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# UNITED STATES
# NUCLEAR REGULATORY COMMISSION

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320TH GENERAL MEETING

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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

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320TH GENERAL MEETING

Nuclear Regulatory Commission
Room 1046
1717 H Street, N.W.
Washington, D. C.

Friday, December 12, 1986

The 320th General Meeting convened at 8:49 a.m.

ACRS MEMBERS PRESENT:

MR. DAVID A. WARD, Chairman

MR. JESSE C. EBERSOLE
MR. HAROLD ETHERINGTON
DR. MAX W. CARBON
DR. WILLIAM KERR
DR. HAROLD W. LEWIS
DR. J. CARSON MARK
MR. CARLYLE MICHELSON
DR. DADE W. MOELLER
DR. DAVID OKRENT
MR. GLENN A. REED
DR. FORREST J. REMICK
DR. PAUL G. SHEWMON
DR. CHESTER P. SIESS
MR. CHARLES J. WYLIE

PUBLIC NOTICE BY THE

UNITED STATES NUCLEAR REGULATORY COMMISSIONERS'

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

FRIDAY, DECEMBER 12, 1986

The contents of this stenographic transcript of the proceedings of the United States Nuclear Regulatory Commission's Advisory Committee on Reactor Safeguards (ACRS), as reported herein, is an uncorrected record of the discussions recorded at the meeting held on the above date.

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P R O C E E D I N G S

MR. WARD: Let's go right into the next agenda item. We are 15 minutes behind. We will have to open up the meeting.

Let's go ahead.

(Slide.)

MR. MICHAELS: My name is Ted Michaels, with the Office of Nuclear Reactor Regulation, the Standardization and Special Projects Branch Directorate.

My talk this morning is on a policy statement that we developed on deferred plants.

(Slide.)

Before I go into the policy statement, I would like to go into some definitions so we clear the air on what we mean by the different terminology we are using here.

The deferred plant -- this is sort of a chronology of when you go from a deferred to a canceled plant.

A deferred plant is a plant where the licensee fully expects to reactivate the construction of the plant but for one reason or another he has ceased his construction or reduced it to a maintenance level and he maintains his construction permit.

A terminated plant is one where the licensee does not plan to reactivate construction. So he has announced

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that construction has been terminated, but his construction permit is still in effect and he may have requested for a withdrawal of the construction permit but that construction permit has not been withdrawn at that point in time.

A canceled plant is a plant without a valid CP.

Now, the policy statement talks about a deferred plant and a terminated plant. It does not talk about a canceled plant.

We do talk about a canceled plant in a staff paper, which is Commission SECY 86-359, and we talk about it also -- we also have a list of tables, Table 2 and Enclosure 4, which covers all canceled plants.

(Slide.)

DR. MOELLER: You could quickly remind us how many are in each group.

MR. MICHAELS: Yes, there are four deferred plants, there are 12 terminated plants, and there are 26 canceled plants.

Now, staff has developed a policy statement in response to the following directives on the subject. The details of these directives are in Enclosure 1 of the staff paper.

(Slide.)

In order to address these directives, the staff task force, consisting of members of the Office of Nuclear

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Reactor Regulation, Inspection and Enforcement, the Office of General Counsel, Research, and the regional offices was formed to consider and develop the following areas.

The first three -- maintenance, preservation, and documentation requirements for deferred plants, applicability of new regulatory staff positions for deferred plants being reactivated, and procedures for reactivating deferred plants -- are addressed in the policy statement itself.

The identification of regulatory improvements and research initiatives is in Enclosure 3 of the staff paper, and the population and status of deferred and terminated plants is in Enclosure 4 of the paper.

(Slide.)

The scope of the study primarily centered around deferred plants. The policy statement does not cover canceled plants because no owner of a canceled plant has expressed any interest in a policy statement to deal with deferred plants or canceled plants at this time.

DR. REMICK: So if there was one, I assume you would handle that on a case-by-case basis?

MR. MICHAELS: That is right. We would handle it on a case-by-case basis.

DR. MOELLER: And terminated plants?

MR. MICHAELS: No. Canceled plants, reactivation

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of canceled plants.

DR. MOELLER: What about activation of a terminated plant?

MR. MICHAELS: A terminated plant. We say in the last few pages of the policy statement that if a terminated plant has any inkling that they might reactivate, that they should follow the same procedures from the standpoint of maintenance, preservation as a deferred plant.

(Slide.)

One of the more important aspects of the policy statement is the identification, the regulations and guides associated with maintenance, preservation, and documentation. The applicable regulations and guides have been identified in the policy statement.

These requirements apply to plants under construction as well as plants on deferred status.

The licensee may choose to modify existing commitments during extended construction delays by developing a quality assurance plan that is commensurate with the expected activities and length of delay.

The licensee is expected to discuss with the appropriate regional office and headquarters the expected construction delay period and the quality assurance program to be implemented during the deferral.

Modified quality assurance programs are inspected

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and reviewed in accordance with appropriate inspection procedures.

One of these procedures is review of quality assurance for an extended construction delay, which is Inspection Procedure 92050.

(Slide.)

Another important aspect of the deferred plant policy is how new regulatory staff positions will be applied to deferred plants when they are reactivated. The policy statement says that plant specific backfits of new staff positions will be considered in accordance with the backfit rule, 10 CFR 5109, which requires that the staff justify value impact analysis, any backfits after October 21st, 1985.

DR. REMICK: Could you elaborate a little bit on that, specifically what that sentence means?

In other words, are you saying that the deferred or terminated plants would be handled in the same way if they were not deferred or terminated from the standpoint of backfit?

In other words, doesn't the backfit rule define certain dates before which the backfit rule doesn't apply and after which it does?

MR. MICHAELS: That is right. The backfit rules applies to plants -- the backfit has a lot of cut-in times.

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I think one of the cut-in times, if your construction permit is after October 21st, 1985, then backfits after that point in time have to be -- you have to have a value impact analysis developed by the staff.

If your construction permit came in before October 21st, 1985, it would be based on when you got your OL docketed, six months before the time you got your OL docketed.

DR. REMICK: That is the same as if you were continuing along with construction; am I correct?

MR. MICHAELS: That is correct. A deferred plant, in a sense, would have the same protection as an operating plant.

That is the issue. That is what the industry is concerned about, that they will not come back for, say, two or three years from now and be required to implement a lot of backfits without a value impact analysis.

DR. REMICK: So they are protected, but they also have the same requirements?

MR. MICHAELS: That is right. They are protected, but they have the same requirements.

DR. OKRENT: Suppose a plant is deferred for a dozen years under this proposed policy.

Does the staff see any difference?

MR. MICHAELS: No. We don't see any difference

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because the plant would be preserved. If it was an operating plant, it would still have to go through the same backfit analysis, and if the design was judged adequate at one point in time we don't see any difference.

Any new designs would have to be, if they were promulgated on an operating plant, they should have the same action for a deferred plant.

MR. HERNAN: This is Ron Hernan, for the Staff.

