone here from DOD who would be willing to start the answer to that question? Steve, do you want to do that, or Hugh, or is--

DR. HANAUER: There's a whole new organization in the Department of Energy called Materials Disposal, behind which innocent name is the job of figuring out what the alternatives are for getting rid of the nuclear weapons material, which the end of the cold war has made surplus to the country's needs and to the other--some of the other hot stuff you were referring to.

There's also another group dealing with the hot stuff at Savannah River and Hanford. These are difficult technical questions. They are also social and political questions.

I don't believe that anything like a proposal has yet emerged from this, much less any kind of a decision by the Department of Energy, the President, the Congress about what actually should be done with them.

Part of the consideration of what to do with these surplus weapons is what led Dr. Bowman, Dr. Venneri and the other authors I quoted to consider whether disposal in rock was a suitable thing to do and to point out the possible dangers of nuclear explosions.

I don't think anybody knows where it's going to go, who's going to pay and how much it will cost. The amount of it is known, but not by me because I haven't spent much time in that particular area.

I haven't heard any serious proposal actually to put it in Yucca Mountain, although its disposal in some rock is obviously one of the alternatives. We all know that there's some rock not very far from here in which similar materials have already undergone some fairly spectacular disposals, and I don't--I truly don't know which rocks they're talking about. But, indeed, work--technical work is going on, to see whether that's a place that would work.

Diane, you know more about this than I do obviously.

MS. HARRISON: Yes, I'm Diane Harrison with the Department of Energy, and the fissile materials disposition program, it's the Office of FMDP at Department of Energy, is looking at evaluating a whole series of alternatives for disposing of the surplus weapons usable fissile material. And there's all kinds of numbers out there. They're talking about anywhere from 40 to 80 metric tons, and it's a number of alternatives that they're looking at for a programmatic environmental impact statement and also a record of decision, which is scheduled to be reached in FY-96.

Some of those alternatives include disposition in a high-level waste repository. Other of those alternatives are taking that weapons usable fissile material and converting it into a fuel, and then putting that into a reactor, and then you would have a spent nuclear fuel to dispose of.

And so that's--those are the alternatives that include the high-level waste repository. They are not considering direct disposal of weapons grade plutonium in a high-level waste repository.

And those issues, as far as cost, who would pay for it, all of those are issues that would have yet been determined and would be part of what would be included in the record of decision and ultimate analysis at some point after that.

DR. LANGMUIR: Another comment or question?

MR. JOHNSON: Cady Johnson with the M & O. I've got something of an ethical problem in this area. It doesn't have to do so much with criticality, but with the scope of these activities being beyond the scope of what's covered in the Nuclear Waste Policy Act.

Now, I know there's work going on within the M & O to look at--I mean I've taken phone calls about this question; you know, request for site information and things like that. People are spending money in the form of their time to look at this issue. And then, Dr. Hanauer, you've appeared to have committed to spend some more time to look at the feasibility of weapons grade material going into Yucca Mountain. Well, we all have to fill out a time sheet.

As far as I can tell from the Nuclear Waste Policy Act, this is not legal. I'm not supposed to be spending my time on that. I'm supposed to be spending my time on spent fuel and defense high-level waste, and that was the first thing I responded with when I got the first phone call.

So it's just--it's a simple--I mean, it's simple, but it worries me.

MS. HARRISON: Hi, Diane Harrison again.

Cady, the activity of the non-managing is funded separately from the nuclear waste fund. The Office of Fissile Materials Disposition has provided me and the M & O with separate funding for this activity so that it is very clearly separate from nuclear waste fund activities.

And I would say if you're doing any level of work, if you've been asked for any level of work, then we need to work out some sort of whatever, DBO or whatever accounting that needs to be done. But anybody who's doing the actual work is funded strictly separately. That has been one of my big issues and concerns also, is keeping the line very clear.

DR. HANAUER: Well, besides the activity described by Diane, which is not being paid for from the nuclear waste fund, to the extent that Mr. Bowman--Dr. Bowman and these other activities suggest that what we're doing may be unsafe, I think that we have to be serious about it and that in considering whether, in fact, our activities are legal, proper and ethical activities to bury commercial waste at Yucca Mountain, if it is found to be suitable, that our responding to this suggestion that we might be incurring a risk is well within our assigned task and within our legal and proper activities.

DR. LANGMUIR: Don Shettel?

MR. SHETTEL: Don Shettel for the State of Nevada.

I have a few comments, most of which seem to be on water. I would just like to remind some of the engineering speakers that the boiling point of water tends to increase as you increase total dissolved solids, and this would reduce the extended dryout period of the repository. And the higher the thermal load that develops, the increased degree of refluxing you'll get above the repository, and this will increase the degree of salinity of fluids that develop. And this needs to be taken into consideration.

For Hugh Benton, on the corrosion of canisters, I'd like to see rates of corrosion on the inside for a pinhole leak that develops in the canister. It seems like a canister could degrade rather atrociously from the inside out due to just radiolysis, and I would personally hate to think about the effect on any microbes on the inside, but somebody has to.

And regarding high-level defense waste, there's a question of the composition, especially the water in the glass, and specifically, if the pour canister develops a pinhole leak allowing humidity in there, the hydration of the glass, which has been termed aging by Bates and his group at Argonne, the volume expansion involved in hydration of the glass could result in the splitting of the pour canister, just possibly from pinhole leaks.

On radionuclide releases, it appears that the performance assessment modelers are still looking at solubility calculations, and this may not be the most conservative calculation they could make. They need to look at the unsaturated dripping type experiments that have been performed on both spent fuel and waste glass at Argonne National Lab; again, John Bates' group.

And a final comment, I've--over the years, it seems I've heard about this capillary barrier concept that's been proposed by INTERA. This would appear to be an effective method for low-level waste, where you have low temperatures and low or non-existent thermal gradients. But at Yucca Mountain, where you're dealing with higher temperatures boiling and large potential thermal gradients, they seem to have ignored the coupled thermal hydrogeological/geochemical effect where, for example, it's a backflow or above boiling, and you drip fracture water, which is below boiling, onto this backfill, or capillary barrier as they call it. When the dripping water hits the backfill, it eventually will evaporate, leaving behind a salt deposit, or precipitate, if you will. Over time, this precipitate could build up and could conceivably form a pipeline or funnel to funnel water later on directly onto the canister.

So that, therefore, in a near-field environment, with any kind of backfill or capillary barrier, it might be harmful to the performance of any repository.

Thank you.

DR. LANGMUIR: Thank you, Don.

We'll take, I think, one more, if we may, from the public.

MS. MANNING: Excuse me, my name is Mary Manning. I'm with the Las Vegas Sun newspaper, but I've just completed a master's thesis on the Nuclear Waste Policy Act amendments of 1987.

There's a little bit of confusion going on because President Reagan in August of 1985 signed an executive order that allowed combining defense wastes and commercial spent fuel in the same repository. That was before the amendments were passed, of course.

In the last three years since the weapons program has been slowing way down at the Nevada test site, there has been roughly $500,000 of taxpayers' money through the defense funds spent on nuclear waste disposal within the program, amounting to roughly $1.5 million, and that's in my thesis. I have tracked it down through the budget.

So to avoid any confusion, Ronald Reagan did sign an executive order, though, combining the fuels.

DR. LANGMUIR: Tom Cotton with the M & O.

MR. COTTON: Just one point of clarification. The defense waste that was addressed in the Nuclear Waste Policy Act was the high-level waste from reprocessing reactive fuel to produce--it does not refer to the plutonium itself. So that's been the assumption that the high-level waste from producing weapons plutonium would go into the repository. It was really built into the act, and there was a presumption in the act that that would happen, unless the President made a finding that it shouldn't be done. He did not make such a finding. He concluded it should be done. So that's really been part of the plan since the act was passed.

The question of plutonium is an entirely different question.

DR. LANGMUIR: Thank you. With that, I think I'm going to, if I may, close the public discussion, and we'll take our break. It's 2:47. We'll convene in 15 minutes for the final discussion.

(Whereupon, a break was taken.)

DR. LANGMUIR: Please take your seats. Before we start, several of us at the table were not speakers during the day. And, I think the audience and we at the panel, as well, would like to know more about you. So, those who are new to the table today, would you please introduce yourselves starting to Steve Frishman's left and tell us what your expertise is?

