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PUBLIC NOTICE BY THE
UNITED STATES NUCLEAR REGULATORY COMMISSION'S
ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

DATE: Tuesday, October 17, 1989

The contents of this transcript of the proceedings of the United States Nuclear Regulatory Commission's Advisory Committee on Reactor Safeguards, (date) Tuesday, October 17, 1989, as reported herein, are a record of the discussions recorded at the meeting held on the above date.

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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

Joint Meeting of the Containment Systems and Structural Engineering Subcommittees

Hyatt Regency O'Hare
Allegheny Room B
Chicago, Illinois
Tuesday, October 17, 1989

The above-entitled proceedings commenced at 8:30 o'clock a.m., pursuant to notice, D. Ward, subcommittee chairman, presiding.

PRESENT FOR THE ACRS SUBCOMMITTEES:

D. Ward, Chairman
J. Carroll
C. Wylie
M. Bender
D. Houston, ACRS Staff Member

PARTICIPANTS:

W. Snyder, SNL
P. North, EG&G
R. Henry, FAI
L. Minnick
A. Walser, Sargent & Lundy
W. von Riesemann, SNL

P R O C E E D I N G S

[8:30 a.m.]

MR. WARD: The meeting will now come to order. This is a joint meeting of the Advisory Committee on Reactor Safeguards Subcommittees on Containment Systems and Structural Engineering. I'm David Ward, the Subcommittee Chairman for Containment Systems.

Chet Siess, the Chairman for our Structural Engineering Subcommittee is not able to be here today. Chet was taken ill last week. He's doing fine at his home and he regrets not being able to attend. He says he will read the transcript with great interests, however, and he'll be with us as we deal further with this issue in the coming months.

Other ACRS members here are Jay Carroll and Charlie Wylie. We also have ACRS consultant, Mike Bender. Mike's serving double duty today in that he's also going to give a presentation just before lunch.

The purpose of the meeting is to discuss containment design criteria for future plants, and we've invited a number of speakers from the nuclear industry and National Laboratories and other resources. Mr. Dean Houston, on my left, is cognizant ACRS staff member for the meeting.

Rules for participation were announced as a part of the notice of the meeting, previously published in the Federal Register on September 22nd. A transcript is being kept and will be made available as stated in that notice.

I request that each speaker first identify himself or herself and speak with sufficient clarity and volume so that he or she can be readily heard. We don't -- the microphones here are just for the reporter. I'll call that to the attention of the members at the table. If you make use of the microphone, it will help the reporter to get an accurate and complete transcript.

As far as making ourselves heard to the people here in the room, we'll all just have to speak up, but it's a small room and I don't think there should be any problem. We've received no written comments nor requests to make oral statements from members of the public.

Before we call on our first speaker, I'd like to make a couple of comments. I think most of us are fairly familiar with what we're about here, the tasks that the ACRS has undertaken, but I'll just summarize it very briefly.

Over the last five to ten years, there's been a considerable growth in scientific information and a general understanding of the nature of severe accidents, core damage accidents in nuclear power plants, and particularly of the role of containments or other mitigative systems in reducing the possible consequences to the public of such accidents.

However, the ACRS and many other people have observed that for some reason, this really almost explosion of new information hasn't jelled into a new synthesis or a new synthesis hasn't been developed to guide designers in providing containments or other sorts of mitigative systems or mitigative processes to deal with what might be the realities of an actual severe accident.

We find ourselves, even with the so-called advanced reactor designs, still using the explicit criteria that were developed a generation ago for containment designs. The large break LOCA blowdown which was adopted as a surrogate criteria and by some measures has served quite well. But the ACRS is concerned that after a generation and many tens of hundreds of millions of dollars of research, certain sorts of experience, that we haven't moved on.

Some months ago, we expressed our concern to the Commission and the Commission sort of tossed it back to us and said, well, why don't you do something about it? Our attempt to do something about it, at least to start, has been to ask people who are expert and thoughtful and experienced in this business and conversant with the issues that are important, to come in and tell us what they think.

So, we have planned three information gathering Subcommittee meetings, and this is the second one. The first one was in San Francisco last month, and I think we learned a lot, a lot of interesting ideas. After we get all of the -- obviously the ACRS is trying to serve as what I'd call sort of a catalyst in a process here to develop - what might become a new surrogate, a modern surrogate.

Maybe we'll decide that it's best to stick with the old one. At any rate, we'll attempt to synthesize out of all that we gather over these few months and develop, in the end -- hopefully early next year -- some sort of advice to the Commission on what course we think the Nuclear Regulatory Commission should be taking in the future in this area.

That's really all I have in the way of background. Any of the other members, or Mike; do you have anything you'd like to say at this point?

MR. BENDER: I'll save my ammunition for my turn.

MR. WARD: Okay. We'll look forward to it. In that case, we'll call on our first speaker. Do all of you speakers have an agenda? Do you know when you're going to speak today? I thought we got them all in the mail.

MR. HOUSTON: Yes, and there are some copies back there.

MR. WARD: Our first speaker is Bill Snyder. Bill, just take the podium.

If you have handouts, we would appreciate getting them as you come up to talk. Give them to Mr. Houston and he'll pass them out.

MR. SNYDER: I appreciate the invitation I received from the Chairman to address this joint subcommittee on containment systems, particularly with respect to future generations of U.S. nuclear power plants.

I appreciate the invitation for no other reason than it's a matter that I've given some considerable thought to, one that has concerned me, and I may, in fact -- my views may be somewhat provocative, but provocative thoughts are not entitled to be endorsed as regulations, and so, you can take my thoughts and use them as you wish, and I'd be happy to explore any ramifications of my thinking on the matter.

I'd like to address some of the comments that were -- questions that were raised in the invitation letter that came from the Chairman.

First of all, I think it's very timely to develop a modern set of containment-system design criteria. In one sense, it might be late, because there are a lot of candidate designs for NSSS and associated balance of plant. They're already on the drawing boards and well advanced in design, and to change criteria now obviously has an impact on those preexisting designs.

However, it is not too late to consider revising the criteria, because none of those designs have been ordered and none have been rendered to concrete, and if we're to make changes, the changes should be made now rather than retrofitting plants after they've been constructed. We've gone through 30 years of that and we all know the inordinate costs of that sort of approach, so if we are to have changes in criteria, now is the time to have them.

Developing such a modern set of containment-system design criteria is, however, a very tall order, and I want to address some of my comments to that, what I call the "tall order".

Now, also, I want to put in a caveat that I'm going to talk about only internal events and not external events with respect to how I see the criteria might be changed.

[Slide.]

MR. SNYDER: First of all, let's look at the question of what is the challenge before us, and it is a difficult challenge, because what we have out there now is a variety of candidate NSSS and plant concepts.

You can count probably upwards of 10 different variations that cut across several primary coolant technologies, and it is difficult to imagine taking the present set of design criteria that have guided to design the containments for the last 30 years and even make modest extrapolations to those alternate NSSS systems and plant concepts and result in a level playing field of competition among all of those sets with respect to achieving the safety objectives.

It's difficult to imagine us managing the design and getting balanced safety among all of the NSSS if we attempt to derive explicit design criteria for each of those plant systems.

The real question, then, is whether the criteria should be explicit design-engineering criteria or whether they should be guidelines or principles to which these designs should respond.

I want to conclude my talk by saying I believe that there is, in fact, a way that we can define a set of criteria that addresses all of the variants of NSSS and plant concepts and provides, at the same time, a level playing field for the competition among all of them, as well as, in fact, will result in, in my estimation, a substantially-improved quality in the containments of nuclear power plants, independent of the NSSS system that's used.

Now, this is a very staggering and a tall order, because we have a bias, we have a legacy of 30 years, and that legacy is dominated by the LWR experience. That bias is embedded in the enormous investment which is made in making a success of the present family of light-water reactors, and it's an investment that cannot, in fact, be ignored, and it, in fact, forges attitudes and preemptive directions, preempts other alternative directions of the future.

Nonetheless, I think there will be competition among other NSSS vendors, other design concepts, and I think we have an obligation to provide a set of design criteria that does not force those designs into the mold that had been created by the 30 years of the LWR experience.

We also have to recognize that there's a bias in the legacy that's even embedded into the institutions that are associated with the nuclear-power enterprise in this country.

Certainly, there is a body of regulation of the NRC, and there's a staff that applies that regulation in making licensing decisions, and it is natural to cause the new concepts to be submitted to fit into the preexisting mold, the preexisting regulation, because, after all, it has taken us 30 years to evolve the present set of regulation, and the change will be very difficult, but we need to look for a common denominator that results in the best designs for safety, as well as provide the level playing field.

Speaking for my own organization, the sharply-focused attention that has been given, in the last 10 years, to the matter of severe accidents has created a technical community of reactor-safety specialists that are really conditioned, intellectually, kind of to a dogma of severe accident research, and I find it difficult, when I talk to my own staff about an MHTGR or an LMR, the first thing I find is that they're countering my observations with a set of criteria and attitudes and biases that are totally driven by the LWR experience and has no relevance to the discussion at hand, and I think we all have to be susceptible or be sensitive to that possibility.

So, again, I would say that the task before us is difficult.

I want to now look at this legacy face to face with what are possibilities in the future.

[Slide.]

MR. SNYDER: First of all, I want to look at the legacy here as Part 1 of this exercise and observe that what we have today is a result of a design approach that may, in fact, be as much institutionally driven as it was objective engineering decisions.

You know, we go around and we make distinctions between the NSSS and the balance of plant, and I argue that that, in fact, is a separation which is, in fact, a consequence of decisions that were made in the early 1960s, after Dresden and Yankee and so forth, where, in fact, the NSSS vendors became, in my vernacular, remote fourth parties to the institutionalization of commercial nuclear power. The real institutionalization at the top was dominated by the utilities, the construction companies, and the AE firms. Hence, the NSSS and the balance of plant.

The other thing that's very important about that, from an engineering point of view, and what I would recommend our viewpoints in the future, is that the NSSS is predominantly a consequence of what I call -- and not my vernacular or my invention, but what I believe to be bottom-up engineering.

If any of you have read Richard Feynman's book, "What Do You Care What Other People Think?", the last half of which addresses the Challenger accident, he talks about bottom-up engineering and top-down engineering. Of course, one has to take that with a grain of salt, since Feynman was a physicist and not an engineer, but his bottom-up approach he defines as one in which the design evolves from a basic set of requirements, and those requirements, in fact, evolve into design of components dedicated to that function and that function only and a unique selection of materials.

The balance of plant in the same model is top-down, in which you integrate largely preexisting components that have broad generic applicability in many industries and have a versatility of applications -- the counter, or the opposite, if you wish, of the dedicated applications.

The balance of plant -- the top-down -- is the classic approach to AE firms in integrating preexisting components to perform a function, but in a nuclear-power plant, we have the interesting mating of these two engineering approaches.

It is the industry's own estimates and not mine that 70 to 80 percent of those causes for the outages of the power plant originate in the balance of the plant, the top-down.

Feynman makes the argument that if you look at many, manv systems, the Challenger notwithstanding, that the unreliabilities, if I may use such a severe term, are a consequence of the top-down engineering.

Another observation about the legacy is the safety systems. They're in light-water reactors, evolving from plants that were nominally -- in, say, the 100- to 150-megawatt level to the present 1,110- to 1,300-megawatt system. As we escalated the power level, the safety systems became amendments to what I call the base plant.

I find it's an interesting intellectual exercise to imagine the type of power plant that we would have designed and built if, in fact, the fission products decayed instantaneously and we didn't have decay heat-removal systems. We would have a very different plant.

In a sense, simplistically, as we escalated the power level to accommodate the decay heat removal, we added the safety system, those systems necessary to protect the integrity of the plant and contain the fission products or isolate it from the biosphere, and so, we have these additions, these auxiliaries, to the plant.

The other thing is -- that our Chairman mentioned -- the containment building, which is the last barrier to multiple defense in depth, is really designed to withstand a surrogate of all plausible accidents -- plausible at the time the definition was provided, and of course, the essence of this meeting, at least with respect to light-water reactors, is to recognize the fact that the design-basis accident, albeit that it's served us well, is not really all-inclusive of the sort of threats to containment that will arise out of a severe accident, albeit a very remote possibility.

The other thing that is important about the legacy is that the multiple barriers that presently are in design, going from the integrity of the clad to the integrity of the primary pressure boundary and to the containment building, are susceptible to common cause and interdependent failures. These barriers are not independent. And we only need look at the case history of accidents to find that they are not independent. The TMI-2 accident, the accident began with really the issue of the integrity of the second boundary, namely, the primary pressure vessel boundary. And of course, it propagated ultimately to encompass all aspects, all three barriers, therefore, in fact, violating the containment of the total containment system. That is sort of the legacy we have.

[Slide]

MR. SNYDER: I would like now to look at an alternative future design approach. And I forewarned you, I may be a bit provocative in some of my ideas here. But I think we need to consider, and consider seriously, whether the total nuclear power plant should be designed with increasing emphasis on "bottom-up" design, both in the NSSS and in the Balance-of-Plant, is it possible for us to think about the total plant system and to achieve balanced reliability performance across the total plant, and drop the distinctions between the NSSS and the Balance-of-Plant. Because I think, from a safety point of view, and from the standpoint even of the best engineering and optimization for safety, the distinctions are probably causing us more trouble than they are in fact giving us a quality plant.

The other somewhat provocative view is that we should not make distinction between safety, safety-related and non-safety systems, again in keeping with the idea that you get, that the total system has relevance to, and all aspects of the plant have relevance to, the safety.

I would argue that a reliable plant, the base plant, without regard to the containment system, contributes as much, if not more, to the safety of the plant than what we define today as the safety systems.

If you look, in fact, at the societal consequences of reactor accidents, you will discovery that the societal costs are totally dominated by the capital loss of the capital value of the plant, putting any reasonable, even conservative estimates, on the "value," in quotes, of health effects, fatalities, et cetera, except in those cases where the accident is extremely severe, almost to the limit of being incredible.

And so if we provide the quality reliability total system that precludes any significant damage to the plant and the loss of the potential capital value of the plant, we have in fact bought an outstandingly safe plant. And I think we need to approach the design in the future from that point of view.

As our Chairman knows, I participated in a meeting in Lyon in the third week in September of this year. And the meeting dealt with the operability of nuclear power plants in normal and adverse environments. Some of the discussion items at this meeting were involved in that meeting, and it was interesting that this question of the distinction between safety and safety-related and non-safety systems came up repeatedly.

At the closing session of that meeting, Pierre Tonguy, who is the head of the regulatory body in France, made the very pointed announcement that in France, in the future, the nuclear regulatory body would make no distinctions between safety, safety-related and non-safety systems, that they found from the standpoint of regulating to increase optimized safety it was a distinction that did not serve them well nor did it serve the French industry well, the nuclear industry well. And they are dropping the distinction with respect to future plants. I think it is something we need to seriously consider when we look at how we direct future designs.

I would like to skip the next viewgraph, the so-called three of three, because I offer for your thought a concept which I have toyed with for some time called "Total Performance Management" in which I define "total" -- let me put this up just briefly --

[Slide]

MR. SNYDER: -- Total Performance Management, as the complete plant system, and over the full projected life.

The essence of this is that we need to look at the design of the plant with full consideration, full objective consideration of both the deterministic and the probabilistic events throughout the total projected life of the plant, whether that is 40 years or 60 years. And we need to look at optimization of the performance of that plant against the indices of safety and against the indices of economics, because I think there are decisions that can be made in which, by changing the costs from operating costs to in fact capital construction costs, and shifting costs from the probabilistic driven component to the deterministic, we can minimize the integrated costs over the plant life. I think it is a concept that needs to be looked. And I might point out that a conference on this subject was held under the auspices of GPU with attendance from many industries, in Parsippany, in early September. And much of the thinking is driven in this direction.

GPU is thinking along the line, and I think interestingly enough, Union Carbide, as a consequence of

Bophal, is thinking about concepts analogous to Total Performance Management.

I think it might be a concept that we ought to include in our thinking on how we direct the nuclear industry in the future, including those aspects of containment and protection of the public.

[Slide]

MR. SNYDER: I want to now sort of bring this to the points that I want to make. And that is, how do all of these observations translate into the matter which is before us, and how do we achieve this desired situation in which we provide a set of criteria that would guide all plants, irrespective of NSSS and Balance-of-Plant design to get optimization from the standpoint of both safety and economics.

Given the fundamental nature of the fission reactors, I see no alternative but to retain the cardinal concept of multiple barriers to attain safety in depth. I think that is an underlying principle, irrespective of the NSSS And the design fo the Balance-of-Plant.

Before I talk about the second two bullets, let me describe to you a perception that I have that is the foundation or the common denominator that we can provide between NSSS and Balance-of-Plant designs.

First of all, I would argue that any nuclear power plant, regardless of the fuel, the primary coolant that is used, et cetera, that they can all be characterized by three state descriptions.

Those three state descriptions are: the clean and cold state, fueled but never operated, not previously operated; we can define a state of the nuclear power plant, which is what I would call the power operation; and then there is the third state, which I will describe as the standby state: no fission process going on, but in fact continued to generate heat from the fission products, and operating systems in place.

Now, the standby state that I described has two substates. One is the deterministic, hot shutdown state. It is in a state which you have chosen by design, and according with your normal operation.

But there is a second category within the standby state. And that is the variable in what we call the safety state.

The transition from the power operation state to the standby state, you can make that transition under two broad categories: the deterministic state in which you say, I want to go from the hot, from the power operation state to the standby state; the other, in fact, in which you are pre-empted by virtue of failure of components, or otherwise, in the system; and the end state is a probabilistic state.

The standby state can be predicted. And we do this in probabilistic risk assessment, based upon the reliability of the components, the various accident scenarios.

But the point I wanted to make is that in all three of these states, elements of the system, components, are shared in common. Many of the components are common to all of these states. And certainly the components that are involved and the subsystems that are involved in the deterministic standby state are the same components that are involved in the probabilistically driven state which we call the accident state.

The point I am getting to is that I think we should stop making the distinctions between safety systems and non-safety systems. We should look at total plant reliability. And I would argue that in all of these states, we can predict with reliability models, which are a complement to fault trees and event trees, but have a broader coverage than the fault trees and event trees as we have used them. And to use these reliability models to make the prediction of the behavior in all three of these major states as well as the two substates of the standby condition, we can track throughout the plant life the adequacy, the validity, of our predictions of the reliability of the system, because we have day in and day out operational data.

Now fortunately, we should not have sufficient experience with the accident state that we can really predict the reliability or the adequacy or the validity of our predictions in the accident state. The fact is that the components are common in the standby -- or the accident state with the normal operational state.

So there is an extremely valid database on which to continue to draw conclusions about the adequacy of the standby/accident state. I think, given that, we can then come about this matter of design or establishing containment design criteria from a reliability point of view.

For each of the multiple barriers from which we derive safety, we can define reliability criteria as indices. One: the reliability of the barrier to withstand successfully the credible threats from credible initiators, and the other we can, in fact, establish reliability criteria for the collective internal systems that credibly through failure and malfunction could initiate a threat to the barrier.

Moreover, I think for the total system, we can define reliability criteria as in an index of successful performance of the composite containment function, which in many respects that which protects the barrier is also that which is used in the standby condition. I think by this approach of establishing a design criteria, we can give the designer considerable latitude against an index of performance which is reliability based, which provides, in fact, a level playing field for all candidates for NSSS and balance-of-plant design or total plant design.

We will not stifle by that means, innovation. We will not force the concept in preexisting molds and we will avoid what can be a detriment, albeit a detrime of a legacy of 30 years, albeit that that has been a successful legacy.

I offer you these probably somewhat provocative comments this hour in the morning. It is not conventional, but maybe it will stimulate some discussion. I thank you for your attention.

MR. WARD: Thank you, Bill. Let's see, one question I have is; you know, from the technological standpoint, the sort of artificial separation of balance-of-plant and NSSS or safety system and non-safety system has some problems. But from the regulatory standpoint, if we end the distinction, I think there might be some problems.

What do we do; double the size of the Nuclear Regulatory Commission? Do they begin to get into more and more things?

MR. SNYDER: I think that's a risk. I would think that a positive view of that is that the suggestion that I have made as to how we approach the design and not making the distinction, that case you make to the Nuclear Regulatory Commission is the same case that you make to the Public Utility Commission and the same case you make to the Board of Directors of the utility.

I think you are not making these -- you don't have to make these distinctions. Now, the question you pose is whether, in fact, in the vernacular, the regulatory body gets into your knickers in areas where you don't want them.

I think that's always a hazard, but I have a lot of confidence in the industry that can handle those sorts of things. Yes, there is that risk.

MR. CARROLL: It seems to me that the points Dave made are all very valid ones, but the one that would really concern me -- and to answer that way, I do agree with you that big pieces of balance-of-plant ought to be treated the same way as the so-called safety-related systems.

The big problem in my mind is that somebody's got to get sensible about what quality assurance means in the United States. I can understand the French perhaps going on the direction they're going, because they have a much more sensible QA program and QA requirements than we do.

But to just say, okay, we're going to make everything safety-related, you might as well kiss the nuclear industry goodbye in the United States, because you couldn't afford to build a plant under our present QA requirements.

MR. SNYDER: I think that while I don't disagree with your observations, but let me put a different spin on it in a different direction. I won't disagree with your observations, but I think that your observations are predicated on all other things being unchanged.

I certainly agree with your conclusion. My view is that other things ought to be changed, and while I don't have time to get into it here, the talk that I gave in Lyon looks at the matter of the reliability of the total plant and what drives or what is the determinate of the reliability.

The determinants of reliability are many. Some of them occur on the designer's table, but some occur in the board room and some of them occur out on the floor by the maintenance personnel.

The thing that is pervasive in my thinking here and in the talk that I reference, is the need to stress quality in all aspects of the plant. But the best index of quality across the board is a reliability index, without distinction of where it resides in the plant. I include in the plant the front office, whether it's the board of directors or whether it's the janitor down on the floor. I think you have to stress quality throughout.

So I agree with your conclusion, given that nothing else has changed, but I think other things have to change and at least in my view, my perception, it is that the leaders -- let me elaborate and say the enlightened leaders of the nuclear electric utility industry are adopting an ethic of excellence, and I think in due course, that will change. I think that's a necessary condition if we are to achieve the quality and be allowed to make the distinctions that I am suggesting.

MR. CARROLL: That's fair enough.

MR. WARD: Yes, Mike?

MR. BENDER: Generally, Bill, I guess that I support the thesis that you developed here. It really is important to use something beside arbitrary standards for deciding what is important to safety. Realistically, though, the people that are developing these concepts for the most part, do not have enough freedom in their philosophical approach to be able to do it that way.

My belief is that there had to be some combination of bottoms up and top down.

MR. SNYDER: Keep in mind that I did not say "only."

MR. BENDER: I didn't accuse you of it. As a matter of fact, that was the sense of what you were saying. Most of it should be bottoms up.

MR. SNYDER: Yes.

MR. BENDER: But there has to be some top down.

MR. SNYDER: Yes.

MR. BENDER: The thought that I wanted to offer was that in developing an approach of this sort, it's necessary to have something near to the French view of things. You have to be pragmatic about it.

The reliability judgment is going to be subjective because the data is not sufficient in the environment which we're talking about to make a judgment, and so in the end, it will rely more on how good the thinkers are in thinking out the scenarios and less on whether you can point to one piece of hardware or another or one system or the other and say it's better for the purpose.

My thought is, in developing these new criteria, that a lot of study of the logic has to be displayed so that the working level designers can understand what the logic is.

MR. SNYDER: Yes. Let me observe that -- and I don't disagree with your observations -- last week, I had a two -day meeting with one of the NSSS vendors for one of the candidate future designs. I was very gratified by the viewpoints that were brought to that discussion by the chief engineer, the chief designer over that project.

Many of the thoughts that we have been discussing here are underlying their approaches to the design, but there is some hesitancy to embrace these concepts fully because there is this legacy, the legacy embedded in the regulatory process, the licensing process, the tendency to want to force all subsequent designs into the preexisting mold.

Also there is a concern about the rate at which the change is made and the direction which we see as the desirable end objective. So, to be pragmatic, we may continue to be in an evolutionary mode, progressively approaching what we define here as the more idealistic.

I think one point I want to leave with you is that from my point of view, regulation as taxation can have two dimensions. It can have a negative connotation, but it can be motivational and leading, depending upon how it is defined and structured. What I am suggesting is that we have an opportunity in the area of talking about containment systems which is a key element in the total safety system, that we can define criteria that will, in fact, motivate and lead the industry. I think that's a role that can be played.

MR. CARROLL: I'm not sure I'm totally clear on your notions about a level playing field for the different reactor concepts when it comes to future containment design criteria. Could you amplify a little bit on that?

MR. SNYDER: Level playing field; what I am saying is to give all concepts equal opportunity at the bar, so to speak, and to serve the public with the best future nuclear power plant design, both with respect to safety and economics.

MR. CARROLL: If I were a proponent of high temperature gas cooled reactors, I'd argue a level playing field would be to allow me to build a plant without a containment.

MR. SNYDER: Whether the playing field is level depends upon where you are with respect to the 50 yard line, but I think there are ways of defining the level playing field. I provided you with my definition, yes.

MR. CARROLL: Okay, thank you.

MR. WARD: Going back again to the safety grade versus non-safety grade distinctions -- not that I'm a fan of that distinction -- but it's been useful as a resource allocation guide. Both the energies and attentions of the regulators have been given to the NSSS system and QA has -- I mean, the strategy for QA has been divided the world in half and give all the loving attention of QA to these systems and none to this.

Now, I think what we found is that that's been too coarse a cut and has, in fact, -- maybe it's failed. I think some of the things that you've said indicate some sort of a partial failure. Still, if we look at those two as some finite resources that regulators have to do whatever their job is supposed to be, some finite resources that a QA process has or some sort of auditing process has, how are you going to decide where to put those resources?

Are you going to use PRA or a new set of judgments or what?

MR. SNYDER: Let me first of all challenge your assumption that that distinction has served us well from a resource allocation. Oh, okay.

MR. WARD: No, I don't think that's true.

MR. SNYDER: Okay.

MR. WARD: I in fact think that the way we have made that distinction between safety and safety-related systems, insofar as that -- the resource being defined as an investment. If we look on the return on that investment, I don't think that the return has served us as well as it would have served us, had we not made that distinction.

By reason of the fact that we have tended to focus on safety as a separate and distinct aspect of the plant, and I think that's largely regulatory-driven. Whereas if I look at it, as a member of the consuming public, I think if we had in fact put some more balanced emphasis on the reliability of the plant overall, to minimize availability, I think we would have had a more economic plant, but we would have had an equally if not safer plant.

Now the cardinal, the key, the keystone of the argument I'm making, and this is the part of the other consideration that I've talked about and the other talk that I gave in Lyon, is that if you achieve reliability in the operating plant, you in fact have bought safety in the plant. I would make simplistic observation that these plants do not have accidents at power.

They have accidents when they're shut down. The stand-by systems, failing to perform their pre-determined or desired operating state, or they're shut down because of falling outside the limiting conditions of operation, and you don't transition to the desirable standby state, but you transition to an accident state that has been determined by which component failed in the operating system.

I think the point comes back to maybe the following observation, that if I have a plant that is -- I'm having a frequency of five or six unanticipated automatic trips per year, versus a plant in which I'm having statistically an average of a half, the fact of the matter just by those unanticipated automatic trips, I'm challenging the safety system ten to twelvefold more frequently than I am with the system that only has a half per year. I think that's a significant observation.

MR. CARROLL: Yes, but I wonder if it's a correct observation. If you look at the body of PRA results, transients, which is what you're describing --

MR. SNYDER: Yes.

MR. CARROLL: --isn't really a very significant risk contributor. It's been conventional wisdom that we've got to avoid scrams, but -- and I think it's important to the utility to avoid them from an economic point of view, but I just really wonder, in light of what we know about PRA results, whether it's a very important risk contributor.

MR. SNYDER: I'm a little -- I've been very close to PRA for 15 years. Sometimes I feel uneasy with the conclusions we draw from PRA. I draw better conclusions with this respect from more conventional reliability analysis. When I look in fact at the reliability of the desired state of the system as distinct from looking at the sequence of events that occur after the transition to the standby/accident state. I'd like to discuss that with you, because I'm not sure I draw the same conclusion.

MR. BENDER: Well, I'd have to disagree totally with that conclusion, as a matter of fact. It seems to me that it's hard to find an accident that didn't initiate as a part of a transient of some sort. The steady state things just don't occur.

MR. CARROLL: Well, transient as defined in PRA does not include, for example, loss of off-site power. That's a separate sort of event where --

MR. SNYDER: Oh. You have a distinction between on-site and off-site events and internal events and external events. I'm not sure I'm prepared to discuss that intelligently. These are subtle distinctions here.

MR. BENDER: Well, I don't make that distinction.

MR. SNYDER: Okay.

MR. BENDER: As a matter of fact, being a non-proponent of PRA as a judgment tool, it's hard for me to even debate the issue. But from the standpoint of operational understanding, the events that upset the systems are the ones that get you into trouble, no matter how they occur.

MR. SNYDER: Yes. What's embedded in some of my thinking here, as you know, if you look at these three states and in the transition from what I call the power operation state to the standby state, the distinction I wanted to make is that you go from the standby state -- from the power operation state to the standby state.

You can do it on command of the operator, or it can go from the power operation state to the standby state automatically when any part of the system exceeds the limiting conditions for operation. But you can go to the standby/accident state by a failure of any number, any of a large number of components.

So the operating state and the standby/accident state is probabilistic and has a very large number of initial boundary conditions. Those are the ones that we analyze with probabilistic risk assessment. We never get around to looking, in fact, at what the reliability of the operating state, and if you improve the reliability of that operating state then you reduce the number of those probabilistically-driven standby/accident states.

I think you can regularly simplify -- I think you can improve the effectiveness of the safety system, which I call a standby/accident state.

MR. WARD: Bill, thank you very much for an interesting and provocative set of remarks.

MR. SNYDER: Thank you.

MR. WARD: At the end of the day -- I mean I hope everybody's going to stick around, so at the end of the day perhaps we can get some comments from some of our other speakers on what you've said also. I look forward to that. Paul North is our next speaker.

MR. NORTH: I would like to thank the Committee for their invitation to attend today and to make some comments on the containment design criteria for future nuclear plants.

[Slide.]

MR. NORTH: The form of the discussion that I shall follow is that first I'll take a look at the philosophical foundation for any approach that might be adopted and try to establish that foundation, then look at some important conditions or what might be considered boundary conditions in the definition of an approach using that philosophical base and then make a few comments about what might be some points in an actual approach and in some related methods that might be associated with implementing that approach.

So first then, let's turn to the philosophical foundation for the approach.

[Slide.]

MR. NORTH: This goes back to really something fairly basic, I guess, and it is what is the service that we are really trying to provide to society over all in the consideration that we're about today. I think obviously we're trying to protect the health and safety of the public but we are trying to do it in the context of nuclear energy as a contributing component of our energy supply system.

My point here is we could certainly protect the health and safety of the public by simply shutting down all of the plants and then we wouldn't need a containment criterion at all, so the word "contribution" or how nuclear energy might make a contribution to our energy supply is germane to our considerations of what might be an appropriate set of containment criteria.

Let's take a look at what that contribution might be for a little while in terms of then establishing the basis for approach.

First of all, within the United States itself it appears that there is a diminution of the electric generation reserve capacity. We are seeing that in a number of locations around the country and it's causing often a reassessment of various generating alternatives including nuclear energy.

Certainly there are environmental concerns and these concerns are particularly related to the generation of electricity with the expanded use of hydrocarbon fuels. There is a great deal of concern with regard to the greenhouse effect, acid rain, et cetera.

There is also the possibility of additional uses of nuclear energy, process heat being one of them. In fact the Japanese are well along in designing and establishing the experimental base for the application of nuclear energy to process heat.

There are other forms of energy substitution which are being discussed and in fact if we're to reduce oil importation these are necessary, so overall, if I look at the prospects for nuclear energy within the United States it certainly appears that there is a potential for significantly increased use of nuclear energy within the United States itself.

[Slide.]

MR. NORTH: But what about the world energy picture? All of the energy studies that I have read of late have forecast relatively large growth in energy demand by the world community through the first half of the next century. The numbers vary from study to study but the basic conclusion appears to be a consensus, that there will be significant energy use growth through the first half of the next century.

Furthermore, embedded in those studies is a statement that the largest growth will occur in areas where the current per capita energy consumption is much lower than is characteristic in the United States today, probably in areas where the ratio of use today is about one-eighth of ours. That is an important point. If this large energy growth, which will occur not only through the growth of our own economy but also through the shifting of the base of the economy of others to be more like ours, occurs through expanded use of fossil fuels it will provide a great deal of environmental concern, to say the least.

Suffice it to say right now the Japanese are extremely concerned about that very prospect in terms of China burning coal, the Japanese being down-wind from the Chinese system.

If you take those factors into consideration and also see that there are indications already that the United States, European and Japanese nuclear industries will seek to serve this global energy market, in fact there will be a move into these energy deficient areas by the nuclear energy component.

Now if we pull all that together, what does it have to say about the subject we are here to address? Basically, it is this: We really must address the possibility of a much wider use of nuclear energy than is evident today and in a much broader geographic and societal setting.

[Slide.]

MR. NORTH: That last point is quite important because it implies that the plants may sometimes in the future be located in places where there is a lack of the support infrastructure which exists within our own society today.