Correct me if I am wrong, but for each CP there is a defined termination date or expiration date. Does that CP renewal process now apply to these deferred plants?

MR. MICHAELS: Yes, it does.

MR. HERNAN: In other words, I don't know of any case where we have approved a CP for a 12-year period.

DR. OKRENT: But they could ask for a renewal?

MR. HERNAN: Yes, they could, but the staff has to go through evaluations of their reason for delays in each case.

DR. OKRENT: Yes.

MR. HERNAN: There is a control at that point.

DR. OKRENT: It is a control of completely different things than what we are talking about when we talk about backfits, however.

Why does the staff think -- let me postulate a situation. A plant is 50 percent done and then is delayed

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for 10 years.

Why should backfit considerations be identical as if that plant had been completed on time and started running?

MR. MICHAELS: Because if the plant was licensed at one point in time and let's say 10 years elapsed, any backfits would have had to have the backfit rule applied.

Now, we consider this plant -- the design has been approved at one point in time by having the OL docketed, in a sense, not completely approved but in any case it has been accepted, and any further plant specific backfits would be applied by the backfit rules.

DR. OKRENT: What does it really mean technically for an OL to be docketed and accepted?

MR. MICHAELS: It has been accepted. It has had an adequate submission. The FSAR.

DR. OKRENT: It has not been reviewed?

MR. MICHAELS: No, it has not been reviewed.

DR. OKRENT: Suppose you had a plant that didn't even have its OL docketed. Would that be different?

MR. MICHAELS: Yes, that would be different. If the plant did not have its OL docketed, then if a construction permit was before October 21st, 1985, we can impose backfits without any backfit analysis.

DR. OKRENT: Do we have any of these plants that

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don't have an OL docketed?

MR. MICHAELS: All the deferred plants have docketed OLs.

DR. OKRENT: So we are talking then about plants to which you see the backfit rule apply?

MR. MICHAELS: Right.

DR. OKRENT: Even if they are deferred literally an indefinite time, no cutoff?

MR. MICHAELS: That is correct in an abstract sense, but in a practical sense we are going to be looking at it maybe on a case-by-case basis.

DR. OKRENT: And you don't perceive that there is some difference -- either in the ease with which modifications can be made to a plant that is deferred for 10 years -- you don't see any possibility that it may be more appropriate to make them in a plant that is being deferred for an extended time compared to a plant that is operating?

MR. MICHAELS: No, I don't.

DR. OKRENT: You treat them the same in your mind?

MR. MICHAELS: That is correct, because the value impact analysis and the final analysis will show whether a backfit is necessary or not.

It depends on the stage of completion of the plant. If the plant -- if something has to be backfitted

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in an area where it is not completed, then the value impact analysis will show that that backfit is needed, in case, you know, there is an issue where backfit is concerned.

DR. OKRENT: So you would do plant specific backfits; is that what you are saying? Generic backfits don't really take that into account?

MR. MICHAELS: I was going to come to that, but generic backfits -- when an issue is resolved from a generic standpoint and it is applied to a particular class of plants, then it would apply to that particular class of plant and would have to be implemented.

DR. OKRENT: Recently -- I am probably missing something, but my impression is most of the generic backfits involved changing of tech specs or an operating procedure or training of someone.

MR. MICHAELS: If that was the case and it applied to that particular class of plant, it would be put onto that plant and the generic backfit analysis would cover that plant, also.

DR. REMICK: For example, if it were decided that this class of plant should have filtered vents on containments, this plant would be subject to that class of plants?

MR. MICHAELS: That is correct.

DR. OKRENT: But the decision might be made,

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thinking about plants that were in operation, whereas you have here a plant that may be 10 years from operation, and I don't know that in the examination of the generic question this group will be carried along as a group to be thought of on its own. I can't tell from what I have read that that would be the case.

MR. MICHAELS: That is the way we are planning to consider it.

DR. OKRENT: You are planning to consider what?

MR. MICHAELS: A deferred plant is a plant that is similar to a plant that is under construction.

DR. OKRENT: I thought you said it is similar to a plant --

MR. MICHAELS: It would also be -- it is an operating plant, also.

DR. OKRENT: It is hard for me to see how it is the same as one and the other both.

But why don't you go on?

MR. HERNAN: It is treated primarily as a plant that is under construction, Dr. Okrent. The only thing that is different is the rate of construction is much slower and can be zero.

We did have quite a few plants that were delayed after the TMI accident, and we had to go through an evaluation on these plants of which backfit should be

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applied.

MR. MICHAELS: I guess I covered this area, generic backfits, and also provisions of other policy statements applicable to plants under construction will be implemented.

That is, for instance, in the severe accident policy statement -- that is NUREG-1070 -- it refers to an integrated systematic approach to examine each nuclear power plant now operating or under construction for possible risk contributors that might be plant specific.

Deferred plants will have to perform this plant specific vulnerability analysis. However, the backfit rule will be used to decide which identified plant vulnerabilities need to be implemented.

MR. ETHERINGTON: Is there a step change in NRC surveillance between deferred and terminated plants?

MR. MICHAELS: Usually, a deferred plant goes through lesser inspection requirements than an operating plant or a plant under construction. This is done semiannually by the region.

MR. ETHERINGTON: Then another change in when it is terminated?

MR. MICHAELS: When it is terminated, it still has to be inspected until the withdrawal of the construction permit.

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MR. ETHERINGTON: On the same level as merely deferred?

MR. MICHAELS: Probably.

MR. HERNAN: I think the answer to your question is no. Terminated and deferred, the same standards and inspection frequencies would apply.

The biggest difference is the applicant has expressed a different intention as far as ultimately completing the plant.

MR. MICHELSON: I thought he had maintained a different level of quality controls.

MR. HERNAN: Between deferred and terminated? I don't believe so.

MR. MICHELSON: The terminated plant still has full QA control?

MR. HERNAN: The terminated plant still has an active construction permit.

The big step change comes when the plant becomes canceled. At that point the construction permit is no longer valid, and the controls go away.

DR. OKRENT: Is there any requirement on the level of engineering staff that a plant should be maintained in a deferred state?

MR. MICHAELS: There is no requirement, but we heard yesterday at a meeting -- and this is for WPPS-1 --

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there were about 50 man-years expended on the maintenance and preservation of WPPS-1.

DR. OKRENT: Let's say a plant like that is deferred for five or 10 years. During that period, for example, INPO comes out with a set of recommendations for things that should be done in plants of that type, but they are not going to have the engineers who understand that aspect necessarily, since they have engineers who are interested in keeping things from rusting, and so forth.

How does the staff know 10 years later whether these or other similar kinds of recommendations are even known, let alone acted on?

MR. MICHAELS: Before they come in for reactivation, they have to list all the outstanding generic issues that have been promulgated on that plant and all other outstanding action items.

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DR. OKRENT: It is not my impression that INPO recommendations are generic issues or action items.

MR. MICHAELS: Well, those will have to be reviewed.