MR. WELLER: My name is Rick Weller and, unlike what the card says here unless John Greeves has pulled a fast one on me, I do work for the NRC and not the DOE. I'm in the Division of Waste Management at the NRC which John Greeves has just recently become the division director, and I'm the section leader in the engineering and materials area and have been working on waste package design issues/EBS issues for seven or eight years and spent the bulk of my prior life in reactor licensing.

DR. LANGMUIR: Thank you. On that side of the table, anybody that--I believe, everyone else here has been part of the--oh, did I miss someone? Will Clark; sorry, Will. So familiar a face, I forgot. Does the word "shanghai" mean anything to you?

MR. CLARKE: My name is Bill Clarke. I am the ex-TPO, current M&O, Livermore manager representing a lot of what you've heard about in the last couple of days. My expertise, I guess you'd say, is in metallurgy and corrosion, basically. And, I don't know why I'm here. This was left over from the last time, I guess, but willing to participate.

DR. LANGMUIR: Okay. By the way, this is a co-chair with my colleague over here, Dan Bullen. So, the two of us together will be chairing this panel.

DR. BULLEN: Actually, I should place some blame here since Don didn't let anybody ask any questions, I asked Carl to drag him in here and do this. So, now he can share the blame with all the adverse conversation that's going to occur in this great heated debate.

But, the other thing I'd like to say is I did give the people who wanted to introduce themselves as panel members the opportunity to say a few words; two minutes at the most. And, if we could start, I'd like to start with Steve Frishman. We might start with that and then yield to Rosa Yang from EPRI.

MR. FRISHMAN: Well, Dan, you're a good one to be talking about two minutes.

DR. BULLEN: Thanks, Steve.

MR. FRISHMAN: I just wanted to make one quick observation before we started and that's that it was something more than two years ago, I think, that I tried to point out to the Board my prediction that whatever became the transportation container would drive a large part of thinking about repository safety. Well, about six months ago, it became very clear that that was going on and this meeting today and yesterday has made it absolutely clear because, even though the Department still says they have made no decision about MPC only because they're doing an environmental impact statement on the deployment and fabrication of MPC, we have the MPC firmly embedded in all of the safety considerations about the repository now. We even had Steve Brocoum yesterday breaking stride with the Department's own statement saying the program evolution related to thermal strategy. The first piece of that is decision to utilize multi-purpose canister. So, I think we're in a position now where we need to recognize and try to sort out what the MPC is doing in terms of safety thinking about the repository because it seems inevitable that that is the container; whereas, even a year ago, it wasn't, and in the last TSPA, it was not. And, I haven't seen anybody sort of trying to make that evaluation of what is the MPC do to repository safety thinking, as opposed to what are the merits and lack of merits of the site and then what type of waste package might be fit to it if it has any merit.

So, I just wanted to point that out and maybe, at least, spark some thinking in terms of what's driving what.

DR. BULLEN: Rosa, can you make a couple of comments and then we'll go to Rick Weller?

MS. YANG: First?

DR. BULLEN: It doesn't matter.

MS. YANG: Go ahead?

MR. WELLER: Let me just make a few comments about burnup credit notwithstanding the fine presentations by Hugh Benton and Steve Hanauer.

First of all, I think we all recognize that DOE is relying on burnup credit for large waste package designs whether they employ an MPC or not. The criticality control issue is there no matter whether there is an MPC or not in the design. I just wanted to advise that the NRC has never really granted burnup credit in its previous licensing assessments. We typically don't with one exception that I'm aware of and that was for a spent fuel storage rack design. And, even in that assessment, there was a design assumption of no boron in the water. So that there was still some regulatory conservatism, if you want to call it that, backing up that design assessment.

In transportation cask analysis, we typically treat spent fuel as new fuel and, you know, in view of the burnup credit argument, I don't know if we're being overly conservative or not. That policy will be ferreted out in the coming years.

I don't want to prejudge the outcome of the burnup credit issue. I think we all recognize there's not a whole lot of data supporting the knowledge of the cross-sections and fission product characteristics, the fission yields and things like that, and that DOE has recently initiated a program at Sandia to augment that database, but the regulators from our point of view, I don't think, have a whole lot of database to support the granting of burnup credit. And, I suspect that if we back off in any respect, it will be perhaps in an augmented fashion, partial credit or credit with some conservatisms employed; treating the ends of a fuel assembly as new fuel, things like that, as opposed to granting the full burnup credit. And, I think DOE recognizes the approach we're going to take.

Related to this, large waste package designs really just kind of exacerbate the criticality control issue, and I recognize the benefits of the comedies of scale in this and the fact that Yucca Mountain is the only or was the only unsaturated site under consideration. The other two sites in the screening process were saturated. I think that's a tremendous benefit and not to be diminished. But, in this regard, a 21 element waste package design is roughly equivalent to 1/8th of a core and, moreover, you're putting that 1/8th of a core in a waste package in its most optimum configuration for going critical; putting it in a configuration that it was designed to go critical to produce power. In my view, DOE is not taking advantage of geometry to alleviate some of these criticality concerns. They could. Ride consolidation has been practiced on a limited scale and some equipment and technology is available.

Another point is that from what I've heard in management meetings with DOE, I heard some discussion of the use of filler material in today's presentations, but quite frankly, DOE management views filler as an option of last resort. And, I think what that would mean is putting filler in at reactor facilities, and I'm not so sure DOE, you know, really wants to do that or the reactor licensees really want to do that.

The other point about the design of the MPC is that I've heard DOE folks themselves--Jeff Williams from DOE--state that the design of the MPC is really being driven by storage considerations. DOE has a contract with reactor licensees to start taking fuel in 1998. And, I know the reactor licensees want DOE to live up to that. So, it's the storage interest that is the driving force behind the design of the MPC and not Part 60 considerations which I think are the most difficult design considerations above and beyond Part 71 and 72 requirements. If you look at the three functions of the MPC, it might be the most difficult function to satisfy is the Part 60 function. In this regard, designing for criticality control for 10,000 years is really a formidable task. I mean, you know, engineers aren't used to designing things for more than a hundred or several hundred years at the most. So, we're in new territory now.

Lastly, with regard to plan testing of basket materials for criticality control, as discussed by Dan McCright, in view of the schedule information that Dan presented today, I question whether the results from this work will be timely enough to feed back into the MPC design process. Recognizing that the MPC design is on a fast track, the balance of the waste package and repository are on a slower track. DOE is interested at least for initial implementation of those MPC designs in 1998.

I toss that out as a few comments.

DR. BULLEN: Rosa, please?

MS. YANG: I have a general comment, but first I just have to react to what you said about the burnup credit. You know, the reactors discharge fuel because they don't have enough reactivity, and when the reactor is running, it's running in a pool of water. So, you know, it has the most optimal geometry. I wouldn't say the Sandia experiment wasn't an important experiment, but there are a lot of experiments being performed daily in reactor. There are all these reactor physics calculations and burnup credit is allowed in Part 50 and reactors are being operated and start up all the time. So, people know how to calculate burnup credit. And, there are also fork detector experiments being supported by the utilities. They have major burnup credit. So, I think there are considerable experience about burnup credit. So, I wouldn't say it's something that's entirely on paper and remains to be demonstrated.

Referring to the general comment, I think I'm pretty gratified to see--I started in this area and first participated in this meeting exactly two years ago. In my first meeting, I was quite impressed with the technical depth and the knowledge being presented here, but I was quite disappointed because to me most of the presentation didn't really focus on what's the purpose of the experiment. We just keep hearing we need to do more experiments in certain areas and we need to do more modeling in certain areas, but really I think had a sense about what all of that led to in terms of enhanced waste isolation. I think we've seen a lot more focus here today, but I think I'd like to encourage more focus in that direction.

I'll just give a couple of examples of what I hear today. For example, on the criticality issue, I think there are a lot of questions to be debated about a Bowman-Venneri paper. I don't intend to do that. And, I think I applaud the DOE for saying we don't want criticality at any time. I think that's great. But, you did say something about there is a negligible probability. I think it will be interesting for DOE before starting all the work to define what is a non-acceptable or non-negligible probability that you would need to consider. From the Bowman presentation a couple weeks ago, he acknowledged that the probability being assigned by the review team about his scenario is 10^{-40}. So, I would like to see if that's the kind of probability that DOE thinks that should be avoided all the time.