There is a very important parameter to all of this. Obviously if I am talking about nuclear energy making a very wide contribution to our energy needs in the future and relatively a large number of plants being distributed throughout the world, then in reality large numbers of people must support the use of nuclear energy. They must support it as individuals and through their institutions if nuclear energy is to make an appropriate contribution to our energy systems. I have underline the word "support" because I think it is very important that we address that specifically and head-on because I am not advocating acceptance. I am advocating support and we must work to get it. If we aim for acceptance and miss, we might get rejection. If we aim for support and miss, then we may end up with acceptance but it is my basic position that we need to look for an action on our part that will generate support for nuclear energy in a much greater context than it has occurred in the past and so as a result we at least have a foundation for an approach.

[Slide.]

MR. NORTH: First of all, the containment criteria should be linked to clear protective or regulatory objectives formulated on the basis of wide application of nuclear energy within the United States and in the world at large. The containment criteria should also be formulated in a way that allows progressive design innovation in meeting the protective objectives. We are going to be living with nuclear energy for a long time, I believe, and we certainly don't want to confine the designers to our current thoughts or concepts if we can avoid that.

I believe that an approach should be based on rising standards of adequacy from design generation to design generation. That is a modification of Hyman Rickover's standard of rising standards of adequacy and the emphasis there is on the word "rising." As you go from one generation to the next, we should look for improved capability.

MR. WARD: Are you going to explain why you think that is important?

MR. NORTH: Yes.

MR. WARD: Okay.

MR. NORTH: As we go through, and if there are subsequent questions I'll be quite happy to discuss them with you.

MR. NORTH: I think the approach should be one in which the approach itself and the related methods provide a basis for strong support of nuclear energy by large numbers of people, as I indicated in the outline earlier.

[Slide.]

MR. NORTH: There is one possible short-term override that I want to recognize and that is in the immediate future there is a possibility maybe of a judgment by the Commission that a traditional containment structure is necessary to ensure support for the further use of nuclear energy regardless of the reactor system design and the details of that, and I would just like to make a few comments about it.

First off, it would be disappointing technically if that were to occur, as you will see in my approach. Obviously I would consider that to be disappointing technically. It would be understandable as a judgement, primarily a social judgment, even if I personally was disappointed with it, but I am going to proceed in this discussion on the basis that no such judgment is made and that we do in fact look at the technical aspects in the foundation for the thing.

[Slide.]

MR. NORTH: Let's move on then to the defining of the approach itself. There are some related conditions that I'd like to address. The first is in making the approach, there is a sound engineering method or outline which I believe we ought to follow. That is that we should base our approach on best estimate mechanistic analyses and understandings, supported by adequate physical understanding, with factors of safety being added explicitly at some point in there, rather than trying to manipulate the phenomena themselves.

I think we've tried that on a number of occasions. We've tried it specifically in the ECCS area, and it ends up being rather difficult sometimes when you're making what are called conservative assumptions which actually manipulate the phenomena, to determine a priori what might be a conservative assumption or what might not.

So I think it's a much sounder approach to recognize from the beginning that you're going to attempt this through best estimate analyses, mechanistically based. You're going to try to support it with adequate physical understanding of the phenomena that are involved, and you're going to add factors of safety explicitly later.

I think this approach can be understandable and convincing to people that are not involved in the work and therefore it is conducive to that generation of support that I mentioned earlier.

[Slide.]

MR. NORTH: Obviously we've got to consider what you might call "fault tolerance," if you like, and I've expressed this by saying any new systems that are aimed at containment or providing the containment function, should be capable of a demonstration that they have robustness in achieving that containment function.

That can be achieved, in my mind, in several ways. It may be through the use of basic physical characteristics, which are clear and will always occur. It may be in the form of a design, which is tolerant of faults in some way. It may be through the very careful implementation of Defense in Depth, with independent multiple layers involved in that, that are effective for the entire accident spectrum.

This is a point that the previous speaker alluded to, and I underline, we need a careful implementation of Defense in Depth. Just because you have multiple layers, doesn't mean that you've achieved it. In fact, in some cases you can think of, the failure of one in the progression of an accident automatically implies the failure of other barriers. In those cases, then you haven't achieved the independent multiple layers of the Defense in Depth concept. So it needs care in application, and it needs it for the entire accident spectrum.

Also, I want to draw out the requirement really of the absence of a possibility of bypass, if possible, because a number of containment functions can be designed, but then additional equipment can allow you to bypass that containment layer in some form.

I've read quite a bit about whether we can deal with this topic entirely through the concept of prevention, or whether we have to err in the direction of mitigation. I believe that there needs to be a balance between prevention and mitigation, and that there will always have to be a balance between these two things because there will always be residual uncertainty in prevention.

In making the prevention case, one is in the position of trying to prove a negative. That is, that there is no transient that you have not considered, and that's very difficult to do. Therefore, I believe that there will be a continuing need for a balance between these two approaches.

MR. CARROLL: On your absence of bypass, I don't see or I don't understand what that means in the context of a boiling water reactor, where normal operation you're bypassing the containment, or in a pressurized water reactor, where there is mechanisms for bypassing containment. How do you eliminate this?

MR. NORTH: I'd leave that to the designer sir, but --

MR. CARROLL: Well, it is inconsistent with the physical nature of the processes.

MR. NORTH: Well, I understand that you're telling me that today's containment systems do have bypass mechanisms, and I think those bypass mechanisms are weaknesses in today's containment systems or can be. You may rely on valves to function and close, etcetera, and while these may have an expressed reliability, I think inherently a system that can reduce or eliminate the possibilities or potentials for those bypasses is an improvement and that's all I'm pointing out.

MR. CARROLL: Okay, but you're not saying -- you're not saying that that should be your sole objective. I mean there could be --

MR. NORTH: No.

MR. CARROLL: --an acceptable new sort of containment system that didn't totally eliminate bypass?

MR. NORTH: Well, the total topic was robustness in achieving the containment function, and what I was looking at in the subheadings below were a variety of ways in which you might attempt to approach that, and demonstrate it.

MR. CARROLL: All right.

[Slide.]

MR. NORTH: There are a couple of other items that I'd like to draw out here. One is the fact that as we look further downstream through the first half of the next century, it's possible to conceive at least of much longer plant lives than we have today. Obviously the basic lifetimes we're looking at right now are on the order of 40 years.

People are already studying the potential for life extension. It may be to 50, 60, 70 years, something in that order, and I don't think that it's entirely unreasonable to see some distance in the future people being -- talking of lifetimes in the order of 80 to 100 years for plants.

If that's the case or anywhere close to the case, then there are some implications as far as society is concerned. First off, a site which was originally remote may become a lot less remote over the period of that length of time, and we've seen it in a lot of cases, for example, with airports that were originally built some distance away from the city which they served, and which later then were engulfed by the city, with all kinds of complications as a result.

I think it will not be a service to society if we're looking at wide use of nuclear energy over a protracted period of our history now. If we attempt to limit the land development possibilities by saying well, don't develop into these areas, it's going to be -- that would be very difficult to enforce and would not be a real service to people anyway.

With an increasing nuclear fleet then, a lot larger than we may have now, approaches that allow even a remote possibility of farmland withdrawal or the closure of neighborhoods, both of which occurred following Chernobyl, will be increasingly unacceptable to the world society, and both of these factors are going to militate for an approach that concentrates on the characteristics of the plant itself and does not rely on any external response by the rest of society.

[Slide.]

MR. NORTH: Taking those basic thoughts, what I'm now going to do is go back to the elements that were in the foundation that we generated from the philosophy, and make a few comments about each. This is where I come back to the topic of rising standards of adequacy.

First off, that philosophical approach is consistent with the advanced reactor safety policy statement, within which it's stated that new reactor designs should have at least as high a level of protection of the public as current designs do, and it is the expectation of the Commission that in fact subsequent designs would be able to demonstrate some higher level.

So it isn't at odds with that policy statement. I think also if we're looking at this growth of nuclear energy maybe over the next 50 or 60 years, we have to recognize that there are levels of advanced designs that are already in process or in conceptual stages. These should be recognized, and the approaches should be defined accordingly.

There are designs that are really logical evolutionary steps from operating light water reactors. A lot of the advanced light water reactors I would fit into that category. They're not substantially different. They have modifications to them, but they are not as different, say, as the PIUS light water reactor concept or the HTGR or something like that.

There, I think that we've got a good foundation to build from, so long as we build out from that, demonstrate compliance with the severe accident policy, demonstrate improved efficient product retention, as required by the advanced reactor policy statement, and couple that with features such as a longer transient response time, designing them to tighter productive objectives. We are in fact following that philosophy, and that so long as we establish the right protective objectives, this is a good approach.

Now designs that represent a greater development step and are aimed at later deployment, I think we should use more performance-related criteria that allow more design innovation and flexibility and that as successive steps then, we establish tighter protective objectives in the application.

[Slide.]

MR. NORTH: Now, let's come to those protective objectives.

In the near term, the type of objectives that are being discussed in terms of the advanced light-water reactors appear to be acceptable. The objective of having a core-damage frequency less than 10 to the -5 per year and a site-boundary whole-body dose less than 25 REM for accidents for cumulative frequencies exceed 10 to the -6 per year seem reasonable objectives to me in terms of the design of the advanced light-water reactors.

If I was going to a much longer-term view, with different types of reactors in mind, I think there we might go beyond the consideration of not having off-site emergency planning as requirement, but make this condition, in fact, a specific design objective. That is to say bring me a solution to the containment problem in the context of safety which specifically does not require off-site emergency planning.

MR. WARD: Paul, would you go back to that, please? I did not understand the first bulleted item. Core-damage frequency less than 10 to the -5th per year --

MR. NORTH: Comma.

MR. WARD: Oh, okay, comma.

MR. NORTH: It runs on there. There should be a comma to separate those twc statements.

MR. WARD: Okay. All right. I got you. That's all. Thank you.

MR. NORTH: Okay.

[Slide.]

MR. NORTH: In the way of implementing that longer-term objective, as an intermediate objective, the statement in SECY-88-203 of stating that we would aim for not violating the Environmental Protection Agency protective-action guidelines -- that's the lower levels -- for the first 36 hours of a variety of accidents, and then, if you take in all accidents, they would have less than some probability, 10 to the -6 per year, of exceeding those Environmental Protection Agency protective-action guidelines appears a very reasonable way to proceed.

The ECI and ECII and ECIII events there are really categorizations that distinguish between those events that are likely to occur in the lifetime of a single plant; that might occur in the lifetime of a whole fleet of plants; that are, lastly, not expected to occur but which perform a design-basis function; and a final category, IV, which is implied in the second bullet, which is beyond that design-basis set.

Certainly, overall, the appears a reasonable way to go and might provide at least a first foundation for not having offsite emergency-action requirements.

Possibly, in the long term, we could aim at the lower-level EPA PAGs never being exceeded at the site boundary by any credible accident condition that would exist within the plant.

[Slide.]

MR. NORTH: I think there's one further point that you might look at then. Not only do you not want to have to have people evacuate, you don't want to deny them their farms and neighborhoods when you get through, either, and so, maybe there should be some consideration about protection of the land, as well.

Now, that might be -- we've been having some discussions at the laboratory, and that might be stated in some residual-activity level associated with the land or some limited cost to restore the land to a condition where people could return to it. I don't know. These needs some more development and some more thought, but the primary thought that I want to lay on the table here is the thought of such a criterion itself, rather than a statement of what it might be.

MR. WARD: On that aspect, there was an ANS workshop on safety objectives out in Idaho Falls in -- I guess it was in August. This issue was discussed, whether the NRC's safety goal, for example, should include something like this or whether the regulatory agency should have some sort of parallel guidelining or whether the industry should have some sort of guideline parallel with the safety goal in this area.

MR. NORTH: Yes.

MR. WARD: And I guess your work is consistent with the discussions that were going on there.

MR. NORTH: Yes, and in fact, it's being discussed, also, in terms of other facilities, various buildings and processes that the Department of Energy has, should you have this kind of thing. I don't have an answer for you, but I am putting the thought in front of you.

MR. WARD: Okay.

MR. CARROLL: I guess the comment I have on the emergency-planning issue is it's one of our legacies. I just don't really believe, given the emphasis that's been placed on emergency planning, that at least the next generation of plants is going to succeed in being able to say we don't need any offsite emergency planning, because our plant is so safe. I just don't think that's going to happen.

I think it may be a very good design objective to be able to say that we meet these EPGs, but I think you're certainly going to have to consider the structure offsite to deal with emergencies for the foreseeable future. I speak with some authority, having fought the battle of emergency planning at Diablo Canyon for about 15 years.

MR. NORTH: I'm sure that in today's environment, it's a difficult topic, and we won't change it overnight.

Let me move on.

[Slide.]

MR. NORTH: I want to come back to that topic of using best-estimate analyses with explicit safety factors, because I do want to point out here that I believe we should do that with regard to the source terms, also.

There is a great deal of research work that has been done over the last decade, and certainly, with well-known fuel types, we've got a lot more capability now to make best-estimate source term calculations. Obviously, as you move to different fuel types, you'd have to do some associated R&D with that to make sure that you could do it.

In any event, as you apply the approach, then there's going to have to be R&D testing to establish the physical basis, and that may involve prototype testing. Depends on the particular design and application. I think prototype testing could be extremely helpful and supportive. I think it's necessary to validate the analysis tools over a very wide range of conditions, so that we provide confidence in the resulting analysis.

Now, there's also an implication there that if you are going to take this approach, there's some need for rising standards in analytical capability. That is, if you're working to tighter protective objectives as you go out in the years, then obviously, it implies the need for reduce analytical uncertainty, and you may have to do a variety of additional work along the way to achieve that. The intent here is to allow design flexibility on the part of the designer, but to ask for increasingly-capable designs and to demonstrate them through this approach.

[Slide.]

MR. NORTH: We should ask for an analytical and, maybe, experimental demonstration of the multiple independent barriers to radiation release -- it's important to be able to have confidence in that -- and also, a demonstration of fission-production retention. We can do this with analysis and R&D testing but, again, with the possibility of full-scale prototype testing.

I'd like to come to that point fairly strongly, because I come from a background where we have done a lot of analysis, a huge amount of analysis, and built some of the major codes which are in use around the world today, and yet, when conditions change significantly, we still find places where the analysis lets us down, and we have to go back and do some more consideration. I've seen a lot of applications where people would tell you we can analyze things, and yet, we should have caution in there.

We only really began to get acceptance of some of the conclusions with regard to the emergency core-cooling systems when we had done some fairly large-scale testing and demonstrated an analytical capability that matched that testing or, at least, that could take advantage of that testing.

I also would like to mention the shuttle and the Challenger accident, because there's a message in there for us, also. The people that designed that joint in the solid-rocket motor had available to them extensive analytical tools, and they used them, and they used them well, and they did a lot of design analysis.

One thing that they did not do was test that machine in its takeoff orientation -- they had never tested the rocket in the vertical configuration, always horizontally -- and there was one other thing that they were not aware of or hadn't taken into account. I guess they may have been aware of it.

When you start off the shuttle, you fire the liquid engine first, which takes a few seconds -- about 6 seconds -- to build up thrust, and it bends the structure, and then when you fire the solid engines and you let them go, the energy which is stored in that deflected structure comes out in the vibration of the structure at about 3 cycles per second, and when you look at the results from the Commission's study, the first evidence of failure was smoke coming out of the seal at a frequency of about 3 cycles per second. It was never tested in that condition, although they had excellent analytical capability and had applied it.

So, I come down, because of my background, fairly heavily towards prototype testing, wherever it can be achieved.

Looking for, obviously, clear demonstration and analytical validation, I think we need to demonstrate fault tolerance, and our experience supports that approach, as I have outlined. Furthermore, that kind of testing is consistent with the objectives of standardization. Once you've gone through such a process, you're likely to have very standard equipment being built and applied.

[Slide.]

MR. NORTH: I have just a few more comments here. The new systems -- I think we obviously need to make sure that they're built, operated and maintained with appropriate safety limits and levels, and I would look for a demonstration of progressively reduced sensitivity to risk to the level of excellence in operations and maintenance.

I'd look to this because we're thinking of maybe applying nuclear energy over a very wide geographic area where we may not have some of the foundations, and to believe that we can sustain excellent operations and maintenance everywhere over a long period might not be valid.

I'd also look for protection from sabotage, that it should be strong and designed in, and that is not dependent upon stable protective forces that may be external or even internal to the plant operating structure.

[Slide.]

MR. NORTH: Finally, I think the basic statement that there should be no core melt accidents within the first three accident categories, ECI, II, and III, defined in SECY-88-203, appears reasonable and a good approach.

So in summary, if you put all these elements together, I believe they not only allow a progressive design flexibility in terms of achieving the containment function, but also provide a basis for strong societal support for nuclear energy application, which is probably going to be desirable for our world society in the first half of the next century.

MR. WARD: Thank you. We have a few minutes. Any questions for Paul?

MR. WYLIE: Well, going back to your protection from sabotage, strong design and not dependent upon stable protective forces, do you consider the insider sabotage with this, I mean to design against the insider?

MR. NORTH: That's a good question. I'm not a sabotage expert, I have to tell you. It's not my main line of work. Obviously, the insider is one of the real concerns in terms of sabotage. I think the design principle holds that you would try to design against it. I wouldn't debate really how successful you would be.

MR. WYLIE: I was curious whether that was what your thought was when you put down protection from sabotage not dependent on a stable force. That would imply outsiders.

MR. NORTH: Right, it would. My thought there lay in the fact that you might be locating some of these plants in an area where a stable protective force might not be applicable.

MR. WYLIE: Did you have any particular design in mind when you made that statement?

MR. NORTH: No, sir. I was hoping to leave as much design flexibility in this discussion as possible.

MR. WYLIE: I might note also, in your earlier part of your presentation, where you produced data that would imply increased use of energy in the United States nuclear energy, that one observation of the upset on the stock market had to do with the realization by corporations that electric energy costs were higher than they thought they were going to be, and therefore their profits were going to be less, and one of the driving forces was that it was going to upset their economics and had some contribution to do with their upset on the stock market. And I think that's true. I think the United States is operating at a decided disadvantage economically in the world market because of this approach on energy, and something is going to have to be addressed.

MR. WARD: Paul, I have a couple of questions. One, I was interested in your comment early on about the need for what you call a balance between prevention and mitigation, and that there's, I think what you're saying, no matter what claims are made for prevention, there's always going to be some residual uncertainty, and that's what mitigation systems are always going to be for. Are you saying something -- did you hear Bill Snyder say something from that, do you think?

MR. NORTH: No, I don't think so. Let me just try to clarify what I'm trying to say.

MR. WARD: Okay.

MR. NORTH: Let Bill speak for himself if he needs to say something different. But if you look at risk as a product of probability of the event and the consequences of the event, then obviously you can drive down risk by claiming that you have driven down the probability of a particular failure or accident, and you might address that as prevention. What I have done is drive that way, way, down, and therefore I've prevented an accident from occurring.

My statement here is based on this thought: that no matter the extent to which you have done that, if you are trying to claim to the world that as a result of that driving down the probability of occurrence, my plant is acceptable, you're in the position of trying to prove a negative. That is, there is no accident sequence that I haven't imagined somewhere out there, that I don't have an oversight in this study, and that's a very difficult thing to do.

So I think it's prudent to have some component of mitigation in there which deals with consequences on the consequence side of that equation, even though you've gone to great lengths, maybe, to push down the frequency.

MR. WARD: The second question relates to the rising standards of adequacy and the need for that. You pointed out that you think this is consistent with the Commissions' Advance Reactor Policy statement, which maybe it is. I think that was softened a little bit.

You've also talked about the issue of depending on central estimates, and I see these two things as coming together. And you've also tried to take the broad approach of what's really good for society, I mean the real societal benefits.

I might make the case that rising standards of adequacy for nuclear power does not really serve society well because it introduces some artificial realities in what otherwise might be a level playing field marketplace for the ways that electric power is going to be made, for example.

It seems to me that if we could believe the central estimates we're getting at any given time, like today, that we'd be quite comfortable with making judgments based on those which might, in fact, turn out to be very favorable toward the development of nuclear power. But it's really the uncertainties associated with that, and the uncertainty in those uncertainties that kind of keeps us extremely uneasy about it all.

At one point, you suggested that rising levels of adequacy are going to make necessary rising standards of analytical capability, and I understand what you're saying, but maybe the rising standards of analytical capability are all that we need. Have you entertained that possibility? I mean, is there any way that we could uniquely deal wiht uncertainty without trying to improve the level of the central estimate of risk, trying to lower the level of the central estimate of risk?

MR. NORTH: Let me answer that in two pieces, break it down. One is the level of the mean, and the other has to do with the uncertainty associated with that mean.

I use the term "rising standards of adequacy" because I believe we have to get the mean to a level where a nuclear plant is recognized as a good neighbor and not something which may imply the potential loss of my home, or my farm, or me having to get out of the house, or whatever, have to ship the children out of school.

Once you got to the stage where the plant was a good neighbor in that sense, in that definition of the word, maybe there is no need beyond that point to further restrict the standards or constrict the standards.

Now, in terms of the uncertainty around the estimates -- I think I liked your phrase, "the uncertainty and the uncertainty on the uncertainty" -- we have to develop analytical methods enough so that we pull down those uncertainty bans to ranges where we can make the kind of judgment that I just espoused, and I think that there's some work yet to be done down that line.

MR. CARROLL: Quite a bit, I suspect.

MR. BENDER: Just one point about the uncontained containment. The impression I got from your presentation was that the kinds of containments that are provided in water cooled systems may not be the best kind. There might be some way of doing without them. Was that your intent, or were you only thinking about what the British did with gas cool reactors in UK and the French in France, and the Fort St. Vrain reactor?

MR. NORTH: My intent is to state that design evolution is not dead, and that in framing our thoughts on containment criteria for future reactors, it's my thought that we should not deliberately or by accident impose current concepts or the restrictions of current approaches on future designers, that we should allow those designers as much freedom as possible.

MR. BENDER: All right.

MR. NORTH: That was where I was going with that approach.

MR. BENDER: I won't argue that idea, certainly. Any designer would like to have that freedom. But I wondered if you had really given any thought to the systems that aren't contained in the conventional way in which we see water cool reactors contained, and whether that influenced your thinking.

MR. NORTH: You mean like the HTGR?

MR. BENDER: This Fort St. Vrain HTGR, or the 25 or so gas cool reactors in Europe.

MR. NORTH: I haven't done an analysis or been involved in analysis associated with the European systems. We've done a little bit in terms of the modular HTGR, and the approach there in terms of confining the fission products within the coating of the fuel particles appears, on the face of it, to have merit. I would like to see it subjected to a great deal of review and analysis before we would reach a final conclusion about it. But at least it does not appear, in my mind, to be foreclosed.

MR. WYLIE: I would also conclude that you'd like to see prototypical testing.

MR. NORTH: Yes. I declare the bias of liking to see prototypic testing wherever possible.

MR. WYLIE: I would assume that would apply, then, to the HTGR and the containment within the fuel.

MR. NORTH: Yes.

MR. WARD: Paul, thank you very much. Let's take a break now until 10:30.

[Recess.]

MR. WARD: Let's get started again, please.

Before we go to the next speaker, I want to address a question to the other committee members that is very important: what time shall we break for lunch?

Right now we have lunch scheduled from Noon to 1:00, and we seem to be staying right about on schedule. The place was so crowded for breakfast, I am concerned about that. We could break after Bob Henry's talk at 11:15 and go to lunch. Or we could have Larry Minnick's talk come before lunch and break at 12:45. I think either way we might avoid the peak in the restaurant.

MR. WYLIE: The early one might be better.

MR. BENDER: Yes. I think the early one would probably be better.

MR. WARD: Okay. We seem to be voting for the early one.

MR. CARROLL; Since I am on West Coast time, I have no preference.

MR. WARD: That's terrible.

MR. BENDER: It is not a matter of what you eat, but when you eat.

MR. WARD: All right. We will do that. We will break for lunch after Bob's talk, and come back with Mike Bender after lunch.

Our next speaker is Bob Henry from Fauske Associates. Bob?

MR. HENRY: Thank you, David. I would like to again thank the committee for having an opportunity to discuss this issue with you this morning.

[Slide]

MR. HENRY: I would like to, I guess, just talk about a slightly different aspect than the first two speakers, Paul and Bill Snyder, and that is, taking what we know today, should we really alter the criteria for design of the containment. And I will kind of give you my own idea of how I interpret the criteria, what they should be and what I believe they are, and how they relate to mostly light water reactors. But I would also like to point to how this would be potentially applied to other types of reactor systems.

[Slide]

MR. HENRY: We obviously have two kinds of containments that we deal with in the United States: the large dry containments, which are LWRs, but one could even represent those which have been designed for the LMFBRs as being large dry containment; and those which deal with pressure suppression.

[Slide]

MR. HENRY: The criteria that I would view the regulations, and my own way of casting it is, that the criteria for containments under accident conditions should, first, the containment needs to contain the fission products that get released from the first two barriers, the fuel and the primary system.

Secondly, it needs to passively contain or accommodate that energy which is stored in the primary system. So for light water reactors, that is significant. And, a large LOCA is merely a way of conceptualizing how that might come about. I really see the LOCA as being a way you write down you want it to do these things, but in essence, this is what you want it to do. You want it to passively contain that energy.

Now, for a system like an LMFBR, as an example, this has no stored energy that can do work, since the coolant is below its normal boiling point. So, in essence, this criteria basically disappears and you really start worrying about a building that can keep the rain and snow off the system, et cetera. But these two still remain.

MR. CARROLL: Does two include hydrogen? Is that a source of energy?

MR. HENRY: Two, in terms of the design basis, I do not see including hydrogen. But in terms of this is where one then takes the extra step into probabilistic, well, hydrogen, in terms of a large break LOCA. But that is really not a significant load on containment.

But the two previous speakers related to the fact that one needs to also consider probabilistic aspects. I think when you get to severe accidents, you can then go beyond the design basis, because I am talking here about includes the codes. So you then use the alternate pressure. So it does include it, and we will come back to that a little bit later.

Lastly, I think the design basis needs to assure that you can remove decay heat over the long term under accident conditions.

Let me draw to your attention, and remind you how we had, we as a country, had conceived of the containment for Clinch River at the time as an example. I think that is a case where this is not determined. Because the containment, the design basis in Clinch River was that the containment would not fail for 24 hours. And that was it. So you had the concrete thickness, you had the sodium-concrete interactions, the overpressure aerosol sodium fire protection in containment. But nothing was stated about how in the long term this system would come to a safe, stable stage.

I think we have moved a long way past that. But I just wanted to draw attention to that, that we certainly had ways of dealing with this over the short term, certainly dealt with this, because there was no significant stored energy from the system, and lastly did not deal with this, which meant that the first one then was threatened on a time frame of more than 24 hours.

[Slide]

MR. HENRY: I apologize if this is a little simple. But you have to have some slides with equations on them, or at least some numbers.

This merely says if we focus in on light water reactors as an example, and we look at a 1,000-megawatt PWR system, and you need to contain the stored energy if it is a large dry containment, this is roughly the volume of the primary system, and you can quickly calculate the amount of energy which is stored in the primary system, determine what you want the final containment pressure to be, what you think is acceptable. And I've said that the steam partial pressure, if you were to release all this energy, should be roughly three atmospheres in the containment, or a total pressure of about 60 PSIA. When you blow down this energy and depressurize it to a stable, final state, you get about 40 percent of the water comes out as steam, and this dictates then that if you are going to have this size reactor, that you have to have a containment which is at least 2 million cubic feet. And that is about as sophisticated as it has to be.

Now, what evolves from the way we have used the large break LOCA, which I think we will get to in a minute, I think could sharpen the way people look at it. I don't think you necessarily change the criteria. You change some of the ways in which it is implemented. Because we don't necessarily just take this. We then take the large break LOCA as an accident and then we start addressing what that means in terms of pulldown mechanisms, deflection plates, et cetera. Some of hat is where I think the changes need to come in, as well as in perhaps the fission product, not so much fission product inventory, but the influence of the chemical state as we know it now. I think paul was alluding to that a little bit earlier. We now know a little bit more about how fission products behave, such as iodine doesn't behave as a gas. We will come back to that in a second. Anyway, given this, given large dry containment, it has to be that big. And I don't think we want to change that.

[Slide]

MR. HENRY: If you look at a pressure suppression system, the calculations are done slightly differently. But again, if we take a 1,000-megawatt electric plant, one whose thermal inventory is about 300 megawatts, we have roughly 300 cubic meters of water in the reactor vessel and roughly 300 cubic meters of steam. So we have a total energy content of 3 times 10 to the 1 th joules that we have to cope with if somehow that is lost from the primary system.

If you have a pressure suppression containment, then you have a suppression pool whose temperature rise is dictated by the fact that you would want to exhibit it most on atmosphere partial pressure in the containment. So it has a temperature rise of about 70 degrees Centigrade, and it says you need roughly a million kilograms of water to do that, and that takes up this much volume.

Then we have the other concept, that if the break happens to be in the drywell, then all the insert gases can get pushed to the suppression pool. So the wetwell gas volume must be capable of receiving all the nitrogen, if it is an inert containment. And if it has a design basis pressure, like 60 PSIA, which we just talked about again, the wetwell needs to be roughly one half the volume of the drywell. So you take all those gases, you compress them, and you have the initial pressure which was there, two atmospheres that come from the drywell and one more from the suppression pool, which gives you the four atmospheres.

[Slide]

MR. HENRY: So given that, and the fact that you need to work in the drywell, realistically you find that it requires something like 200,000 cubic feet just for people to be able to go in and maneuver a little bit. And those of you that have walked around drywells know that that is "maneuver" in quotes, because it is pretty tight in there.

But given that, it says then the wetwell has to be this big and your total containment volume then is in the range of 300,000 cubic feet, closer to 400,000.

So given that you are going to build a pressure suppression containment, then it has to be at least this big.

[Slide]

MR. HENRY: So the criteria that you have to cope with the stored energy in the primary system dictates that.

So this is the first part of the message that I would like to bring to the committee. And that really is that the containments as designed to date, typically, that we are discussing, I think are pretty well done. And I think that is one of the things that needs to come through both in terms of criteria that we may sharpen for future containments as against something that is also good for the public to know, and following on Paul North's presentation, to develop confidence that things have been well done.

But anyway, you need to contain the energy. The calculations, while approximate, show you, once you say I am going to have that kind of containment, this is how big it has to be. Given that pressure and size, you now know how thick the walls are. You have to decide whether it is a steel or concrete type of containment. And now you have pretty well dictated how big, how thick the walls are, et cetera. And now you are only down to talking about the configurations of the systems.

So other aspects definitely need to be considered. The systems, you definitely need to consider it, no question about it.

I am really not going to talk about that this morning, because I was focusing on the containment structural part. But the other parts you really need to concern yourself with as normal operation. And that dictates, as Adolph can certainly tell you, dictates a lot of what you deal with in terms of the plant configuration, containment configuration.

My conclusion is that what we have used for the current plants, while it has not been applied necessarily uniformly, if you go back to the very old plants, I think that what we have developed, with the regulation, it is enveloping, I think it is well conceived, and I think that aspect you want to retain for plants in the future.

[Slide.]

MR. HENRY: How about the containment of the fission products and the liner? Well, for the two concepts that I just mentioned, pretty well talking about light water reactors now, the containment could potentially pressurize for tens of minutes of longer during an accident.

We just looked at two types of accidents in general where we could lose the water inventory from the primary system, therefore the containments would be pressurized to maybe a maximum of something like three bars over pressure, which means, in order to make sure we can contain the fission products, we must have an integral steel liner. So, from one follows the other.

Now, if that liner happens also to be the containment shell, so be it, but if it's a concrete container, then you need to have the liner separate, of course. I think, again, this is a criteria that's most effective.

[Slide.]

MR. HENRY: I put this slide together, which I always hesitate to do, to compare our experience in reactor accidents.

This gives us some idea maybe of just how sound the containment design principles have been. If we compare our experience at TMI versus that with Chernobyl, we look at the three barriers of fuel encladding, the fuel matrix and the cladding surrounding the fuel, the pressure vessel which is the pressure tubes in the Chernobyl system, and that which leaves the containment and we'll call it containment/confinement here to avoid the controversy of that system.

We have numbers from the Russian report that tell us that what got released to the atmosphere, in their estimate -- and many other people after that have said that their numbers should be higher than this -- but we have at least 20 of the iodine that went to the environment, at least ten percent of the cesium.

We don't really know exactly what these are, but they're obviously at least this big, probably more in the range of 80-90 percent here and 80-90 percent here. But we do know from TMI that the core examinations, longer term, said that we lost most of the noble gases from the fuel matrix, and we also lost most of them from the pressure vessel.

One of the points, that I'd like to make with respect to how we design containments is the way in which those noble gases got into the containment is through the in-core instrument tubes which, as most of you know, I'm sure, the central region of those tubes is containment atmosphere and they go all the way up through the core.

So that's one path whereby these noble gases got into containment and also we lost some out through the PORV, which was always discharging water. It never discharged just pure gas. I should say that differently:

The fission product path through the PORV was always water-filled, because the pressurizer always had at least 40 percent water inventory. So everything that went out through this path through a soluble material had a chance to be dissolved in the water. I make this point about the in-core instrument tubes because that's typical.

You find that at just about all plants. They have other ways that things can get out of the reactor vessel so that it's wise to have a containment that certainly encompasses everything. There are ways in which fission products can get out independent of valves such as the pressurized power operator relief valve.

We also know from the work done at EG&G on the fission product inventory on debris which has been taken off of the top of the damaged fuel zone and that which is in the lower plenum, that those regions are retaining maybe, at most, 20 percent of the cesium which they should have for the fuel inventory which they have.

So, if we assume we damage maybe 60 percent of the core, that says maybe we have something like 50 percent of the cesium. The iodine is somewhat more volatile than that as cesium iodide and we likely lost most of that, both from the first two barriers.