DR. OKRENT: I don't see anything in what I read that -- can you point me to the page that tells me how to know or I&E information notices? I don't know that they have to even have read I&E information notices durin that period.

MR. HERNAN: INPO initiatives are, of course, voluntary on the part of the utility. As you state, information notices do not require a response from the utility, so we do not have a control on either of those.

DR. OKRENT: How about things where you put out -- I don't know what you call it -- but where you ask for the utility to respond. If it is a deferred plant, do they have to respond?

MR. HERNAN: Yes. Any bulletins and engineering letters, we require a response from the utility.

DR. OKRENT: But they might say, we haven't gotten to the part of the instruction where this is a relevant item. Let's say it is a question about something in the control room, and they haven't reached that yet.

So that will be their answer.

MR. HERNAN: That will still have to be carried

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as a commitment, as if they were actively constructing the plant.

It will be part of the overall review.

DR. OKRENT: So if the NRC had a formal request, they would still have to, at some point respond?

MR. HERNAN: Yes, they would.

DR. OKRENT: But all these other things, you just don't know what they would do and whether they would even know about this industry experience or recommendations, and so forth.

MR. HERNAN: That is correct. Our experience has been that the engineering staff is dictated by the extent of the preservation program; however, I think our experience -- and correct me, if I am wrong, Ted -- also indicates that they do keep a cadre of licensing people actively on the project during that point in time to maintain a liaison and to maintain the construction permit in terms of regulations.

MR. MICHELSON: They are housekeeping, though. They are keeping things in order, that is all.

MR. HERNAN: It depends on the project and the activity of the licensing activies to that class of plants.

DR. OKRENT: You know, there have been situations where vendors have set up notices which some licensees said you never even saw. Is Salem a good example?

DAVbw

Over 10 years, there may be many of these.

How is this kind of thing going to be ascertained? You know, that you know, if and when this thing comes in for a real OL, that a host of things which are nontrivial in their effect on safety, whether the Staff has made it a formal request or not, have been dealt with or even known about.

MR. HERNAN: Again, I guess I would have to say, we would operate probably in the same manner as we did after TMI, and that is, that there was a certain amount of catch-up, in terms of what has happened, in terms of new regulations. I am not sure about vendor regulations, how closely we track those, but there had to be a conscious effort in those plants that will reactivate after TMI on where were we at the time we stopped and what has transpired since that point in time, and what is needed to complete our operating license review.

Each of these projects does have an advantage of following that in those cases.

MR. MICHELSON: At the point in time a utility desires to start a project back up, he obviously has to hae a whole lot of people. You just can't sustain that kind of a force. Does the NRC go back and reexamine the competency of the utility to start that project back up? That is where you will look to see, have they got the right kind of people

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now to back and do all this catch-up and whatever.

Is that a part of the requirement, that the NRC do a competency review?

MR. MICHAELS: Yes. Their procedures for reactivating are stated in the policy statement.

MR. MICHELSON: Is that included? I didn't recall seeing that kind of a statement in there.

MR. MICHAELS: Yes, it is in there.

MR. MICHELSON: That is where you would ask these kind of questions, and you would not let them reactivate until such time as they had the kind of people with the capability and the ability to go back and play catch-up.

MR. HERNAN: I think you have to distinguish between actual reactivation and construction, which is the utility's option, and our progression with the operating license review. Two different things.

MR. MICHAELS: Here it is. Under Item 3(a)(6).

DR. OKRENT: Could you read?

MR. MICHAELS: It says:

"Information to be submitted by licensee when reactivating."

Now in response to this question, it says:

"A description of the management and organization responsible for construction of the plant."

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DR. OKRENT: That is a very general answer.

MR. MICHAELS: Let me read it. It saus"

"A listing of any regulatory requirements applicable to the plant that have become effective since the plant construction was deferred, together with a description of licensee's proposed plans for compliance with these requirements."

DR. OKRENT: Well, again, you are obviously going to want to see anything that is an NRC rule or regulation or some other requirement met, otherwise somebody could, I suppose, say you are being illegal or whatever for not meeting your own regulations, but there are other things that are important for safety, as well.

MR. MICHELSON: The other thing, basically, the crux of it is the ability to self-regulate. You are trying to move more and more of the self-regulation back to the licensee.

Now what has been visibility or what is even a requirement that he self-regulate himself during this interim period?

Well, the answer, I think is no. He just has to maintain his records, but he doesn't have to pick up on things INPO suggests doing, and so forth.

DAVbw

DR. REMICK: But a active licensee does not.

MR. MICHELSON: But we are doing it, to some extent, right now, on an informal basis. We are looking at what the licensee is doing, in terms of implementing INPO requirements, implementing vendor requirements, and so forth. We are doing it now. And they aren't NRC requirements, but in this case, I assume we don't need a monitor, or there is no requirement, I guess to monitor the self-regulation aspects.

DR. REMICK: Self-regulation, I think, is the important term. The best they could do is to self-manage, not regulate themselves.

MR. MICHELSON: The idea was to turn the regulation back to the licensee. Self-management is fine.

DR. REMICK: Don't turn the regulation back.

DR. MOELLER: Is there no plan on the part of the NRC Staff, when a deferred plant is reactivated, to go back and look at the same things that they have done on a regular plant, meaning if they look at INPO?

DR. MARK: There is certainly a plan to go back and treat it as de novo, unless it was a very specific thing.

DR. REMICK: And there is a list of specifications. If you are going to reactivate, an advance notice of 120 days, and so forth. That type of thing. I

DAVbw

interpret it to mean it would be a typical OL review.

MR. MICHAELS: Yes, it would be the initiation of an OL review, essentially reviewing where the application stood at what point in time and taking it from there.

(Slide.)

The next Vugraph deals with the proposed regulatory improvements. During the development of the policy statement, the task force was requested to identify any areas where the regulatory framework could be improved. We found that the present regulations are not sufficiently prescriptive with regard to withdrawal of a CP. For example, nothing is said in the present regulations on what happens if the licensee requires a plant to be terminated and a construction permit expires before the NRC can process the cancellation.

The policy statement addresses this uncertainty by requesting the licensee to provide notice to the Staff sufficiently far in advance of the expiration of the CP to permit the Staff to determine appropriate terms and conditions.

It also says that prior to formal cancellation of the permit by the Commission, the permit holder is expected to comply with the Commission's rulings, including taking affirmative steps to extend construction permits which may expire before Commission action and the termination request

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can be completed. However, even though the policy addresses this uncertainty, the regulations may have to be revised to provide more specificity in this area.

We may need some more internal NRC guidance on specific information needed for termination of a CP. For instance, what are the criteria for site stabilization and other aspects of site termination?

We also may need detailed guidance for inspection of deferred plants prior to reactivation.

(Slide.)

We have coordinated this with the industry, and we have met with the AIF on February 19, 1986, and requested their views on whether a policy statement for deferred and cancelled plants was necessary.