Personally, with my background, I'm quite gratified also to see finally fuel cladding as being acknowledged to exist in the repository. I listened with great interest about there's going to be some modeling performed and some experiments. But, I look at the table; I don't see any experiment really focused anything on zircaloy, at all. Maybe that's just an omission.

But, anyway those are just two examples. I think in the future I would like to hear both in the program planning and the program management point of view and in the presentations. I think it would be very helpful to, whenever we plan any new experiment or do any new work, to ask ourselves before we even start what is the significance of it.

DR. BULLEN: Thank you, Rosa.

With that, I'd like to turn it back over to Don Langmuir because he truncated all questions today, and I'll let him run the meeting for a little while.

DR. LANGMUIR: Okay. Let me start with Kal Bhattacharyya. I think maybe I said it better that time, Kal. You told us this morning that there was allowance or consideration for ventilation by end drifts in the proposed repository as a means of providing cooling. And, I wondered, I didn't hear a word though about DOE considering heat pipes or, more specifically, design features that might cool the system as part of your analysis of how to proceed. I wonder if you could comment on that?

DR. BHATTACHARYYA: As you know, Professor George Danko of E&R has done some work in the past on heat pipe area, showed some efficacy of cooling the drifts. In our consideration, we have at this moment concentrated on the efficacy of the ventilation process to take the heat away, as a matter of fact. That's how our work is right now concentrated on. So, we are not looking to heat pipe at this time as a part of this.

DR. LANGMUIR: The reason why it was suggested to us some time ago by George Danko, I believe, was that they're passive. There's no cost, no fans, no cost to having them performed.

Another question has to do with the ventilation effect on the repository. I presume you're going to have to ventilate as you proceed through site characterization from the ESF. What that means to me is you're going to mess up the moisture content in any fracture zones that are in contact with the workings. You'll have to inevitably be moving some water in and out, mostly out, of the system. I wonder if there's been thought in the program about what this effect might have on people's ability to sample and analyze waters that are coming in contact with the ESF.

DR. BHATTACHARYYA: I don't feel that's a question that's in my area to answer. Maybe, somebody in the hydrology can probably address that. Is there any volunteers?

DR. LANGMUIR: Dwight Hoxie is here. Perhaps, he or Dale Wilder might have an idea.

MR. WILDER: I was trying to hide. Actually, what I'm going to talk about responds to part of your question, Don, not much in terms of how we sample the water under the typical sampling situation, but in terms of what will that do to us in our thermal testing where we're looking at mobilizing the water from heat. I don't think that it will have a great impact on us because the first couple of meters will probably be modified in its moisture conditions. But, there's enough water left in the matrix of the rock which will not be removed efficiently by the ventilation after the first meter or so of rock that we'll still be able to do our thermal testing and look at what the thermal hydrological response would be. But, there's no question it will be removing some water.

One of the things we are looking at also, by the way, is the positive impact that could have on the performance in terms of the waste package if we do rely on removing a significant amount of water vapor in the ventilation system, and I think Tom Buscheck has done some calculations showing that if we could remove one facility volume of moist air that we will have significantly removed water that could come in contact with waste package.

MR. WELLER: Don, I might be able to add a little bit of something from a trip I took to the underground research lab a couple of years ago. The experience there is that the operation of the ventilation system is drawing the rock out. I understand it's dried out to about a foot and that's non-fractured rock basically, although they did purposely cross a fracture and you do get dripping at the fracture, but every place else, it's my understanding, the rock is dried out to a depth of about a foot.

DR. LANGMUIR: So, you've got to get there in a hurry to see where the water is naturally seeping out of the system or you lose the information.

MR. WELLER: I don't think there's any question that the heat and the ventilation system is going to affect the ambient moisture conditions in the rock.

DR. LANGMUIR: Yeah. Tom Buscheck?

MR. BUSCHECK: Almost had two days of substantially complete containment.

Professor George Danko is working with us at Lawrence Livermore doing ventilation calculations, and in his model in the past, he's assumed that the surface of the rock maintains a certain wetness coefficient. He has a simple way of referring to it, but he hasn't really explicitly modeled the thermohydrological effects of ventilation and we're doing that right now. We're coupling enough code with his ventilation model and we're planning to do some optimization studies. What Dale was referring to is if the water vapor mass fraction of the gas you remove is close to 1, you can remove a tremendous amount of moisture and heat per unit volume of gas removed from the ventilation system. So, we're going to consider schemes perhaps whereby you let the system rest and the water vapor mass fraction come up to 100% and then you ventilate and watch the decline of the water vapor mass fraction as you ventilate. And then, you get a point of diminishing returns. You'll probably want to go to the next drift and start ventilating, and I think you can learn a tremendous amount about the hydrological behavior and the response to heat by watching that cyclic drying and then behavior during ventilation and recovery between these ventilation pulses.

DR. LANGMUIR: And, you can do that without frying the occupants of the tunnel and leaving them breathless, I assume.

DR. BULLEN: Before we leave Kal, I had one more question for him. You showed a couple of scenarios whereby you were going to emplace the waste either on a railed vehicle or a tract vehicle or you were going to set it on a pedestal. I had a question about when you transfer it from the transport cask to whatever little vehicle you put it on, how are you doing that? Is it like the new Holmes container where you just have a hydraulic rim and slide it along the surface and gouge the daylights out of the container or are you going to have a little more elegant way of doing it, I hope?

DR. BHATTACHARYYA: Sounds like a loaded question.

DR. BULLEN: Yeah, well, I'm trying to lead you in the direction I want to go, but that's probably inappropriate.

DR. BHATTACHARYYA: We're really showing conceptual data. We are just pushing that waste package out of the transport cask at this time. Please, don't ask me how yet. We have probably some screw feed mechanism and so forth.

Right now, we are simply looking at the emplacement equipment one at a time. This is the first we are looking at. If you're already in a cart--you know, the waste package sitting on a cart, then there's no banging or moving it around. It's on this wheel and it rolls out; hopefully, in a controlled manner, as a matter of fact. So, there shouldn't be any banging. The gantry concept you saw, if you recall, the waste package had kind of a lip at the end. It hangs out a little bit, a few centimeters. I don't know exactly what. That's where the two arms comes and grabs it and picks it up. So, it really never gets around the waste package to squeeze it or bang it. So, hopefully, it will treat it well.

DR. BULLEN: Okay. Well, the only concern I have is if you have it on a little cart and you're rolling it out, in 100 years when you want to roll it back, the same kind of question arises; will the wheels work and will you be able to roll it back?

DR. BHATTACHARYYA: Yeah, we talked briefly about that. Hopefully, using some ceramic bearing and so forth, double flange wheels, we'll be able to. But, again, that needs to be proven.

MR. CLARKE: Yeah, I've got a question for Kal. It's something that's bothered me for a while. In most radiation contamination incidents I've been involved in, the problems always come from airborne contamination. HEPA filters are not only expensive, but have to be changed quite frequently. I'm just wondering how are you going to filter the air coming down through those drifts assuming you get some kind of an early failure?

DR. BHATTACHARYYA: We're not planning to pass--if you recall, the ventilation quantity is quite huge; 270,000 cubic feet or something. You know, if you're going through a tunnel, it gives you essentially some more dust. So, I'm not a HEPA filter expert, but I've been told that it will fill up a HEPA filter in a matter of minutes. The only place you are considering putting a HEPA filter is at the exhaust shaft where it will short circuit if there is a radiation leak detected. It's not a daily feature where the ventilation air is going--we expect that the waste package is going to be clean when we receive them and we would monitor them to determine any leakage out of it. But, there is no plan to put any HEPA filter in the exhaust shaft, as such, unless there's an accident.

DR. BULLEN: With a followup to that, are you going to monitor each individual drift and then when will you know when the package has failed; I guess is the key question.

DR. BHATTACHARYYA: The whole question of monitoring and the requirements are in its infancy, really. It should be a systems type of study. We've just begun to look at it. We have not developed any requirements yet; how many packages, how often. We know that it's a requirement to dissuade risks at this time.