But what we got out was essentially zero in noble gases and cesium. We got a little bit of iodine out because there was a water path or maybe multiple water paths -- there were four candidate ways in which water could have gotten into the auxillary building, and of course, that can bring some iodine with it and some dissolved noble gases.

They're pretty insoluble, but if we have a little bit of the iodine volatilized when in the auxillary building, of course, that has much more health consequences than the noble gases. In essence, we find that the containment did a marvelous job, simply because we had that steel liner as well.

MR. CARROLL: If you would turn your signs around?

MR. HENRY: He's exactly right. This should be this way. I should also note down here for you, I think, -- I said that the releases from the containment were through water.

These came from the USSR report in Vienna two years ago. Also, the way in which people talked about the confinement and the containment for the RBMK-1000 could only accept the primary system inventory for a very specific set of accidents.

It could do it, but it had to be a very specific set of accidents. If you had taken this system -- this is strictly my opinion now -- this is a little tough to analyze since you don't have all the details of the accident, but if you were take this and put this in a containment which was designed in the U.S. -- I don't address issues related to missiles or molten material being ejected -- our containments would have contained this accident in terms of the criteria we're talking about here.

That's why I think it's extremely powerful in terms of what has been done in the past.

[Slide.]

MR. HENRY: In essence, as far as failing the first two barriers, TMI had the kind of releases -- not the same timeframe, but the kind of releases that were at Chernobyl and all of it was contained.

Our conclusion is, while values shown are approximate, it's clear that what's been done with the liners is well conceived and, again, should be retained for future plants.

[Slide.]

MR. HENRY: What other lessons do we get from looking at the reactor accidents? I'm just taking two out of the accidents here. The first is; TMI was caused by a lack of water. The accident was stopped because water got put back in. So that's something we certainly don't want to lose sight of.

One of the things that was not really discussed much in the Russian report and you have to piece maybe four of five writeups together, is that the damage at the plant was stabilized for several hours, maybe even a day, by water. Remember that they always talk about the firemen putting the fire out and leaving at 5:00 in the morning with everything under control and they kept it under control -- the world didn't know what had happened yet -- all day Saturday by water, but they eventually stopped that because, since they didn't have an integral design, the water started spilling over into the other units and contaminating them

They need those units and so they stopped putting water on. Of course, it boiled away, heated back up and then the world knew more about it on Sunday, Monday and the rest of the two weeks that followed.

MR. WARD: But the world didn't learn more about it because they stopped putting water on it. The world knew about it because he stuff was already moving through the tmosphere; isn't that right?

MR. HENRY: Because they stopped putting water on it.

Well, the original stuff that came through the atmosphere, you're quite right, Dave, was the initial energetic event. They also had additional releases thereafter as well because they stopped putting water on it.

They were not significant. It was roughly 50/50; that which came out in the initial event was roughly half the radiation release and that which came out longer term is roughly half, but your point is that the stuff that world saw was what came out early on, and that's true. The rest of it, I think, stayed pretty much in the USSR.

What we get from this is that water is a very effective media for recovery from an accident state, regardless of the configuration, except if we have sodium coolant, of course. We're talking about light water reactors here.

Current plants and, I think, future plants should focus on and maybe even improve on ways to have water in the containment and to remove the decay heat long term, because now you have to put water in, but you have to get rid of the energy, so that may change the requirements of the system somewhat from what we have now, even though we have a significant fission product inventory, a load which is imposed upon the systems to remove decay heat.

[Slide.]

MR. HENRY: One of the lessons which came out of TMI is that you should look at all the aspects of those systems to make sure their radiation load doesn't impact long term, the ability of that system to carry out its function -- the filters which were hydrocarbon related which degraded, et cetera.

For future designs with respect to the containment or to the liner, I think we may also want to focus on making sure that the liner can stay cool by making sure that water gets close to liner if, indeed, the liner is bare.

I think it's a wise idea that where we can, when we're building new containers, to embed the liner in concrete, such that we never have strong thermal loads applied to the liner because debris comes out of the vessel, or we could do both.

I would recommend that we do both. For most systems, we currently do that, but there are some which have bare steel linings.

MR. CARROLL: There's a lot of attraction, though, to a bare steel liner if you're going to cool it with some sort of a passive system.

MR. HENRY: There is. I thinking more of cooling it on the inside with the sprays, in which case these two are not mutually exclusive.

MR. CARROLL: Yes.

MR. BENDER: I'm not sure I understand what you mind by debris in this case.

MR. HENRY: The debris here, considering that we have not stopped the accident in the primary system and core material has come out into the containment, so we have a lot of decay heat -- I should say decay heat that we have stored energy in the debris which would be fuel dominated.

MR. BENDER: So it's a combination of whatever it is that falls out of the core.

MR. HENRY: Right, and if it comes in direct contact with the liner, then that --

MR. WARD: Did I miss a point? You said if you cool the liner on the outside, those are not mutually exclusive?

MR. HENRY: Inside, be cooled on the inside. I was thinking in terms of containment sprays as an example. Usually, for operational purposes, people will find that it's to their benefit to embed the liner anyway, so that things don't fall on it and so on and so forth.

But it also has a benefit in the accident sense in that, regardless of how you can see the material comes out, the line doesn't see an immediate strong thermal transient by high temperature liquid material which would be if the debris liquification temperatures which may be in the range of 4000 Fahrenheit or 2500 Kelvin and higher.

This is a very strong weight of attack which comes in direct contact.

MR. WARD: By "embed the liner," do you mean concrete on the outside or the inside or both?

MR. HENRY: Certainly on the inside, but usually both. I mean, if liner is a steel shell, it's a steel containment, then the concrete is strictly on the inside. If you decide you want the concrete on the outside for post tension or reinforcement, then you can have it in both places.

If you were to search through all of the containments that we have in the U.S., we'll find that the most recent ones basically do both of these, but the older ones have some where the debris can come in direct contact with the liner, and there are some where it's a little difficult for water to get down in them.

MR. CARROLL: The advantage, of course, of a bare steel liner that you can pour water on exteriorally is that that can be a big gravity tank that really doesn't require any -- in fact, that is the approach that it appears Westinghouse is using on the AP-600.

MR. HENRY: And some of the ALWR designs, the big designs, also have a similar kind of thing. You can just try to make more effective use of all that water which is kept on site.

[Slide.]

MR. HENRY: I think one of the areas -- I apologize for not being very explicit here -- that we need to focus on is how the criteria actually gets implemented, because far too often we take the LOCA, which is a way of conceptualizing the pressure loads which are delivered to the containment, as an accident specifically. Of course, those which have gone through PRA studies know that the large LOCA is always a very non-dominant accident sequence.

I think that we begin to get into conflict -- it's good to have a conservative assessment on one hand that says we can certainly deal with all the passive energy which is stored in the primary system. But if start dealing with the specifics of the LOCA accident, on how it would actually occur, we make that part of the design calculation. Then things get to be a little hard to cope with. We spend a lot of time on things which are not essential.

As an example, we have spent a lot of time on such things as how big is the break, you know, is it 200 percent or okay, we can make it 200 percent because we can all live with that. But then what's the break opening time? With those PRA probability numbers, we were back -- when RELAP was first being started, there was an incredible amount of detail put into codes to try and figure out how compression -- how the refraction compression waves ricocheted through the primary system, all dictated by the break opening time.

The size of the opening and whether it's a guillotine or fishmouth -- all these things begin to impact upon the jet restraints that you have. So I think there is where we can definitely start using some of the analyses on what are dominant sequences. Again, also some of the physical properties that we know of piping systems now, to simplify and sharpen these, so that we actually use our talents where they're really required, as opposed to deflecting some of them in those areas.

Now you all remember -- there are such things as suppression pool dynamics. But when you start tying into large break LOCA, we spent a tremendous amount of money on that. I mean this is -- Forgossin (ph) certainly said to you there are things you have to look at, but you can certainly get carried away with it and you're going to miss why you got this large LOCA in the first place.

MR. BENDER: I have a comment I make as well make here. I think in principle you're right, but remember the suppression pool dynamics came from the way in which the BWRs are designed for blowdown. They were there all the time and they are sort of independent of this instrument.

MR. HENRY: Well --

MR. BENDER: And that's just an observation.

MR. HENRY: I agree with you 90 percent Mike, except when you start looking at the rate at which you have to clear the downcomers, that's all dictated by the LOCA, how big you say the LOCA is.

MR. BENDER: Fair enough.

MR. HENRY: But you're talking about of course --

MR. BENDER: That's very important.

MR. HENRY: The very important part of this that I think is your point is such things as how the SVRs discharge into the pool, was not included and should have been, so that the system was purged, and all of it probably would have gone away.

MR. BENDER: If they designed it properly for that, the other problem probably wouldn't have arisen --

MR. HENRY: Yes, that's right. I agree with that. Other things which I didn't put on here that I should have, and it goes back to again -- so did Paul North made this. I'm not sure if Bill Snyder made the same point or not, but one of the things which gets implemented from here of course is the source term.

We only take the large break LOCA when we specify the source term. Source term is noble gases, 50 percent of the volatiles and one percent of the non-volatiles. Well, the part that's damaging in that is the fact that the iodine is a gas, and is modeled therefore in that manner.

If we were take best estimate analysis, which say in essence that the iodine is much more found to be tied up with cesium iodide, which is an aerosol as opposed to a gas, much more easily stripped out by the water and of course is highly soluble, and if we were to take the current design basis criteria that we have for the containments in terms of the leak rate, which we have the integrated leak rate test, which we do every five years to make sure that the system can indeed live by that, we would find that the releases at the site boundary were dramatically reduced, even by our current criteria, just by carrying out the best estimate analysis.

I agree wholeheartedly with the comment made previously by Emergency Planning, because I've lived through those wars myself and I know that when you start telling people you're going to evacuate the beaches on a site alert, they believe that there's a good reason that you're going to evacuate the beaches on site alert. You've already conditioned them to that.

So you can't go telling them we've changed everything and we now do a better job. So one of the ways of perhaps starting to deal with that is to make the criteria best estimate for chemical composition of the -- for the fission products, namely cesium iodide, cesium hydroxide, and then apply that to the current design basis we have for the leakage rate and we'll find what we have at the site boundary for the design basis accident is dramatically reduced, because we will get back to essentially having releases which are noble gas oriented. Now you'll find that it's going to be something very light, a TMI-kind of release, if you take the iodine away.

So if you do that, you'll find that indeed we have already improved the system tremendously just by our understanding of the source term itself. Perhaps that's one of the ways of starting to address this confidence problem. Because you certainly can't go out and with the plant that I was familiar with, we were attempting to restart and the people were well-conditioned about WASH 1400, etcetera.

We do best estimate analyses, the exposure to site boundary for station blackout is 1R, and we're down to the lower end of the pegs. There was absolutely no reason to evacuate anybody. In fact, you're actually increasing the risk of the populous to evacuate. But there was no way you could out and make that a public argument.

So I think that's maybe one of the ways to try to cope with that, instead of the criteria where we force the use of best estimate source terms, in terms of the chemical composition.

[Slide.]

MR. HENRY: We also then have in the future systems, you have address issues which are still there for current plans. I think thinking things ahead, you could address those so that they are easier to cope with in terms of the uncertainties. It's easier for the people to deal with the uncertainties which are perceived to be there and easier to deal with the ways in which you can make the uncertainties less influential to the whole argument.

Those include hydrogen combustion; liner attack we talked about. Any potential for debris dispersal and containment, which is called the direct containment heating. But I'd rather put it on this, just talk about this dispersal. Then long-term coolability, because if it does come out to the reactor vessel, we need to assure that we can extract the heat long term.

[Slide.]

MR. HENRY: In essence -- I guess I got a few slides here -- from the hydrogen point of view, I don't think that we need to do a whole lot more than what's currently been done. I think the kind of design basis which has been looked for ice condensers and Mark III's can be -- could and should be applied to the -- to future designs, which in order to protect against it, you make sure that the volume and here's the alternate pressure you asked me about earlier -- can accommodate a complete burn of hydrogen which is generated by 75 percent active cladding.

So you take the active cladding of the core. You react 75 percent of it and you burn that completely, and you ought to be able to take that, which is exactly what was imposed upon the current designs.

Now there are ways in which you can force the containment to live by that, and that's -- you can inert it which you'll have to do for small containments anyway; intentional ignition, if that's chosen to have an oxygen-bearing containment that's small enough in volume; or you can just have a large enough volume.

We also need to then protect the liner, water --- this is for LWRs, of course -- or imbed it or both and I would certainly say both out to be part of the criteria.

MR. CARROLL: You use 75 percent of the active cladding.

MR. HENRY: Right.

MR. CARROLL: The NRC Staff today argues for 100 percent.

MR. HENRY: I think that's the issue which is being debated.

MR. CARROLL: Yes.

MR. HENRY: My recommendation is when you do best -- when you get down to this, I think best estimate analyses is a redundant thing to focus on. A hundred percent always gives you a great deal of comfort in a way --

MR. CARROLL: Well, it's 100 percent of active fuel cladding is not 100 percent of the zirconium available to react. There's a hell of a lot more zirc than just that.

MR. HENRY: That's right, but I think -- well, I shouldn't say what the NRC position, but the one that -- the issue that I've heard discussed is 100 percent of active cladding. If it's 100 percent of all zirc in the core, then the containment is so large that it's pretty tough to even construct it and live by the seismic loads that you have to reduce on. Okay.

MR. CARROLL: And your basis for saying 75 percent is a best estimate approach to --

MR. HENRY: I would take the probabilistic approach. I would go -- I mean I would do a sort through the dominate axis scenarios, and then best estimate analyses of the hydrogen loads that they impose on containment, and also impose recovery from that, because recovery -- some of this that people have worked out in the past, for instance, has steam inerting in the containment. When you recover, you can get rid of that. So you have to include that in the assessment.

MR CARROLL: How important do you think development work on catalytic igniters is power failure or is the failure of the power source to low plug igniters a very important issue?

MR. HENRY: Well, the advantage of a catalytic igniter is just the passive nature of them.

MR. CARROLL: Yes.

MR. HENRY: I really am not that familiar with the kind of performance testing which is going on. I think the first place being unfamiliar with the first question I would ask is how do -- what do we think these things would do in the midst of an accident, with all the other stuff that's going on? Would they survive?

If so, then I think they have a lot of positive aspects to them. The one that I had heard of previously was one in Germany where it was a curtain like this that got unrolled in the midst of an accident. That's just another active system replacing another active system. But if it's something which is truly passive, then there's a lot of promise to it.

MR. CARROLL: So what you're saying is the loss of power to low plug igniters seems to you to be fairly important in looking at severe accidents?

MR. HENRY: I think if you --

MR. CARROLL: If I could replace it with something that was really passive?

MR. HENRY: I think if you're -- for future plants, they'll have to be designed to be able to accommodate some way in which they can address station blackout-like conditions, and if -- the way the igniters are currently set up are all AC-driven. For future plants, I think if you have igniters, they'll have to be DC-powered as well, and that certainly complicates the control circuitry. It certainly complicates education of the operators, which is something -- the operators have enough to worry about now anyway.

So if there was a way that you could do it passively for a small volume containment, and assure yourself that the consequences of the accident, that there's a low likelihood of impacting on that success, then I would say it would be very promising to look at. I have to beg off on it, because I'm really not that familiar with what's been done research-wise.

I know the Sandia people have done a lot of work on it. They may want to talk about that later. I'm just not familiar with it myself.

MR. CARROLL: Okay.

MR. HENRY: We have already talked a lot about the liners.

[Slide.]

MR. HENRY: I think in general one conceives of a variety of accidents. It is not a good idea for debris to be in various spots in the containment. Certainly if you think of PWRs going into an accident, the operators are already trained to depressurize for a number of reasons. They would like to access the low pressure systems, whether they are accumulators or whether they are the low pressure injection systems that would be available.

It is also a major way that you protect one of the most important containment boundaries, which are the steam generator tubes, because if you have an inverted U-tube generator at high pressure there is a substantial pressure and thermal load on the steam generator tubes. It's always wise to depressurize because that bypasses all the containment things we are talking about here.

Lastly, depressurize because if you couldn't recover it in vessel and it was coming into the containment you would like to minimize those dynamics, so depressurization is a key part of it but I am only talking about the containment today. If you still assume that there is enough pressure to disperse things I think it is a wise thing to set up the containment configuration, which you can do ahead of time, to minimize the potential for debris to be pushed around the containment. In essence, that simply means that you can design the reactor cavity and instrument tunnel kind of configuration ahead of time. It says debris can pretty much stay in the reactor cavity.

That's a benefit. I don't mean it's a benefit in terms of actual safety. I mean it is a benefit in terms of being able to license it and say I am pretty much independent of the uncertainties that people put on this issue of direct containment heating, et cetera. You reduce your sensitivity to discussing those issues.

You also want to maximize the capability of putting water on the containment floor because you must take water away -- must use water to take the heat away from the debris.

I am going to recreate an old issue by saying it --

MR. CARROLL: When do you put the water in?

MR. HENRY: Before. Before. It's to your benefit. I know the issue of -- in a former life I spent a long time talking about steam explosions. I would certainly recommend that water be there before. Once debris is going into the containment any explosive interactions that you see are to your benefit. It's taking heat away from the debris. It's putting it in the form of steam. The containment is designed to cope with steam. If you look at the rate dependent processes, again my assessment, you can't come anywhere close to having either a pressure load or a shock wave which challenges the interval containment system and if it gets pushed that far, that's another reason for embedding the liner in close proximity to the debris. Simply the liner is another cushion -- I mean the cover for the liner is another cushion, if people want to really look at shock waves but steam explosions are notoriously weak shock wave generators.

Maximize the potential for accident recovery by maximizing the available area. This goes back to just looking at what you can do for the containment, how big can we make it realistically without jeopardizing our normal operation. Let's try to have the maximum area to spread this stuff out and get some water to it. That's all different than the current systems because if you go through all the types of containments, some can't really spread it out very far. They all can get water to it one way or another.

MR. WARD: Back on the first one. It's not clear to me how that really deals with the direct containment heating problem. At least part of the direct containment heating problem as I understand it is the dispersed stuff interacts with -- I mean reacts with the air, the oxygen and you have another thermal source, the pressure load to the containment.

How does this really deal with that?

MR. HENRY: Dave, it's much more in terms of if you have something which is a Millstone-like configuration, which if you do it ahead of time it's fairly easy to do. It just merely says that there's a fairly square configuration and a room that sits off down at the very end of the cavity. The debris being heavier than gas, if there is any pressure in the primary system and it blows down and tries to move the debris, it acts as a separator so the debris just stays in the cavity.

It is strictly a configurational issue to be done at the time of the original design.

I think you had a presentation from Bill Sugnet in the previous session?

MR. WARD: Yes.

MR. HENRY: Did he show configurations of the reactor for the ALWR?

MR. WARD: I have seen them somewhere, yes.

MR. HENRY: That is in essence what -- this merely says that there is a very limited time of flight and a very limited potential for interacting with the atmosphere.

MR. WARD: Okay.

MR. HENRY: You don't give it a long path to interact with the oxygen and steam.

MR. CARROLL: Talking about the EPRI requirements document and SP-90 and ABWR, you seem to be taking a different position on water first or water later than they are.

What are their arguments in your mind for the fusible plugs that put water in after debris gets into the cavity?

MR. HENRY: I am not quite sure what they may have said about water first, water later. As I have heard the question, it's been framed somewhat differently in terms of how soon do we have to get it there, as opposed to is there a problem putting it there before.

I think if you -- when you walk into a control room and you watch what the people are coping with in the midst of an accident, it's pretty tough for them to put all that instrumentation together to assimilate where material might be and I think if you could ever put water into a vessel you ought to then put it there. If you could put water into -- if you can't get it into the vessel you should put it into the containment, even if you know the system -- even if the vessel still has pressure. If you can't get it in, put some in the containment to protect it. If you can put it in both places, and that is one of the places that both -- I would use what both of the previous speakers said and put it in my own context.

One of the things that doesn't get discussed in PRA space or operator action space is split use of the same system. When you look at the size of the systems that we have for either high pressure injection or low pressure injection, if they have to remove decay heat the amount of water you are dealing with is far in excess of what is required. There is never a time that I know of when if the procedures tell you you should put it into the vessel, they should put it into the containment, that these systems can't cope with putting it in both places at the same time, even with one system.

I'm not quite answering your question. That's one of the things they should look at for the ALWR.

My arguments for putting it into the -- any time you think it has to be there is you are not quite sure where you are in the accident. You want to arrest the accident as soon as possible, give the containment the maximum potential for recovery from the accident. I think it is best to have water there to do that ahead of time.

MR. CARROLL: All right.

MR. HENRY: Mike, let me go back. You had mentioned BWRs earlier and one of the points I would make here again, which is slightly off the point -- I apologize if it is, but it's been our experience that when you look at BWR systems, for instance, the operating procedures are extremely well done but when you go through a PRA kind of approach you look at the spectrum of accident sequences, you'll find that there are possible times when the operator is put into conflict with the procedures.

One of the ones that we found was for some of the procedures as they were implemented at the plant, if the temperature gets higher than saturation, 212, he's not supposed to put water into the containment ever. The reason for that goes back -- if you trace it back goes back to the performance of the vacuum breakers. You don't want to damage the vacuum breakers if you spray it in.

Typical of many of these things and the procedures themselves have a technical basis for them and in many cases that is very conservative. It is well thought-out but very conservative. In this particular case it was so conservative that it dictated the whole result and if you merely just look at the dynamics of how fast one of these things can open you will find that there is really no limitation to putting water in the containment.

So as you go through these designs you also find it is worth your while to focus on not only how water comes in but is there ever any limitation of when you think water ought to go in and you'll find that, yes, they are there in the procedures but they are usually there because it's been a very conservative type of calculation.

MR. BENDER: There is a counterpoint to it that says it would have been very easy to design the vacuum breaking system in such a way that it wouldn't have been jeopardized by this. It took very structural improvement to do that.

MR. HENRY: That's right. My point is more with current plans.

MR. BENDER: Sure.

MR. HENRY: But I'll use it as an example of what you ought to go through for future designs as well. That is exactly right, that they were certainly changed.

[Slide.]

MR. HENRY: Yes, I think that the more we can focus the criteria on best estimate, the better off we will be. I certainly don't think we ought to deviate from being able to accommodate the kind of stored energy we have in the primary system however.

I think the general criteria that have been used for the current plants were pretty well conceived. The prudence of the criteria is demonstrated by our experience in reactor accidents, just comparing what happened when we violated the first two barriers in TMI and the same thing for the Chernobyl system.

The general criteria for the plants are applicable to the future designs. The implementation is the place where things can be streamlined. I don't think my estimate -- we're not talking so much about changing the criteria as how we actually get it implemented.

Future designs can address the severe accident issues ahead of time to merely reduce the influence of these uncertainties that are conceived because I think you can address those things. You just kind of take it away from being something which is of considerable importance.

Dave, I put these together before I got your letter that said feel free to talk about the other designs, so I'll just make one comment since I talked about CRBR before if I go back to this slide up front.

[Slide.]

MR. HENRY: If we are dealing with an electric metal cooled reactor, you can still satisfy all of these. This one has no stored energy to do work on the system, so it really doesn't apply. It needs to contain the fission products. I think again it needs to be steel-lined or a steel containment, period.

This is the difference. It needs to be able to remove decay heat long-term and to do this I think you have to transfer it through the shell of the containment, so I would recommend for LMFBRs in the future that we deal with small containments -- in fact, not a whole lot bigger than the reactor vessel, inerted, with cooling from the outside, so that you could achieve all of these. With the sodium coolant you do not have the flexibility of being able to add coolant to the future and you don't want to cope with the sodium - concrete interaction so we would be dealing with something which is a small containment, steel, inerted, and cooled from the outside so the sodium reflux is from the debris to the shell. It has to be able to take basically sodium boiling at one atmosphere inside of the -- if there were any kind of debris configuration.

I think that's about all I have.

MR. CARROLL: You don't want say anything about gas-cooled reactors?

MR. HENRY: No. It's been a long time since I worked on gas-cooled reactors, and I'm just not the best one to talk about them, but I would certainly apply these same criteria. This is not a very meaningful criteria for gas-cooled systems either, but the others are.

MR. WARD: Okay, Bob. Thank you very much.

Will you be able to be here until 5 o'clock or so?

MR. HENRY: Yes, I will.

MR. WARD: Okay. Rather than take any questions now, then, let's break for lunch and come back at 12:30.

[Whereupon, at 11:25 a.m., the Advisory Committee recessed for lunch, to reconvene at 12:30 p.m. this same day.]

AFTERNOON SESSION

[12:34 p.m.]

MR. WARD: Our next speaker is Mike Bender.

Mr. Bender.

MR. BENDER: I'm reminded that somewhere around 1970 -- I'm not sure exactly when it was -- that the ACRS had a meeting not very different from this to talk about containments, and at that time, not all that many power plants had been engineered, and I was asked to give some comments on containment, and at that time, I noted that it wasn't very much different from sexual contraception, that it was something you did to avoid accidents that you didn't really want to be exposed to, but it didn't always work, and so, in talking about it today, I concluded that maybe I ought to remind people that no matter what we do about containments, you can never make the probabilistic argument that they'll work every time, and so, what I have in mind to do today is to talk about some concepts of how to deal with containment in the current environment, and I've listed these four items on the board as things that I'd like to deal with.

[Slide.]

MR. BENDER: First of all, I'll try to develop a definition, and then I'm going to talk a little bit about what experience we've had so far with respect to accidents and my own interpretation of the research business, and hopefully, at the end, I can concentrate on a design basis, but there's not a lot of time to cover all these things, so it's going to be kind of sketchy.

It turns out, in looking back through the things that I had around, that believe it or not, when the containment business was being developed as a technology, the water-cooled reactors were not the things where it was explored, because the AEC then didn't want the research community tampering with what was a developing technology.

So, they tended to encourage people to work on the experimental things, and I was, at that time, working on the experimental gas-cooled reactor, and General Dynamics -- GA now -- was developing the HTGR, so the ACRS review, in those days, gave a lot of attention to gas-cooled reactor containments, not in the sense of whether the containments worked but what the things were that went into dealing with the containment capabilities. So, it turned out that that period of development was a useful one to learn things. Later on, the AEC got into the LOFT concept and the semi-scale things to deal with pipe breaks, and those had an effect on containment requirements.

So, there was a developmental period back then, when the accident definition was being developed, and that was the time when people began to think about what the containment function really was.

For those that haven't looked at it recently, the book by -- I can't remember who edited it -- "The Technology of Nuclear Reactor Safety" has a section by Thompson and McCullough which discusses the containment concepts in about the 1972 period. I think that's when that thing came out. It's worth reading, because things haven't changed all that much. So, I'm going to work from that context -- what I knew then and, in a way, what I know now.

[Slide.]

MR. BENDER: First, there is a definition of containment to be dealt with, and this is the definition that I think is commonly understood: a boundary closure, a heat sink. A heat sink controls temperatures. That's what Bob Henry made some comments about. And thirdly, some way of stabilizing the radionuclides when they release the containment. That's a system concept, but hardly ever, in review of these systems, do we talk about it as a system concept. Almost invariably, we're talking about either the structure or the radionuclide-trapping system or whatever causes pressurization.

[Slide.]

MR. BENDER: There are a lot of things that have to be considered in designing a reactor system that affect containment. I have listed some of them up there, but unless you know what they are, it's hard to say whether the containment system will function or not, and for the most part, these are implied in the current water-cooled reactor systems. Whether they apply to gas-cooled reactor systems or not apply to water-cooled reactor systems is debatable, and the point that was made earlier today that maybe we ought not to be so constrained relates, in some degree, to those things.

Now, the PWR system, if you didn't have an ice condenser, requires a higher-pressure containment, because the inventory of steam in there invariably will cause a high-pressure release. If you had some kind of heat sink, you could take care of it, but the ATWS, as it presently exists in PWRs, can't be contained unless there is sufficient relief capacity in the system, because you could blow up the pressure vessel. If the pressure vessel itself bursts, that's another issue that has to be dealt with. It isn't considered today. Whether it should be or not is something we have to think about.

The BWR systems have a really good heat sink. It's designed in, and it has enough redundancy to be reliable, but the ATWS may not be containable, and we haven't really studied.

The HTGRs have a different situation. With all their inherent heat capacity, with the supplemental cooling that's used in some pressure vessels, they have a very reliable heat sink capability, but the one point that seems still to be not fully established is what could be done if the pressure vessel bursts. It's not an admissible event. I'm not going to argue that it should be admissible or not. I'm only pointing that the question has to be dealt with.

[Slide.]

MR. BENDER: Now, changing the direction for a minute, I'd like to talk a little bit about the reference reactor experience, the real experience that exists. Never mind what's been done with PRAs.

First, we've never had a radionuclide release at high power. Can we? I don't know, but from the standpoint of containments, we have to think about what that release is and how it could be dealt with. There are some events that I could identify that could have caused that.

Brown's Ferry had a partial ATWS. There was the Davis Besse feedwater thing that, if it had gone in certain directions, could have caused high power releases. I don't say that they would have, but they could have.

The previous practice has not dealt with this and it hasn't deal with core coolant blockage. Probabilistic questions would ask you whether you should deal with it.

Now, most of the design having to do with containment, and many other reactor safety things, came from these three events -- the SL-1, the NR-X, and the Windscale accident -- all of which contributed some radioactivity to the environment, all of which were serious accidents, and all of which influenced containments.

There hasn't been enough review of what happened here recently to know whether we understand how those events relate to our current concepts.

Now, there are a couple of events that we've talked about.

The Chernobyl event has been discussed to the extent that it's pretty well understood right now.

There was an accident in India at Tarpur with a BWR that released a lot of radioactivity to the environment. I have never been able to find anything that discussed the event, but those events are containment circumstances that need to be dealt when we're thinking about what the accident problems really are.

Now, TMI has been discussed ad infinitum. What it really showed was that a core could melt and the containment wouldn't be violated. All that was necessary was to have a little bit of cooling and some way of keeping the containment pressures low. In that particular accident, it was no problem, because the reactor was shut down. Furthermore, it leaked some, and that didn't make any difference either, because the pressure was low enough so there was no real driving force to get radioactivity out of the system.

Now, I want to turn, for a minute, to the question of what we learned from the safety research.

[Slide]

MR. BENDER: Here what I am going to try to do is go through a listing of things. I am sure I am not going to cover everything. And it is a personal opinion.

But first, all the studies have shown that if you start operating on the basis of anything that can happen will happen, you can't get to any design that is totally acceptable. Somehow or other, you have to eliminate some things. And which ones you eliminate may have to be done probabilistically. But the issue has never been completely developed.

Secondly, I think it is clear that if you do not do anything about the accident, there will be some circumstances in which you will not understand what the consequences are. We have been through that a few times. The steam explosion issue has never been resolved. And unless you accept some position on it, you will never be able to decide what kind of containment you need.

The third point is that there is time available to do something. All these studies show that accidents, while they happen, do not get to catastrophic conditions instantaneously. The worst that has ever been observed is the condition having to do with a BWR ATWS. And even there, there is time to do something in the sense of recognizing that whatever happens, it will have some limiting condition.

The operator, in all cases, is an important part of the accident control. And we have more or less given up on him in these analyses. But he is there. He needs to be considered. And from the standpoint of developing containment concepts, it is important that we consider what the operator can do.

My view is that he ought to be depended upon but not to do very complicated things.

[Slide]

MR. BENDER: Now, if I turn to the question of what the structures have to do, I have a few different thoughts to lay on the table.

First of all, I think the tests that have been looked at so far show that if you stay within elastic limits, concrete has very good structural capability. And the liner will do fine, too, if you just watch the discontinuities, in the anchors or in the studs. If you understand what the loadings are under the accident conditions, generally you can design to make the liner as reliable as the reinforced concrete structure. But so far, we have not developed the understanding that is needed to defend that particular condition.

The point that most people have worried about is the question of these flange closures, especially if they are sealed with elastomers. The testing that I have seen and the experimental work to date says that as long as it does not start to leak, it will stay there, mostly because these gaskets are isolated from overtemperature and radiation. If they were not isolated, then we would be in trouble. But because most of them are double-sealed, that has never been a serious problem.

[Slide]

MR. BENDER: Now, the point that has created, I will not say controversy, but at least concern, is what happens to radionuclides when they get out into the system. For the purpose of defining the containment, you have to assume that some radionuclides are out there. But in fact, there are a lot of natural holdup capability inside the containment. And all the studies nave shown this.

There is blockage to stop aerosols. There are absorptive surfaces. There are fluids in the system. All of which do some good, and if collectively considered, could knock out most of the radioactivity that could escape.

Now, when we start looking at what is going on in [illegible] system, we find there are chances for the ameliorating co[illegible] to make the problem difficult, mainly because they might generate error samples. That issue is still out there. It is not really clear that we understand what to do about aerosols.

So, in developing a design, it is necessary to think about what can be done to deal with it.

[Slide]

MR. BENDER: Now, the things that have been talked about are the use of sprays, which have been in containments for 20-odd years. They are certainly capable of knocking out a lot of the radionuclides if they are reliable. And we have not worked very much on establishing their reliability.

They were put in as a device to take out heat in the containment and to take out radionuclides when we had a very simplistic concept of what was going on. Since then, they haven't been given much credit. But they have always been there. And by doing some engineering of it, including thinking about what you might do with the fire treatment equipment that is in, which is full of spray capability, it is possible to enhance the spray capability in containments a good deal more than presently exists.

Using things like caustics, which admittedly are a problem to the hardware, if used at the right time, can be very helpful.