The AIF provided a letter March 31, 1986, on reactivation of construction projects. They agreed that a policy statement was necessary, that the policy statemenmt should apply to plants with construction permits, that the plants with withdrawn construction permits should be handled on a case-by-case basis, that the backfit rule should be ruled to implement new requirements in effect for plants with construction permits and that the current preservation requirements were adequate, as long as records are maintained.

The review and comments on our final draft were

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submitted to us on July 2, 1986.

(Slide.)

In summary, this policy does not have any new safety requirements, and there are no new safety issues that have been addressed. It essentially consolidates existing requirements and focuses on the requirements for deferral and reactivation of deferred plants.

It also clarifies how the backfit rule will be applied when a deferred plant is reactivated.

It has been coordinated with the industry, through the AIF and additional coordination will be forthcoming, since the policy statement will be issued for public comment.

DR. REMICK: Does this do anything really new, other than what you are doing, just indicating to people what the current status is of these plants or how you are handing the plants, what they must do for reactivation?

MR. MICHAELS: Yes.

DR. REMICK: So there is no change in the policy or regulations?

MR. MICHAELS: That is correct. There is no change in policy or regulation.

MR. HERNAN: This reflects the experience we have had with those plants that fall in those categories.

MR. WYLIE: Could I conclude from this that

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when reactivating the plant, that the Staff would do a complete review of the construction organization and all the other programs to reaffirm their accuracy and that there would be a thorough and complete inspection of all equipment which had been installed prior to putting the plant on hold, is the main thing during that deferral period, to assure that it had not deteriorated during that period?

MR. MICHAELS: Ther is a semiannual inspection performed by the regions.

MR. WYLIE: Of everything?

MR. MICHAELS: The whole preservation and maintenance program.

MR. WYLIE: A lot of equipment is dependent on the state of the plant when it was placed on hold. There is a lot of equipment put into the plants that may be exposed to weather and changing conditions.

Now I know they will say, okay, you preserve that, but over long periods of time, unless you disassemble that equipment and laook at it, you don't know what condition it is.

MR. MICHAELS: There are criteria for inspecting that equipment.

MR. WYLIE: That is what I am saying.

Can you conclude from this policy that it will be thoroughly inspected, disassembled, if necessary, and looked

DAVbw

at?

MR. MICHAELS: The policy doesn't address that specifically, but the procedures are there. Deferred plants have been reactivated. Limerick has been reactivated. So the deferred plants have been reactivated.

MR. WYLIE: I am saying that, but we are talking about plants that have been deferred for long periods of time.

MR. MICHAELS: As I said, there are maintenance and preservation programs in effect. The purpose of these preservation programs is to assure that the equipment is properly maintained, and I am sure that they have started procedures when they come back on to determine whether the equipment can be operational.

MR. WYLIE: Okay.

My question, basically, was, could I conclude that that would be done, that everything would be inspected, to the extent of determining anything that had deteriorated during the referral period?

MR. MICHAELS: I would say the ongoing maintenance and preservation program is as an assurance of that, in addition to start-up procedures that the licensee would have to conduct before preoperational testing would assure that.

MR. EBERSOLE: A question.

DAVbw

A case in point would be the long-term storage of carbon steel pipe, you know, miles of it, sitting there, maybe with full water. The stuff grows rust like you wouldn't believe. Or a case in point, what do they do to clean this gunk out? Do they take on explicit investigations? You can't flush it out. What do they do?. Do they hot store it? Do they put anticorrision inhibiters in it?

MR. MICHAELS: They put desiccants in it. I am not familiar with that particular area, but they do have anticorrosion protection and desiccants, inert atmospheres, when necessary.

MR. EBERSOLE: I know, even for one-year storage, it remains static during shutdowns, and that is a problem, as far as inerted water.

MR. MICHAELS: I don't have anybody here from Inspection and Enforcement to answer that question at this point, but I am sure, you know, as I said, they have determined that the present maintenance and preservation requirments, the quality assurance program that is in place, is adequate, if looked at and approved by the regional offices at headquarters.

DR. REMICK: Dave, this is your subcommittee, and I only acted on your behalf, ran the subcommittee the other day, but a couple of comments I would like to make.

DAVbw

We determined that Staff still mandated a proposed policy statment to go out for public comment.

At the subcommittee meeting, of all those present at that time, the recommendation was that they didn't see a need for a letter at this time.

DR. OKRENT: I understand that is the case. I have a differing view, and there is a draft, possible letter, and I have been adding a couple of sentences as I listened to what Wylie and Michelson were asking about. And so I will put a draft letter on the table Saturday morning, and the committee can do what it wants with it.

DR. REMICK: Did you have anything else?

DR. OKRENT: I don't have any more.

DR. REMICK: Mr. Chairman, I suggest, unless there are other questions, we are finished with this topic.

MR. WARD: Very good. Thank you, Ted.

Let's take a 15-minute break.

(Recess.)

DR. OKRENT: If I recall correctly, in November, the committee had some consideration of the implications of Chernobyl. And in prior meetings, we've heard something about what was known about the details of the accident itself.

The subject today is ACRS comments on the implications of the accident at Chernobyl. On this subject, there was a subcommittee meeting held November 5, 1986, and one again Wednesday of this week, which I couldn't attend. And Forrest Remick chaired.

I'll ask him to add any comments that he wishes. At the time of the November 5th meeting, the staff had a draft document. Something was mailed out before the December subcommittee meeting. I'm not sure if what we have now is not only the complete document and it's in the book?

MR. SAVIO: Yes.

DR. OKRENT: There is a yellow draft of a possible ACRS letter on the implications, which includes some, but not all, comments provided by members to drafts one and two.

But, in any event, it presents for the benefit of the committee members at least one possible approach that the committee could take in responding to what is, I think, a formal request to comment; the Commissiners themselves wanted to hear what the ACRS has to say on this.

DAV/bc

So there is an agenda which shows that we run from 10:45 'til 12:45, according to what I have here -- is that correct? -- which includes a half hour presentation by the staff, a half hour discussion with the staff. I mean, there's an hour there. Maybe it will be an hour presentation on that point.

And then time for some general discussion.

I think, if members want to see points available for possible inclusion in the letter, which will be considered in the material tomorrow morning, I think it would be very valuable for them to advise me during this general discussion period, so I can try to draft something up to meet their wishes.

We normally drop things out quickly, but generating new paragraphs takes somewhat more time. In any event, let me leave my aspect of the report as such.

I recommend that you do look at the yellow draft while the staff is giving their presentation. Let me ask Forrest Remick first, and then other subcommittee members for any comments.

DR. REMICK: The things that I would add is, originally, the staff had thought they would be going to the Commission this month with this. It's our understanding now that they're waiting for the fact-finding report to be issued, the other report. So they probably now will not be

going this month.

So the urgency of getting the letter out this month is not there, although we can still do it. And I think we should.

But it has slipped a little bit. The other is there's a fair amount of material and it took a good half day in the subcommittee meeting, so we suggested to the staff that, rather than having a number of presenters, that perhaps one person should give a summary, realizing that he is going to need backup from other staff experts.