DR. BULLEN: My other concern is the same as Bill Clarke has that I know HEPA filters can fill up in a few minutes. And, if you're using the ventilation to cool it and you find a leak in one of the drifts and you have to filter the air coming out of it, you're going to drop your flow rate significantly and the temperature may go up. In the event that you'd have to go in there and try and remediate, it's going to be a little more hostile environment if you have to HEPA filter all the air coming through. You might want to consider that when you look at your design.

DR. BHATTACHARYYA: You're absolutely correct. We are using ventilation to manage the temperature if it is necessary. It almost is a last resort. Our first preference would be to emplace the waste and shut it down. This is no different than the SCP/CDA concept, you know, of a borehole in the wall where you put the waste package and put a door on it and you are done. I view these emplacement tunnels just like long boreholes from an access. You are done, you close it on either side, and that should be the passive mode of controlling that. If you have to ventilate it, then you have to make sure that the ventilation is fail safe and all that stuff and gets into one order of magnitude of difficulty, as a matter of fact. So, we are simply looking at it as a concept, but hopefully the mainstay of waste isolation here.

DR. CANTLON: Let me pursue your answer, Kal. You indicated that there's no intent to filter the incoming air. There are massive amounts of pollen released during pollen pulses and spore releases. You're going to feed the bacterial microbial population down there pretty well if you don't filter the air.

DR. BHATTACHARYYA: I have not considered that yet. I didn't realize that that's a problem, frankly. Someone on the waste package MIC area probably should get together and talk about it.

DR. DiBELLA: Kal, one of the overheads you put up showed waste packages in drifts that were separated by empty drifts and I think you called this localized disturbance concept. What's the purpose of the empty drifts? I mean, why would you mine them in the first place? And, if you would mine them, how would you get something licensed with drifts in it that seemingly have no purpose? Could you explain that?

DR. BHATTACHARYYA: Sure. That's a concept for maintaining the flexibility from going--initially starting out at a low thermal loading, for example, 25 MTU. But, since we do not envision it being an easy idea to go and excavate between a previously emplaced drift, it's better to --if you want to maintain the flexibility, we would rather excavate four drifts and emplace only in one and, if at a later date we want to go up in the thermal loading, then we do come back and we can emplace in the empty drifts. You know, if you have four and you emplace two of them, then they're doubled and ultimately quadrupled them. That's the purpose.

Did I answer your question correctly?

DR. DiBELLA: You didn't answer the licensability aspect of it. Maybe, Rick could address that or somebody else.

DR. BHATTACHARYYA: Okay. The concern about licensability, I'm not exactly sure. It would be simply like licensing a 25 MTU repository if that's the way we're going. Leaving empty drift really should not affect its licensability in the sense that we are still loading it to a 25 MTU. Now, how we load that--and I showed two pictures; one was loaded in a maximum density which Lawrence Livermore described as a localized disturbance type or we could load it in a square pattern which it's often referred to as minimally disturbed. It depends on waste isolation and things like that.

I see Dale wants to speak to something.

MR. WILDER: I would like to follow up on the question very briefly in terms of some of the advantages of having those drifts excavated even though they are not used as emplacement drifts until you make the decision later. That is that one of the issues that we're looking at with this localized disturbance is that the pillars can serve as effective drainage for the water that is driven off by the heating. By having those drifts emplaced, that gives us a great monitoring ability for performance confirmation. And so, I don't think that they're there without purpose and I think they would be part of the licensing strategy, if you will.

MR. FRISHMAN: I noticed in your presentation that all of your layouts seem to be based on the smaller MPCs, the 75 ton. Is there some reason for that and also are there significant differences in your story if you go the way the Department story goes right now which is the majority of the packages will be large MPCs?

DR. BHATTACHARYYA: If I gave you the impression that these are 75 ton MPCs, that's not correct exactly. The layout depending on--to accommodate the larger MPC which is commonly referred to as 125 ton, although they don't weigh 125 ton. Actually, the waste package does weigh about 65,000 kilograms or 65 metric ton. So, the 125 ton MPC is actually the 65,000 kilogram waste package. That's the larger of the two. A 75 ton MPC when you put it in a waste package weighs somewhere around 54,000 kilograms, I believe. 40? Okay, 40,000 kilograms. So, the pictures I showed you of the 65,000 kilogram is the larger of the two MPCs.

DR. PRICE: I'd like to ask Kal a couple of questions. The first one is about ventilation. I saw the volumes which were in units of cubic meter per second and the question is what's the peak wind speeds that you might expect?

DR. BHATTACHARYYA: Okay. We have done some network analysis for ventilating under normal repository operation, many when we are not trying to cool it or do some thermal management. At that stage, you're looking at an emplacement drift about 410 feet per minute velocity. The allowable velocity is 1500. So, we are, you know, one-fourth or more so the velocity. In the vertical shaft, the speed velocity is 840 feet per minute. And, we can go up to 4,000. So, we are one-fifth of that. So, we are within certainly the industry standard.

DR. PRICE: That's some of the speeds you showed. During the cooling period, what kind of speeds are you looking at?

DR. BHATTACHARYYA: Okay. We have not done a network analysis for the total continuous ventilation time. So, I cannot answer your question when there is a peak cooling period of time we have to do.

DR. PRICE: Okay.

DR. BHATTACHARYYA: What we have done is we have taken only one drift, as a matter of fact, at a time and seeing where the cooling--the two scenarios, maybe I can make that clearer. They are continuously ventilating and maintaining a certain target temperature which is kind of an extreme scenario, I would imagine. Or if we had closed the repository--you know, closed each of the emplacement drifts and then only individually went in to cool it off, then you would need much less number of immoderate air and shouldn't have any problem pushing that immoderate air through individual drifts.

DR. PRICE: Okay. The second question I've got has to do with I think you generally said this morning that you've really got a long ways to go in the operations area. That there are a number of things that you really haven't had time as yet to consider, and you'll probably have time at some time later to look at. So, I'm assuming that really when you get down to the nitty-gritty of how you operate this thing, you really don't know yet. And, things like removal in line, if you've got something that has to come out and it's tenth in line, do you take all nine out ahead of it? If you have a gantry loaded and it fails and is non-responsive and you've got into the hundreds of tons to handle, you can't just pick it up and push it out? You know, if everything is locked up, you've got a real problem. If there's a rock fall, how do you handle it? And, if you're putting in golf ball size backfill, how do you do so without damaging what's in there? I'm just sort of making a statement, but I'm asking whether or not you agree with it or not. To work all of these things out and there's probably a lot more in the list, there's a long ways to go.

DR. BHATTACHARYYA: I agree with you fully. We are taking this one step at a time. Our first goal is to meet some of the reasonably available technology questions that are raised by the site suitability. We need to meet them. And then, eventually, of course, provide the reasonable assurance to NRC that we can do all of that. We have about five years to do so.

DR. PRICE: And, you generally have the engineering attitude that most engineers have that you can do most of this stuff; that it's doable. That, you can do. And, the second part of that is that the operations themselves, as from your last statement, not really connected with the question of site suitability? That last one is a little loaded.

DR. BHATTACHARYYA: Please, repeat that. Do you mean the concept of the operation is not related to site suitability? Is that what you're asking?

DR. PRICE: That since these are doable in your mind that there is nothing that relates to the operability of the repository as it's presently designed, envision that that would affect the decision of site suitability?

DR. BHATTACHARYYA: No, I don't view it that way because

I think it's our charter to show that we can construct, operate, and close this repository safely for the site suitability question. There is available technology, the pre-closure, rock characteristics, and all these high-level findings. All ask that we show not there is assurance to a degree that NRC would require, but to a degree where we can convince a reasonable person that this can be done without endangering the public or the person who is working on that. So, although we may have a can-do attitude, people are not going to take me on my face value. We have to show that.

DR. PRICE: In other words, you'll have to show in your mind for that to be suitable--for everything to be suitable, you'll have to show how you can go down there and rescue a couple of hundred tons of equipment that may have to be withdrawn from the drift?

DR. BHATTACHARYYA: Absolutely.

DR. PRICE: Okay.