Now, whether to trap the radionuclides that are gaseous in form and are not chemically active is an issue that has been around for a while. The best that I see we could do would be to work on the concept of holding them up long enough so that at least the more vicious ones decay to a form where they can be trapped by something.

[Slide]

MR. BENDER: So we are brought around to a question of, given these things, what can we do about developing a design basis? And so I am going to take a few minutes here to talk about what kin [illegible] design basis might evolve.

First, a little bit about the philosophy.

First, there has to be some kind of concept based on accident frequency. But we still have to think about non-probabilistic limits in some way. That is the mixed bag we are dealing with.

But, every accident has to be contained in some manner so that the public is not excessively jeopardized. And I use that word "excessively" in the sense that we can never prove that there is no risk to the public. We will have to think about some kind of bounding limit. And that limit is the thing which brings about attention to emergency planning. Somebody would say well, emergency planning is not part of containment. But in fact, we are talking about protecting the public, and if the containment doesn't do its job, there has to be something to back it up. I will come back to that in a minute.

But the thought is that we need to have a strategic reserve that we can think about as a way of dealing with accidents if all other things fail. Some of that strategic reserve is being sure that the people that can be evacuated get evacuated. And some of it has to do with what we can bring in from the outside, to protect the public if the containment cannot do the job we want it to do.

Now, it is necessary to have a design basis accident concept. And that is what has been developed really by analysis in the past. We have never said, here is the design basis for containments. The only basis we have ever had is so many radionuclides out there. But we have gone through some very elaborate, elaborate accident analyses of the kind that Bob Henry talked about earlier to develop what the containment had to accept.

In a sense, the problem has been made worse by the way in which we do it. The Appendix K analysis has gotten confused with containment requirements. Appendix K was put in for one thing. And that thing was to be sure the fuel was properly designed and the operating condition were properly specified. But it has been used to define how the containment would work. So we have worst-case LOCAs and we have many other condition set forth for accident analysis purposes that probabilistically I think could be argued are unlikely to occur, from the standpoint of providing public protection.

Now, my thought is that we need to think about things like the ATWS as being accidents that are uncontainable. And we have not done the analysis that tells us what the consequences are.

So to my mind, I would go back and do some of that work.

Secondly, we have not spent enough time thinking about what the realtime relationship is between the accidents that occur and the radionuclide releases. Everything that has been looked at says nothing happens instantaneously. And for the most part, you know the accident is happening in some form well before the bulk radionuclide release occurs. So there is time to do some protective actions that can at least minimize the consequences of them.

[Slide.]

MR. BENDER: The thought which I had, which admittedly is a personal opinion only and not tested on very many people, is the following: You ought not to assume that an accident is going to go to the last circumstance unless you really can't do anything about it. There are a lot of accidents that something could be dome about. The small LOCA heat sink bypass that has been such a problem in the probabilistic analysis is something that could easily be corrected by design. The fact that the designs don't accommodate it makes the accident seem very serious, but it really has only to do with a little bit of engineering improvement.

The thing that seems to be missing right now, to me, is an effective accident sensing device. We don't yet have enough information to provide to the operator for him to really know what's going on. And if I were working on something, I would work more on this point than I would on system design improvements because the operators' information is crucial to any kind of accident control.

The second point which I'd like to make is that containments ought to be designed for controlled failure. Designing it as a one-horse sleigh where it can fail anywhere guarantees you that you can't allow for all of the circumstances that might have to be dealt with, but just as a crank shaft has a shear pin, you can put into the containment something that will fail before everything else fails.

In the old days, when I was in the white oil refinery business, where I spent a few years early in my career, we put lots of rupture disks in, and the purpose of a rupture disk was well known. It was to be sure that things burst at a certain time and in a certain place.

In the case of containments, the testing has shown that you can go up to structural yield without catastrophic failure. I wouldn't propose to go that far, but I certainly would think about putting some kind of pressure relief in to go before I reach that stage. I don't know that I need to make that rupture disk protect the concrete pressure vessel if it's properly reinforced, but I think it would wise to think about protecting the liner in that way.

And it has a special value, because if you can control the failure, you can direct the failure. If you put in some kind of trapping medium, whether it's caustic or filters or what not, and the failure directs the flow to those devices, the likelihood of having a reliable containment device that isn't dependent totally on containment structural capability is enhanced tremendously. So my thought is to think along those lines.

[Slide.]

MR. BENDER: Now, there have been some proposals for venting, and, in my view, venting under certain circumstances is something that people ought to think about.

If you'll recall a previous point that I made, if you don't do something, then there is an implied circumstance where a containment bursting will occur. Not to have it occur requires something else to be done. Either you have to enhance the strength to a point where it's impractical, or alternatively you've got to work on the premise that you'll let it fail and live with what happens.

Some people have talked about this early venting. somebody said that that's been in the BWRs since they were designed. I think it's a worthwhile concept. You can't do it without having some kind of back-up trapping system, because you can't trust yourself to let it go without at least doing the minimal amount of protection.

The second point is one which a lot of people have talked about, namely venting after the accident as a way of being sure the containment doesn't burst, and I think that's worth thinking about. Also, [illegible] the reliability under certain circumstances is [illegible] has not been addressed, and because it hasn't been addressed, I'm not sure we can build a case for it.

[Slide.]

MR. BENDER: The next point I want to deal with is this question of a strategic reserve. What should it be? We're going to have to worry about common fault failures, and we don't really know which ones to think about. They're going to exist.

The thing which we have to deal with, in my opinion, is just saying, Well, whatever happens, there will be some time to do something, so let's concentrate on getting the operating personnel to understand how to get the emergency controls in place. My view, which is not unique, to me, is that firefighting logic is what ought to be developed as part of the containment concepts. We ought to be thinking about how to get secondary support for accidents if they occur.

Some of that has been talked about, but, so far, I think we're not yet in a position to say that we understand what really should be done. But showing that you can start the fire alarms going, or the emergency alarms going, is not really much help. Monitoring the streams to see whether there are radionuclides out there is not much help. It'll just tell you whether the accident is out of hand or not. But as far as fighting the accident, we haven't given enough attention to what really might be done to make things better if the containment itself failed.

[Slide.]

MR. BENDER: So the thrust of my point is that we need to think more about what those supplemental capabilities ought to be.

Now, I'd like to deal wiht one last point before I sum up here, namely that the question of what the design requirements are in the end is going to boil down to what kind of codes and standards exist. My contention now is that about all we've got is the pressure vessel code and a few regulations. They're not integrated, we don't tie them together very well, and it's not really clear that you can relate the codes to the accidents that are now of concern.

So it seems to me we have to think some about how to modify the existing requirements so that they have some order to them.

The second point which I wanted to make was that we've concentrated so much on the details of these structural requirements. We've got a pressure vessel code now or a concrete code that is covered by a document that's about six inches thick -- I think that's about the size of it -- and only the technologists interested in the details understand what's in that code, and that's not very many people. But that's where all the emphasis is. There is not enough emphasis on the question of physical arrangement of the systems and how to make them more resistant to whatever the accident circumstances are.

There has been some talk of late, and I don't know it stands, that the new BWRs will have minimal amount of piping in them. I think that's a good step. But even more important, I think, is the location of the equipment, so that if the accidents occurred, there is a minimal amount of jeopardy from the accident conditions.

Separation for fire protection purposes has had some value, but that separation has been developed for fire protection purposes, and the question of whether it's adequate to deal with the accidents we're really concerned about is still to be examined.

So the thought I have right now is that we need to go back and see whether we have the right basis for evaluating containments when the new concepts are developed.

I think those are the basic points that I'd like to lay on the table. I don't have any real philosophical view about what the best containment is going to be in the long run. My view right now is that we don't have a good enough logic basis for dealing with the things that are out there. So until we've built that logic a little better, and have it in a form that the design organizations can understand and using organizations can interpret, I think we're still in no man's land, and that's where I'd like to quit, Mr. Chairman.

MR. WARD: Thank you very much, Mike. Any questions for Mike?

[No response.]

MR. WARD: Let's go right on the next speaker and then we can catch you as part of the wrap-up at the end. Larry Minnick is our next speaker.

MR. MINNICK: It really is a privilege to be here, although we might all wait and see how productive it will be. Frankly, when I sought this opportunity, my intention was to describe what I believed to be a better mousetrap and then to sit back and wait for the world to do its thing.

But last Friday, I received a long letter from Dave Ward suggesting that what ACRS is really looking for is a synthesis of the new knowledge with respect to severe accident risk which will provide a basis for new, practical and comprehensive containment specifications. Since I am by no means expert, and I am made well aware of that by listening so far today, in the new knowledge, I've thought long and hard about what more I could say that just might be helpful.

So, first, you'll hear some background, background that qualifies somewhat the growing view that not much thought was given to containment design in the earlier reactors. That is also directly pertinent to today's quandary. Next will be some opinions that will be less than popular and that perhaps only a very independent and retired consultant like myself can put forth.

Third, will come some thoughts as to an approach that might be appropriate for the ACRS to pursue, prior to establishing containment specifications and finally, I still plan to try to get you all thinking about a better mousetrap. As for the pertinent background, over the weekend, I reread several sections of the Yankee PSAR and FSAR

Actually, we called them hazards reports in our naivete. That plant was built between 19857 and 1960, therefore, the PSAR was dated 1957. You may recall that the plant has a 125 foot steel sphere as a containment vessel. The sphere has an ASME code stamp as a Section VIII, unfired pressure vessel.

In effect, that steel sphere is a dandy heat exchanger which inherently and passively eliminates concern for containment over pressure or if not eliminates it, certainly dramatically reduces it. As to the necessity for a pressure relief device, there was a special code case existing at the time that stipulated that no such device was required on nuclear containments.

The finding specifically mentions the potential of such a device to inadvertantly release noxious gases. One thing that Yankee did have and still has is a system which continuously monitors containment leakage. That system was demonstrated and has demonstrated its usefulness many times.

I recommend that such a system be seriously considered for future containments. The Yankee Hazard Summary

Report specifically acknowledges the remote possibility of core melting and stipulate a source term for the hypothetical accident, so called, of 20 percent of the gaseous and volatile fission products, combining that with the tested leak rate and worst case meteorology, yielded acceptable offsite dose limits at that time -- dose levels.

When Connecticut Yankee came along about 1963, it was to be more than three times as large as Yankee. So a steel containment became impractical. Reinforced concrete was determined to be the best alternative. Most people took comfort in the multiplicity of rebar which did away with the concern of cracking of steel plates and welds.

On the other hand, in effect, the containment was insulated with several feet of concrete, thereby effectively bottling up a considerable and very long-lived heat source. All the usual safeguards, even those that exist now -- sprays, heat exchangers, filters and lord knows what -- were included to assure by strictly active means, that the decay heat could be removed.

Nevertheless, there was a fairly vocal minority which felt that in addition, containment pressure relief should be provided. The debate was finally resolved by a summit meeting within Yankee. Beyond the joy of reminiscing, that meeting is of interest today, I believe, as a demonstration of the caliber of people that the utility brought to bear, and more especially for the precedent that I believe was established at that time.

Connecticut Yankee was, I think, one of the first very large concrete reactor containments on PWRs. It's also that I think the result of the meeting is interesting because of the overriding reason that I'll relate -- the reason for not installing a relief device in that containment.

There were only five people involved in the meeting. They were: William Webster, Chairman of New England Electric Systems and of Yankee -- he happened to be an engineer and during and after World War II for several years, he was on the Manhattan project and then on other federal nuclear activities. At the time of the meeting I'm discussing, he was also a member of the General Advisory Committee of the Atomic Energy Commission.

Any of you remember, know that that was a high honor. There was also Roger Coe who was vice president in charge of everything technical at both Yankee and Connecticut Yankee. There was Glen Reed and I will not have to go into his background because I think most of you know it. At that time, he had been project manger of Yankee and plant manager throughout its startup and by that time, three years of Yankee operation.

I was there as Roger's assistant, and to state the case as I saw it, for the need for pressure relief in which I believed. To assist the four Yankees, we had the benefit of

Dr. Theos J. Thompson. Tommy was a member of the ACRS and later became an AEC Commissioner. To the great regret of everyone who knew him, he subsequently met an untimely death in an airplane accident.

Some of us think that the course of nuclear power might have been different. Obviously, I lost the debate. There was no device installed. As far as I could tell, and I believe that this is true, the overriding reason I lost was that no one, including myself, could propose a device that didn't have somewhere near as much potential for causing difficulties as it did for curing them.

I think that is a very fundamental point. Sometimes the things that we need, the things that must be required have to depend on what can be done. What is practical? What is useful to address our response to very low probability situations?

So much for the good old days. We didn't agonize quite as long then as now, but I do think we were just as concerned. Now, it's time to look hard at where the industry stands today as to severe accidents and on containment venting. I have to qualify these remarks. It is obvious to me and it will be obvious to you all that I'm not conversant with the immediate happenings and the developments, so maybe you should think of these remarks as those of an interested and concerned and somewhat informed bystander.

There are a lot of us around, and for that reason, maybe they're valuable. Well, anyway, to start with, we spent $500 million to study the situation That's a lot of money. Are plants significantly safer as a result? Are we in a position to synthesize the results into a basis for a new, practical and comprehensive containment specification?

I submit that today's answers to both questions is clearly no. Will more, similar research provide the needed answers? I don't think so. Instead, I feel the time is now to mount a major effort to provide for passive cooling of the core on the floor and to provide passive filtered venting of the containment.

I have only one major reservation -- incidentally, the same one the Yankee management had in 1963 -- can this be accomplished without increases in risk that counterbalance the gains? Obviously, I believe it can. Let's look first a little closer at the new knowledge.

It seems clear to me -- maybe not to anybody that knows more than I do -- that severe accidents didn't deserve the massive effort unless we felt that they were credible. Further, they pretty much stem from assuming that actively engineered safeguards don't function, or at least not properly or at least in time.

If that's the case, isn't it equally credible that containment relief will be required.

It also seems indicative of a real concern for over-pressurization, for the industry to plead for allowing higher, untested pressure levels in existing design. I agree there's a very large safety factor in the current designs, but taking advantage of that to gain a few theoretical hours that may not be enough doesn't strike me as a valid engineering approach, not does it do a think to reduce risk.

I do recognize that the industry is mired in a combination of eminently valid quandaries. I also believe that our concept of risk as the product of frequency and consequences is a valid one, and I also agree that the probable frequency of a severe accident and/or over-pressurization is very, very low, but in the opposite direction -- and other speakers have mentioned very pertinent things -- the Defense in Depth could use some buttressing. It is difficult to prove a negative, but anyway, in this situation, in the opposite direction to the low probability of the situation, we all know that the consequences of containment failure will be very, very great. Furthermore, they're essentially indeterminant in kind, in quantity, and in duration.

My judgement, any judgement of the level of risk that is the product of these two situations -- I express it as being all too likely to be in the eye of the beholder. Others have pointed out different ways to look at it, with the same basic message.

Further, even sometime after the reactor is shut down, the decay heat in the core is not a trivial quantity of ongoing energy. One percent -- only 1 percent of the thermal power in a large reactor is on the order of 40 megawatts, or 40,000 kilowatts. That is one hell of a lot of low-pressure steam to hope to relief through a few cracks in an overstressed containment.

It scares the hell out of me to even consider that, partially because when I was even younger, I started up an 80,000-kilowatt machine. That was the largest electrical generator in New England in 1952. It produces 80,000 kilowatts of electricity. We're talking 40,000 here as just lying there waiting to bite us. That's a lot.

I also have a very basic belief that the overriding responsibility of the engineering profession is to provide the best achievable design within the limits of the knowledge available. In questions of public safety, that dictum should not be overridden by any other pragmatic consideration.

Now comes my recommendation to ACRS.

I believe they should take the bull by the horns -- no pun intended -- and suggest -- I believe they should suggest that passive means for cooling core debris and for relieving containment overpressure should be seriously and specifically considered for incorporation in future reactors.

As a follow-on, I suggest that ACRS develop a scheme for evaluating the pros and cons of the specific devices to accomplish this objective. There are already a number on the street, and more will be forthcoming, I'm sure, if ACRS urges such an approach.

As examples of items of merit, I think two of the paramount considerations -- and I've already, really, alluded to both -- is that, first, the device or devices should have minimal effect on the basic design of the plant. I don't want to lose the lessons we have learned and the improvements we have made if I don't have to. Second, the devices must provide a substantial improvement in safety assurance, even after careful examination of every credible detrimental effect or risk.

Obviously, those two items are only two of a very long list, but for now, I'd really like to proceed to talk about a self-actuated pressure-relief device for reactor containment, sometimes referred to as SAPRD, and I hasten to admit it's an approach in which I have a deep personal interest and one which I hope will merit a high score under any objective evaluation.

You and I will be both glad that I can now talk from slides.

[Slide.]

MR. MINNICK: That slide is pretty self-explanatory. The thing to emphasize is the fundamental purpose of the device

I am going to describe.

[Slide.]

MR. MINNICK: But fortunately, we were able to combine other desireable functions that the device can also perform and perform effectively.

It will scrub, or, in effect, filter everything except the noble gases. It will provide for diluted, elevated, and heated release of the noble gases. It has the ability to condense all the steam that goes through it and return the condensate, the water form, to the containment, and of course, the place to put it in the containment is under the vessel.

The device will also, incidentally, reestablish containment integrity whenever the containment overpressure is terminated, and it does this with no operator action, no help, and incidentally, it provides relief of potential containment vacuum following an incident.

[Slide.]

MR. MINNICK: Have I whetted your interest?

It does all of this with some very important inherent characteristics. It's actuation is totally passive, and that is true as it sits there waiting and during its functioning and after the accident is over. Need no active device or mechanism, no operator action, no requirement for electricity, no instrumentation or control, and no makeup water, and incidentally, just for a kicker, it shields everything it collects and ultimately contains whatever has not been returned to the containment in a single, underground tank.

[Slide.]

MR. MINNICK: Having made all these wild claims, let's get some idea of how the thing works.

I thought it would help if I took them in sequence.

This is how it looks in normal operation. Fundamentally, it is a manometer, and it's as simple as every manometer you've ever seen. Inside, it's large. I have to counterbalance the pressure in the containment with a column of water, and depending on what that pressure is, I have to assume roughly 2 feet of water to compensate for it.

During normal operation, when the containment is atmospheric, the manometer sits there with the end of the standpipe seal under several feet of water. When the pressure increases in the containment, water is forced downward in the chamber and up into the standpipe, until -- and this is the only calibration necessary for this -- the water level drops to the level of everything that rises up, and as you will see later, there's more than just the standpipe. That calibration is fundamentally based on the size of the room and the diameter and length of the pipes.

Okay. That would provide for relief at a preset pressure, but we need to do more than that. Among other things, we really want to cool the effluent from the hot containment. In order to do that effectively, I've shown here one recirculating drain. The end of it, you will notice, is lower than the end of the standpipe. Therefore, it's always sealed underwater, but it's purpose is to stabilize the process and provide for recirculating heat removal, recirculating pre-cooling, and mixing.

As you'll see later, this is misleading, in that I have shown only one. A multiplicity, of course, is necessary to remove the substantial amounts of heat we've been talking about.

Now, there is one more drain, appears both necessary and desireable. We simply want to limit the height of the column in the standpipe, and more particularly, we want to take the overflow, which is really the condensed steam or the water formed by condensing the steam, and if we didn't remove it, the level would continue to rise.

So, when it rises to the level of the upper drain, it flows, by natural processes, down to a sump, and the sump is only provided to provide a seal, so that when the pressure rises, water is forced up into this, just like it is into this and this, but that seal is of small -- relatively small size and, as shown, in effect, a weir will drop the water and allow it to go, hopefully, directed under the containment.

[Slide.]

MR. MINNICK: A little better picture of the whole thing looks something like this. I've added some other niceties or necessary niceties. The whole business is supported by a seismically designed chimney, which also provides for several functions. It's really better to look at this picture that you have in your handout, along with this slide.

[Slide.]

MR. MINNICK: But the chimney supports the whole business, provides seismic support for all the piping inside. It provides a shield during the accident so that when the activity, some of it at least, is up in the standpipe, that activity is shielded from anything outside the chimney, and additionally of course the chimney with a source of heat inside of it, some 120 feet high, provides a very significant updraft.

That's what we depend on really to remove the heat. I've already mentioned that there is a multiplicity of recirculating drains, for the basic purpose of providing heat transfer area. Heat transfer area, of course, within the draft that I was talking about.

Inside this standpipe, and it doesn't really show on the picture, but it quickly became apparent when we started looking at this thing n a little detail, it would be desirable to break up bubbles. It makes a whole operation more predictable and also enhances mixing and scrubbing. So we can assume that there are perforated baffles of some kind up the length of this damn thing.

At the top of the standpipe, you'll see a moisture separator which is a pretty typical piece of gear, to take droplets of water out of any turbulent situation. Now I told you that it didn't need any instrumentation and now I'm telling you we need level indicators. However, the level indicators are only significant prior to the need for the device, basically to assure the operators that at all times the ends are covered and the thing is in calibration to perform the role that we've described. So it's just a monitoring ahead of the situation.

I was glad to hear today that we're less concerned with concealable rapid pressure transients than we used to be. But I think they would be pretty well squelched anyway by the fact that this is really only a relatively small hole in the containment, and pressure transients tend to ignore small holes, I think. But in any event, it's perfectly practical to put in some kind of a muffler, not unlike an automobile muffler only bigger, to damp out any rapid transients that may be taking place inside the containment.

[Slide.]

MR. MINNICK: Finally, just as another refinement, it seems beneficial to put fins on the standpipe and enhance its ability to reject heat to the draft in the chimney. This thing

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IMAGE EVALUATION
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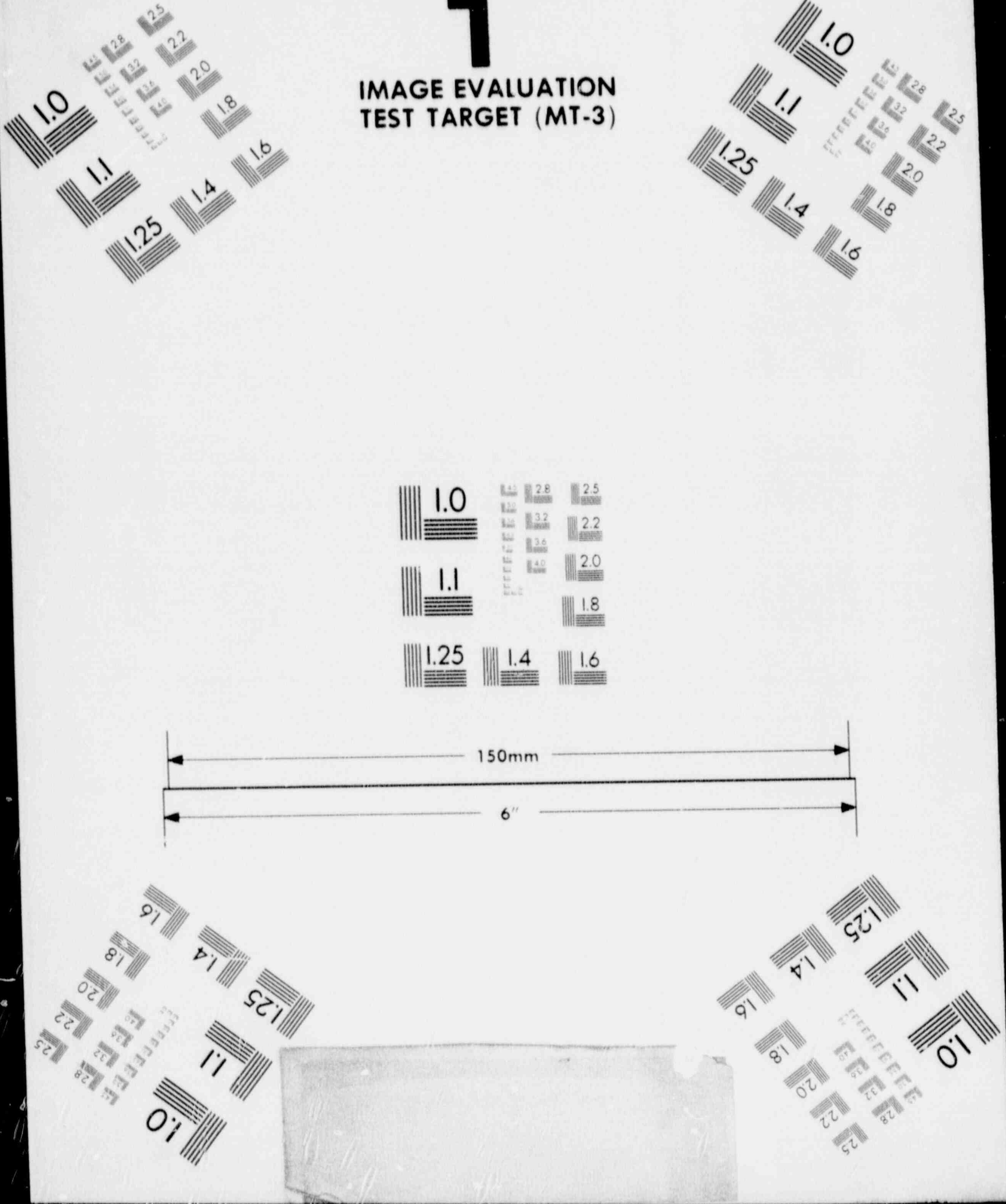

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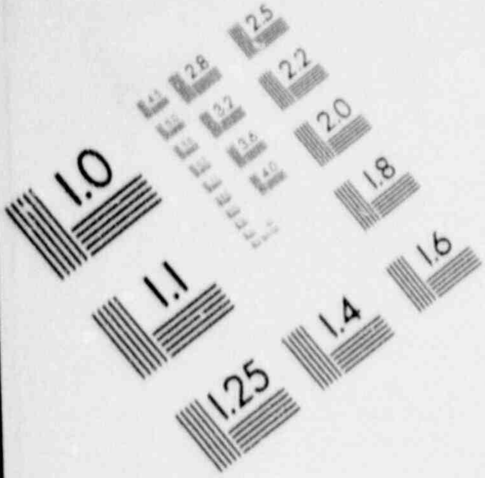

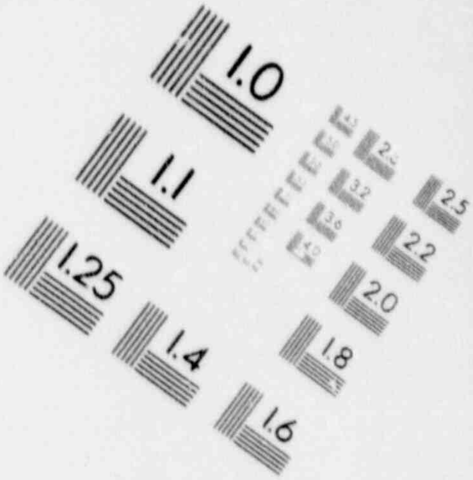

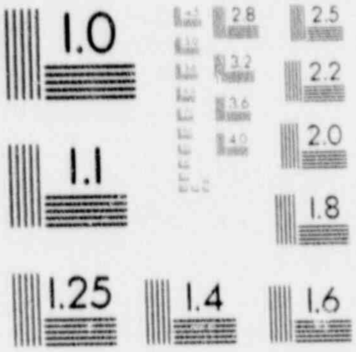

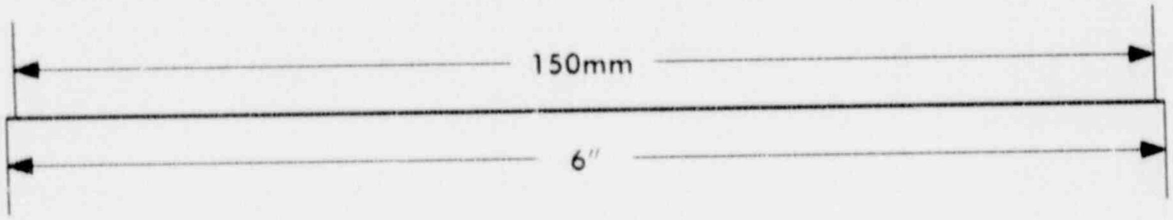

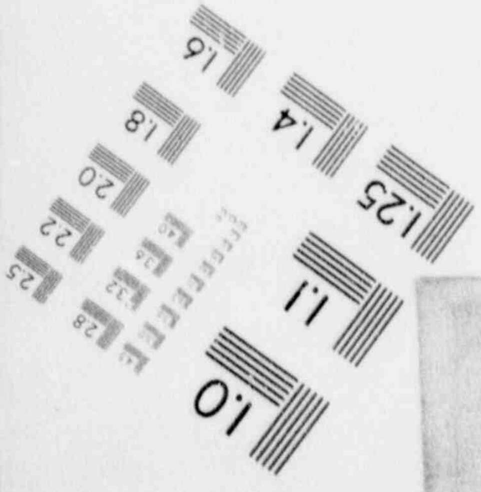

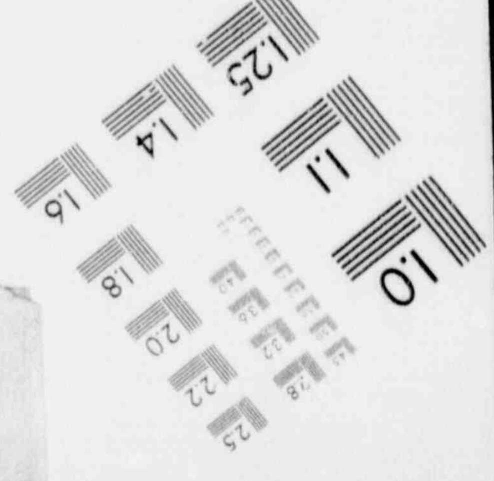

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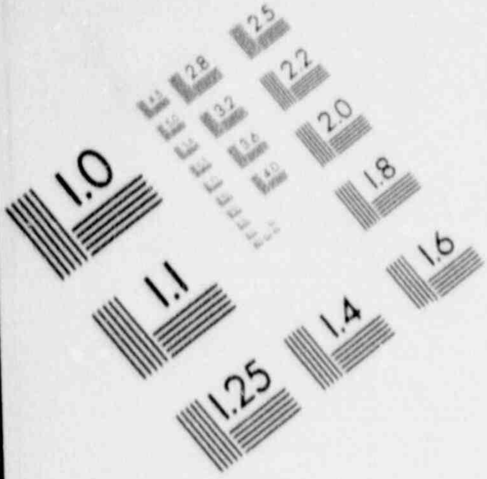

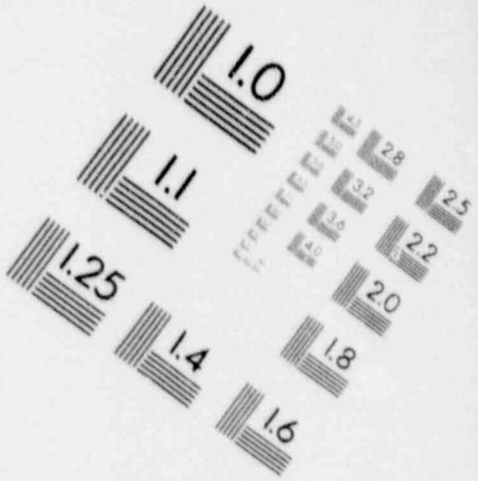

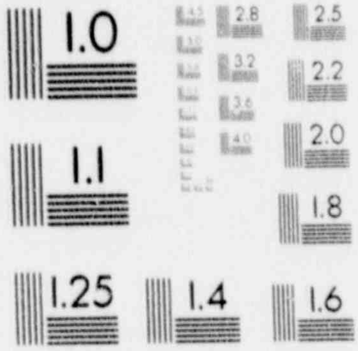

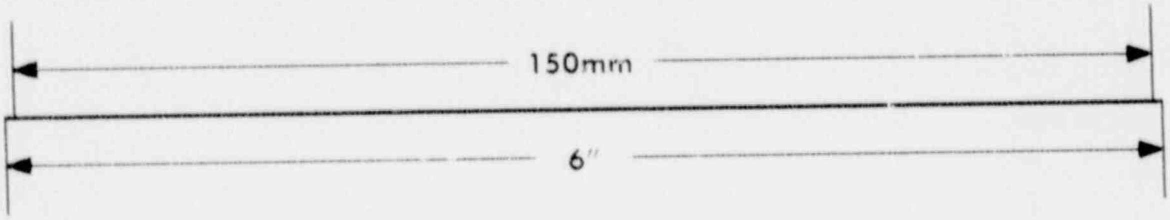

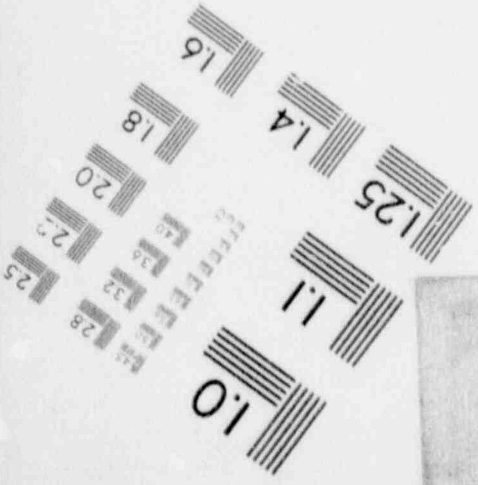

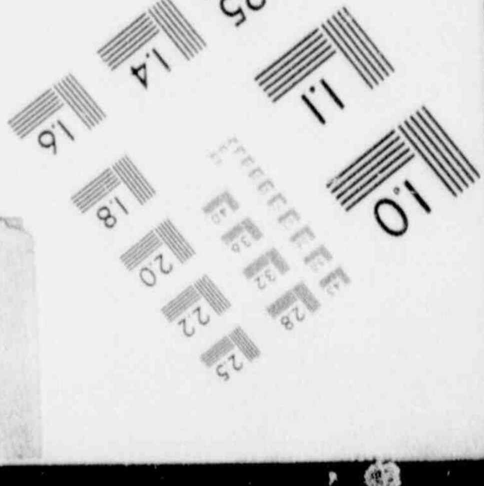

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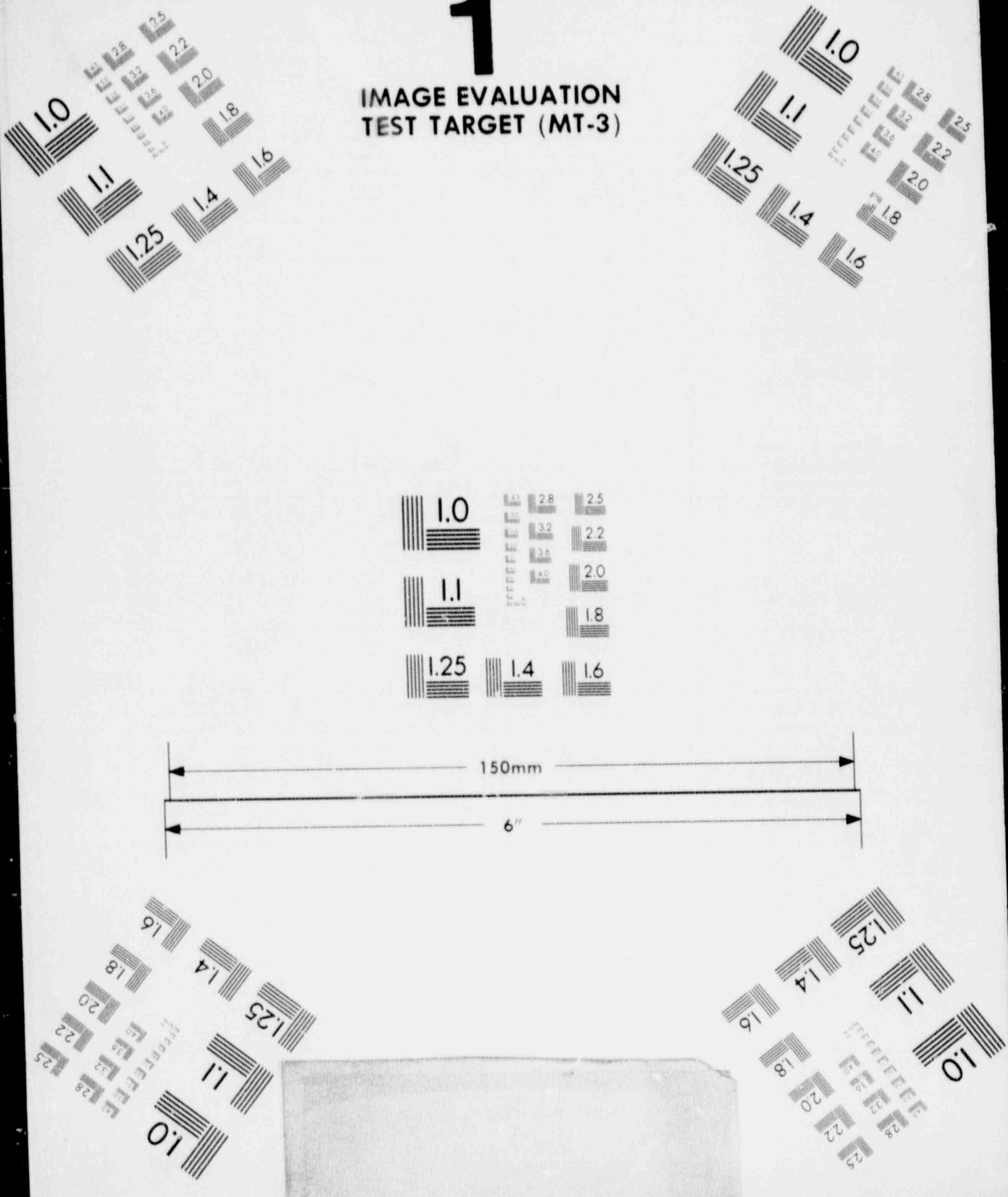

was studied, as I showed on the first slide, by S. Levy, Inc., of which I was an employee at the time, under the sponsorship of EPRI.