So, Brian Sharon, with the understanding of the fact that he won't be able to perhaps answer all the detailed questions, but will have backup people, but in the interest of saving time, we've asked him to give an overview, paying particular attention to not only the overview but any changes since the subcommittee last saw the reports -- either additions or modifications.

Those are the guidelines we gave the staff in the subcommittee meeting.

DR. OKRENT: Any other comments by subcommittee members, or questions?

DR. MOELLER: I have some questions that I'll try to bring up as we go along.

DR. OKRENT: If not, let's proceed with the staff.

DAV/bc

(Slide.)

MR. SHARON: My name is Brian Sharon, with the NRC Staff, the Division of Safety Review and Oversight.

I will be going through more or less an update of the implications report. I'll try and stress primarily the areas that have changed since the last presentation rather than go through the whole thing again.

I will rehash all of our conclusions, or assessment and our conclusions in each of the areas. If you have any questions, hopefully, we have the staff here that was responsible for preparing the various sections. And if I can't answer the questions, hopefully, they'll be able to.

Let me start out first by talking about the status of our factfinding report. You just heard that the Commission presentation on the implications report was postponed because of the need to get this factfinding report out first and on the street.

Just as a reminder, the report was prepared prior to the Vienna meeting last August, and it was primarily to aid the delegation going over there in becoming familiar with the RBMK reactor. And basically the Soviet power system and the Soviet philosophy as best we can understand it.

Basically, we dredged out all the available

DAV/bc

literature that we could on their reactors and safety philosophy, et cetera, and tried to compile it.

The report did serve its purpose. It was very helpful in preparing for the meeting. We went over there probably knowing as much about their reactors as one could expect.

We were surprised also, if you remember, that the Soviets were very forthcoming at the Vienna meeting. They gave us a very indepth understanding of their reactor and the accident itself. They gave us a rather large report on the accident as well as their design.

Subsequent to that meeting, the INSAG, the International Nuclear Safety Advisory Group, which advises Hans Blicks, Director General of IAEA, prepared a report on the meeting.

This report has now been completed. INSAG 75-1 is, I believe, the number of the report. We can get you a copy if you don't have one.

This also contained a good deal of faction information about the accident and the reactor as well as recommendations made to IAEA.

The bottom line is that there is really no new information that has been received since the Vienna meeting. The Soviets have not provided any more information.

DAV/bc

All indications are they probably won't. They would most likely like this thing to be put behind and forgotten.

(Slide.)

This is just a quick reminder of the content of the factual report. There are nine chapters. As you can see, NRC is responsible for the introduction, the summary, the accident scenario and the disbursion and transport of the fission products.

We've had a number of meetings on this with various authors. We had the draft chapters submitted last month. We put them together into a draft report. We reissued it to the participants and then told the review meeting on November 18th. That meeting, hopefully, was to finalize the report so we could publish it.

There were a number of comments and concerns primarilry from DOE, and they had a number of concerns also primarily with the FEMA and EPA writeups.

We agreed that DOE should go off and resolve their concerns with FEMA and EPA. We were not mediators in this report. This is not a report where everybody has a vote. And if there's a one that they vote on, it doesn't go out.

Each agency would most likely write a report on the implications to their agency. They would also have to

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look at the facts as they related to their agency. And rather than have four or five different agencies in the government issuing factual reports in their area, we decided it would make more sense for the government to issue one report.

The only thing we wanted to make sure that these participants understood is that everybody doesn't have a veto in this.

My understanding right now is that DOE has not met with either FEMA or EPA. The FEMA meeting was scheduled once and then cancelled. And they have not done anything to meet with EPA.

We're assuming that either their concerns were resolved or they just weren't in substance, that they wanted to pursue it. There is another meeting scheduled for the 19th, a week from today, which is designed to finalize the factual report and go ahead and issue it.

That meeting will be attended by Mr. Vaughan from DOE and Mr. Denton from NRC to make sure we all understand that the object is to finish it.

So we will hope that following the meeting on the 19th, we will probably just have some minor cleanup and editorial things to get rid of, and then we'll have the report available.

(Slide.)

DAV/bc

With respect to the implications assessment, let me preface it by saying that the Commission briefing on the implications was indeed postponed by Mr. Stello. He wanted to have the factual report completed before he went down and told the Commission about the implications.

The Commission meeting has been rescheduled tentatively for the week of January the 19th. So, in keeping with Dr. Remick's point that he made about a letter, we would probably need one no later than I would guess by the 16th of January.

That's the latest. That gives us one week in which we can reflect any comments into the Commission briefing. Obviously, if we had it earlier, that would be very helpful because we could factor it in that much easier.

Let me go through very quickly the implications assessment report. Just recall that our approach was, first, we wanted to identify the candidate issues that we saw coming from Chernobyl. We wanted to assess them by looking at (a) what was the current regulatory practice that the NRC was following? And, then (b) when we did the assessment, how did that relate to the regulatory practice? Was regulatory practice adequate? Was it not adequate? Did it need to be strengthened in some areas, or what?

From that, we then drew our conclusions as to what actions may or may not be needed. I would point out it

was tough but we tried not to get ourselves in the post-TMI type of mentality or anything that ever bothered us about nuclear power we shoveled into this report and said this is an implication of it.

We were very careful that the issues that we indeed put down are directly related to Chernobyl. And it was not really something that we were using as a vehicle to bring to the surface an ongoing concern.

For example, there are a lot of regulatory areas where perhaps some people thought progress was not going as well as it should or the staff should be doing more in those areas.

These concerns were already raised and available. In other words, people knew about them. And while we look at Chernobyl and say that, yes, it certainly has some relation there, it does not make this a new issue.

In other words, the issue was already there. So we shied away from those and we just tried to stay with those that came directly from the Chernobyl accident.

The report itself is not that extensive. It's not much over a hundred pages. And there's a summary in it. Each of the issues is assessed in the report, again highlighting what the issue was, why it's an issue, why we think it's an issue, what the current regulatory practice is and our assessment of whether we need to do anything and, if

we do, on what schedule?

DR. REMICK: Brian, I did not make the subcommitte but I like that format of having, you know, what is our current practice and what's the ongoing work. I thought that was very a logical format.

MR. SHARON: We felt that it presents it in a very clear way so you can easily see what the relationship is.

(Slide.)

These are the list of issues. Let me point out right now for you what changes have been made since the last time you saw this.

First off is that we have done a general updating of these issues. We've improved them. We've made modifications to the rewrite. For example, the reactivity accident section was rewritten based on some comments we go to provide a little more clarity and background as to current NRC practices.

So there was an overall just cleanup and updating of the report. The operations section did have some changes made. This was primarily to reflect the ACRS comments we received last month. However, it was not a radical change but more or less just to accommodate all the comments.

There is one new section, 1.7 Accident Management, which discusses the operator procedures, and so

DAV/bc

forth, and their relationship to severe accidents.