DR. BULLEN: In the interest of moving along, I'd kind of like to ask a question of Rick Memory and maybe Hugh Benton and Dan McCright will chime in because it has to do with the use of burnable poisons both for criticality control and--I guess, the fundamental question is if you're using boral which is going to be in some of the MPCs that close up early, first, what the expected life of the boral might be, how long before the BPs are gone, the burnup poisons are gone, and could you address the materials compatibility issues; compatibility with the waste package materials that you're selecting and long-term compatibility issues? Maybe we could get either Dan or Bill Clarke to talk a little bit about microbial influence corrosion because there was some very interesting developments along the lines of boral with respect to that. But, I'll go to Rick first and ask him if he'll at least say a few words about how long do the BPs last?

MR. MEMORY: Well, I'm going to have to let Hugh address that.

MR. BENTON: The criticality control material that will wind up in the MPC will be tested and Dan McCright's material testing program at the end of that time will have a good feel for how long that will last. If it turns out that that criticality control material will not last for the period of isolation, then we have a number of options that I went through. First of all, clearly, we don't need criticality control material for all of the fuel or even for most of the fuel. We can use fillers and so on. If the criticality control material is in the first group of MPCs which would be a relatively small number and requires some kind of special operation at the repository, that won't have any major impact on the total program. There will be lots of opportunity in subsequent MPCs when the time period between when you're spending the money for the criticality control material and when you are using it is less. Then, you can afford perhaps more expensive solutions.

DR. BULLEN: This remediation that you meant at the repository would be the control rod insertion or the repackaging of the fillers, that you might have to open a small fraction of the packages to remediate?

MR. BENTON: Yes, it's possible that we would have to open some, maybe even all, of the first procurement of MPCs, but that's still a relatively small number.

DR. BULLEN: Moving on to Dan or maybe Bill Clarke, could you comment a little bit, Dan, on--

DR. MCCRIGHT: Again I, first of all, thank Hugh. You answered that so diplomatically because it's my opinion that the aluminum is not going to last very long because if we have penetration through those outer barriers for whatever caused that penetration and when that hits the inside of the container in a highly radiolized environment, I just don't think that aluminum is much, much less corrosion resistant than any of the barrier materials we've chosen. And, again, when you consider that you'd have a highly radiolized environment in there--peroxide, nitric acid types of things--this will attack the aluminum very readily. We've also probably contributed to the problem by having so much boron in there, there would be grained out reactiveness, the way aluminum likes to go. That's my opinion.

I discussed this with many of my metallurgical colleagues. But, it's because of that that we've--with some of those other materials that I've listed, we've thought that again--and Rick Weller pointed this out at the beginning because with the large MPC we thought it was difficult enough dealing with a disposable container, but now with the basket material that has to last much, much longer, it really is pushing the site, say, maybe even beyond the frontiers of material science. And, that's why in the SIP that Rich Van Konynenberg wrote, he was very interested in looking at, we might call, advanced materials. Again, he thought very highly of the commercially pure zirconium and maybe even the surrounding material again because you're so interested in this long-term structural stability.

Granted, the basket material is I think one of the more difficult things. You're asking the material to be a high thermal conductor at the early stages. You're asking it to be structurally stable for very, very long periods of time and then to have the property of being a good neutron absorber. We're finding now that one simple material isn't probably going to be there. We've talked about there's different composites of shunts and so forth, of having the aluminum in there just for the thermal role and then relying on something else that would engender the long-term stability.

I'd like to say one other thing, too, and Rosa Yang brought this up again and I think maybe two things will tie together. She mentioned about our lack of testing for zircaloy and some of the beneficial properties that the zircaloy might offer. But, first of all, in the great scheme of things, that's in a different WBS element than the containers with the waste form. And, again, because of the project and whether we're going to use zircaloy as a barrier, that's been an on again/off again thing; and trying to compete for money, again it's been an on again/off again thing of whether to test for zircaloy. Right now, we're not doing that work. However, if we are to become more interested in zirconium as a basket material--and as consummate as I am where I'd like to get a dollar's worth of knowledge out of a quarter's worth of research investment--I think gives us a very good opportunity to bring two elements of the program together and where we have a lot of commonality in corrosion problems and degradation problems of being able to solve more than one problem at a time.

DR. BULLEN: Bill, did you want to comment on MIC or--

MR. CLARKE: Well, I wanted Hugh to wear a seat belt because I'm going to support him. Actually, I'm going to support them both. I think that what Hugh was referring to was during the period of time that these things are being constructed during the surface storage which they're certainly going to have some, hopefully we are going to complete the corrosion studies on the various basket materials, and then at that point, if we discover that aluminum just cannot cut it under long-term disposal, then they can make some adjustments on the earlier ones. I don't think that what he was referring to is if we actually get a leakage of some kind during disposal, we'd have to pull them out and do something.

DR. BULLEN: Rosa?

MS. YANG: I'm glad to hear there is going to be more work in that area. But, the reason I brought it up is because, you know, it was said in TSPA-95 you're going to include the model in it. I don't see how you can include the model without the data. There are a lot of corrosion data at higher temperature in a much different environment, but not in this environment. So, I guess, my point maybe is more from a planning programmatic point of view, but there seems to be a disconnect.

DR. CANTLON: Let me pursue this a little bit further. Since the boral is a very costly part of the container development in the basket and since there is this problem of its questionable long life, why not have the utilities provide assemblies that already have the control rods in them?

MS. YANG: That's a good idea because there's a problem to dispose of those control rods. But, do the designs of the canister allow for that because it's not just the rod, there's some other structure above it, too.

DR. CANTLON: How feasible is this? It seems to me a lot of things flow from it. It cuts a big cost element out of your container manufacture. It shifts one of the expenses back to the utilities where it belongs and doesn't take away from the disposal portion of the budget. So, Hugh, I'd like to have--

MR. BENTON: Well, without commenting on where the money should be shifted which is out of my scope, but just looking at the criticality control one question would be how used are these control rods? Are we going to be able to get effective criticality control out of them?

MS. YANG: I think the nuclear use is probably only 10%. I think from a mechanical and other points of view, the integrity of the rod, there might be cracks and stuff like that on the control rod, but I don't think it matters here.

MR. BENTON: So, I think the question would be how much variability are we going to have? What kind of a testing program is going to be involved in determining that those rods will perform their function for the period of time that they have to since obviously they weren't designed for that. But, clearly, the vast majority of the PWRs have nice open, empty guide tubes and it would be a good place to put some criticality control material.

DR. LANGMUIR: Okay. Since we're bearing down on Dan some of the time here, Dan McCright, I had several questions for him which occurred because of the presentation this morning. It occurred to me--and this is my naivety, I think, that we're dealing with here. Some of the metals that might be emplaced and used for construction of the canister and within the system could on weathering create bacteria sites. Copper sulphate is used in swimming pools. Chromium maybe provides a bacterial side effect. That's one side of a question. In other words, what can you do that's going to help you by how you design your system to keep the bugs from being in there where they're going to raise havoc with all of your lovely inorganic rates? Another one is radiolysis. To what extent does radiolysis of the fuel itself on the surroundings limit the growth of bacteria locally and, therefore, their effects?

DR. MCCRIGHT: Well, first of all, Don, I was like you some years ago. I thought, well, copper ought to be wonderful because it certainly is a biocide for higher forms of life, but my understanding is that the microbial, the really small bacteria, there's certain ones that can thrive in a copper environment. Also, it was pointed out at the microbial workshop, there are even some that can live in a lead environment. So, again, the idea of using a metal as a toxic, I don't know. Silver has sometimes been promoted as also having chemical toxic properties, but again it would only work to have it in the ionic form. So, the question is whether something would work and whether it would be in the right form.

DR. LANGMUIR: Silver is going to weather to the ionic form.

DR. MCCRIGHT: That's right. But, I guess, what I'm getting to is I don't know that there is a good chemical approach to try to counteract the bacteria. But, again, I'd have to call the experts on that.

DR. LANGMUIR: The other thing that occurred to me was if you're using fillers or backfill, placing some of these substances in them at a contact with the metal. If you get to the point where you're going to use backfill, maybe later on it might be very constructive.

DR. MCCRIGHT: It would be a wonderful idea.