We spent the grandiloquent sum of $65,000 and I think we got our money's worth, at least relative to $500 million. Anyway, our conclusions are here somewhere if I can find them. We did analyses of the thermohydraulic considerations. We did analysis of radioactive releases through the water, as compared to postulated accident releases by others.

We did a reasonably good structural design of the thing, so that we could cost it, and -- well, anyway. Our investigation at least indicated that a DF of at least a factor of 1,000 was appropriate for everything except the noble gases. That's better than the pressure suppression people take credit for, but after all this thing's a hundred plus feet high.

Just for interest, we determined that hydraulically without baffles, in order to prevent slug flow, the diameter of the standpipe should be at least six feet. The volume of the chamber below should be about 15,000 cubic feet, just to provide stability in the system. We can obviously make it larger if you want to provide more water available in the initial stages of this.

Let's see. Cost is always of interest. We made what I think is a very conservative estimate, and felt that the thing would cost about $15 million for an 1,100 megawatt plant.

Finally, like most other people, our major recommendation was that the device required considerable verification, optimization and further study, which of course it does, and that study should be at something approaching full scale, maybe a third or something like that. But it should be under actual operating conditions as far as steam, air, water and so forth are concerned.

I believe we'll get no surprises, but of course that would also reveal those, and unfortunately at least in that first test, I don't see how it could be done with including radioactivity. But I'm not sure that's necessary to demonstrate basic feasibility and effectiveness. So that's my presentation. Questions.

MR. WARD: Any questions?

MR. BENDER: That concept, I think, is interesting from the standpoint of providing a passive system. Did your group do any looking at the heat transfer requirements, just how much?

MR. MINNICK: Oh yes we did. If I didn't mention that, I should have.

MR. BENDER: Well, I just didn't get an impression.

MR. MINNICK: Indeed, and we are certain that with the draft we calculated, sufficient area to reject the heat which I already mentioned in the slide, could be accommodated by 5,000 one-inch aluminum tubes. They would be thin. Now for gravy, we should also thin the risers, which would give some of more feet. Now I won't guarantee that it's not 5,500 or something --

MR. BENDER: Oh no. I was just trying to get the planning of the scale.

MR. MINNICK: --but that gives you a feel, and we did look at it quantitatively, analytically.

MR. CARROLL: Did the study include looking at an accident for a spectrum of accidents, perhaps, for a large dry containment with and without the device and what kind of off-site doses resulted?

MR. MINNICK: Yes. We felt that the -- and this I should have mentioned. Our analysis showed that this release of noble gas from this in a period of time which might be sufficient to turn on the engineering safeguards prior to vessel failure, the ratio of releases of that situation using our device to a situation where the containment ruptured, was 26 to 1.

Now I think it's more than that really. Intuitively, at least personally, I'd rather be submerged in noble gas anytime than a hodgepodge. But that's qualitative, and I didn't express that very well today. But the disadvantage -- and I should have touched on this -- one of the disadvantages of this thing is to postulate a situation where this, which is set somewhere -- we can debate exactly where it should be set, should be somewhat above design, but as has been said, less than where things start to become plastically -- where they are depending on plastic deformation, so somewhere in there.

But in any event, at least theoretically, this thing will open at some preset value ahead of containment failure. Now that says that there's a time interval between when this opens and when the vessel would have failed if it hadn't been there, if you hadn't used it.

Now if you then postulate that gee, I turned the engineering safeguards back on again a split second before it actually failed, then this thing has released an amount of that radiation which will yield a dose of 1/26th of what will be released when the containment finally does fail.

Now to me, that's a very small price to pay, especially -- and that's why I mentioned to me the totally indeterminate nature of what really happens if that damn containment does fail. Who the heck knows what's going to come out, how it's going to come out and when they can shut it off?

MR. WARD: Larry, is this draft report something I can distribute to the Committee, or is it --

MR. MINNICK: I don't seen any "Proprietary" on that, but then -- as far as I'm concerned, you're more than welcome to.

MR. WARD: Okay. All right. We'll count on that. Okay. Thank you very much, Larry.

MR. MINNICK: Thank you.

MR. WARD: Let's see. Our next speaker is Adolph Walser of Sargent & Lundy.

[Slide.]

MR. WALSER: My subject of presentation is, as it says, containment design criteria for future nuclear power plants considering severe accidents. I'm addressing this subject from the standpoint of a structural engineer who is in charge of designing containments.

[Slide.]

MR. WALSER: As a guideline for you people involved in phenomena, determining loads, and so on, I'd like to emphasize that the structural engineer needs, as a basic input, to know something about the size and configuration of the containment, and really important, almost most important, we need to know loads to which the containment is subjected to.

As far as loads go, we need to know where are those loads applied, what is the magnitude, what's the time dependency, and what's the probability of occurrence for those loads.

Next, we need to know whether there are any requirements for particular construction materials, steel containment or reinforced, or a pre-stressed concrete containment, and what are the material stress and strain allowables which go along with the loads, specifically with the probability of occurrence for those loads.

Based on that, we can select material stress/strain allowables, and the applicable codes, and the categories under which this containment may fall.

[Slide.]

MR. WALSER: Let's review what we know about at present regarding the containment, the structural containment design. The containments to date and present containments are designed for LOCA loads. LOCA loads are well defined. The nuclear steam supplier is supplying these loads. The loads are coupled to the final capability of the reactor in one way or the other.

The present codes which are used for the design of the containments are ASME, Section III, Division 1 for steel containments, and Division 2 for concrete containments. The codes are complete. They have been developed and are presently maintained by industry research institutes, universities, of course with the participation of the NRC. These codes are based on that the containment is designed for LOCA loads.

[Slide.]

MR. WALSER: Just to show some examples of the loads we are talking about, for any plant which is presently designed, the nuclear steam supplier can give me a containment pressure versus containment volume relationship. So I can size the containment, and I can select my design pressure. A similar relationship is provided for the accident LOCA temperature.

[Slide.]

MR. WALSER: In another form, the pressure load is given in terms of time after pipe break. Such a curve tells me the peak pressure. I have to add a margin to the peak pressure, of course, and the graph tells me whether this load is dynamic or static. It's well defined. Of course, as you mentioned, the containment design at present may not be designed for the real thing, but let's continue.

[Slide.]

MR. WALSER: Containment capabilities for existing containments have been determined. I define containment capability is an upper bound pressure load beyond which the containment may fail in one form or the other.

Safety margins have been computed by dividing this containment capability pressure to LOCA loads, and factors in the order of three have been arrived at.

Of course, the acceptance criteria to determine these capabilities are creating the containment to stresses beyond code-allowable limits. Actual material strengths, behavior stress/strain all have been factored into those studies.

These containment capabilities have been used in the PRA studies to determine the risk to the public of releases, and, to my knowledge, the containment that the PRA studies have shown that the risk to the public is acceptable using the present definition of "acceptable."

The testing by Sandia and others of scaled containments built of steel and reinforced concrete have shown that the containments behave ductile in most cases, and that the ratio between containment capability and LOCA pressure is a real number. It doesn't just come from analysis; it has been backed up by these tests.

MR. WARD: When you say in most cases, have there been some experiments that have, some tests that have indicated non-ductile failures?

MR. WALSER: I'll let Walter Von Riesemann address that.

MR. von RIESEMANN: The answer is yes from the steel but that needs a lot of application before it's taken out of context.

MR. WARD: And you are going to talk about that, or are you?

MR. von RIESEMANN: A little bit.

MR. WALSER: I would say that the containments which are totally modelled and which are representative have shown ductile behavior with no reservations.

MR. WARD: All right.

[Slide.]

MR. WALSER: The work which went into the determination of the containment capability were sponsored by the IDCOR program, by utilities who commissioned PRA studies, by the Sandia and NRC sponsored workshops and the Sandia effort on NUREG-1150.

The work was done by a large, large group of people. If you are interested I can -- I have viewgraphs about the results of the IDCOR studies.

[Slide.]

MR. WALSER: In the IDCOR program, the following containments were investigated. You see on this slide. They are a variety of shapes and ages and large containment PWRs and a variety of BWRs.

[Slide.]

MR. WALSER: The failure conditions are shown on this slide and as you see, in general the containment is expected to fail in the hoop direction, which is logical since the containment is also designed for seismic loads in combination with the LOCA loads so the vertical direction has some reserve.

[Slide.]

MR. WALSER: Here we see the load factors which are the safety factors which I was talking about which range from 2.7, 2.4 here to 2.1 for the Browns Ferry, a Mark I.

[Slide.]

MR. WALSER: So the safety factors are beyond what you would expect from a typical structural analysis where the safety factor might be in the range of 1.7 or thereabouts.

[Slide.]

MR. WALSER: Here is an evaluation why these safety factors come about. Of course the largest one is the LOCA pressure load factor, the load factor for the pressure load which comes out of the ASME code.

[Slide.]

MR. WALSER: I remember that on one occasion we were asked to provide a confidence level for these studies -- in other words, probability of failure versus the ratio of that safety factor and as you see, the consensus was that up to a point the confidence is very high that the containment will survive but when you get to a state of yield in the containment materials anything can happen and the confidence drops rapidly.

MR. WARD: That was a summary of expert opinions, is that what that was?

MR. WALSER: This was -- no, I wouldn't say -- it was a very limited opinion. The people who worked mainly on the IDCOR program thought it was a reasonable representation.

MR. WARD: Okay.

MR. WALSER: It wasn't a scientific approach but it shows that in the containments if they behave ductile you are pretty certain that nothing is going to happen until the materials yield, until you get into large deformations.

MR. CARROLL: The lower part of that curve monotonically increased and then -- is that --

MR. WALSER: That is a guess.

MR. CARROLL: What I am trying to get at, is that -- when we talk about containment capability are we talking only of structural failure or are we talking about seals and elastomers and things like that?

MR. WALSER: Including the containment as a structure, including the penetrations and seals and everything.

MR. CARROLL: Okay.

MR. WALSER: Of course there is a little flaw in this curve. Actually, since the containments have been pressure tested to the design pressure the curve should be starting from here and not from zero.

MR. CARROLL: They have been pressure tested at 1.25, the design pressure.

MR. WALSER: About, yes.

[Slide.]

MR. WALSER: The lessons learned from these containment capability studies in number one, the containment has a large safety margin. The containment designed to LOCA loads has a large safety margin.

To arrive at that large safety margin the containment must be ductile. It cannot have any weak links. It can strong links but no weak links below the major failure of the containment.

This ductility is called for in the present ASME code. It is not something which is left to the ingenuity of the designer. It is a requirement of the present ASME code.

MR. CARROLL: I heard a comment this morning that designers should or are going to have to learn how to avoid, even though they meet the code, avoid discontinuities and things that can lead to failure.

Do you subscribe to that?

MR. WALSER: Absolutely. That's where experience comes in and yes, definitely.

MR. CARROLL: Does that suggest the code has problems that should be corrected?

MR. WALSER: No. You cannot codify things like that.

MR. CARROLL: Okay. I heard that someplace before too.

MR. WALSER: Anyway, to come to the conclusion of a long story is that in our opinion the present structural containment design criteria is adequate and should not be changed in the near future.

[Slide.]

MR. WALSER: We did think about to provide you some recommendations for future developments if you should desire to undertake that.

Number one, we recommend that future developments be done as an industry effort. Utilities, research institutes, universities, architect-engineers, nuclear steam suppliers, and of course the NRC should jointly undertake such a future development.

The goals of this effort from a structural design or standpoint should be that loads in terms that they can be used by the structural designer be defined. We need numbers, not ideas. We need numbers.

Of course we also can use probabilities. We are not necessarily tied to, you know, numbers in psi but probabilities can be factored into that.

A consensus has to be reached by this recommended industry group as to the events involved in severe accidents. I still hear now that sometimes somebody says this load doesn't exist. Somebody else says yes, it does exist and tell me. A consensus has to be reached regarding the probability of the risk to the public in case of severe accidents.

[Slide]

MR. WALSER: Future structural designs will be based on probabilistic assessments of loads and resistance to achieve a safe structure. The structural steel code has taken the first step in that direction, and as all codes are behind, but they are following. When this can be done when such design codes are available, it is time to also change the containment design.

But the present ASME design code had to be changed from the present deterministic to future probabilistic in terms of load factors, or in terms of allowables, allowables in terms of stress and strain, and again ductility has to be emphasized.

MR. CARROLL; Going back to your first bullet, you mentioned that there is a trend towards coming up with load assessments based on probability.

Can you give me an example of that, give me a feel for what that is all about? Where is that effort taking place?

MR. WALSER: For instance, if you have a load, such as that load, which you know is there, the load factor has to be high, let's say. Higher than for a load which you expect not ever to see to its full extent.

MR. CARROLL; So you are talking here at least in the efforts that are presently ongoing about loads in the classical civil engineering sense?

MR. WALSER: That is correct.

MR. CARROLL; Not loads in terms of molten corium?

MR. WALSER: No. No.

MR. CARROLL; Okay.

MR. WALSER: No. Strictly structural design codes.

MR. CARROLL; Okay.

MR. WALSER: Of course, in answer to your question about molten corium, the people knowing about those phenomena may say I need so and so much of some type of concrete to protect against breaching of the containment. And that is, of course, a requirement other than load.

The Advanced Light Water Reactor Industry Group studies at the present time only two types of containments: a BWR and a PWR. And whereas now we have a whole series of different shapes of containments, the fact that fewer containment shapes may be involved in future plant designs may simplify all these matters considerably.

Of course we anticipate that all these efforts in our recommendation will take a considerable amount of time. I had a number here in my draft, and I was asked to remove the number, in terms of years.

[Laughter]

MR. CARROLL: Are you willing to tell us what it is orally?

MR. WALSER: Well, I said 20. Which may be a little optimistic.

MR. WARD: Any questions? Yes, Mike.

MR. BENDER: I want to go back to the probabilistic question for just a minute.

MR. WALSER: Mr. Amin from Sargent & Lundy I am sure is very qualified to answer that question better than I am.

MR. BENDER: You I think suggested something that many of us have talked about, of having a lower load factor for lower probability events, which I think the pressure vessel code already has in it for conventional kinds of vessels.

But you did not say anything about load combinations. And it seems to me that that is a part of the probabilistic issue.

MR. WALSER: Yes. Yes. The load combinations of course have to be based on a deterministic line of thinking. What loads can exist at the same time is the basic question. And of course, depending on the number of loads you consider to act at the same time, load factors may vary from load to load as it does now in the ASME code.

MR. BENDER: But existence at the same time is for certain a probabilistic issue?

MR. WALSER: Yes. Yes. Absolutely.

MR. BENDER: So the load factor might be different.

MR. WALSER: Yes.

MR. BENDER: Depending upon what combination you are talking about?

MR. WALSER: Absolutely. Yes, indeed. Yes, indeed. Now, that is where the probability comes in. We should have a consistent probability of loads and load combinations occurring.

MR. BENDER: Okay. Thank you.

MR. WALSER: You might be interested that several years ago -- Do I have some more time?

MR. WARD: You do. Go ahead, please.

[Slide]

MR. WALSER: We made a study for Commonwealth Edison which had to do with determining the location of a potential containment failure. We were asked to determine the Zion containment could take so and so many PSI load result failure. The question was asked, where is it likely to fail once it goes beyond that point?

So we looked at that containment shell very closely and we found that we had these likely points or locations of failure. And I have some slides showing this event here.

[Slide]

MR. WALSER: If you look at the containment growth as the pressure increases, we see here that up to the threshold, actual design threshold we have, of course, a quite linear behavior where cracking of the concrete occurs. And there we have a slightly softer containment, which then gets softer and softer as cracking increase. And at this point we determined that the tendons, the loop tendons, would start building at about 1 percent strain, and then from then on of course it was a matter of just stretching those tendons to their failure point.

MR. CARROLL: And then what happens?

MR. WALSER: Well, then we have a big bang, if that should ever get that far. But we are certain that that is not going to happen. That is why we studied those phenomena. And as you can see, the containment will not sustain a pressure increase and deform in the radial direction from four inches to 30 inches. That will not happen without prior small failures.

MR. CARROLL: Small failures of the liner? Is that what you are saying?

MR. WALSER: For instance, yes. In other words, localized failures are anticipated.

MR. WARD: But not in the first five inches?

MR. WALSER: Yes. That is correct. Yes.

MR. WARD: Is that because there is explicit design of the details to accommodate the five inches?

MR. WALSER: Yes. And the strains, materials strains, up to this deformation, are not small, but they are not large, either. Most of the items, most of the steel materials are below yield. The tendons are the first ones to yield, or in case of reinforced concrete containment, the reinforcing steel is the first to yield.

QUESTION FROM THE FLOOR: All of this is true for reinforced concrete containment?

MR. WALSER: Yes. We have not studied steel containments in detail. So that is why I can't address it.

[Slide]

MR. WALSER: The point I would like to make here is that the containment of course has lots of penetrations. Here is a penetration area. There the quarter-inch liner is equipped with an insert plate to accommodate the many electric penetrations here and there.

And here we have the two valve openings. And what we figured out in our study was that the proximity of this to the insert blades, and the fact that this penetration is held against the concrete, so is this penetration assembly, and as the concrete moves, these points will move.

And the liner in between these two anchors, let's call them, simply will be stretched beyond what the typical liner will be stretched.

MR. CARROLL: Now, if you were doing this over again, I take it, you are saying you would provide more separation between?

MR. WALSER: No, we would watch that separation, which we have always watched that separation. But we see with this study that the separation really has to be watched.

[Slide]

MR. WALSER: And this was our line of thought. We have here the containment deformation versus liner strain, showing here the typical progression of the liner strain in the loop direction.

The liner strain we expect to be at containment, absolute containment failure, to be in the range of about 3.6 percent, which a piece of steel can sustain under ultimate test conditions.

Now, the loop liner between those penetrations will get stretched more. And it will of course come to about this strain level at an earlier deformation time.

So we have classified that this location would be one of the location where the containment would leak, start leaking before it reaches this breaking point. And we have found several such locations. Of course, they are all within the design. The design deformation is somewhere here. So we see the enormous amount we have in terms of strain. Not stress, but strain.

MR. CARROLL: Okay, now. If somebody said here is a clean sheet of paper, design me this same Zion containment, and space things better, do whatever you can, what would happen to these special areas? Would you end up with something closer to the 30 inches of deflection, or how much closer? Do you have enough room to physically separate them?

MR. WALSER: Yes. Number one, there is enough room. Yes. But this particular case was not considered a failure, or a brittle failure, from a design standpoint. It is well within the original design code.

MR. CARROLL: But my question is, how much relief would you get if you were to start over again? Could you design a containment that there would be no point on it that could not accommodate the 30 inches deflection?

MR. WALSER: Well, that would get you to the other concern, the sudden failure, rather than the leak before break.

So I would not, that is a philosophical question again.

MR. CARROLL: No, not necessarily. Because I might way you, as a structural engineer, give me all I can get on this containment. And then I may want to put a rupture disk on it at some pressure less than that.

MR. WALSER: Yes.

MR. CARROLL: And control where my brittle failure is going to be.

MR. WALSER: Yes. It can be improved.

MR. BENDER: I think I don't quite understand the nature of the deformation. Is this a concept with reinforced concrete behind it? Or is the shell freestanding?

MR. WALSER: No, the liner is attached to the concrete.

MR. BENDER: A 30-inch deflection is the number that I heard.

MR. WALSER: That's the projected ultimate, where the confidence level is absolutely zero.

MR. BENDER: Can it physically do that, without buckling or in some form?

MR. WALSER: Yes.

MR. BENDER: I would have thought the peripheral restraint would stop it from moving, but never mind.

MR. WALSER: We have, of course, some buildings in the research which would put a dimple into this glowing containment, which was not considered a cause of failure.

QUESTION FROM THE FLOOR: Thirty inches in a 130-foot diameter containment is not very large.

MR. WALSER: It's equal to the containment wall thickness.

MR. BENDER: Well, I haven't done the arithmetic, so maybe it's there. It surprises me a little bit.

MR. WALSER: If you take the Sandia model test results and proportion them up to the real thing, you get much beyond the 30 inches.

MR. BENDER: Okay. I'll buy that.

MR. WALSER: Thank you very much.

MR. WARD: Okay. Thank you very much, Mr. Walser.

Let's take our afternoon break now. We'll make it 15 minutes and come back at 2:40.

[Recess.]

MR. WARD: Our next speaker is Walt von Riesemann.

MR. von Riesemann. I'm going to make some off-the-record comments. It's almost like saying "ditto" up here to a lot of the comments today.

MR. WARD: That's all right, we're slow learners.

MR. von RIESEMANN: I'll just wait for a moment for the viewgraphs to be handed out.

[Slide.]

MR. von RIESEMANN: As you can see almost a tone, I'm calling this Thoughts and Reflections on Containment Design Criteria. It's based on about 12 years of experience with the analysis and testing of either scale models or actually full-sized panels of containments.

[Slide.]

MR. von RIESEMANN: Having worked in the industry for a while, I realize that I should put up some caveats maybe first. The comments are my own. They're not necessarily those of Sandia nor any other organizations that I deal with, both in the U.S. and in Europe.

Another way of paraphrasing this might be to say, what's true is not mine, what's false is mine. The examples I'm going to cite are only used to illustrate a point and shouldn't be construed to be absolute. I'm sure we could sit here for maybe hours or days looking at the details of the examples. But again, forget the little nits, if you will. I'm trying to illustrate some points and develop a thrust.

My majority comments are based on light water reactors and as mentioned previously today, the conclusions could be quite different for a different type of reactor. I also feel that right now in the light of day that we're in with a slowdown, if you will, in the industry, that it's a time to look at design requirements when perhaps rational minds will prevail rather than waiting for a crisis to occur.

[Slide.]

MR. von RIESEMANN: I'll also mention my summary right up front to tell you where I'm going. I feel that we've had at least a decade of knowledge on containment behavior, both severe accidents and operational experience and these have not been factored into the actual -- I should say design requirements. I shouldn't say the ASME code there. That's one facet of it.

My recommendation is that a committee, of course formed of a cross section of people, be formed to rewrite the design requirements, taking into account what we've learned, and particularly what we've learned from severe accident studies. We must consider the containment as a system, not as a single component.

As mentioned this morning, I think the first step really would be to determine what philosophy should be used before we get into the fine details.

[Slide.]

MR. von RIESEMANN: I don't want to bore you with this material, but just as a refresher, if you will, even for myself, the purpose of the containment obviously is to contain the radioactive material that might be released from the system, but there are other functions.

It's a radiation shield. It's also protection against external threats, missiles, tornados, sabotage and it supports perhaps equipment like crane rails.

[Slide.]

MR. von RIESEMANN: That is, it's not an isolated component. It just doesn't consist of the shell. We do have penetrations and bellows, drywell head on the boiling water reactor, fuel transfer tubes, isolation valves, basemat instrumentation to know the status of the system. I feel that the response of the containment, if you will, depends on the interaction and the behavior of all of these.

In the NUREG-1150 study, for example, there was one possible scenario where the pedestal for the reactor vessel would be eroded away. This would then take the steam lines with it and possibly fail the containment. You can sse the interaction that might occur in a severe accident.

[Slide.]

MR. von RIESEMANN: Let me briefly -- and I say briefly -- just hit a few highlights on the current approach to LWRs. I'm being brief, so there might be a few little slights in my coverage. We assume the loads are known. We assume that the pressure is known, temperature and even the earthquake.

There are different philosophies and Adolf Walser has since indicated this, between the steel and concrete containments, and these, in fact, do lead to different margins of safety against internal pressure. One could ask; why?

[Slide.]

MR. von RIESEMANN: We also have jurisdictional boundaries in the code between steel and concrete and this possibly could lead to some inconsistencies. As mentioned before, the liner is given zero strength, but here is maybe one of my -- two key points: design is based on essentially elastic behavior.

There's no provision for going inelastic, and yet there is reserve capacity. Also, the leakage requirements that are put on the containment are very stringent.

[Slide.]

MR. von RIESEMANN: Let me discuss -- observations, I'll call them -- lessons learned. They're primarily based on severe accident work, and some of these also come just from experience with containments. Current design personnel air locks and electrical penetration assemblies behave very well, both from a leakage viewpoint and strength.

There was some problem with their electrical performance in the high temperature and high pressure environment. I'll maybe go through these rather quickly. You can read them also.

Equipment hatches, the sleeve will ovalize and leakage may occur through the equipment hatch; also some designs have pressure unseating hatches where the pressure tends to form a gap. I would say that they're not desirable.

MR. CARROLL: Do you have any notion as to what percentage of today's containments are in that category?

MR. von RIESEMANN: A small number. I don't have the exact count. I couldn't venture a guess now. I'd have to do some looking.

MR. CARROLL: Okay.

MR. von RIESEMANN: Seals and gaskets; we tested a variety of those. They also performed very well up to about 500 degrees Fahrenheit. In other words, they did not degrade in their ability to keep pressure. Inflatable seals which are used only on about 13 -- this number I have -- 13 reactors, are used on personnel air locks. They're essentially a balloon to fill up the gap. Well, they will, of course, leak at overpressurization.

They do fine in design conditions. It's the severe accident they have a problem with. Basemats; it's just something new that's come up in the recent tests. The results will have to be interpreted and there might have to be some additional work.

MR. CARROLL: What does that mean?

MR. von REISEMANN: Well, I would rather not say on the record.

MR. CARROLL: Okay.

MR. von REISEMANN: I suggest you go to the water reactor meeting next week.

[Slide.]

MR. von REISEMANN: Well, perhaps I could mention that there was a test of Sizewell B 10 scale model in the U.K. and the base mat failed. Now, whether that's indicative of difficulty or not, it's too early to tell, because one has to interpret all the data.

MR. CARROLL: In what kind of mode?

MR. von REISEMANN: In a bending mode, if you will -- dishing. There was concrete spoliation from the bottom of the base mat and excessive deformation.

Let me talk in particular -- this one has been addressed to some extent by a few people already. The stiffening around penetrations and the "area replacement" rule causes strain risers. This performs very well in the elastic region and the design region, but going on loads beyond design, it may lead to an early failure.

Let me give you two examples.

[Slide.]

MR. von REISEMANN: This is a picture of a scale steel model. This is 14 feet in diameter, 21-feet high. It typifies a either ice condenser or free-standing steel containment. It was built to the code by CB&I.

This is an equipment hatch showing the -- here it's open, obviously. There is a stiffening area around the opening. We have stiffening rings in the circumferential direction and, also, around the opening. In the test, right in this area, there was a strain concentration. The well failed here, and that caused a failure in the shell and then caused a catastrophic failure.

[Slide.]

MR. von REISEMANN: This shows, again, a picture of the model. The aerial view of this site after the failure mode -- these are pieces of the containment some distance away. The detonation or the failure was catastrophic.

However, quickly add, there's a competing horse race, so to speak, going on. The equipment hatch ovalizes, the cover stays circular, and you have a mismatch of the two.

This shows the calculated displacement versus pressure for the equipment hatch versus the experimental data. You notice it's flattening out. The deformation is growing rather rapidly with pressure. At a few psi additional pressure, there very well could have been leakage going in this path, but what the major point I'm trying to raise again is these little details can cause a failure. It's a high level, mind you. The failure occurred at 195 psi. Design pressure was 40. The containment formed well, but you could add additional capacity by attention to the details

[Slide.]

MR. von REISEMANN: Now, conversely, or in a like manner, let me talk about a concrete model 6 scale.

This shows a picture of doing construction of the liner. This is the steel coming out. This is an area where piping penetrations will come through. The material is removed, and a thicker plate is put into place.

[Slide.]

MR. von REISEMANN: The thickness was 1/16th of liner right in here and 3/16ths in the stiffened area. These are the penetrations coming through. These show the studs that are used to attach the liner to the concrete. This is actually scaling, 2-inch spacing.

[Slide.]

MR. von REISEMANN: This is the major failure mode for that containment, looking on the inside. This is the penetration. Before, we were looking from the outside. This is the insert plate, the liner. This is the tear that developed. Depending on where you're sitting, you might be able to see the studs. Can you see them?

MR. BENDER: Yes, the shadows.

MR. von REISEMANN: At first, we thought the failure was due to the heat-affected zone. That's not the case. It's due to the, if you will, prevention of slip by the studs that are attached.

[Slide.]

MR. von REISEMANN: Analyses have been done of that particular area. This shows the insert plate and the studs, if you will, are right here, that failed at 138, 140, 142, and essentially 145 1/2 psi. The blue indicates a very low strain field, on the order of 2 percent or less. The red is on 26-percent strain. You have a high strain concentration right in the area of the studs.

A couple of thoughts on this: One is we are going to do some separate tests on this to see if this, in fact, is valid and test different designs, but analyses have also been done by removing these studs, and the strain increases. You have now the ability to move in that area.

MR. CARROLL: Selectively removing the studs around the penetration.

MR. von REISEMANN: Yes.

Again, they were designed for certain conditions, not for the severe accident.

MR. BENDER: Just to clarify the point, if you took those two studs out, the deformation would go somewhere else.

MR. von REISEMANN: Yes. You would now force the failure over here, at a higher load.

MR. BENDER: You have to carry the load at some other stud.

MR. von REISEMANN: Well, load and deformation.

MR. BENDER: And deformation.

MR. von REISEMANN: If you go far enough away, you can smear out that deformation and then the load would drop off.

MR. BENDER: I agree with what you're saying, but I think you have to look at whether the studs that are out there are seeing a bigger loading, a bigger lever arm or whatever it is they get, due to that deformation. I think you can't totally ignore the fact that when you take these two studs out, something else is going to be carrying the load. It's not just deformation, but loading.