MR. EBERSOLE: Brian, in 1.4, is aux feedwater now considered an engineered safety feature?

MR. SHARON: Auxilliary feedwater? Yes, sir.

Under item Roman II Design, not very much changed here. On multiple unit protection, the last time we spoke with you, I think we had pointed out there was one area we were concerned about. And that is that multiple units, an undamaged unit, in order to shut it down, operators had to leave the control room.

The question was: Is that -- well, fine, if the undamaged unit are protected when they're in the control room because of control room design features, is there a concern about operators leaving the control room and going out into the plant? Is there a radiation hazard or otherwise?

We first said we'll look at that because we didn't know. We've done some additional assessment. The conclusion was that we really don't see that there is any hazard from the standpoint of being able to shut down and keep an undamaged unit shut down.

We can discuss that a little further later.

Roman IV Emergency Planning. This section was rewritten to be consistent with comments we received. It also was rewritten just to be more consistent with the way

DAV/bc

it was presented last month at the committee meetings.

The basic conclusions aren't really changed, but the writeup is a better reflection of the what the staff had told you.

And, then, last, we have a new section, Roman VI, which is Graphite Moderated Reactors. This is primarily the Fort St. Vrain assessment.

What did we learn from Chernobyl and how does it affect Fort St. Vrain?

We'll be telling you about that.

DAVbur

(Slide.)

Let me walk you through the overall conclusions. You have seen this before. It is the same slide I believe we used last month.

One is, and I think foremost, that there are no immediate regulatory actions needed. Our assessment shows that there is nothing that would prompt the staff or the Commission to take some immediate action by means of an order or bulletin or the like, or a 5054(f) letter.

Our conclusion is that U.S. reactors are protected against Chernobyl-like events, primarily by the nature of their nuclear design. We don't have the positive void coefficient. We have the fast acting shutdown systems with sufficient shutdown margin. We have a containment around our reactors, and we have taken a hard look at the human action safeguards, and we have concluded that we think we have done a pretty adequate job of assuring that the human actions would not result in the gross negligence, I guess, that the Soviets had with their operators in causing that accident.

Number two, we would take the Chernobyl experience into account in reinforcing some of the aspects of requirements already existing or being developed; in other words, stuff that is ongoing would certainly benefit by taking into account what we learned from Chernobyl.

DAVbur

These are in the area of human action controls, emergency planning, and containment performance.

The third conclusion is that there are some areas where we think some further study or research is probably warranted. This is not to say that there is a definite problem here, but we want to make sure that there is not.

Some areas here are like the reactivity accidents. We will explain this a little further, that there are some accidents that we want to take a look at to make sure that they are indeed low probability or precluded by the design.

Accidents at low power, shutdown, there are questions about the adequacy of the tech specs under those conditions.

And then source term characteristics. We want to make sure Chernobyl is factored into the source term research.

And then lastly -- and I think this is true not just of Chernobyl but of every event that happens, even like Surry -- we need to take into account all of these events that have occurred as a background, as a general background, as we go about our regulatory duties and making sure that we take them into account when we formulate rules, regulations, or make decisions on safety.

These are the general conclusions. I would now

DAVbur

propose to walk through the specific areas in each of the six main chapters.

(Slide.)

The first area is the operations. I think our general conclusions here are that we have taken a hard look at the administrative controls that we have in our regulatory process, and we think they are generally adequate to assure that there is a safe operating envelope for the plants.

This is not to say they are perfect. This is not to say they can't be improved. But we looked at them, and we say there is nothing here that says we need to either go out and fix everything immediately or shut the plants down until we do.

We looked at the procedural adequacy and compliance with regard to approving tests and unusual operations towards bypassing safety systems, engineered safety feature availability, and we also tried to take a look at a very elusive subject -- operator attitude toward safety and basic general safety philosophy.

This is an area we discussed at the subcommittee, and I think we don't really want to call it operator attitudes towards safety. I think it is more just plant personnel attitudes towards safety, which can be expanded to the utility's attitude towards safety, and someone suggested

DAVbur

how about the NRC, too?

But I think we have taken a look and we think that the attitudes that prevailed at Chernobyl, as the Soviets told us, would not and could not exist over in this country without being caught somewhere in the management chain and stopped, so that the type of violations that occurred there would not take place here.

The last one in management systems -- this is accident management, and I will get into this one a little later -- deals with the severe accident area.

Some of the things we came up with under operations was we think it might be worthwhile to consider a high level onsite nuclear safety manager with no other duties. This is a person who has some authority in the plant, but his primary purpose is to make sure the plant is running safely. He doesn't have any other duties.

I think the word is "ombudsman" that was used, although I think there is a question on that definition whether that is appropriate.

But we think this might be a very useful thing to have in a plant, one person who is -- I won't call him the person responsible for safety, but he is the person responsible for overseeing and making sure that the plant is operating safely and things are not being done to the plant for the sake of expediency or perhaps just production.

DAVbur

MR. EBERSOLE: Why isn't the function fulfilled by this so-called adviser? Is he not high enough in the organizational ladder?

MR. SHARON: What? The shift technical adviser?

The shift technical adviser I think performs some of that function, but again their role is really as an adviser to the operators. They are not in the position where they are stepping back and looking at safety from a global aspect at the plant.

DR. SIESS: He only functions after the accident.

MR. SHARON: Sam, I don't know if you want to add anything.

MR. WRIGHT: Sam Wright, NRC.

The shift technical adviser is of course onsite, and he responds to the situation when something goes awry. He is not the one that looks over procedures, planned actions, and that sort of thing.

We are looking really at a high level safety type who will do those kinds of functions.

DR. SHEWMON: We added one guy to fix the one that happened before. Now we are going to add another guy to fix the last one that happened.

MR. SHARON: I think what we said is we think it should be considered. We are not adding anyone yet.

DAVbur

Obviously, there's drawbacks, just what you said. Every time something happens we can't add another person, but there may be some merit to having such a person there.

This is not a person that maybe is going to be sitting and overseeing the operators.

DR. SHEWMON: There is an Onsite Safety Review Committee which was supposed to accomplish this in at least DOE-operated reactors and I would think on the commercial ones, also.

MR. SHARON: There is usually a plant safety committee.

DR. SHEWMON: Don't they review procedures for tests that haven't been done before?

MR. SHARON: Yes, that is their job.

Sam wants to add something.

MR. WRIGHT: I just want to add that he has competing duties. All these people who sit on that committee have competing duties -- production activities, maintenance, and that sort of thing, independence and oversight.

DR. KERR: You can't be completely independent of the plant operations because if you are you won't know what is going on.

The plant is there to operate and operate

DAVbur

safely.

If I see someone who is involved in safety and who is also intimately involved in operation, I would say they would have to know what it is going on, and it bothers me to separate the safety from the operation.

It seems to me the plant management has to be responsible for safety. Otherwise it doesn't get looked at. If you think someone else is responsible for safety and he is only responsible for production, then you have in a sense what may exist in some cases in the NRC versus the operators.