DR. LANGMUIR: Yeah.

DR. MCCRIGHT: And then, your question about radiolysis and so forth, again there are certain bacteria--and again this becomes very bacteria-specific which can live where and which don't live where--but there are again certain kinds that can live in a rather high gamma dose rate. I don't recall the numbers, but there are some. Again, that may sterilize some, but not all. And again, remember that they can go into a very, very dormant life form, essentially as a spore and when conditions are unfavorable. When conditions become more favorable to their growth, then they're ready to go. So, again, I don't know. We'd have to investigate that further of whether one can make use of the radiolysis effect to our benefit.

DR. LANGMUIR: We'd love to have them go dormant though. That would be just fine.

DR. MCCRIGHT: Yeah. Again, the thing that will really make them dormant is dryness. Again, don't harp on this, but from a materials point of view, I haven't heard anything that makes dry unsound.

DR. LANGMUIR: From your presentation, I saw an extraordinary array of matrix of experiments and I wondered to what extent some of those questions could be answered or might have been answered by archeological metal, analog studies, metals that have been buried in the past. There's been a lot of work on those over the years and, obviously, the only ones that we can get a hold on are copper and in some cases iron. And, also, how are you planning to use the results of the corrosion work on metals in the New Zealand geothermal systems as part of your analysis of what to worry about in performance?

DR. MCCRIGHT: Okay. There are a number of points here.

First of all, again we have actually put some of our own metal candidates in certain places in the New Zealand geothermal area and we're planning to do more. The results have been really quite surprising. It's a very, very hostile environment. So, I don't really color this very detrimentally. But, even Alloy 825 corroded very readily which surprised me. Again, pH_2, 90 degrees Centigrade, very, very high concentrations of chloride sulfate, and I would presume sulfide. That's probably the reason why the 825 went so rapidly. We haven't tested some of the other Hastalloy-C types and titanium and we're planning to do that soon.

Let's see, you had another--

DR. LANGMUIR: Well, I like that one for a minute. Can you discount the formation of conditions like New Zealand in a reflux system in a repository?

DR. MCCRIGHT: Well, that's a good one. But, see, there again, you can say, well, that's again what you get, reflux especially the microbial things again because what the microbes like to do, they'd like to take sulfate and make sulfide out of them. So, again, that could be just an awful learning experience. And, like I say, I really want to be skeptical about that. I even hate saying this at a public meeting because again it's whether those results are really relevant to Yucca Mountain or not. It's just we need more analysis on that. I haven't personally been to New Zealand yet and so I'm getting my information second and third hand and I don't know that I've gotten all the chemical story correct.

The other part of your question on dealing with the matrix of things that we are proposing and how much of this we could gain from archeological analogs, again like I say, that's one of the comforting things sometimes about the copper based materials. We have pieces that have been around for long periods of time, but it's often what we don't know is what the exposure history has been. We have to surmise a lot of that. And, again, it's value will certainly put some bounds on things, but to be able to use that directly, again I have some reservations about our ability to do that.

DR. CANTLON: I'd like to follow up on something I think Dan and James Houseworth both were commenting on and that is, as the TBM moves down and begins to approach the repository horizon, there's clearly going to have to be a change in behavior on the part of the crews there. Having been in the tunnel day before yesterday, they're using wood excelsior to hold back the sand that they're putting in behind the steel sets. And, that clearly is a practice that they should already be working in an accelerated way to replace.

Furthermore, I think I counted about 15 styrofoam cups and probably 100 or more cigarette butts in about 25 feet of traverse along there. So, you know, there's sort of style that has to be--you have to make some kind of a determination at what point in the area you're going to shut that kind of behavior off and move into this low organic contamination. It's pretty substantial.

MR. HOUSEWORTH: Well, I certainly agree on the issues of styrofoam cups and cigarette butts that we don't need those in the ESF environment. As far as the wood use, up where the TBM is now, I understand that is not a permanent use of wood. But, there are some uses further up in the tunnel; my understanding most of that for blocking of steel sets.

DR. CANTLON: Yeah. Well, I can't imagine they're going to pull the wood excelsior out where they put it. I don't think that's practical. But, getting the shift away and put other material--you could use rock wool, a whole series of other things to fill in the interstices that would work, more or less, the same way.

DR. PRICE: I'd like to follow up on something Mr. Memory said. You said that you intended to connect to the nearest existing rail line or something like that. So, the obvious question is have you selected the rail route? I think I know the answer, but I think you did say that.

MR. MEMORY: Yes, I did say that and, no, we haven't selected a route. We just concluded a transportation study that expanded on some of the earlier work that was done in the '90s, '91 time frame. And, as a result of that study, we classified routes as being--we came down to basically four primary corridors that need to be considered, continue to be considered, three that need to be considered and removed from consideration, but re-monitor in case options or conditions change. And then, there were a few that we decided needed no more consideration. But, the final selection of that route, if we do, in fact, select a route, will come out of the EIS process. The way it will be selected will be determined from the outcome of the scoping meetings and so forth.

DR. PRICE: So, you have four corridors alive now, to use your words?

MR. MEMORY: Yes.

DR. PRICE: We used to talk about Jean, Carlin, and so forth routes. Is that still the same--

MR. MEMORY: There's Jean corridor, a modified valley corridor, Caliente corridor, and a Carlin corridor.

DR. LANGMUIR: Let us shift gears here. I'm hoping I'm not interrupting anything because I was just talking momentarily with Tom.

Bob Andrews, could we go to your last overhead which is the summary of what you describe as conceptual underpinning of some detail process models and uncertainties. It's on Page 40 of your overheads.

I think one of the most useful things I've done since joining this program to learn about it in some depth was to read TSPA-93 which brought me to the point of realizing, as you have, I'm sure, that there is some major gaps you're trying to fill between '93 and '95. Your list suggests a number of things which are EBS tied or source term tied is another way I'd put them. One of the concerns I've had as a geochemist has been that I didn't sense that you were getting the information you needed from the geochemists. You were not getting enough that you could take from them to tie into a well-defined source term for your TSPA equations. There was a gap. Between the waste packages and the geologic environment, there was a large unknown in terms of performance. I see that being expressed here. What I'd like to have heard from you perhaps was more discussion of sensitivity of the importance of these things to the final TSPA performance. You've listed a number of things. The hydrologic models, I would assume you mean their--I'm not sure what I assume there. Is the question the absence or presence of backfill, perhaps, is part of the question. What do you decide to do with it? But, could you go down the list? I'll stop talking and maybe go down the list a little bit telling what specifically your concerns are and what you think the importances of these things are to overall TSPA?

MR. ANDREWS: Yeah, I'd be glad to. Maybe, I should start at the top. Let me make some general comments first.

I think when you look at this list or any list that we in PA come to, we talk about the conceptual models, you know, the foundation models, which are developed either from the design side or from the site characterization side substantiated by either laboratory information or in situ information or analog information. But, the fundamental basis of all of the conceptual models that we use in performance assessment are tied to those data collection interpretation synthesis model development, model testing, model verification, if you will, of programs within the design areas which many of these are and within the site characterization which other ones of these are. We in performance assessment, of course, take those best estimates, best based, as fundamentally based as the 1-2-3, 1-2-2, 1-2-4 worlds, give us and then evaluate whether it makes a difference. We abstract from them clearly and I talked about some of that abstraction today and then evaluate does it make a difference?

Having said that as an introduction, if we walk through that list and we talk about first the drift scale thermohydrologic assessments--so, I'm talking about in-drift thermohydrology which is to say temperatures, humidities, water contents, liquid saturations, if there are such things as advective, i.e. dripping features which intersect the drift. As you're well aware, we talked about yesterday at some length the development of the fundamental data which are being developed in the site characterization program to help --I don't want to use the word "validate"--but help substantiate the conceptual models used in the thermohydrologic analyses. There are a number of conceptual models out there for thermohydrologic analyses that have been presented to the Board and that are being used in performance assessment, as well as in the design area in thermohydrology. I think what everybody feels a little uncomfortable with is what's the substantiation of those detailed conceptual models. The substantiation of those clearly is both from a laboratory testing program, as I think Tom Statton pointed out, and an in situ testing issue.