MR. von REISEMANN: Let me go in the extreme, Mike. The studs in free field did not fail. The studs -- and this is not an isolated case; we had other areas where the containment failed -- did fail.

MR. BENDER: That part is understandable.

MR. von REISEMANN: Now it's deciding what an optimum design might be and which way to do it.

MR. CARROLL: Do we really care? Why do we care about this? Suppose I was a venting advocate and said at 90 percent of design pressure, I'm going to have a rupture disk open, and the heck with it. I don't need to worry about these details.

MR. von REISEMANN: If you had a rupture disk, yes. I'm not saying this is a great concern, but it's something to look at and tie it into the overall philosophy on how you're going to design or the design requirements for a containment. This is something that's known; factor it in.

MR. CARROLL: Yes, I guess.

MR. von REISEMANN: Don't change the rules just because this occurred. Use it as a piece of information.

MR. WARD: It seems to me the problem is predicting the behavior of these details in advance.

MR. von REISEMANN: That's another problem. You have to do some fairly detailed analyses to do that, and we're hoping to do some tests, first of all, to confirm the analysis and the sixth scale model, and then see if there are any simplified ways of doing that analysis.

MR. WARD: But the code -- I guess Adolph alluded to this earlier -- the code doesn't really provide much guidance on how to avoid details that are going to fail well beyond design loads but before something else does.

MR. CARROLL: The people would rather it didn't --

MR. von REISEMANN: The code is written very well for the rules that they were given, and now we're changing the ballgame, so to speak, maybe.

MR. BENDER: Just to add to your point, if we were to go to a rupture disk concept, where we would be allowing the loading to go up near to the failure point, I think you'd really want to be more concerned about how these studs are designed than you do if you base it totally on elastic loading.

[Slide.]

MR. von REISEMANN: While you're mentioning that, let me skip one viewgraph and get to this one. I'll get back to the other one in a moment. This is an article in existing ASME code on overpressure protection, and it states, in essence, that a pressure-relief device is not required -- and that's my underlining -- where the service or test limits specify design specification and not exceed it.

Now, since this was written, severe accidents have come on the scene. It's my understanding that existing containments don't need this overpressure protection, because they know the loads, and my conclusion is I think this article, in fact this whole topic needs reexamination.

[Slide.]

MR. von REISEMANN: Getting back to the previous viewgraph, some lessons learned, and these are just experience, if you will. Substantial corrosion can occur where the steel enters concrete. Both aerosol retention and retention in secondary buildings, I say, has not been quantified. Really, it hasn't been given credit, even, and then we know that containments have had isolation valves left open for extended periods. In fact, there was a comment made previously about continuous monitoring, if you will, of the condition of the containment.

[Slide.]

MR. von RIESEMANN: Again, back to the summary, it's my feeling that we've had this database, this information that's not been factored into the design requirements, and I feel that the first step would be to discuss philosophy, how to go at it, and then recommend that we look at the design requirements by a committee of industry researchers and the regulators, and look at it as a system and also address, if you will, severe actions.

[Slide.]

MR. von RIESEMANN: Now let me raise some goals that I would charge the committee with. They're not all-inclusive and some people might argue with some of them. I'd like to see benign failure modes.

MR. WARD: What do you mean?

MR. von RIESEMANN: I don't want a catastrophic failure of the containment versus a leak. That might suggest weak link-strong link design, a ruptured disc or some -- well, inherent knowledge of the behavior of the containment for overloads.

MR. WARD: That seems -- you seem to be assuming that there could be a leak from a failed containment that's large enough to relieve the load, but not so large that there's significant contaminated atmosphere release?

MR. von RIESEMANN: Right.

MR. WARD: Does that -- is there really room where they can be doing this too?

MR. von RIESEMANN: I don't know there is, Dave. That's something that has to be looked at. The other thing of course is you have to look at filtered venting, if you will.

Obviously long life -- these are pretty self-explanatory. Let me discuss this one. I have a word on there that might not be the best phrase.

In our work and I think Adolph can back me on this with the severe accident people, we find we talk a different language. He's smiling. It's a completely different scheme of things. We have to bring the people together, that we understand one another and what they're talking about, and they know what the other side is.

And we have to have the designers become familiar with severe accidents and loads beyond the design bases, and the fact that in some of the cases, the threats are not well-defined. Now I say in here that I've changed the mindset -- I don't know if that's a proper thing -- but the thinking if you will, the communications must be there.

[Slide.]

MR. von RIESEMANN: I also feel that with a little thought and at not very much expense, in fact maybe zero expense, you can make changes to the containment design to minimize the effects of fire, say flooding and hydrogen combustion. I would like to see realistic leakage requirements. My own bias is that the tenth of one percent tech spec leakage requirement is very stringent, and from a risk viewpoint, you can go considerably higher and not affect the risk.

This might -- that stringent requirement might be quite a penalty on the design. Then a very personal personal bias if you will, a realization that buckling per se is not failure. I call this the civil engineering, if you will, syndrome; the oiler column buckling is failure, but in the shell it's far from failure.

Los Alamos National Labs did tests on the torispherical heads and failure, i.e. leakage. It didn't occur until six times the design pressure. Buckling occurred at about two times design pressure. So there's quite a bit of reserve between buckling and leakage for that particular application.

[Slide.]

MR. von RIESEMANN: Now can we get everyone together in the room and agree on this? I don't think so on all these points. There's a lot of potential difficulties. The definitional loads. These -- in fact the next one, design criteria versus performance requirements. As was mentioned, a designer needs numbers and yet we're trying to say accommodate these threats that we're not too sure of.

It becomes a difficult topic, and this is where the work will have to occur on these two -- defining loads, if you will, and deciding whether you should have design criteria and performance requirements or how to handle, if you will, the severe threat.

What I'm getting at, if you make the severe accident a design criteria, by the very nature you put a factor of safety on it and we're getting stronger and stronger and where does it stop? Overpressure protection, i.e., filtered venting is a problem also in the sense that a lot of people have different feelings on that. It's hard to say what will happen in that area.

Also leak rate testing, there's quite a bit of work going on perhaps on modifying that. Perhaps it's not as difficult as I've indicated here. I mentioned this morning too a legacy of the system, the way we do things. I think current licensing is set up to do things in a very prescriptive manner, and it might be difficult to make modifications where you have guidelines and not firm design loads.

Then finally, my own feeling is that currently probabilistic design beyond the -- there's a few words missing here -- probabilistic design is actually beyond the current state of the art. I think one can design a structure and then look at it and see what the probability of failure is, but trying to design a structure for a given probability of failure is rather difficult.

[Slide.]

MR. von RIESEMANN: I'd like to leave, I guess, three thoughts and you see I made this up while I was listening this morning and this afternoon. The containment is a system. It's just not a shell. It's just the many parts and they interact. We should look at them all together. We have to formulate a philosophy for the new design requirements and we have to be able to accommodate uncertainties in -- instead of saying loads, I ought to say uncertainties and threats. Thank you.

MR. WARD: Okay. Any questions for Walt at this point?

MR. BENDER: I would like to raise one question, because it's something that has troubled me somewhat. In the slide before your last one, you had a whole list of things that you might do.

MR. von RIESEMANN: Yes.

MR. BENDER: I'm convinced that you can't just throw those out on the table and ask a designer to accommodate them. It seems to me it can only be done by example. Has anyone given any thought to how to illustrate the concepts?

MR. von RIESEMANN: In some cases as you know, there's commentary written to the Code, for example, that give illustrations on how to do things. There's also documents that could be prepared on, if you will, guidelines to the designer. But really, I think the real answer that I have at this point in time is this is for something for industry to get together with researchers, with regulators and see what can be done. What is practical, what will work. I don't have a hard and fast answer at this point.

MR. BENDER: Well, I'm not. I'm acting in my role as a consultant to the committee at the moment. It seems to me that if there's a recommendation that it might be worthwhile in giving some thought to, it's whether some illustrative kinds of analysis and design can be used to further develop these ideas. I don't think you can just throw them out on the table.

MR. von RIESEMANN: These can be prepared. Now I would like to -- as I mentioned in the beginning, these are my comments and with feedback from other people, they could be modified. But I'd like to see which is the best way to go.

MR. WARD: Thanks, Walt. What I'd like to do next is spend some time perhaps getting some of the feedback that Walt just alluded to, but I'd like to give the speakers a chance to add to their comments based on what they have heard throughout the day or perhaps to question some of the other speaker or amplify some of the things other speakers have said.

I think the most logical or one systematic way to do that is just to ask each of the speakers in turn if they have anything they would like to add along either of those lines.

Bill Snyder.

MR. CARROLL: I am unfamiliar with this concept of ACRS being systematic about it.

[Laughter.]

MR. WARD: Oh, okay. I'm sure we'll kind of follow it up sooner or later. Just give us a chance.

Bill, did you have anything you would like to add? I'd welcome you to come up, if you would.

MR. SNYDER: Not having been forewarned that I was going to have this opportunity, I suddenly find myself scrubbing back through my memory for a day of exposure and trying to put into context all of the aspects of it.

One thing that did strike me when Larry Minnick was talking about his concept for a pressure release system that I found conceptually no difference in that concept. If I take the concept inside the containment, I can accomplish the same thing interestingly enough by a standpipe in the suppression pool of the Mark III containment in which I vent the top of the pipe, the standpipe, through the top of the containment.

I can achieve the same function. I've internalized it. I may solve a lot of problems in construction and design.

I'd have to study the hydraulics of the system but it struck me that it could be brought inside containment -- because in your system your external pool, so to speak, in which the standpipe stands is in fact in pressure communication with the inside of the containment, so I can move that cell inside the containment and not change the pressure distribution.

I also do not change the hydraulics of the situation and so instead of having an external free standing pipe I simply take the top of the pipe out through the top of the containment shell, which has an interesting ramification in the sense that if you don't have the problems of maintaining temperature of water and bad climates and so forth if it is inside the containment.,

MR. CARROLL: Except you don't have the chimney effect of outside air available.

MR. SNYDER: Yes, the only thing you are missing is in fact the heat removal that you get from the standpipe being external but it was one of the observations that I made.

One of the other observations I would make is that, and Walt Von Riesemann made this point, and that is that we have a gap so to speak between those people who have done the research and have a perception of what the reactor safety research means to the safety of the nuclear power plant system, a gap between that group of people and those that in fact design the structures against a set of criteria which has a classical origin but in an industry which is different from the nuclear industry.

I am not a civil engineer but my general impression is that civil engineers don't design systems that -- and when driven beyond the elastic limit. In other words, they don't design to accommodate situations where they go into irreversible processes. I think we are talking about there are two totally different disciplines, two different attitudes and philosophies that somehow have to be brought together, I think, if we are going to optimize the design of future plants.

It will certainly be a requirement if we are going to make significant changes in the codes that are used to design future systems.

I certainly didn't have any reason to disagree with Bob Henry's observations on the ramifications of the research, having spent a long time in that field myself.

I do observe and maybe this was because I tended to look globally at the future in the sense of all possible nuclear steam supply systems and how they might be implemented in an operable design that most of the conversation here in the discussions pertained to light water reactors and I am of the opinion simply on practical matters that the first portion of the second generation of nuclear power in the U.S. is for many reasons going to be light water reactor based. It is not at all clear that the second phase of the second generation of nuclear power plants will be light water reactors. I think maybe the solution to this is rather than make a radical departure in basic concepts in requirements for containment between light water reactors now and the whole spectrum of possibilities in the second generation we might look at the next generation or the future generation of nuclear power plants is in two parts -- the logical evolution from what we presently have as well as setting in fact the basis for maybe more motivational criteria and more system overview that accommodates a large spectrum of nuclear steam supply systems in the future.

I am very practical about that in the sense that I think that if you try to define a single set of criteria for the future that encompasses light water reactors and in fact, say, modular high temperature gas reactors and liquid and metal reactors I think you may have created administratively almost an insurmountable hurdle in making that transition.

Those are all my comments.

MR. WARD: On the last point, is that not in fact what is happening, though, with the ALWR program is going ahead without consideration of the sort of thing we have been talking about today?

MR. SNYDER: What I find very interesting, I alluded to this in my talk in my discussions with a number of the nuclear steam supply system vendors for their next generation of plants, particularly the mid-size plants, is that I -- and this is a simplistic -- a simplification of what really is there but those designers are going a half step to what you might regard as sort of the ideal state in the next generation of plants but they are going to step beyond what in fact has been conventional practice, and even a half step beyond what in fact is sort of the mold in which the regulatory might try to force the design concept.

I think they already have made the directional change and they are acutely aware in taking that half step that they have created for themselves a series of questions when they go in for license and I think that is an interesting point.

I think if nothing more if the ACRS through the Commission can create the sort of a containment systems requirement that will be used as a reference in licensing the next generation of plants that they recognize that the industry has already made the half step.

MR. WARD: Thank you, Bill.

Paul, you have had the advantage of a few more minutes to collect your thoughts.

MR. NORTH: I, in fact, have no significant comments on the other presentations. I found no major points of disagreement with them.

As a result of one item of discussion that I've had in the mean time, I would like to amplify one point only in own presentation. That is that if we're looking at the wider application of nuclear energy in maybe some countries where there is some concern about the depth of the infrastructure that may exist -- and this is, in fact, in our own environmental best interest that this occur in the first half of the next century; then the Nuclear Regulatory Commission within the United States may have a perfectly legitimate interest in plants that are placed overseas in this sense -- those countries may, in fact, look to the United States for leadership, for standards, and, to some extent, for actual examination of plants.

In fact, we see this, for example, in the application of the combustion engineering plants to Korea. The fact that the US safety analysis was done on those plants was certainly a factor in the Koreans deciding to go with those plants. And, as a matter of fact, the Korean Government has come to us as a national laboratory and paid us to do independent analyses on those same things.

So, I think that's an important point that would be supportive in the long haul. That's the only thing I wanted to add.

MR. WARD: Let's see. Bob Henry.

MR. HENRY: I just had a couple of things, David. There's really two reasons you set about to even concern yourself with severe actions. The first is to find out how you can better prevent them, obviously, because that's where all your financial interest is; and the second is to figure out, God forbid you should ever have one, what do you do about it, because in 1975, WASH-1400 pretty well proved that the risk is low for nuclear power plants, and everything since that time says it's even lower than what they concluded at the time. So the real reason you look at it is to say, "If I have it, how can I recover from it?"

Well, this has some implications in what everybody should think about, I think, because it's true, as Walt said, and Adolph educated me in the past, about eight years ago, we do talk different languages when we get into these things.

We've got to be very careful about it, very careful, because, from my perspective of looking at severe accidents, I would encourage the people who do structural type calculations to think more in terms of thermal loads for severe accidents, not pressure loads, because you can think of recovery and mitigation. Recovery implies mitigation. Mitigation does not imply recovery.

So if we're dealing with relieving pressure, we have not yet dealt with the accident. That's why you have to think about that. Ninety-percent of what you ought to think about is how to recover from the accident, and, by that, do I also solve my pressure, quote, "loads."

So, I think, when we start evolving severe accidents into designs, most of the focus, 90 percent of it, should be on how do I provide the containment -- I gather you have to use best-estimate analysis because we're way down the pike towards the plant having a very bad day -- but how do I help the containment extract that decay heat, because if I don't do that, the accident is not over? Once I do that, then I no longer have to worry about further pressurization. If I deal only with pressure loads, then I still haven't solved the other part of the accident.

The reason I bring that up is both Adolph and Walt made the point, a very valid one, they need numbers to design to. You can't design to just very general philosophy.

I really believe that from a pressure point of view, the numbers they have now are very valid, and the fact that you go through these margins that Adolph listed here give you the confidence that it can tolerate a lot more things.

So, those numbers I find to be very helpful for the designer, very helpful for me to know that there are a lot of things, even in severe accidents, I can -- it gives me a lot of time, because to get -- Adolph showed -- Adolph, can I borrow that curve of displacement versus pressure? In essence, I tend to think of things as two asymptotes.

[Slide.]

MR. HENRY: In one case, we have -- this is pressurization, so the pressure is increasing, and from here out, pressure stays constant and displacement changes. To get to this point for the containment that Adolph was talking about is maybe 30 hours. At this point now, it takes a long time to generate all this, so this is maybe another 20 hours to actually generate the gas, if it's gas, and if you're talking the concrete, not steam. So, in essence, you have to look at that really as it's margin, it's time for me to recover because I somehow have to eventually get water into the system.

So we're talking about a very long time here, so there's a lot of things that one needs to then trade off. Where do I really put my resources? We're not going to solve it here, but I want you to think about that in terms of I think the way in which the numbers are constructed now for pressure loads is very valid, has a lot of margin in it, people are used to it, they know how to apply it, they know what it means.

The thing to start thinking about is thermal loads. You want to make sure that when you have a severe accident, that you don't jeopardize this in any way, like the liner which is your -- your sealing capability is jeopardized because corium comes out of the containment.

Let's see. I have one other -- ah, yes. Walt made a very valid point about the ILRT in the neck. But, again --

MR. CARROLL: It is also a fraud, by the way, because I think a lot of people take great comfort in the fact that, hey, everybody meets the test. But I don't think anybody knows what's behind it unless you've been there.

MR. HENRY: That's right.

MR. BENDER: What does ILRT stand for?

MR. CARROLL: Integrated Leak Rate Test, ILRT.

MR. BENDER: Thank you.

MR. HENRY: You do all this pretesting to make sure you don't flunk the test, you know where to put the putty, and everything.

[Laughter.]

MR. HENRY: Let me give you a suggestion of how we go about changing it, because while it is applied somewhat different than most people conceive, it also is a very tough thing to discuss in public because it's a protection.

One of the ways I would suggest that we start dealing with that is to again try to force the source term to be more realistic, because that's chemistry, and you don't have to have full-scale test for it, etcetera, because I think the first thing to do is first show that you have a much reduced source term, and then, with the leak rate that we have -- gee, look what the releases are at the site boundary: extremely small, orders of magnitude less than the design basis.

When we start doing that, then you perhaps have the leverage to take a step back to say, from a, again, probabilistic point of view, is it really necessary to do this every five -- is it still every five years, because it is -- it's -- as I recall, Zion by itself takes two days just to pump up the containment. So it's a very expensive test because it requires a lot of time.

I would recommend that we deal first from the source term point of view, which says, "Look, with the tested leak rate that we have, these are the releases of the site boundary -- extremely low compared to what was considered the design basis," and then perhaps move to somewhat relaxing the requirements for the leak rate test itself.

That's all I have, Dave.

MR. WARD: Thanks, Bob. Mike, you were next.

MR. BENDER: I'm not sure that I can add a lot to what's been said. The thing that strikes me first of all is that there's fairly good agreement in here that a systems approach is needed and it seems to me that ought to be emphasized. We tend to look at these parts of the containment as individual pieces when they need to be collected together. I think that ought to be emphasized more.

Secondly, I'm not comfortable, as I pointed out in my earlier discussion, that I understand how bad the accidents can really be. I don't expect to have an ATWS and a BWR and I don't expect to have a burst pressure vessel. But when I'm looking at the question of what the severe accident limits are, I feel like there's a need to really look at those accidents.

I think that has been submerged in worrying about the detail of accident mechanics, associated about the very benign circumstances associated with a slow core melt. I would suggest that that be looked at.

The third point that I would like to emphasize is one that's also been made a number of times here, and that's the time versus load conditions. That will tell us a lot about how long the structures are subjected to both temperature and pressure loadings, and it might tell us too about what kinds of load combinations have to be considered.

So far, the ASME Code has served us well as a design tool. But it's pretty abstract, and the basis for it is not very closely related to severe accidents. My intuitive feeling is that it has set the designs into areas where the expenditure of resources might better have been handled in a less generous way, and whatever was not spent there could have been spent on something else. Particularly, I think, on the question of internal structures and how they might influence the containments has been distorted by the LOCA question. That could be straightened out in a reexamination of the criteria.

The last point I'd like to make has to do with the gas-cooled reactors and their future. I don't pretend not to be biased in favor of gas cooled reactors. I've worked on them a long time and for the most part, I found their sins are more a matter of distorted interpretation than serious safety problems.

But the one thing that does stand out is that they contain a lot of very hot graphite, very much like the situation that existed at Chernobyl. So the question of what kind of failures can be tolerated needs to be examined more carefully. The problem might be very serious, but so far the basis for accepted gas-cooled reactors has been the failure in the pressure vessel can't be larger than a certain amount, and that didn't change when the containment was put around the concepts. So the whole thing needs to be looked at a little bit more carefully.

That's far enough, as far as I'm concerned, for a discussion like this. I think many of these points could be elaborated on in future discussions.

MR. WARD: Okay. Mike -- Larry, would you like to add something?

MR. MINNICK: I have nothing to add.

MR. WARD: Okay, Adolph?

[Slide.]

MR. WALSER: In response to Henry's remark about removing temperature out of the containment, I have here a slide, a cross section that shows an advanced light water PWR, which has these water tanks up here, which are designed to sprinkle from the outside of the steel shell containment, and which then is vented through the chimney, to get rid of the steam and although I'm very much -- my background is all concrete and especially pre-stressed concrete, but you can take this tremendous advantage way from a steel containment.

MR. CARROLL: Well, until molten corium gets up next to the liner?

MR. WALSER: Well, but that is a matter of construction provisions. You just don't let the corium, which is supposed to fall into this cavity, get anywhere near the outer steel shell. If you need more than the four or five feet which you have here, you make it bigger, and that doesn't change almost anything. It doesn't change much.

The other remark I have has to do with the ASME Code. I think it is a -- it's a good code. It has its limitations. It responds to the task which the Committee had on hand, both for steel and for concrete containments. Now, as I said, it has its down -- drawbacks. It's a deterministic code, and the materials are all limited to basically behave in the elastic range.

For the low probability loads, that is really conservative, and to develop an ASME Code for -- on a probabilistic basis, going beyond the elastic range will be quite a task because ASME, especially Section 3, Division 1, doesn't recognize strain and that is going to be a big step to talk to them about strain.

MR. BENDER: For the fast -- the metal-cooled reactor systems, a fair amount of work has been done on strain as a structural criterion, and I think something could be drawn from that. But it is going to be difficult, I agree.

MR. WALSER: Yes.

MR. WARD: May I?

MR. von RIESEMANN: You're next.

MR. WARD: Well, I want to interrupt you number one. You're referring to Code Case N-47 for the breeder work, but you see again they were pressed to come up with something, because a breeder was there. Right now there's no pressure, no pun intended here if you will, for the Code to change. I think a little push has to occur.

There's an inertia if you will; there's no -- very little activity in the industry, and I'd rather see it done, as I said, in the cool of day then a crisis basis, where in fact the N-47 was almost done that way. The Code has served us well, in the NRC guidelines and all the work and the industry. But I think now it's time to look at maybe making some improvements in the Code, and doing that with industry, if you will, research and all together.

I sort of fell into this now. Some other comments, I guess. I think I was surprised, if you will. Coming to the meeting, I thought there would be a greater difference of opinion in the topics that were addressed. There was a diversity of topics, but yet a common threat through them all. There seems to be agreement. There's some small points that we might have to talk about, but yet overall there's an agreement.

There's also a warning raised by Mr. Snyder and started throughout the day. You have to be careful about talking of requirements and saying they're for containments per se. We have to be concerned about what type of reactor system we have. Obviously the emphasis has been on light water reactors.

I think I feel that we should start working on the other type of reactors, again because they take time to develop the criteria and it's something new. We just shouldn't let them sit in the background. Finally, I guess I would recommend that after the ACRS has time to digest this material in the meeting in Washington, that perhaps they consider a workshop, maybe of the three groups that have been participating in others, to discuss this in greater detail.

Thank you, Walt. I'd like to give -- I don't want to prolong this too long, since it looks like we can get away a little early. I'd like to give others in the audience a chance to say something, in particular, the members of the NRC staff here.

Mr. Bagchi or Mr. Hardin, do you have anything you'd like to comment on?

MR. BAGCHI: I think I agree with all the other speakers who talked about general design philosophies. Particularly, I felt that Mr. Bender's remarks about having some controlled way of venting the containment and knowing where the containment might fail and having some control over that is very important.

If we are going to have a design philosophy on future reactors, that's something we ought to seriously look at. Several points that Walt made; they all made sense. We ought to have inspectability. We need prolonged life for future reactors. Somebody mentioned 8200 years. Particularly for those kinds of ranges of life, we need assurance that no corrosion or other kinds of degrading influences would compromise the containment function.

I also feel that a workshop of the nature that Walt proposed would probably be the best compromise to come up with a set of criteria for future reactors. All of the advanced reactor designs are here, at least for the NRC staff, for approval and review, and I think it's very important to have a set of general criteria that everybody could work on agree upon.

Based on today's discussions, I feel there is a lot of things in common, moreso than I had thought possible before I came to the meeting. That's all I have to say. Thank you.

MR. WARD: Okay, thank you. Brad, would you like to say something?

MR. HARDIN: I'd like to just make a couple of comments about some things that came up during the discussions today to make some observations about some things that are happening in the staff.

One of the interesting questions that I think comes up when we start talking about criteria for new plants, is how far do we have to go? We've had a number of discussions on that within the staff.

In particular, the Commissioners asked us to answer a question that was raised by Commissioner Rogers about whether or not we were going to have to change the adequate level of protection for severe accidents. We agonized over that for a long time, and in writing that paper and in discussing it, we were making reference to the severe accident policy statement that did indicate that it was expected that there would be improvements in safety standards in future plants.

We presume that includes containments as well as the other parts of the nuclear system. In looking at that, and I guess Dave, you raised the question about this rising standard as time goes by. It appears that the industry has already offered us significant improvements in the designs that we're seeing the evolutionary designs.

Aside from the question whether we believe that we must require some improvements, we're seeing that those are already being offered. I guess I'd just offer a thought that we believe that as technology improves, that we would expect that we would be seeing improvements and rising standards anyway.

Whether we feel that we have to require that or not, it appears that it's going to happen. This has been something important for us to recognize in our review of the evolutionary designs. I guess another thought on that same aspect is; in looking at the evolutionary designs, we believe that there's been enough information and knowledge gained about those types of designs, both PWRS and BWRs, that we feel that we know pretty well what improvements can be made.

We feel we understand the technology that's available and if there are things that might be identified as weak areas from PRA studies, that those have been identified fairly well and that if you get into a position where a designer is offering a design and there is some feature that we're aware of that they are not offering to us, we believe the cost-benefit is an appropriate consideration to make if, indeed, we're satisfied that they have met our basic criteria.

As you know, we have been looking at what kind of criteria that we should use in reviewing evolutionary designs and there is an attempt being made to use the safety goals as we understand them at this point in order to have something, because we have to have something now. We're faced with making decisions.

There was a comment -- Bob Henry asked me during the break about the status of the Source Term. It may not be known to you all yet, but you're going to be hearing about this soon, I'm sure, if you haven't; that after a long period of time, not having really put much effort into Source Term improvements by the staff -- and I guess, as far as I can tell, this is due to lack of resources -- in the last few months, there has been a concerted effort made to address the known problems with the present Source Term regulations, 10CFR-100.

There is a paper that's being drafted

MR. CARROLL: Footnote, not regulation.

MR. HARDIN: Okay. There is a paper that is in draft form that's been requested by the Commissioners. Len Sofer in the Office of Research, is the principal author. I don't think it's been through complete review yet, but there has been some recent efforts to look at the very things that I think Bob has raised today, so I'm sure we're going to hear more about that in the near future.

One comment that I'd like to make is that when the staff has been faced with looking at the question of how to define criteria for the future plants, we have iterated back and forth over the last couple of years on this, and right now, we are aiming towards having some very general criteria which we hope will be applicable, not just to the evolutionary designs, but will be broad enough and will be in the form of something like general design criteria that will be useful for other designs as well, like the passive designs.

We haven't done much thinking yet about the HTGR and the liquid metal, but it appears that it would be possible to write some criteria that would be something like in the form of general design criteria, stating broad objectives. To implement that, we are writing a reg guide, which we're going to have drafted sometime toward the end of this year, and it's going to deal with all of the same issues that we've been talking about here.

Our aim is to try to be very cognizant of the direction that the ACRS is going in, to try to factor that into our reg guide so hopefully we'll have something that's consistent with whatever direction that you're going in. We have plans to propose to you a process for your reviewing that with us probably early next year.

Right now, just to finally make a statement about that, we do intend that it would have fairly general, generic criteria in the major text and then to address plant-specific concerns, we would probably have appendices. There may be an appendix for light water evolutionary designs; maybe an appendix for the passive designs and then as we learn more about the HTGR and the liquid metal, to add appendices for that, too, so that we have something that we can kind of keep up to date as we're learning here.

I guess one last thing here: on the disagreement between the 75 percent active cladding and 100 percent as far as the hydrogen generation, we seem to be at kind of a standstill on that, and yet there may be some further information coming along. I just wanted to comment that basically the reason the staff is stuck on a hundred percent now is that we haven't been able to accept the industry's position on the coolability of the debris, ex vessel.

The hundred percent is being viewed as a stat by the staff as a surrogate to allow some additional hydrogen generation for the ex vessel. My understanding is that the industry has done some more calculations on that which may indicate what quantity of additional hydrogen generation over what would be generated in vessel might be expected.

We haven't seen those yet as far as I know, but I think we're open to looking at other information and also there's some experiments being done in Argon right now, which I understand there are some early results in the last month or so on that. I don't know if there's anybody here who knows anything more about that.

Those experiments could be very useful to resolve questions like what the effects of crust on top of the debris and the coolability when you have water standing over the debris, so we're looking to that to hopefully resolve some of these differences we have on the 75 percent/100 percent.

MR. WARD: Thank you, Brad. On the issue of rising expectations, I guess -- And there is a considerable difference of opinion on whether it is appropriate for the NRC's regulations to chase the improvements that are possible or gained by the industry for reasons of investment protection or something else. And that is why I think in some of the discussions, for example, of the safety goal, I think there has been kind of a smearing or overlapping or something of goals that are appropriate for the industry to have and goals that are appropriate for the regulator to insist upon. So somehow I think those things need to be kept more separate than they have been kept. But we'll see what happens.

Before we finish, do any of the other members have something they would like to say at this time?

MR. CARROLL: I thought it was a very useful and productive discussion today. I learned a lot.

MR. WARD: Okay. And I would like to thank all of you who participated. I agree. It was very useful and I hope something somewhat useful will come out of all this. Thank you very much.

[Whereupon, at 3:55 p.m., the meeting was adjourned.]

REPORTER'S CERTIFICATE

This is to certify that the attached proceedings before the United States Nuclear Regulatory Commission

in the matter of:

NAME OF PROCEEDING: ADVISORY COMMITTE ON REACTOR SAFEGUARDS

DOCKET NUMBER:

PLACE OF PROCEEDING: Chicago, Illinois

were held as herein appears, and that this is the original transcript thereof for the file of the United States Nuclear Regulatory Commission taken by me and thereafter reduced to typewriting by me or under the direction of the court reporting company, and that the transcript is a true and accurate record of the foregoing proceedings.

Marilyn Nation

MARILYNN NATIONS
Official Reporter
Ann Riley & Associates, Ltd.

T-1 ①

INTRODUCTORY STATEMENT BY THE JOINT CONTAINMENT SYSTEMS/STRUCTURAL ENGINEERING SUBCOMMITTEES - CHAIRMEN'S REPORT

OCTOBER 17, 1989
ROSEMONT, ILLINOIS

The meeting will now come to order. This is a joint meeting of the Advisory Committee on Reactor Safeguards Subcommittees on Containment Systems/Structural Engineering.

I am D. Ward, Subcommittee Chairman of Containment Systems.

Not Here → C. Siess is Subcommittee Chairman for Structural Engineering.

The ACRS Members in attendance are: J. Carroll and C. Wylie.
ACRS Consultant - M. Bender

The purpose of this meeting is to discuss containment design criteria for future plants with invited speakers from the nuclear industry and national laboratories.

Mr. D. Houston is the cognizant ACRS Staff Member for this meeting.

The rules for participation in today's meeting have been announced as part of the notice of this meeting previously published in the Federal Register on September 22, 1989 (FR39067).

A transcript of the meeting is being kept and will be made available as stated in the Federal Register Notice. It is requested that each speaker first identify himself or herself and speak with sufficient clarity and volume so that he or she can be readily heard.

We have received no written comments or requests to make oral statements from members of the public.

(Chairman's Comments - if any)

We will proceed with the meeting and I call upon Bill Snyder of SNL to begin.