There are some operators who say, well, if we satisfy all the NRC regulations everything is okay. You could have one that says, well, if this guy doesn't tell us to stop it will be okay.

MR. WRIGHT: I understand what you are saying, and I believe those are the kinds of things we have to consider. It is not clean in every regard, but we believe that it warrants an examination.

DR. CARBON: Is this like the Japanese concept?

MR. WRIGHT: I am not really familiar with the Japanese concept.

MR. WARD: I would just like to endorse the general thrust of what Bill says. Separation between safety questions and nonsafety questions isn't always obvious.

DAVbur

I think this high level person, whoever, or organization or something needs to be involved in the overall operations so that he or she or it is in a position to recognize a safety issue even if it hasn't been identified by someone else.

MR. MICHELSON: The only problem, of course, is sorting out the difference between being involved and being responsible for. If you are responsible for production, then you might be a little clouded in terms of your thinking on safety. If you are responsible for safety, then you perhaps are a little clouded on production.

The two together have to work. They can't be one and the same person.

DR. KERR: There has to be a plant manager that is responsible for managing the plant, and hence I can't see giving some guy who tells the plant manager to shut the plant down, it is unsafe, and the manager thinks it continues to operate.

MR. MICHELSON: That is where his thinking is being clouded by his production responsibility, though. That is the argument you get into.

DR. CARBON: I would urge you to check the Japanese system. They do exactly what you are considering here. It would probably be of considerable help in finding out what their experience has been.

DR. REMICK: The Japanese, of course, throughout their history they have a mechanical engineer and an electrical engineer -- and I can't think of the formal name, but it is a person, basically by their utility law, that they must designate as kind of a professional engineer in that category, and they have added the engineering category to that.

In one of my trips there, talking to the MITI people, they did not necessarily recommend that concept for U.S. consideration, but they do have a long history of having a mechanical engineer identified, an electrical engineer identified, and now a nuclear engineer, a chief nuclear engineer.

DR. CARBON: One of those people -- I assume it is the nuclear engineer -- has this authority to shut down the plant if he feels it is being operated unsafely. Not only does he have the authority; apparently he will lose his license or be penalized if he doesn't take action if these kinds of events are occurring -- not an accident, but operating unsafely. He is liable to lose his license.

They have one in each plant.

DR. REMICK: Not on every shift, though.

DR. CARBON: No, not on every shift.

MR. SHARON: One analogy -- I bring up two points -- one analogy might be similar to a QA organization.

DAVbur

Again, that is sort of a separate organization that provides to some extent an oversight function and does report directly to a high level manager.

The analogy may not be perfect, but I think it is similar.

DR. REMICK: Not all of us would like to see you follow the QA example.

MR. SHARON: Keeping in mind that this is strictly a recommendation to take a further look at something, what we did is, okay, what went wrong at Chernobyl?

Obviously, these operators were able to compound the problem by each one agreeing or somehow not seeing that safety systems were being bypassed and stuff. They went on allowing it.

We asked ourselves the question: is there something that should be done or could be done in U.S. plants that might prevent this from happening?

One could say, well, obviously, if I had 10 safety people there that oversaw every aspect, that might be one way.

We also said, what about if we had one person?

What we didn't have is we didn't have any clear information that said this is no good or this is good. So we couldn't dismiss it. We couldn't say, well, obviously,

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we don't want to have the safety czar there because we don't have the information together yet that could dismiss it.

It may not be right, it may be wrong, okay? But we don't have the information today to dismiss it. So we need to study it more and see if it makes sense.

That is all we are really saying right now, is we want to study this more and see if it makes sense. Obviously, if we come up with a conclusion that says ultimately it makes sense, we will be back down here talking to you.

MR. EBERSOLE: Brian, I think you might have a look at the FAA's rule. They say if you have got two good men up front that is enough. That might be better than half a dozen. I am not sure.

MR. SHARON: That is certainly a factor we would want to take into account when we take a look at this.

MR. ETHERINGTON: If you ran exactly the same tests as were run at Chernobyl in the U.S. plants, would you have a runaway?

MR. SHARON: No, because we would presume we would not be bypassing, for example, all those protection systems.

MR. ETHERINGTON: Well --

MR. SHARON: Chernobyl would not have happened had the operators left the safety systems intact and not

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bypassed them. The plant would have automatically shut down long before they ever got to that condition.

Had they bypassed everything but the last scram, which was the reactor trip on turbine trip, we don't think the accident would have happened.

MR. ETHERINGTON: Let me say, suppose you bypass all of the safety systems on a U.S. plant. Would you have a runaway?

MR. SHARON: It depends on what accident. If you are saying, is there a way that I can, for example, withdraw enough control rods to make it go prompt critical?

MR. ETHERINGTON: Exactly the same experiment.

MR. SHARON: I don't think it would.

MR. ETHERINGTON: That is what I thought. Isn't that point worth making for the benefit of the public, anyway?

MR. SHARON: I think we could certainly consider it in the writeup. I think we tried to capture that in Chapter 2.

MR. EBERSOLE: I don't think that is true in the case of a boiler. We proclaim loudly, you know, we don't have that, but we don't say that we operate under boiler conditions. If you intercept the safety systems on a boiler, you are going to be in trouble.

DR. REMICK: But that is not a positive void

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coefficient. It is a positive insertion of reactivity upon the collapse of the void. That is not anything surprising.

MR. EBERSOLE: That can produce some spectacular consequences.

MR. SHARON: You are saying we can induce an ATWS in the plant?

That is basically what it is. The answer is yes.

MR. EBERSOLE: He said that I think you can intercept the scram system on a boiler.

DR. KERR: His question was if you ran exactly the same tests on a U.S. reactor would you have a problem? The answer is, no, you wouldn't.

DR. OKRENT: It is impossible to run exactly the same experiments because we don't have exactly the same reactor.

MR. EBERSOLE: If you disable the trip system.

MR. ETHERINGTON: I wonder why you say that, Dave. The experiment was to test the output from the generator during rundown.

DR. OKRENT: But if you look at the configuration of the feedwater system and the recirculation pumps, you know -- I don't know what the words "same experiment" means, but if you mean in detail, we can't do that experiment.

MR. ETHERINGTON: The point is they had almost an

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explosive runaway due to the big increase of reactivity.

My real question is could that occur in U.S. graphite reactors?

MR. EBERSOLE: You qualified it when you said graphite.

MR. ETHERINGTON: Did I say graphite? That is right.

MR. EBERSOLE: Graphite, okay.

MR. ETHERINGTON: That is what people are comparing them with.

MR. SHARON: We will discuss the Fort St. Vrain reactor in Section 6, I believe. So we will get to that in terms of reactivity accidents.

MR. EBERSOLE: The fire was a consequence of the original explosion, wasn't it? As a matter of fact --

MR. SHARON: We understand it was basically the pieces of hot graphite and fuel that landed on combustible surfaces, like the roof, that started the fires.