DR. LANGMUIR: Let me jump in for a second. I'd like you to comment on the relative importances of these to the overall performance and compliance that we're seeking in a repository.

MR. ANDREWS: This one is going to be pretty darn important because the initiation of whatever aqueous or humid air corrosion processes we have are going to be directly tied to the in-drift thermohydrologic environment, humidity/time relationship, temperature/time relationship, in the drift. All of the water contact with the waste form which directly relate to dissolution of the waste form are directly tied to this. Whatever EBS release we might have that's controlled by either advective or diffusive processes are directly tied to this. This really is number one of the waste isolation and containment strategy, if you will, from Steve Brocoum's talk.

DR. LANGMUIR: What are you going to know about it in '95 that you didn't know in '93 that's going to bring closure to it in terms of bounding it and establishing--

MR. ANDREWS: Some of these things are not going to be closed. I mean, one of the purposes--and I hope that came out clear of the PA sort of effort is to evaluate does it make a difference? I'm saying significant.

DR. LANGMUIR: Well, are you saying that's--

MR. ANDREWS: Maybe not. If I look at long-term, maybe it's not. You know, it depends on my time scale, it might depend on my performance measure that I'm looking at. You know, I'm making a guesstimate that it's significant on all those features.

DR. LANGMUIR: I'm particularly interested in one a little further down your list, not to make this take the rest of the afternoon.

MR. ANDREWS: Okay.

DR. LANGMUIR: But, the waste package thermal and chemical solubility models, I'm wondering what you are being told here and what this--I'm presuming you're talking about things like neptunium solubility, for example. How important the uncertainties in these things are to your evaluation of suitability?

MR. ANDREWS: Some of these things--and I put solubility as a subset here, by the way. There's a number of other waste package scale thermochemical issues that effect corrosion, as well, the very near-field thermochemical environment affecting pitting corrosion rates, for example, of both mild steel and the corrosion resistant materials, although there's very limited information to support that. With respect to the solubility, although there is a pH dependence for neptunium solubility from the analyses that we've done in the past, that sensitivity was relatively minor in comparison to the temperature effect on--no, no, I'm sorry, I take that back. The pH impact on neptunium solubility is relatively high. We have not directly coupled any of the very near-field thermochemical analyses and near-field environment studies that are going on at Livermore with the range of pH that might be expected from a performance perspective. So, we are, more or less, assuming ambient geochemical environment inside the package for these solubility assessments.

I think there was a question earlier about the complex nature of the coupled thermochemical hydrologic environment in the drift and its potential impacts on performance. Some of those things, you know, we can address in sort of qualitative ways. Many of them are difficult to address in a very quantitative, rigorous, sensitivity analysis mode.

DR. LANGMUIR: If I recall correctly in '93, the corrosion models were you turned a switch and, all of a sudden, things were released if I remember correctly. You didn't have a statistical frequency of failure. Am I correct in that?

MR. ANDREWS: No. There was statistical distribution of failures, but per package.

DR. LANGMUIR: Per package.

MR. ANDREWS: So, there was a statistical distribution of package failures all of which were defined as the first pit going through each of the 10,000 packages.

DR. LANGMUIR: If you add bugs and kick that rate up by 10^5 to 10^7, what does it do to TSPA?

MR. ANDREWS: Well, maybe we'll have to try that one.

DR. BREWER: I'm not a geochemist, but I'd like to follow there on the--no, I'm not. But, I'm sitting here and I'm wondering what difference TSPA really makes for site suitability or setting priorities or anything else. I mean, you're asking very specific questions. I've got the general question. I mean, you do the work, who cares? How does it enter into the larger issue here? It's not real clear.

DR. LANGMUIR: We pick up on that because there was a question which wasn't going to get asked which was basically have you found that what you're observing is being fed back into the research programs and defining priorities and the funding and the next activities that are relevant to the program in the time scale to '98?

DR. BREWER: Right. That's my question, thank you.

MR. ANDREWS: You know, sometimes in PA, we make the general comment on a conceptual model and the fundamental underpinning of certain conceptual models seems not to be there, not to be well enough developed to have a lot of confidence with that conceptualization as used in performance assessment to make predictions. And, yet, we've identified some of these throughout TSPA-91 and TSPA-93 and make recommendations that those should be the focus. There are in this year's planning package, a lot of those models or the top five or 10 hitters of those conceptualizations are being hit directly saying, look, PA says that these are the things and these are a subset of them. Are the things that drive their performance and which they don't have a lot of fundamental confidence in. You can do a lot of sensitivity studies, some of these things--or all of these are, in fact, significant. You know, that they do make a difference from both a subsystem and system performance perspective. You know, maybe differently for different time periods and different regulatory measures of performance because if I go out to very long time periods and, you know, dose type standard, a lot of things become not so significant; you know, package lifetime, for example. But, for the time periods that we're generally concerned on, you know, the 10 to 100,000 year time period, these are the key model uncertainties right now.

So, I would say, do people listen? I think they do. Maybe, I'm dreaming, but I think they do. They listen on the modeling side, you know, the detailed process level understanding side, and the data that's required to support those models. Clearly, there's a number of issues involved.

DR. DOMENICO: I defend Bob. Of course, he doesn't need any defense here, but I look at Page 10 in his presentation and where he's looking to get more reasonable estimates of waste package degradation, et cetera, I get the feeling that --and, you can maybe answer me on this--that you are trying to improve your source in your model for transport. You're trying to get a better source for that which is the whole thing. Is that correct? Now, your source now where you get a drip, you get a break, and is it diffusion controlled or solubility controlled in terms of the releases as the '93 one operated?

MR. ANDREWS: For '93 or '95?

DR. DOMENICO: Well, the '93, how would this be different?

MR. ANDREWS: With regards to whether it's dissolution controlled or solubility controlled?

DR. DOMENICO: Yes?

MR. ANDREWS: It will depend, as it did in '93, on the nuclide.

DR. DOMENICO: Okay.

MR. ANDREWS: You know, neptunium is going to be sitting there on defense, but a number of the other nuclides are still solubility limited and other ones are alteration/dissolution limited. It will, by the way, be a function of the--it might be slightly different this time around because the amount of water in contact with the waste form in TSPA-95 is going to be a function of the water content in the drift rather than just pull out of the air assumption like it was in TSPA-93.

DR. DOMENICO: Yeah. The other thing is that if you can find out all these things, is your source code sophisticated enough to incorporate these details? That's the other thing. Or can they not be?

MR. ANDREWS: Yeah. I mean, I wouldn't have--yeah, everything here that I talked about can be incorporated.

DR. DOMENICO: You can incorporate in the source term. Okay.

MR. ANDREWS: Uh-huh.

DR. LANGMUIR: I had one for Steve Hanauer and perhaps for you, Benton. Carl DiBella did a service to us on the

Board in the closed session the other day telling us what we would learn quickly and a short course in criticality for most of us. But, it left me with some questions about whether the DOE is viewing the thing as I would perhaps like to see them view it. That is it sounds to me like a large part of what's being assumed by Bowman, et al., is related to geochemistry, hydrology, and geology; namely, you've got to create a 14 foot ball of silica with uniformly distributed plutonium and water in it. And, I find it very hard to see that happening any way I can imagine with some knowledge of hydrology, geochemistry, and so on. So, I wondered if you folks in DOE were taking advantage of the expertise of your in-house geochemists, hydrologists, and geologists in assessing the reality or unreality of the assumptions in the Bowman criticality problem?

MR. HANAUER: Of course, this is just what Dr. Bowman didn't do. He assumed that such an assembly was formed and only sketched vaguely what might produce it. There have been other studies, notably one at Sandia, about what might happen. It's on our plate, but we, of course, haven't done it. What we have to do includes such things as putting into the scenario the geological processes which would have to take place for any of this to have any reality. The Los Alamos Review published by Canavan and his co-workers states that the probability of these things coming together is really very small, but he was particularly interested in plutonium, water, and rock and the Bowman scenarios. As we look more generally for possibilities, if we find any, we will then have to go into the question of how it could really happen, if it could really happen. This is now my opinion because we haven't formulated what we're going to do. I think we will stay fairly schematic unless and until we find something that seems real enough to merit such detailed study.