T-

Presentation
to
ACRS Joint Subcommittees' Meeting
on
Containment Systems and Structural Engineering

A. Wm. Snyder
Sandia National Laboratories

October 17, 1989

AWS: 10/15/89

T1 ①

Containment System Design Criteria for Future Generations of U. S. Nuclear Power Plants

A difficult challenge, given:

- the variety of candidate NSSS and plant concepts
- the bias of the legacy being limited to the LWR experience
    - the investment in making a success of the concepts and designs of current plants
    - current institutionalization of the U. S. nuclear power enterprise
    - the sharply focussed attention being given to
        - the understanding of severe accidents
        - the predictions of threats to and the response of contemporary containments

T-2②

(1 of 3)

The Concept and Design Legacy
Vis-a-Vis
An Alternative Future Design Agenda

The Concept and Design Legacy:

- design approach
  - NSSS; predominantly "bottom up"
  - Balance-of-Plant; predominantly "top down"
- safety systems; mostly additions/auxiliaries to the base plant
- the containment building, last barrier of the multiple defenses-in-depth, designed to withstand a surrogate (DBA) for all plausible accidents
- the multiple sequential barriers of the defenses-in-depth susceptible to common cause and interdependent failures

T-30

(2 of 3)

The Concept and Design Legacy
Vis-a-Vis
An Alternative Future Design Agenda

An Alternative Future Design Approach

- NSSS & BOP; both designs mostly "bottom up
- no distinctions between safety, safety-related, and non-safety systems

T-3 ④

(3 of 3)

The Concept and Design Legacy
Vis-a-Vis
An Alternative Future Design Agenda

An Alternative Future Design Approach (continuing)

- **Total Performance Management (TPM)**

  Total - complete plant system; over the full projected plant life

  - **optimization of the performance of the complete plant system to all vital performance success indices (safety, economics, etc.)**
  - **include in design, full objective consideration of both deterministic and probabilistic events and their costs**
  - **excellence keyed to plant system reliabilities as metrics of quality attained in design,operations, maintenance, and management.**

T-3

Translation to the objectives of safety, the "language" of containment and containment systems, and the definition of design and performance criteria w/r/t internal events

- Retain the cardinal concept of multiple barriers to attain safety-in-depth
- Define multiple reliability criteria as indices of successful performance for each of the multiple barriers to attain safety-thru-quality, e.g.,
  - the reliability of a barrier to withstand successfully credible threats from credible internal initiators
  - the reliability of the collective internal systems that credibly, thru failure and malfunction, could initiate a threat to the barrier
- Define a total plant system reliability criterion as an index of successful performance of the composite containment function

T-6

Idaho
National
Engineering
Laboratory

COMMENTS ON CONTAINMENT DESIGN CRITERIA FOR FUTURE NUCLEAR PLANTS

P. NORTH

T-6

FORM OF DISCUSSION

- PHILOSOPHICAL FOUNDATION FOR AN APPROACH
- IMPORTANT CONDITIONS IN DEFINING THE APPROACH
- THE ACTUAL APPROACH AND RELATED METHODS

T26

PHILOSOPHICAL FOUNDATION

- SERVICE TO SOCIETY - PROTECT THE HEALTH AND SAFETY OF THE PUBLIC IN THE CONTEXT OF NUCLEAR ENERGY AS A CONTRIBUTING COMPONENT OF OUR ENERGY SUPPLY SYSTEM

- PROSPECTIVE NUCLEAR ENERGY CONTRIBUTION IN THE UNITED STATES
    - CURRENT DIMINUTION OF ELECTRIC GENERATION RESERVE CAPACITY
    - ENVIRONMENTAL CONCERNS
    - POSSIBLE ADDITIONAL USES (PROCESS HEAT, OTHER FORMS OF ENERGY SUBSTITUTION)
    - POTENTIAL FOR INCREASED USE OF NUCLEAR ENERGY WITHIN THE UNITED STATES

T-6

WORLD ENERGY PICTURE

- LARGE ENERGY CONSUMPTION GROWTH PROJECTED BY WORLD ENERGY STUDIES
- LARGEST GROWTH IN AREAS WITH LOWER CURRENT PER CAPITA ENERGY CONSUMPTION THAN IN THE UNITED STATES
- GLOBAL ENVIRONMENTAL CONCERNS IF THIS GROWTH IS PROVIDED BY FOSSIL FUEL BURNING
- INDICATIONS THAT UNITED STATES, EUROPEAN AND JAPANESE NUCLEAR INDUSTRIES WILL SEEK TO SERVE THIS GLOBAL ENERGY MARKET.

CONCLUSION - WE MUST ADDRESS THE POSSIBILITY OF MUCH WIDER USE OF NUCLEAR ENERGY THAN IS EVIDENT TODAY AND IN A MUCH BROADER GEOGRAPHIC AND SOCIETAL SETTING

T-6

AN IMPORTANT PARAMETER

LARGE NUMBERS OF PEOPLE MUST SUPPORT THE USE OF NUCLEAR ENERGY (AS INDIVIDUALS AND THROUGH INSTITUTIONS) IF IT IS TO MAKE AN APPROPRIATE CONTRIBUTION TO OUR ENERGY SYSTEMS

T-6

A RESULTANT FOUNDATION

- CONTAINMENT CRITERIA LINKED TO CLEAR PROTECTIVE (REGULATORY) OBJECTIVES FORMULATED ON THE BASIS OF WIDE APPLICATION OF NUCLEAR ENERGY WITHIN THE UNITED STATES AND IN THE WORLD AT LARGE
- CONTAINMENT CRITERIA THAT ALLOW PROGRESSIVE DESIGN INNOVATION IN MEETING THE PROTECTIVE OBJECTIVES
- AN APPROACH BASED ON RISING STANDARDS OF ADEQUACY FROM DESIGN GENERATION TO DESIGN GENERATION
- AN APPROACH AND RELATED METHODS THAT PROVIDE THE BASIS FOR STRONG SUPPORT OF NUCLEAR ENERGY BY LARGE NUMBERS OF PEOPLE

A POSSIBLE SHORT TERM OVERRIDE

THE POSSIBILITY OF A JUDGMENT BY THE COMMISSION THAT A TRADITIONAL CONTAINMENT STRUCTURE IS <u>NECESSARY</u> TO ENSURE SUPPORT FOR FURTHER USE OF NUCLEAR ENERGY, REGARDLESS OF REACTOR SYSTEM DESIGN

- DISAPPOINTING (TECHNICALLY)
- UNDERSTANDABLE (AS A JUDGMENT)
- IN THIS DISCUSSION, PROCEED ON THE BASIS OF NO SUCH JUDGMENT

I-1

DEFINING THE APPROACH

RELATED CONDITIONS

- SOUND ENGINEERING APPROACH
  - BEST ESTIMATE, MECHANISTIC ANALYSES
  - SUPPORTED BY ADEQUATE PHYSICAL UNDERSTANDING
  - "FACTORS OF SAFETY" ADDED EXPLICITLY

- THIS APPROACH CAN BE UNDERSTANDABLE AND CONVINCING TO PEOPLE NOT INVOLVED IN THE WORK AND IS THEREFORE CONDUCIVE TO THE GENERATION OF SUPPORT

T-2

DEFINING THE APPROACH

RELATED CONDITIONS

- THE NEW SYSTEMS SHOULD DEMONSTRATE ROBUSTNESS IN ACHIEVING THE CONTAINMENT FUNCTION
    - USE OF BASIC PHYSICAL CHARACTERISTICS
    - FAULT TOLERANCE
    - CAREFUL IMPLEMENTATION OF DEFENSE IN DEPTH WITH INDEPENDENT MULTIPLE LAYERS, EFFECTIVE FOR THE ENTIRE ACCIDENT SPECTRUM
    - ABSENCE OF THE POSSIBILITY OF BYPASS
- BALANCE BETWEEN PREVENTION AND MITIGATION (THERE WILL ALWAYS BE RESIDUAL UNCERTAINTY IN PREVENTION)

DEFINING THE APPROACH

T-7

RELATED CONDITIONS

- POSSIBILITY OF LONGER PLANT LIFETIMES (80 TO 100 YEARS)
  - ORIGINALLY REMOTE LOCATIONS MAY BECOME MORE POPULATED
  - IT WILL NOT BE A SERVICE TO SOCIETY TO LIMIT LAND DEVELOPMENT POSSIBILITIES
- WITH INCREASING "NUCLEAR FLEET" APPROACHES THAT ALLOW EVEN THE REMOTE POSSIBILITY OF FARMLAND WITHDRAWAL AND CLOSURE OF NEIGHBORHOODS (CHERNOBYL) WILL BE INCREASINGLY UNACCEPTABLE TO SOCIETY
- BOTH OF THESE FACTORS MILITATE FOR AN APPROACH THAT CONCENTRATES ON THE CHARACTERISTICS OF THE PLANT ITSELF AND DOES NOT RELY ON EXTERNAL RESPONSES BY THE REST OF SOCIETY

APPROACH AND RELATED METHODS

T07

FOUNDATION ELEMENT - RISING STANDARDS OF ADEQUACY

- CONSISTENT WITH THE ADVANCED REACTOR POLICY STATEMENT
- LEVELS OF "ADVANCED DESIGNS" SHOULD BE RECOGNIZED AND APPROACHES DEFINED ACCORDINGLY
  - DESIGNS THAT ARE A LOGICAL EVOLUTIONARY STEP FROM OPERATING LWRS - BUILD FROM EXISTING RULES; DEMONSTRATE COMPLIANCE WITH SEVERE ACCIDENT POLICY; DEMONSTRATE IMPROVED FISSION PRODUCT RETENTION; COUPLE WITH FEATURES SUCH AS LONG TRANSIENT TIME; DESIGN TO TIGHTER PROTECTIVE OBJECTIVES
  - DESIGNS THAT REPRESENT A GREATER DEVELOPMENT STEP AND ARE AIMED AT LATER DEPLOYMENT - USE MORE PERFORMANCE RELATED CRITERIA TO ALLOW DESIGN INNOVATION; ESTABLISH EVEN TIGHTER PROTECTIVE OBJECTIVES

APPROACH AND RELATED METHODS

I-8

FOUNDATION ELEMENT - CONTAINMENT CRITERIA RELATED TO PROTECTIVE OBJECTIVES

- NEAR TERM ADVANCED LIGHT WATER REACTORS

  CORE DAMAGE FREQUENCY $\leq 1 \times 10^{-5}$ PER YEAR SITE BOUNDARY WHOLE BODY DOSE LESS THAN 25 REM FOR ACCIDENTS WHOSE CUMULATIVE FREQUENCY EXCEEDS 1×10^{-6} PER YEAR

- LONGER TERM OBJECTIVES

  GO BEYOND <u>CONSIDERATION</u> OF NO OFFSITE EMERGENCY PLANNING REQUIREMENT AND MAKE THIS CONDITION A SPECIFIC DESIGN OBJECTIVE

APPROACH AND RELATED METHODS

FOUNDATION ELEMENT - CONTAINMENT CRITERIA RELATED TO PROTECTIVE OBJECTIVES

T-8

- LONGER TERM OBJECTIVES (CONTINUED)
  - INTERMEDIATE CRITERIA:
    - LOWER LEVEL EPA-PAGs NOT EXCEEDED AT SITE BOUNDARY WITHIN THE FIRST 36 HOURS OF ECI, ECII AND ECIII EVENTS

    AND

    - PRA INDICATES A CUMULATIVE MEAN VALUE FREQUENCY OF EXCEEDING THE LOWER LEVEL PAGs AT THE SITE BOUNDARY WITHIN THE FIRST 36 HOURS DOES NOT EXCEED APPROXIMATELY 10^{-6} PER YEAR (REF. SECY-88-203)
  - POSSIBLE LONG TERM CRITERION:
    - LOWER LEVEL EPA-PAGs NEVER EXCEEDED AT THE SITE BOUNDARY BY ANY CREDIBLE ACCIDENT CONDITION

APPROACH AND RELATED METHODS

T-8

FOUNDATION ELEMENT - CONTAINMENT CRITERIA RELATED TO PROTECTIVE OBJECTIVES

- LONGER TERM OBJECTIVES (CONTINUED)

  ALSO ESTABLISH DESIGN OBJECTIVES THAT PROTECT THE ENVIRONMENT SO AS TO ELIMINATE LAND REMOVAL OR USE RESTRICTION

  - COULD BE A RESIDUAL ACTIVITY ON THE LAND OR A LIMITED COST TO RESTORE
  - NEEDS FURTHER DEVELOPMENT

APPROACH AND RELATED METHODS

J-8

FOUNDATION ELEMENT - PROGRESSIVE DESIGN INNOVATION

- BEST ESTIMATE ANALYSIS WITH EXPLICIT SAFETY FACTORS
  - MECHANISTIC ANALYSIS
  - INCLUDING SOURCE TERMS
- DEMONSTRATED PHYSICAL BASIS
  - R&D OR PROTOTYPE TESTING
  - VALIDATE ANALYSIS TOOLS OVER WIDE RANGE
  - PROVIDE CONFIDENCE IN RESULTING ANALYSIS
- RISING STANDARDS OF ANALYTICAL CAPABILITY
  - TIGHTER PROTECTIVE OBJECTIVES IMPLY THE NEED FOR REDUCED ANALYTICAL UNCERTAINTY

APPROACH AND RELATED METHODS

T-8

FOUNDATION ELEMENT - PROGRESSIVE DESIGN INNOVATION

- DEMONSTRATION OF MULTIPLE INDEPENDENT BARRIERS TO RADIATION RELEASE

- DEMONSTRATION OF FISSION PRODUCT RETENTION
  - POSSIBILITY OF FULL SCALE PROTOTYPE TESTING
  - CLEAR DEMONSTRATION - ANALYTICAL VALIDATION
  - DEMONSTRATE FAULT TOLERANCE
  - EXPERIENCE SUPPORTS THE APPROACH - ECCS METHODS
    - SHUTTLE ACCIDENT
  - CONSISTENT WITH THE OBJECTIVES OF STANDARDIZATION

APPROACH AND RELATED METHODS

T-9

FOUNDATION ELEMENT - PROGRESSIVE DESIGN INNOVATION

- ASSURE THAT NEW SYSTEMS ARE BUILT, OPERATED AND MAINTAINED WITH APPROPRIATE SAFETY LEVELS
  - DEMONSTRATE PROGRESSIVELY REDUCED SENSITIVITY OF RISK TO THE LEVEL OF EXCELLENCE IN OPERATIONS AND MAINTENANCE (WIDE GEOGRAPHIC APPLICATION)
- PROTECTION FROM SABOTAGE STRONG AND DESIGNED IN (NOT DEPENDENT ON STABLE PROTECTIVE FORCES)

APPROACH AND RELATED METHODS

T-9

FOUNDATION ELEMENT - PROGRESSIVE DESIGN INNOVATION

- ENSURE NO CORE MELT ACCIDENTS, POSITIVE REACTIVITY FEEDBACK OR OTHER ACCIDENTS WITH A POTENTIAL FOR LARGE RADIATION RELEASE IN ECI, ECII OR ECIII SPECTRA (REF. SECY-88-203)

- THESE ELEMENTS NOT ONLY ALLOW PROGRESSIVE DESIGN FLEXIBILITY BUT ALSO PROVIDE THE BASIS FOR STRONG SOCIETAL <u>SUPPORT</u> FOR NUCLEAR ENERGY APPLICATION

T-10

CRITERION: CONTAIN THE ENERGY STORED IN THE PRIMARY SYSTEM

(Large Dry Containment)

- 1000 MWe PWR primary system inventory.
  Volume ~ 12,000 ft^3 = 340 m^3
  T_{AV} ~ 570F (300C)
  Mass = 24,000 kg at h = 1.34 x 10^6 J/kg = 3.2 x 10^{11}
  Plus stored heat in fuel (5 full power secs) = 1.65 x 10^{10} J
  Specific energy = 1.41 x 10^6 J/kg
  Assume PP_{ST} = 0.3 MPa in containment or a total pressure of 0.4 MPa (~ 60 psia)
  Steam mass fraction after blowdown = 0.39
  Steam mass = 94,200 kg, $_{ST}$ = 1.65
  Volume required = 57,100 m^3
  (2,000,000 ft^3)

I-10

CRITERION: CONTAIN THE ENERGY STORED IN THE PRIMARY SYSTEM

(PRESSURE SUPPRESSION CONTAINMENT)

. 1000 MWE BWR PRIMARY SYSTEM INVENTORY.
VOLUME OF WATER ~ 300 m^3 - 220,000 KG
VOLUME OF STEAM ~ 300 m^3 11,100 KG
STORED ENERGY IN FUEL ~ 1.7×10^{10} J
TOTAL ENERGY = 3.3×10^{11} J

. SUPPRESSION POOL TEMPERATURE RISE ~ 70C.
$M_W = 1.2 \times 10^6$ KG
VOLUME = 1250 m^3 = 44,000 ft^3

. WETWELL GAS VOLUME MUST BE CAPABLE OF RECEIVING ALL THE NONCONDENSABLES IN THE DRYWELL, I.E. FOR A DESIGN PRESSURE OF 0.4 MPA (~ 60 PSIA) THE WETWELL NEEDS TO BE ONE-HALF THE VOLUME OF THE DRYWELL.

T-10

BASIC CRITERIA FOR ACCIDENT CONDITIONS

1. CONTAIN FISSION PRODUCTS RELEASED FROM THE FUEL AND THE PRIMARY SYSTEM (FIRST AND SECOND BARRIERS).
2. PASSIVELY CONTAIN (ACCOMMODATE) THE ENERGY STORED IN THE PRIMARY SYSTEM COOLANT AND FUEL AT NORMAL OPERATING CONDITIONS. (LARGE LOCA IS A WAY OF CONCEPTUALIZING THIS REQUIREMENT.)
3. REMOVE DECAY HEAT OVER THE LONG TERM.

TWO DIFFERENT CONCEPTS
USED IN THE U.S.

- LARGE DRY CONTAINMENTS.
- PRESSURE SUPPRESSION CONTAINMENTS.

T-10

SHOULD THE CONTAINMENT DESIGN CRITERIA BE ALTERED?

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ACRS SUBCOMMITTEE MEETING

CHICAGO, ILLINOIS
OCTOBER 17, 1989

(T-10)

CRITERION: CONTAIN THE ENERGY STORED IN THE PRIMARY SYSTEM

- THE PREVIOUS CALCULATIONS ARE ONLY APPROXIMATE TO ILLUSTRATE THE SIZES NECESSARY TO SATISFY THE CRITERION.

- OTHER ASPECTS NEED TO BE CONSIDERED, PARTICULARLY THOSE ASSOCIATED WITH NORMAL OPERATION.

- CONCLUSION - THIS CRITERION FOR CURRENT PLANTS:
  - IS ENVELOPING.
  - IS WELL CONCEIVED.
  - SHOULD BE RETAINED FOR FUTURE PLANTS.

T-10

CRITERION: CONTAIN THE ENERGY STORED IN THE PRIMARY SYSTEM

(PRESSURE SUPPRESSION CONTAINMENT)

(CONTINUED)

- NECESSARY DRYWELL VOLUME FOR WORKING CONDITIONS ~ 6000 M^3 (211,000 FT^3).
- WETWELL GAS VOLUME ~ 3000 M^3.
- CONTAINMENT TOTAL VOLUME 10,250 M^3 (360,000 FT^3).

T-11

CRITERION: CONTAIN FISSION PRODUCTS RELEASED FROM THE FUEL AND PRIMARY SYSTEM

- FOR THE TWO CONCEPTS USED IN THE U.S., THE CONTAINMENT COULD POTENTIALLY PRESSURIZE FOR TENS OF MINUTES OR LONGER DURING A SEVERE ACCIDENT.
- TO SATISFY THE CRITERION, THE CONTAINMENT MUST HAVE AN INTEGRAL STEEL LINER.

T-11

EXPERIENCE FROM REACTOR ACCIDENTS

(FRACTIONAL RELEASE OF NOBLE GASES IODINE AND CESIUM FROM BARRIERS)

	FUEL/ CLADDING	PRESSURE VESSEL(1)	CONTAINMENT(2)
TMI-2			
NOBLE GASES	0.8-1.0	0.8-1.0	ESSENTIALLY 0
IODINE	0.5-0.8	0.5-0.8	>> 0.01
CESIUM	0.5-0.8	0.5-0.8	ESSENTIALLY 0
CHERNOBYL-4			
NOBLE GASES	1.0	1.0	1.0(3)(4)
IODINE	?	?	0.2
CESIUM	?	?	0.1

1. RELEASE THROUGH THE IN-CORE INSTRUMENT TUBES AND THE PORV.
2. RELEASES FROM CONTAINMENT WERE THROUGH WATER PATHS TO THE AUXILIARY BUILDING.
3. FROM THE USSR REPORT TO IAEA (AUGUST, 1986).
4. THE RBMK-1000 CONFINEMENT/CONTAINMENT SYSTEM CAN ONLY CONTAIN THE STORED ENERGY IN THE PRIMARY SYSTEM FOR A SPECIFIC SET OF ACCIDENTS.

I-11

CRITERION: CONTAIN FISSION PRODUCTS RELEASED FROM THE FUEL ON THE PRIMARY SYSTEM

. CONCLUSION: WHILE THE VALUES SHOWN IN THE PREVIOUS SLIDE ARE APPROXIMATE, IT IS CLEAR THAT THE CRITERION FOR CURRENT PLANTS IS:

- WELL CONCEIVED, AND
- SHOULD BE RETAINED FOR FUTURE PLANTS.

T-11

OTHER LESSONS FROM REACTOR ACCIDENTS

- THE TMI ACCIDENT WAS CAUSED BY A LACK OF WATER.
- THE TMI ACCIDENT WAS TERMINATED BY ADDING WATER.
- THE DAMAGED CHERNOBYL REACTOR WAS STABILIZED FOR SEVERAL HOURS BY WATER ADDITION (FIRE FIGHTERS) BUT WAS HAULTED BECAUSE THE WATER WAS SPILLING INTO AND CONTAMINATING UNITS 3, 2 AND 1.
- CONCLUSION: WATER WOULD BE VERY EFFECTIVE IN RECOVERING FROM AN ACCIDENT STATE AND FUTURE DESIGNS, LIKE THE CURRENT PLANTS, SHOULD FOCUS ON WAYS TO SUPPLY WATER TO THE CONTAINMENT AND TO REMOVE THE DECAY HEAT.

T-11

CONTAINMENT LINER INTEGRITY IS IMPORTANT

FUTURE DESIGNS SHOULD FOCUS ON

- COOLING THE DEBRIS TO PROTECT THE LINER.
- IMBEDDING THE LINER IN CONCRETE TO MINIMIZE THERMAL LOADS FROM DEBRIS.
- OR BOTH.

over for 1st slide for T.12

T-12

CRITERIA IMPLEMENTATION

- LARGE LOCA WITH A SIGNIFICANT SOURCE TERM IS A REASONABLE DESIGN BASIS TO ASSURE THE CRITERIA ARE SATISFIED.
- DETAILS OF THE DYNAMICS OF THE LARGE BREAK SCENARIO, SUCH AS BREAK OPENING TIME, SIZE OF THE OPENING, SHAPE (GUILLOTINE OR FISHMOUTH) CAN BE, AND SHOULD BE SIMPLIFIED. THESE IMPACT:
  - PIPE RESTRAINTS.
  - JET DEFLECTION DEVICES.
  - SUPPRESSION POOL DYNAMICS.

T-B

CONCLUSIONS

③

- THE GENERAL CRITERIA USED FOR DESIGNING THE CURRENT PLANTS ARE WELL CONCEIVED.
- THE PRUDENCE OF THE CRITERIA USED IN THE U.S. IS DEMONSTRATED BY THE EXPERIENCE FROM REACTOR ACCIDENTS.
- THE GENERAL CRITERIA USED FOR CURRENT PLANTS ARE APPLICABLE TO FUTURE DESIGNS.
- THE IMPLEMENTATION OF THE CRITERIA CAN BE STREAMLINED.
- FUTURE DESIGNS COULD ADDRESS SEVERE ACCIDENT ISSUES TO REDUCE THE INFLUENCE OF UNCERTAINTIES.

T-12

⑥

SEVERE ACCIDENT ISSUES

- Hydrogen combustion.
- Liner attack.
- Debris dispersal/DCH.
- Debris coolability.

① T-13 T-13

FUTURE DESIGNS CAN ADDRESS SEVERE ACCIDENT ISSUES BY
(CONTINUED)

. USE A REACTOR CAVITY/INSTRUMENT TUNNEL CONFIGURATION WHICH DRASTICALLY REDUCES OR ELIMINATES THE POTENTIAL FOR DEBRIS DISPERSAL GIVEN A HIGH PRESSURE MELT EJECTION CONDITION.

. MAXIMIZE THE CAPABILITY OF PUTTING WATER ON THE CONTAINMENT FLOOR.

. MAXIMIZE THE POTENTIAL FOR ACCIDENT RECOVERY BY MAXIMIZING THE FLOOR AREA FOR DEBRIS ACCUMULATION.

FUTURE DESIGNS CAN ADDRESS SEVERE ACCIDENT ISSUES

T-12

. LIKE THE CURRENT PLANTS, FUTURE DESIGNS SHOULD PROTECT AGAINST OVERPRESSURE DUE TO HYDROGEN COMBUSTION.

  - VOLUME AND ULTIMATE PRESSURE CAPABILITY TO ACCOMMODATE A COMPLETE BURN OF HYDROGEN GENERATED BY THE OXIDATION OF 75% OF THE ACTIVE CLADDING.

  - INERT THE CONTAINMENT.

  - INTENTIONAL IGNITION (IGNITERS).

. PROVIDING PROTECTION FOR THE LINER.

  - WATER.

  - IMBEDDED.

T-15

CONTAINMENT DISCUSSION

Presented to the NRC ACRS Subcommittees on Containment and Structures--Chicago, Illinois October 17, 1989

Prepared by M. Bender, Querytech Associates, Inc.

- 0 DEFINITION OF CONTAINMENT, A SYSTEMS CONCEPT
- 0 REFERENCE EXPERIENCE
- 0 CURRENT UNDERSTANDING FROM NRC AND INDUSTRY SPONSORED RESEARCH
- 0 DEVELOPING A DESIGN BASIS

T-15

CONTAINMENT DEFINED:

A SYSTEM INTENDED TO PREVENT THE SPREAD OF RADIONUCLIDES, RELEASED IN BULK FROM THE REACTOR CORE, BEYOND SPECIFIED SITE LIMITS IN THE EVENT OF A NUCLEAR ACCIDENT.

ESSENTIAL SYSTEM PROPERTIES:

1 Boundary closure sufficient to limit dispersal of radionuclides postulated to be present during and subsequent to an accident,

2 An effective heat sink to absorb nuclide decay energy and stored energy in coolants and surrounding structure for the purpose of controlling temperature conditions to limit subsequent chemical, physical state, or fluid perturbations that would aggravate radionuclide dispersal conditions,

3 Radionuclide trapping or stabilizing capability to prevent further dispersal of all but the noble gases during and subsequent to an accident including those caused by transient effects. (Holdup to permit noble gas (xenon) decay can be a valuable capability, but the trapping mechanisms must be of high reliability; the physical flow path may be the most effective device for this purpose.)

T-15

RELATIONSHIP TO THE NUCLEAR POWER SYSTEM:

0 Nuclear power production and conversion systems vary in:

transient characteristics,
nuclear instigated accident initiators,
dynamic response,
accident limiting capability,
radionuclide dispersal mechanisms.

0 Both **inherent** power plant **properties** and **designed-in safety provisions** affect the containment needs and capabilities.

0 **PWR System containment**, absent built-in heat sinks such as the "ice condenser", invite structural overload under accident contingencies such as loss-of-coolant accidents. Strong (high pressure) containment is the evolutionary design response, but **may not be adequate for either an ATWS with insufficient primary system relief capacity or pressure vessel bursting.**

0 **BWR Systems** by design provide reliable heat sink capability but **ATWS threat may not be containable.**

0 **HTGR systems** because of the **inherent heat capacity** of the graphite core (high permissible temperature and large mass) combined with supplemental cooling provisions of the concrete pressure vessel liner are **immune to catastrophic containment loadings** when reasonable containment pressure holding capacity is available. Long term containment response to **severe core damage events has not be adequately examined**. ATWS events seem to be self limiting as a threat to containment. **Pressure vessel bursting** is not admissible as an accident scenario.

T-15

REFERENCE REACTOR ACCIDENT EXPERIENCE

1. NO RADIONUCLIDE RELEASES AT HIGH POWER

2. OPERATOR ALERTNESS HAS PREVENTED FUEL FAILURE AT POWER (E.G. BROWNS FERRY ATWS, DAVIS BESSE FEEDWATER TRANSIENT)

3. PREVIOUS PRACTICE HAS EXCLUDED SEVERE EVENTS FROM CONTAINMENT REQUIREMENTS (BWR ATWS, CORE COOLANT BLOCKAGE)

4. EARLY ACCIDENTS IN SMALLER INSTALLATIONS HAVE GUIDED SAFETY REQUIREMENTS (SL-1, NR-X, WINDSCALE)

5. TARAPUR AND CHERNOBYL SHOWN POTENTIAL RISK (NOT AS EXTENSIVE AS "DOOMSDAY" PREDICTIONS BUT EXTENSIVE AND SERIOUS)

6. TMI-2 SHOWED THAT CORE MELTING DOES NOT NECESSARILY VIOLATE CONTAINMENT. WITH MINIMAL COOLING UNDER SHUTDOWN CONDITIONS LOW CONTAINMENT PRESSURES EASILY MAINTAINED. LOW LEAKAGE WASN'T HARMFUL.

FYI T-15

A CONTAINMENT OVERVIEW FOR THE NINETIES

M. Bender, Querytech Associates, Inc., October 17, 1989

I was initiated by the ACRS to containment issues in 1966 when the Brookwood (Now Ginna) Nuclear Power Plant was being reviewed for a construction permit by the AEC. At the time the subject of containment design adequacy was just coming into focus. There were no design standards and only an extremely vague impression of the accident circumstances to be contained. Now, almost a quarter century later we're trying to determine whether we know enough to satisfy public safety needs of a nuclear power plant containment system. We have to remind ourselves that a "systems" concept has never been established; it has just evolved by accident analysis procedures.

Surprisingly, the gas cooled reactor systems technology contributed as much to our current posture on containment as did the water cooled technology. During the ACRS review of the Experimental Gas Cooled Reactor (EGCR) and the Peach Bottom HTGR in the 1958 to 1968 decade most of the containment logic evolved. ECCS redundancy, double end pipe break effects on coolant systems, emergency cooling dynamics, etc. Later the safety technology was translated to water cooled power reactor systems through the AEC's then-safety research programs. LOFT and the smaller "Semi-scale" related experiments further defined the accident mechanics. Eventually the functions of containment evolved. A good characterization of that "state of the art" has been provided by Thompson & McCullough [1].

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[1]. The Technology of Nuclear Reactor Safety, Volume 2, Chapter 21 by T. J. Thompson and C. R. McCollough, former ACRS members, provides an excellent discussion of containment rationale. Nothing in the current state of knowledge alters the substance of their discussion.

In 1966 the containment emphasis was different from now. The accident being contained was arbitrary, an event which released all of the noble gases, 50% of the volatile radionuclides and about 1% of the particulate matter. These quantitative bases came from small bench scale experiments with reactor fuel. The TMI-2 accident is the only large scale reference for this accident basis. Surprisingly, the TMI-2 radionuclide releases matched the laboratory experiment-based estimate quite closely. As a design condition for TMI-2 the regulatory basis was quite sound.

What TMI-2 showed was that serious accidents can occur, bulk fuel melting can be present, and containment can leak without public health or property injury. But, what containment functions weren't needed?

1. High pressure resistance was not utilized, even though the industry crowed about its capability; in fact, the containment leaked.

2. Containment sprays weren't needed because the heat remained in the reactor vessel.

3. Structural strength to resist internal and external loadings was unnecessary because the structure wasn't subjected to high pressure.

4. The built-in fire protection was useless to deal with the hydrogen fire that appeared, but the containment effectiveness was not lost thereby.

This list could be further extended. But, this is enough to show that NRC's containment regulations didn't directly apply to the TMI-2 accident even though the reference radionuclide release occurred.

When the Chernobyl accident occurred everyone pointed to U. S.

Containments as the basis for saying "it can't happen here". But it can, because accidents happen in vague ways and "precursor " conditions can affect containment effectiveness. We've seen other accidents that could have had more serious consequences than TMI-2-- the Browns Ferry fire, the Browns Ferry partial ATWS, the Davis-Besse Feedwater event, the Fermi-2 simulator induced operator error to list a few. None caused any direct public injury. We've mesmerized ourselves in to accepting the impossibility of a BWR ATWS or a PWR burst pressure vessel, even though they are not beyond conception.

Reexamining the basis for containment and its needed capabilities is a timely event. We've been fumbling with the problem for at least 25 years. We need to know whether we understand the goal and whether we have reached or can reach it. When standing under the black cloud of nuclear accident doomsday predictions, containment is the public security blanket. What can it do and what does it have to do?

What I plan to offer is one observer's thoughts about the current situation as learned from 25 years of continuing association with the technology in a regulatory environment. These views might not stand up to legal attack, but they will come nearer to meeting the concerns of the true public safety advocate than the Ludite actions of those overly concerned by the power plant nuclear safety threat.

T-16

ACCIDENT REFERENCE EXPERIENCE

1. The range of accidents involving commercial nuclear power systems, have not included concurrent high power and radionuclide dispersals (essentially all arose from low power transients).

2. The consequences of the few high power events that might have been accompanied by radionuclide dispersal were limited by operator alertness and intuitive response. Their consequential potential, absent operator response, are difficult to envision (e.g. the Browns's Ferry ATWS event, the Davis Besse Feedwater Transient, BWR Channel Box degradation). The frequency of such events is not documented, but enough events have been experienced to warrant their consideration in containment capability.

3. The most severe types of events (BWR/PWR ATWS, PWR/BWR pressure vessel bursting, total blockage of coolant flow to the reactor core) have previously been excluded from containment design consideration on probabilistic grounds. Under "severe accident" reasoning strategic reserve provisions should be considered for inclusion as a part of emergency planning.

4. Most of the reference radionuclide-dispersal accident experience has been associated with non-power reactor systems. Many of the events have influenced current generation design practices and provided the guidance for acceptable reactor design practices. Notable are the SL-1, Chalk River NR-X, and Windscale events.

T-16

ACCIDENT REFERENCE EXPERIENCE

5. Of the power reactor events, only the recent Chernobyl event and the earlier one at the Indian Tarapur installation resulted in significant radionuclide releases beyond site boundaries. Neither resulted in consequences as severe as those envisioned by "doomsday alarmists" (thousands of deaths and unlimited ground and water contamination), but both were sufficiently catastrophic to bring about radical changes in accident control philosophy.

6. The most extensively examined accident, TMI-2, was effectively contained in spite of the extensive core damage that far exceeded the conditions judged to exist at the time when the accident was in its critical stages. The ultimate consequences to human health were negligible but the threat of containment violation was ameliorated by very low power conditions and adequate, though degraded, core cooling provisions. Although the containment leaked, low pressure conditions minimized the dispersal potential and secondary housing minimized radionuclide releases to the surrounding environment.

WHAT ARE THE LESSONS FROM ACCIDENT RESEARCH?

1. ACCIDENT PROGRESSION

1.1 "Murphy's Law" logic does not give effective design guidance.

1.2 Unencumbered accident progression will inevitably lead to imponderable accident conclusion.

1.3 Time is available for control accident interdiction.

1.4 The operator is an important part of accident control and operator interdictive provisions should not involve complex logic based on accident progression analysis.

T-16

LESSONS FROM SEVERE ACCIDENT RESEARCH

2. CONTAINMENT STRUCTURAL RESPONSE

2.1 Containment structural behavior is predictable and reliable up to elastic response limits. Reinforced concrete appears to provide non-catastrophic failure capability beyond elastic response limits.

2.2 Liner reliability contingent on assuring controlled structural movement under accident loadings--discontinuities still the major uncertainty in liner response.

2.3 Closures sealed ~~with elastomers~~ are the main source of leakage vulnerability. Experimental testing suggests that up to the point of significant leakage (observable flow) gasket materials in current use are functionally effective over the anticipated times of active accident progression if protected from overtemperature and intense radiation.

T-16

LESSONS FROM SEVERE ACCIDENT RESEARCH

RADIONUCLIDE TRANSPORT

3.0 Existence of radionuclides at the containment boundary is a necessary postulate for establishing containment requirements.

3.1 Natural radionuclide holdup is an inherent property of any containment system. Its effectiveness is a function of the radionuclide dispersal path and the absorptive or other trapping capability of the physical surroundings e.g. surfaces that could trap iodine by chemical reaction for sufficient time to permit decay or particle filters that would knock out aerosols.

3.2 CONDENSING STEAM, LIQUID SPRAYS, AND EVEN WATER POOLS WHICH MAKE CONTACT WITH RADIONUCLIDES IN THE RELEASE PATH CAN PROVIDE IMPORTANT TRAPPING CAPABILITY BUT THEY CAN ALSO BE SOURCES OF AEROSOLS. Their trapping effectiveness depends on circumstances associated with contact conditions e.g. boiling or bubbling water in the path of radionuclide release can generate radionuclide bearing aerosols but cold condensing surfaces can coalesce and collect aerosol constituents.

T-16

LESSONS FROM SEVERE ACCIDENT RESEARCH

RADIONUCLIDE TRANSPORT

3.3 CONTAINMENT SPRAYS, a well established feature of many current generation nuclear containments, are effective in trapping radionuclides and appear to have been discounted as effective containment features. Their behavior during accidents must be understood and deleterious effects of malfunction cannot be ignored, but with appropriate reliability they can satisfy much of the radionuclide trapping capability, even under severe accident conditions. Their value has NOT BEEN GIVEN ADEQUATE CREDIT.

3.4 Trapping near the end of the release path by fluid impact barriers, caustic sprays and possibly activated carbon filters can minimize dispersal beyond containment boundaries. Release path control is essential to this approach.

3.5 Trapping of noble-gas radionuclides is technologically feasible but the conditions related to the event are so demanding that resisting them cannot be assured during complex accident circumstances. When the accident effects have subsided refrigerated carbon traps or fluorocarbon absorption systems for radionuclide might be feasible, if the noble gases have been held within the containment boundary.

T-16

DESIGN BASIS

"DESIGN BASIS ACCIDENT" DEFINITION

1. ACCIDENT INITIATORS NEED TO BE POSTULATED--LOCA'S, LOPA'S, STEAM GENERATOR RUPTURES, ETC.

2. SEVERITY OF THE CONDITION NEEDS BETTER RATIONALE I.E. WORST CONDITION LOCA'S DISTORT BEHAVIORAL CHARACTERISTICS AND MISUSE SAFETY RESOURCES--EXAMINE SYSTEM PROPERTIES FOR A REALISTIC ACCIDENT BASIS.

3. ATWS TYPE EVENTS NEED TO BE INCLUDED IN SOME FORM. ENOUGH EXAMPLES EXIST TO DEFEAT ANY PROBABILISTIC ARGUMENT THAT THEY ARE OUT OF THE REALM OF PROBABILITY.

4. RADIONUCLIDE RELEASES SHOULD BE BASED ON REAL TIME EVENTS--ARBITRARY RELEASES DO NOT PROPERLY CHARACTERIZE THE ACCIDENTS AND DO NOT EFFECTIVELY COMBINE RELATED CIRCUMSTANCES.

T-17

DESIGN BASIS

"DESIGN BASIS ACCIDENT" DEFINITION

5. ACCIDENTS SHOULD NOT BE ASSUMED TO PROCEED TO THEIR NATURAL ENDPOINT UNLESS THE INTERDICTIVE OPPORTUNITIES ARE BEYOND ACCESS. AN ATWS MIGHT NOT BE CONTROLLABLE; A SMALL LOCA HEAT SINK BYPASS COULD BE CORRECTED IF KNOWN TO EXIST. ACCIDENT SENSING NEEDS TO BE BUILT IN TO THE DBA ASSESSMENT.

6. DESIGN CONTAINMENT ENCLOSURE FOR CONTROLLED FAILURE; ALLOW CONDITIONS NEAR TO STRUCTURAL YIELDING AND PROVIDE RUPTURE RELIEF THROUGH A KNOWN TRAPPING PATH BEFORE BURSTING.

7. PROVIDE FOR EFFICIENT ~~TRAPPING MEDIA SU~~CH AS CAUSTIC SPRAYS, CHEMICALLY ACTIVE TRAPPING PONDS, RUGGED AND ACCIDENT INSENSITIVE TRAPPING DEVICES LIKE "SAND FILTERS".

T-16

DESIGN BASIS

1. CONTAINMENT PHILOSOPHY

1.1 CONTAINMENT REQUIREMENTS SHOULD BE BASED ON PUBLIC RISK VERSUS ACCIDENT FREQUENCY LOGIC BUT THE APPROACH MUST ADDRESS NON-PROBABILISTIC LIMITS IN SOME MANNER.

1.2 ALL ACCIDENTS MUST BE "CONTAINED"; the higher frequency events deserve proportionately more stringent limits on the consequences from radionuclide dispersal, but A BOUNDING LIMIT IS NEEDED FOR EMERGENCY PLANNING REGARDLESS OF THE OCCURRENCE FREQUENCY.

1.3 SOME ACCIDENTS, though held to extremely low probability by carefully implemented design, ARE UNPREDICTABLE IN THEIR BEHAVIORAL CHARACTERISTICS (including violation of the containment boundary) AND MUST BE CONSIDERED IN EMERGENCY PLANNING PROVISIONS I.E. A STRATEGIC RESERVE.

DESIGN BASIS

2. HEAT SINK PROVISIONS

2.1 Control of heat release is the first consideration in containment system design. Pressure rise within the containment boundary is established by showing how heat is dispersed and what effect it might have on the fluid environment within containment.

2.2 Heat content of primary and secondary coolant systems due to sensible heat and change-of-state must be included in establishing internal pressure and temperature conditions.

2.3 fectiveness of the heat sink in limiting temperature rise is a ary consideration in determining containment performance quirements. Secondary reactants become important as heat ources when temperature conditions rise to rate-accelerating ritical limits (zircalloy-water reactions, hydrogen combustion, concrete disintegration).

DESIGN BASIS

3. ENCLOSURE PERFORMANCE CAPABILITY

3.1 Pressure holding (Ultimate accident load vs ultimate strength) should take full advantage of structural strain capacity, permitting inelastic response within limits known to be non-catastrophic, especially if self limiting, can assure effective use of safety provisions. Failure mode control is a part of this type design treatment.

3.2 Leakage control is related to the path of leakage. Venting to radionuclide trapping systems can combined with leakage control provisions to establish a suitable leakage path. Allowable leakage should large enough to make monitoring of leak tightness easy.

3.3 Containment structures by design should be tolerant of pipe whip and missile forces without complex "tie-downs" that add undesirable structural demands at the time of an accident. Leak-before-break has eliminated some complexity but improved design arrangements could eliminate virtually all such provisions.

DESIGN BASIS

3. ENCLOSURE PERFORMANCE CAPABILITY

3.4 External loadings (Prior to, during and subsequent to accidents) and their interactive effects have to be treated, but

(a) site related requirements can be made less demanding than seen in many current designs. (e.g.use of innovative features like elastomer membranes in foundations, as European designers have done), and

(b) more attention to geometries that minimize wind and flood loadings

are actions that can enhance containment effectiveness with relatively less resource expenditure.

3.5 Internal loadings from accident phenomena (hydrogen combustion, hot gas circulation, steam explosions) that severe accident analysis predicts to be possible under some circumstances need to be addressed but explicit designs to resist extremely severe conditions are probabilisticly unreliable and failure consequences from such events as uncontained accidents must be examined in pragmatic terms (the Chernobyl accident fits this scenario.)

3.6 Containment failure mode and consequence control must be related to emergency responses during and subsequent to accidents.

T-12

DESIGN BASIS

4.0 VENTING AS A PART OF CONTAINMENT STRATEGY

4.1 Containment bursting or gross leakage is implicitly a part of current containment designs because some "severe" accident conditions can exceed the strength or leakage control capability of the strongest containments. Control of failure properties is important to containment strategy in such circumstances.

4.2 "PRE" VENTING--Early venting prior to radionuclide release CAN MINIMIZE CONTAINMENT ACCIDENT LOADINGS WITHOUT PUBLIC RISK but must be keyed known conditions; a trapping system should be directly in the path of any venting arrangement.

4.3 "POST" VENTING--Venting subsequent to radionuclide release HAS PUBLIC SAFETY ADVANTAGES IF THE RATES ARE CONTROLLED AND AN EFFECTIVE RADIONUCLIDE TRAPPING SYSTEM IS IN PLACE. Reliability of the trapping system under uncertain conditions is a crucial consideration.

DESIGN BASIS

5.0 STRATEGIC RESERVE

More thought should be given to a "strategic reserve" because its independence from accident circumstances eliminates much of the concern about failure commonality, the "achilles heel" of almost all safety provisions.

5.1 Every examination of accidents has shown that aside from nuclear criticality events (not considered to be physically possible in water cooled power reactors prior to core damage) containment of radionuclides is not an instantaneous need. Most and certainly the most probable of envisioned "severe accidents" that become a threat to public safety occur hours and sometimes days after the accident conditions are identified.

5.2 A "strategic reserve" to augment built-in containment features enhances the public risk protection. "Fire fighting" risk control logic should be applied. Ability to bring in outside containment cooling devices that would be isolated from the initiating conditions (e.g. fire water hookups) adds important independent risk protection. Accessibility for such provisions needs to be considered in the design criteria.

5.3 As shown by the Chernobyl accident response, barriers to radionuclide transport should be considered pragmatically. How to deter radionuclide transport, isolate principal radionuclide sources, or even alter weather factors are matters of importance to emergency response. More emphasis should be placed on these elements of emergency planning that aid emergency response in preference to elaborate warning systems that, at most, bring fringe public safety provisions into play. ("The rains in Spain fall mainly on the plain" but crop dusting and fog treatment have been applied to thousands of acres in short time periods for crop protection purposes.)

T-17

DESIGN BASIS

5.0 STRATEGIC RESERVE

COMMON FAULT FAILURES, THE ACCIDENT "ACHILLES HEEL"

TIME IS AVAILABLE TO IMPLEMENT EMERGENCY CONTROLS

FIRE FIGHTING LOGIC SHOULD BE USED i.e. AFTER ACCIDENT HOOK-UP, ISOLATION OF NUCLIDE SOURCES, COUNTER WEATHER CIRCUMSTANCES, EXTERNAL COOLING.

T-1c-[illegible]

SELF-ACTUATED PRESSURE RELIEF DEVICE
FOR
REACTOR CONTAINMENTS

(CONCEIVED BY L. MINNICK;
INVESTIGATED FOR EPRI BY S. LEVY, INC.)

PATENT APPLIED FOR BY EPRI

FUNDAMENTAL PURPOSE

TO PREVENT OVER-PRESSURIZATION OF REACTOR CONTAINMENT DURING ANY POSTULATED ACCIDENT OTHER THAN INSTANTANEOUS RELEASE OF ENERGY

T-19

SELF-ACTUATED PRESSURE RELIEF DEVICE
FOR
REACTOR CONTAINMENTS

ADDITIONAL FUNCTIONS PERFORMED

- SCRUBS RELEASED GASES OF PARTICULATES AND ANY MATERIAL HAVING AN AFFINITY FOR WATER.

- PROVIDES DILUTED, ELEVATED AND HEATED RELEASE OF NOBLE GASES.

- CONDENSES ESSENTIALLY ALL STEAM AND RETURNS THE WATER FORMED TO THE CONTAINMENT.

- REESTABLISHES CONTAINMENT INTEGRITY WHENEVER CONTAINMENT OVER-PRESSURE IS TERMINATED.

- PROVIDES RELIEF OF POTENTIAL CONTAINMENT VACUUM FOLLOWING INCIDENT.

T-19

SELF-ACTUATED PRESSURE RELIEF DEVICE
FOR
REACTOR CONTAINMENTS

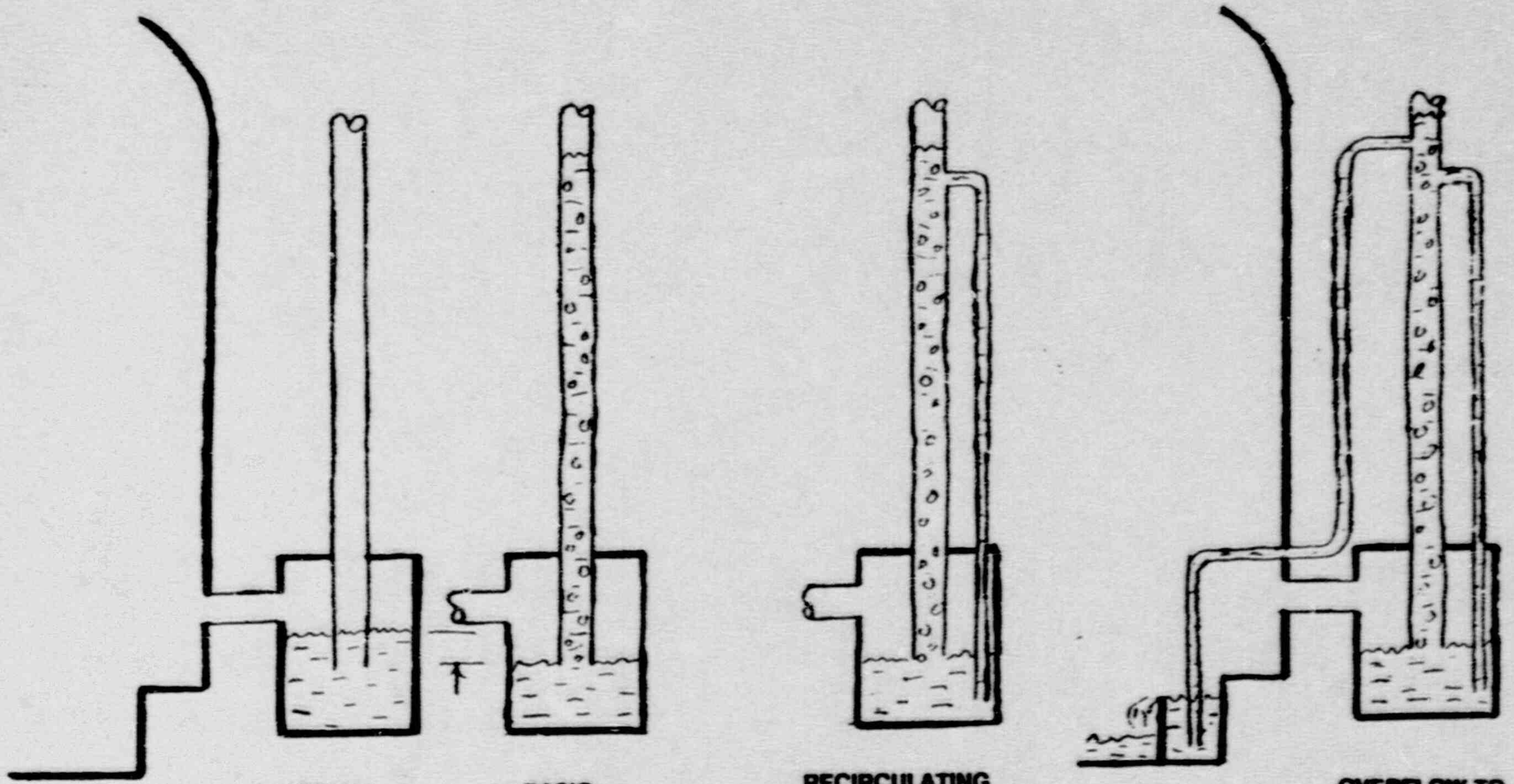
INHERENT CHARACTERISTICS

- TOTALLY PASSIVE ACTUATION, OPERATION AND RESET:
  - NO ACTIVE DEVICE OR MECHANISM,
  - NO OPERATOR ACTION,
  - NO POWER REQUIREMENT,
  - NO INSTRUMENTATION OR CONTROL, AND
  - NO MAKEUP WATER

  ARE REQUIRED THROUGHOUT THE COURSE OF THE TRANSIENT, REGARDLESS OF DURATION.

- SHIELDS ALL RADIOACTIVE MATERIAL COLLECTED AND, ULTIMATELY, CONTAINS WHATEVER HAS NOT BEEN RETURNED TO THE CONTAINMENT IN A SINGLE UNDERGROUND TANK.

SAPRD ~ IN PRINCIPLE



NORMAL PLANT OPERATION

BASIC RELIEF & FILTRATION

RECIRCULATING DRAIN

ENHANCES:
- STABILITY
- HEAT REMOVAL
- PRE-COOLING & MIXING

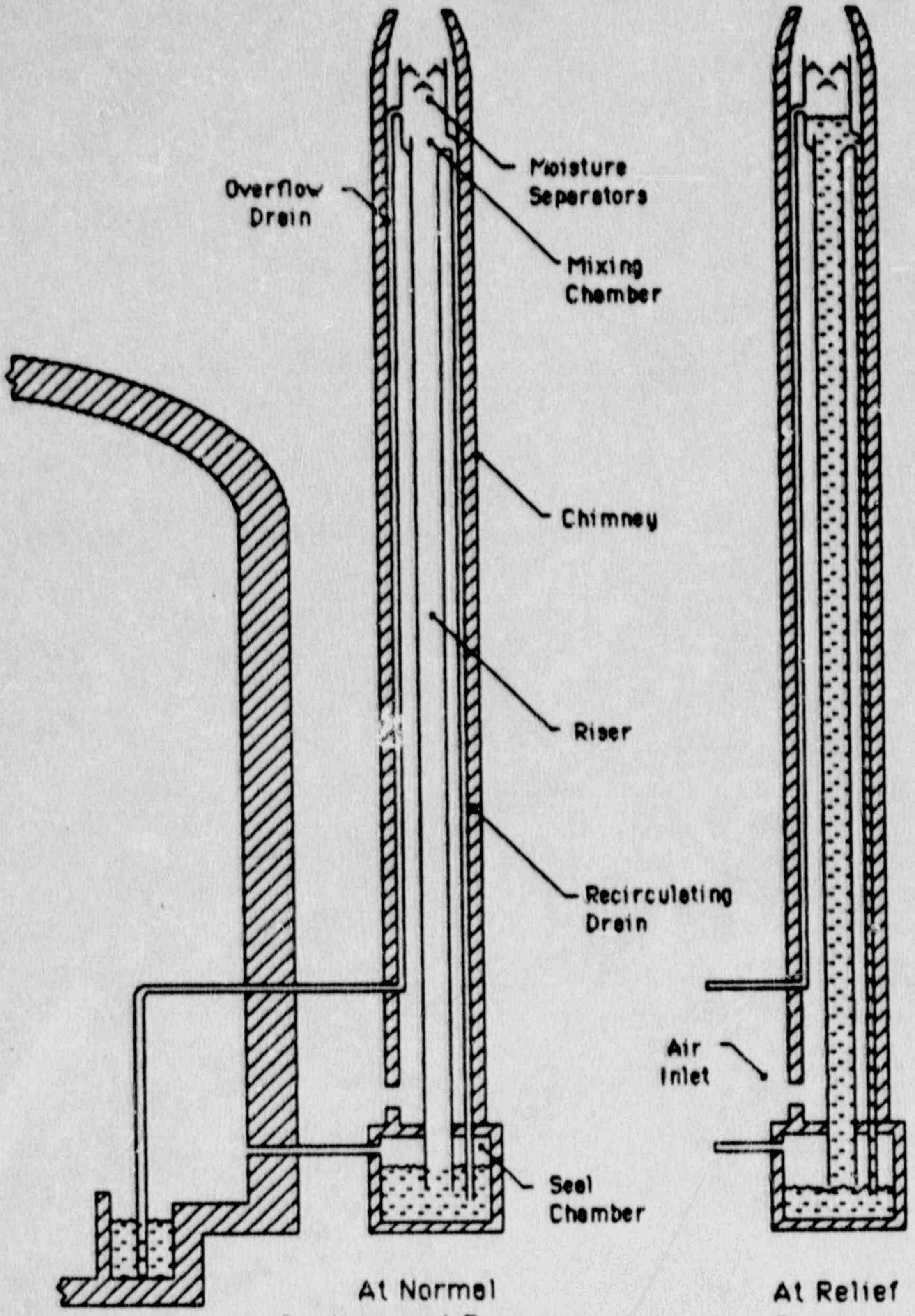
OVERFLOW TO CONTAINMENT

- MAINTAINS LEVEL IN STANDPIPE
- MAINTAINS POOL OF WATER UNDER REACTOR

7-19

7-20

Schematic Diagram
Self-Actuating Pressure Relief Device



At Normal Containment Pressure

At Relief Pressure

REFINEMENTS

T-20

- CHIMNEY, WHICH:
  - SUPPORTS
  - SHIELDS
  - PROVIDES UPDRAFT OF AIR
- MULTIPLICITY OF RECIRCULATING DRAINS, WHICH: -
  - PROVIDE HEAT TRANSFER SURFACE AREA
- STANDPIPE BAFFLES, WHICH:
  - BREAK UP BUBBLES
  - ENHANCE MIXING AND SCRUBBING
- MOISTURE SEPARATOR, WHICH:
  - PREVENTS CARRY-OVER OF DROPLETS
- LEVEL INDICATORS IN SEAL CHAMBER AND SUMP, WHICH: -
  - ASSURE OPERATORS, DURING NORMAL OPERATION, THAT SAPRD IS PREPARED TO FUNCTION
- MUFFLER, WITHIN CONNECTION TO CONTAINMENT, WHICH: -
  - PROTECTS SAPRD AGAINST CONCEIVABLE RAPID PRESSURE TRANSIENTS IN CONTAINMENT
- FINS ON STANDPIPE, WHICH:
  - ENHANCE HEAT TRANSFER

T-2|0

Subject of Presentation

"Containment Design Criteria for future nuclear power plants considering severe accidents"

- The subject is addressed from the standpoint of a structural design engineer

7-810

Basic Structural Design Requirement

- Size and configuration of containment
- Loads
    - Location
    - Magnitude
    - Time dependency
    - Probability of occurrence
- Construction materials
- Material stress or strain allowables
    - Applicable codes
    - Failure criteria

T-81

③

Review of Present Structural Containment Design

- The LOCA load is well defined. The NSS supplier provides this load. It is coupled to the reactor's thermal capability.
- The ASME Containment Codes are complete.
  They are:
  Section III - Division 1 - Subsection MC,
  Section III - Division 2 - Subsection CC,
  and have been developed and are maintained by the Industry, Research, and Universities with participation by the NRC. These codes are based on LOCA loads.

T-21 ④

Review of Present Structural Containment Design

- The containment capability of existing containments for an upper bound pressure load have been determined and safety margins compared to LOCA loads have been computed. The acceptance criteria in all these capability evaluations were beyond code allowables.
- Based on these studies, containments designed to current codes show considerable margins.
- Some of these results used in PRA have shown acceptable risk to public within current understanding of acceptable risk.
- Testing by Sandia of scaled containment models in steel and reinforced concrete have shown that in most cases, the scaled containments behave in a ductile manner. (leak before break)

T-22

Review of Present Structural Containment Design

- The work required to determine the containment capabilities was sponsored by:

  The Industry Degraded Core Rulemaking Program,

  Utilities commissioning plant unique probabilistic risk assessment studies,

  Sandia-NRC sponsored workshops,

  Sandia effort on NUREG 1150.

- The Advanced Light Water Reactor Study utilizes a containment designed for LOCA loads and using the ASME Code. System and layout provisions are made in consideration of severe accidents.

T-22

Review of Present Structural Containment Design

- Lessons learned from the Containment Capability Studies have highlighted that the containments must be ductile and must not have a weak link anywhere. Designs and care of details is of utmost importance and can be provided within current design basis.

Conclusion

- The present Structural Containment Design Criteria is adequate and should not be changed in the near future.

T-22

Recommendations for Future Development

- It is recommended that an industry effort, in participation with research, universities and the NRC, should be undertaken to develop loads and design criteria for containment based on severe accidents.
- The goals of this effort should be: Define severe accident loads in terms and ways that can be utilized in structural design without ambiguity.
- A consensus has to be reached regarding the events involved in a severe accident. Loads, in terms of time dependent pressures and temperatures and their probability of occurence have to be established.
- A consensus has to be reached regarding an acceptable probability of risk to the public in case of a severe accident.

T-23

Recommendations for Future Development

- Future structural designs will be based on probabilistic assessement of loads and resistance to achieve a safe structure. When this can be done appropriately, it is then the proper time to change the containment design basis.
- Revise present ASME design codes from deterministic to probabilistic in terms of load factors and allowables, and emphasize ductility.
- Based on the present work of the Advanced Light Water Reactor Industry Group, future containments may have only one of two configurations: the large dry containment for PWRs and a modified Mark II containment for the BWRs. Limiting consideration to these possibilities will facilitate the above tasks considerably.
- It is anticipated that such efforts will require a considerable amount of time.

T-25

THOUGHTS AND REFLECTIONS ON CONTAINMENT DESIGN CRITERIA

W. A. von RIESEMANN
CONTAINMENT TECHNOLOGY DIVISION
SANDIA NATIONAL LABORATORIES

PRESENTATION TO
ACRS JOINT SUBCOMMITTEE MEETING
CONTAINMENT SYSTEMS/STRUCTURAL ENGINEERING

OCTOBER 17, 1989

T-25

⑤

CAVEATS

- COMMENTS ARE MY OWN AND DO NOT NECESSARILY REFLECT THE OPINIONS OF SANDIA OR ANY OTHER ORGANIZATION

- EXAMPLES THAT ARE CITED ARE ONLY USED TO ILLUSTRATE A POINT AND SHOULD NOT BE CONSTRUED TO BE 'ABSOLUTE'

- MAJORITY OF COMMENTS ARE BASED ON LWRs. CONCLUSIONS COULD BE DIFFERENT FOR DIFFERENT REACTORS (GAS-COOLED HTGR, BREEDER)

T-25 ③

SUMMARY

- A DECADE OF KNOWLEDGE ON CONTAINMENT BEHAVIOR AND SEVERE ACCIDENTS HAS NOT BEEN FACTORED INTO THE ASME CODE

- RECOMMEND THAT A COMMITTEE (INDUSTRY, RESEARCHERS, REGULATORS) BE FORMED TO REWRITE THE CODE (DESIGN, FABRICATION, INSPECTION INCLUDING LEAK RATE MEASUREMENTS, SEVERE ACCIDENTS) CONSIDERING THE CONTAINMENT AS A <u>SYSTEM</u>

  FIRST STEP WOULD BE TO DETERMINE THE PHILOSOPHY

CONTAINMENT

- PURPOSES:

PRIMARY	—	TO CONTAIN ANY ACCIDENTAL RELEASE OF RADIOACTIVE MATERIAL FROM 'PRIMARY' SYSTEM
SECONDARY	—	RADIATION SHIELD PROTECTION AGAINST EXTERNAL THREATS MISSILES TORNADOS SABOTAGE SUPPORT FOR EQUIPMENT (e.g. CRANE, ETC.)

CONTAINMENT (cont'd)

Test

✓

- CONTAINMENT IS A SYSTEM--NOT AN ISOLATED COMPONENT (SHELL)

I.E. SYSTEM CONSISTS OF

Structure (Shell)
Penetrations (Operable and Fixed)
Bellows
Drywell Head (BWR)
Fuel Transfer Tubes
Isolation Valves
Basemat
Instrumentation (Status of System)

THE PERFORMANCE (BEHAVIOR) DEPENDS ON THE RESPONSE OF ALL OF THE PARTS AND ANY POSSIBLE INTERACTIONS; e.g., REACTOR VESSEL SUPPORT FAILURE WHICH THEN WILL LOAD CONTAINMENT THROUGH THE STEAM LINES.

CURRENT APPROACH TO DESIGN (LWRs)

- ASSUME LOADS ARE KNOWN
  (Includes Pressure, Temperature and Earthquake)

- DIFFERENT PHILOSOPHIES

  Steel - Allowable Stresses
  Concrete - Factored Loads

  Leads to Different Margins of Safety Against Internal Pressure

(T-25)

CURRENT APPROACH TO DESIGN (LWRs) cont'd

- JURISDICTIONAL BOUNDARIES BETWEEN STEEL AND CONCRETE MAY LEAD TO INCONSISTENCIES
- LINER (CONCRETE CONTAINMENTS) IS GIVEN ZERO STRENGTH
- DESIGN IS BASED ON ESSENTIALLY ELASTIC BEHAVIOR
- LEAKAGE REQUIREMENTS ARE VERY SMALL

LESSONS LEARNED

- Current Design Personnel Airlocks and Electrical Penetration Assemblies (Except for Electrical Peformance) Behaved Well (Leakage and Strength)

- Equipment Hatches
  - Sleeve Ovalizes -- Leakage May Occur
  - Pressure Unseating--Not Desirable

- Seals and Gaskets -- Performed Well Up to About 500°F

- Inflatable Seals -- Leakage will Occur at Overpressurization

- Basemats -- Data from a Recent Test Result has to be Interpreted; Additional Work may have to be Performed.

T-26

①

LESSONS LEARNED (cont'd)

- Stiffening Around Penetrations and 'Area Replacement' Rule Causes Strain Risers and May Lead to Early Failure

  In Particular, for Liners With Studs and (on Ring Stiffened) Steel Cylinders

- Basemat -- Cylinder Intersection in Reinforced Concrete Containments is Overdesigned

- Tori-spherical Heads do Buckle but do not Fail (i.e. Leak) till the Pressure is Several Times the Buckling Pressure

- Consequences of a Core/Concrete Interaction Depend on the Chemical Composition of Concrete

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- OVERPRESSURE PROTECTION

ARTICLE NE-7000 (ASME CODE, SECTION III) STATES THAT "PRESSURE RELIEF OR VACUUM RELIEF DEVICES ARE NOT REQUIRED WHERE THE SERVICE OR TEST LIMITS SPECIFIED IN THE DESIGN SPECIFICATION ARE NOT EXCEEDED."

HOWEVER, SINCE THIS ARTICLE WAS WRITTEN, SEVERE ACCIDENTS ARE BEING EXAMINED.

THIS ARTICLE WILL NEED RE-EXAMINATION

LESSONS LEARNED (cont'd)

- Substantial Corrosion of the Steel (Where it Enters the Concrete) May Occur
- Aerosol Retention in Concrete has not been Quantified
- Retention in Secondary Buildings has not been Quantified
- Containments have had Isolation Valves Left Open for Extended Periods

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SUMMARY

- A DECADE OF KNOWLEDGE ON CONTAINMENT BEHAVIOR AND SEVERE ACCIDENTS HAS NOT BEEN FACTORED INTO THE ASME CODE

- RECOMMEND THAT A COMMITTEE (INDUSTRY, RESEARCHERS, REGULATORS) BE FORMED TO REWRITE THE CODE (DESIGN, FABRICATION, INSPECTION INCLUDING LEAK RATE MEASUREMENTS, SEVERE ACCIDENTS) CONSIDERING THE CONTAINMENT AS A <u>SYSTEM</u>

  FIRST STEP WOULD BE TO DETERMINE THE PHILOSOPHY

T27

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GOALS FOR THE NEW REQUIREMENTS

- Benign failure modes
- Long Life
- Simple Inspection, Including On-Line Monitoring
- Construction Ease
- Designers must become Familiar with Severe Accidents and Loads Beyond the Design Basis and the Fact that some Loads are not well Defined; i.e., Mind Set must Change

T-27

GOALS FOR THE NEW REQUIREMENTS (cont'd)

- Internal Structure (Compartments, Rooms) should be Designed to Minimize Effects of Fire, Flooding and Hydrogen Combustion.
- Realistic Leakage Requirements
- Realization that Buckling, per se, is not Necessarily Failure

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POTENTIAL DIFFICULT POINTS

Definition of Loads

Design Criteria vs. Performance Requirements

Overpressure Protection

Leak Rate Testing

Current Licensing is done on a Prescriptive Basis--Difficult to Accommodate Guidelines

Probabilistic Design Beyond the Current State-of-the-Art