DR. LANGMUIR: I guess as a followup question/comment, after having thought about it myself, it occurred to me that if you were ever going to concentrate plutonium from a waste package release, it would end up being codings on the base of your drifts along with the silica perhaps. Or it might end up pin fractures below the drift filling those fractures with fracture filling minerals. If you can reach criticality from that, then maybe it's an issue. But, that's about the only way I can envision that you'd concentrate the silica, water, plutonium that might come from waste packages.

MR. HANAUER: I haven't done the calculations, but I don't think there's enough plutonium in our waste packages to do that. I think you would have to include uranium which there also isn't very much of, Uranium-235. I haven't done these calculations, but it is my impression from some numbers I've seen that filling the fractures won't do it. You're going to have to get some fissionable material into the matrix, as well. Now, that's really without serious looking at the numbers. If that would transform the problem into one that was even more difficult and if this turned out to be a scenario we had to look at, we would have to look at how much volume there is in the fractures and whether you could put enough uranium in there to matter. And, obviously, we haven't done that.

DR. LANGMUIR: Just a last thought on that. My understanding is plutonium would likely be in colloidal form, either absorbed or as a radionuclide colloid itself, which means it's probably going to get filtered out without getting into the matrix and, therefore, remain in the fractures in some form. Just a thought.

MR. HANAUER: I don't know enough plutonium chemistry under those conditions to comment.

DR. LANGMUIR: Do we have any more questions? I've covered the ones I had. Staff questions?

DR. REITER: I have a question for Kal. This has to do with the emplacement rate. Looking at 10 CFR Part 60, as a part of the performance confirmation, DOE is supposed to do geologic mapping to determine whether the assumptions made in receiving the construction authorization are correct. Now, I know in like reactor licensings, lots of extensive--after they get the construction authorization, they have to do a lot of extensive mapping and excavations. Have you taken that into account? Have you talked with the NRC of what kind of a map? Are you going to have to map like all 100 miles of the drift? Are you taking that into account in your emplacement rates and what effect that may have? This is more of a question. I don't know how much you're going to have to do.

DR. BHATTACHARYYA: Let me try this. We are looking at excavating, say, in seven or eight drifts at a time. After the excavation, we assume that each of these drifts would be mapped and then they would be supported and then we'll put invert rails, whatever it is we need. And then, we'll turn that set of drifts to emplacement side. So, we'll always have a set of drifts being excavated, equipped, mapped, and so forth. So, that may answer some of your questions. As I discussed earlier, we haven't really formulated the requirements of this performance confirmation monitoring requirements here, but the thought is there.

DR. REITER: But, you are planning on mapping all the drifts?

DR. BHATTACHARYYA: That's my understanding.

DR. BULLEN: Before we continue with the staff, Dennis Price would like to make a few comments.

DR. PRICE: I was wondering whether or not we were going to end up getting to make a summary statement or something as each person--but, since I guess this is my chance--

DR. BULLEN: You are it.

DR. PRICE: Okay. I'd like to make a request that the DOE provide us with regard to the Calico Hills study those overheads which we did not see. I understand you have data. You have information that we didn't have that, in fact, they were available on overheads and I'd like to have those sent to the Board, if you would, so we could look that over. I think there might be some stuff of real interest to us because, for example, the two examples you showed us were well within the comfort zone, as I talked to you previously about, with respect to the regulatory limits. And, surely, all your data, we're not so far down the line, we'd like to see what everything looks like. And, anything else that might have been in those overheads that we did not see.

And then, I just had one little quick thing I wanted to add and that is I believe that Kal said that operations are part of site suitability and I think that's an important concept to have. That everything is not earth science as with regard to site suitability. People have to get into this thing to make it work; it's got to work. And, sometimes, I think we tend to overlook that with respect to the issue of site suitability.

DR. LANGMUIR: Any more questions or comments from those at the table who'd like to make perhaps a closing remark, if you'd like? Staff member questions?

(No response.)

DR. LANGMUIR: If not, we're open for comments from the floor, but I'd like to see that they're related to our discussions in the panel, if possible. Anybody have any brief comments related to the proceedings?

MR. MCGOWAN: How brief do you prefer? This is apparently public commentary, is that correct? Mr. Chairman, what is the time allotment?

DR. LANGMUIR: Five minutes, if you would, sir.

MR. MCGOWAN: I'll try to be briefer than that, thank you.

I hear a lot of listening in the dark. My final commentary will be candid and self-explanatory. No such thing as almost pregnant; none of us is smarter than all of us combined--that's a surprise--fail to plan, plan to fail; those who ignore the lessons of history; decide in haste, repent at leisure; and you can probably construct the rest of them yourselves. The fact is that's an accurate summary of the generally perceived state of the site characterization suitability study process to date and I appreciate all of your efforts getting us at least that far.

I'm going to skip around here in the interest of time, mine and yours. As you all know, the first nuclear chain reaction achieved by Dr. Fermi at the Argonne

Laboratory, University of Chicago, 1941, very nearly resulted in a catastrophic explosion and meltdown. It would have saved us an awful lot of time which was apparently adverted by Dr. Fermi in the final scant moments; okay, boys, put them back. In other words, knock it off. Chernobyl, Bopal, Gologna (phonetic)--Juarez, Three Mile Island--West Virginia, and the entire litany of catastrophic and near catastrophic events of nuclear and non-nuclear contexts were not the result of any single egregious breakdown, but of an accumulation of respectably non-egregious errors which at the time assumed Draconian proportion and all of which were the result of ordinary human error. In the realm of nuclear physics there is no such thing as forgivable human error. No such thing as an acceptable level of uncertainty and/or level of public acceptable risk. That's agreed apart from our time from now on. There is only negligence based on human quality deficiency carved in the damningly transparent armor of limited special interested egocentricity. I hope you take this to heart. I very sincerely hope so.

Consequently and ultimately, the underground storage of this fissile materials and high level waste is entirely out of the question. You know it and I know it. There is a viable alternative and that's of even greater significance. You could wrap this up and get to the alternative as soon as possible without spending another dollar or minute. The alternative is interim storage solely and directly essential to drastic reduction trans-elimination via deep burn down enhanced by ABC technology. Completely and permanently, eradication of toxic radioactivity from this treasured environment. The final disposition of it via transport. The--technology which you're well-acquainted with. I'm talking about a ballistic cargo projector, extra-orbital, escape velocity, sun targeted, distant planet targeted, Black Hole, Sigmas X1 targeted, and ultimately omni-radially, universally targeted for ultimate dispersion dilution, non-retrievable.

By the way, you forgot to mention how you get the emergency workers retrieved from the repository in the event of an egregious accident. You ought to discuss how you get the property back up. What if it has some bodies in it? I don't mean to be so direct and abrasive to you, but maybe you should speak to each other in tomorrow's summary meeting and get a little further down the line into the discussion.

Simultaneously, to what I proposed as the sane way to approach this, you can fully integrate and compatibly interface extant and evolving alternative removal energy systems and then create an abundance of inexpensive energy appropriate for historically unprecedented benefits, omni-applicable, universally throughout the next millennium any time you're ready at a profit and it will more than pay for the whole smear including your bill today.

I want to just say this. That vision is attainable. It doesn't come out of a textbook; it comes out of your heart and mind. It is entirely dependent upon your personal, individual, and societal decision making process and I'm including everybody. That can take a nano second or it may take the rest of human time. But, I think it's time that one of us, maybe two, maybe 12 billion, one, however tumorous, courageous, but faltering step down from the primordial tree. That is your--responsibility from this individual member of the public.

Thank you for your time and your interest.

DR. LANGMUIR: Thank you. I believe we're adjourned. Excuse me, the Chairman wants to speak?

DR. CANTLON: I'd just like to thank all of the speakers. I think this has been a particularly meaty session. I think these are important topics. We're beginning to get the kind of synthesis that I think is useful. The thing that I think we have tried to strive for in the Board's public sessions is to focus primarily on candid technical exchange, technical and scientific exchange, in a context that really sticks with the facts. I think we dealt with some fairly sensitive topics today in a very rational and pooled scientific way and I'd like to commend all of the speakers.

Thank you very much and we look forward to continuing to work with you.

(Whereupon, at 4:35 p.m., the meeting was concluded.)