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MR. EBERSOLE: The fires didn't initiate the accident. It was a consequence of it.

MR. SHARON: That's what we understand; yes.

DR. OKRENT: Some might argue that we should let the plume just go up and carry it off.

DR. OKRENT: I think we'd better let Mr. Sharon move along, so that we have some time at the end for general discussion.

(Slide.)

MR. SHARON: Again, we would also state that there should be consideration of a program for accident management, particularly training and procedures for scoping with severe core damage and for managing and insuring that containment stays intact.

This area is mostly going to be addressed under severe accident policy implementation program, where we wil go out and do the individual plant examinations and look for the vulnerabilities. We think that one area that might fall out of this is to look at the need for accident management procedures for severe accidents, in order to help reduce vulnerabilities in the plant, as a result of operators not being able to properly cope with a severe accident.

This program, as you know, is under way.

If anybody has any questions, Dr. Rosztoczy is here. If anybody has any questions, he could probably

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elaborate a little more on exactly what areas we would be looking at.

The next area is, we would take a look at the administrative controls that govern technical review and approval of changes, tests and experiments.

What we are talking about here is the 5059 reviews. Whenever a plant does a change or wants to perform a test to the reactor or the plant, it must review it against the criteria in 5059, which are, does it create an accident of a different nature? Does it create an accident more severe than the one previously analyzed, or does it increase the frequency of an accident that was previously analyzed, and if it passes on any one of those three, then one has to do an evaluation, and it has to be submitted to the NRC for review.

This review, this assessment, is typically done first internally by the plant safety committee, PORC. One of the things that we recognized previously, while certainly, these committees are diligent in their review of this in making sure that the 5059 criteria are examined.

There seems to be a diversity in the depth to which one does an evaluation against 5059. Obviously, different changes to the plant would warrant different depths of evaluation.

One program that is ongoing is at NSAC, the

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Nuclear Safety Analysis Center, which is part of the EPRI organization.

It is developing a report, a guideline, which would try to standardize the 5059 reviews, in terms of, given what kind of changes you would like to make to the plant, what type of evaluation, what depth of evaluation should you perform. This would give you some degree of consistency in these reviews. So we are working with them. We are encouraging them to complete this effort.

DR. OKRENT: I wonder if that kind of general guidance can take the place of actual detailed knowledge of the plant design, the plant safety issues and so forth and so on by the engineers and operators, whom are involvee in such a review. As one just listens to operating experience, as we did this week, you see fine points about each plant, that were they not recognized ten years from now while somebody was proposing to do something, could create a severe vulnerability, things that would never be in general guidance.

MR. SHARON: I think you have raised a question that the Staff has already latched on to in some aspects. I don't know if you've heard any presentations on this PRISM program.

DR. OKRENT: I have.

MR. SHARON: I think this is a first step, and

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what it is is to try and computerize the PRAs.

DR. OKRENT: If the Staff was running the PRISM program, is that correct?

MR. SHARON: Right now, it is Research that is developing it, but I think the industry is very interested in it as well. I think, typically, if we use it -- if we take it on as a useful tool, the industry typically follows and picks it up, because they do not want us, number one, telling them when their plants are not safe. So we are hoping that the industry would take the lead in this area as one way of assuring that they don't put the plant into an unsafe condition or a high risk condition by making changes that haven't been properly evaluated from a PRA type of standpoint.

DR. OKRENT: You sort of lead me to ask a question.

Do you really think the PRISM program is the wave of the future, and why bother with IPEM? Why not move strongly in that direction for each plant?

MR. SHARON: I just don't think the PRISM program approach is ready yet for global. Number one, it has to be based on an existing PRA, but I think there is only about 30 plants that have PRAs today. We would have to get PRAs for the rest of the plants before it could really be applied. So it is something that is coming down in the future, and I

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don't think it is appropriate at this time to delay doing a vulnerability search, using the IPEM, the individual plant examinations, because this will be something that is done in a relatively short period, say, about a year and a half, two years, and hopefully, it is going to bring to the surface any glaring vulnerabilities which, I think, is appropriate to fix rather than wait several years before this method really becomes useful, and even then, we may not have PRAs for all the plants.

DR. OKRENT: Go ahead.

MR. SHARON: The last item here is the review of engineering safety feature status displays and their availability and see if there is any areas that are worthwhile for making improvements to it.

I think this is straightforward. We just want to make sure that these displays are to be available to the operators. Some of them are not available all the time. They are not required to be, so this is an area where we want to see if there is some improvement.

MR. EBERSOLE: Brian, may I ask a question?

Consider the effects of not having what I call having the red book, which is a detailed listing of prohibited actions and operations. There is a lot of freedom above a set of constraints that you presently have and ingenious operators can take actions, which are not

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explicitly prohibited.

I will give you a case in point.

In bypassing blow down on a boiler, the man's got 90 seconds to do something, but he isn't told what to do in that 90 seconds. There is a set of explicit prohibitions that prevent him invading the proper action of the automatic function. You can find these all over the place. The operator, in the absence of being absolutely forbidden a course of action, is pretty well on his own. Who is going to tell him not to. So I understand the need for the red book.

I would have thought such a thing like that might have prevented Chernobyl. It is a piece of paper in front of you, saying that you will never enter this regime.

MR. SHARON: My understanding was that it didn't exist, not maybe as a red book that says you will not do this, but for example, we have tech specs which say systems can't be taken out of service, and if they are taken out of service, then they go in an action statement that says these should be restored to service or you shut the plant down in so many hours.

MR. EBERSOLE: But it is very low key. It is not the kind of thing you can remember.

MR. SHARON: I agree that we should it under consideration, and it is something we should look at as a

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longer-term item.

Unless there is any other question on the operations, I would move over to II, which is the design aspects.

(Slide.)

And our conclusions here, the first one, which is the reactivity accidents, we took a hard look at these. We concluded that a) we don't have positive void coefficients that the Chernobyl reactor had. It is not there. We do have some positive moderator coefficients early in cycle, usually, but once you start burning up the fuel, these again, go negative. We did a systematic review of the design basis accidents, the transients that we look at in our FSARs. We did not see any of those that indicated there was something that we were doing wrong.

What we did look at is, if I carry these design basis events one step further, I guess, to extend them, for example, instead of having one rod withdrawal, I can withdraw many rods, or instead of one bank of control rods withdrawing, I would withdraw many banks at the same time.

There are ways that one could theoretically put reactivity into the reactor, in essence, of what we analyze -- what I should say is, a long time ago, a judgment was made, when the design basis for these plants was being developed, that these events were either precluded by

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physical features in the design or were of low enough, extremely low probability, that one need not consider them in the design base. It is kind of hard to go back and dredge out all the rationale for why these events were not put in.

Our look at them right now indicates that we still think they are in a very low probability, or they are, indeed, precluded by design features.

Nevertheless, we are a lot smarter now than we were 15 years ago when these things were probably first developed and came about. And we think it might be appropriate to go back and take a look at these, using the more sophisticated tools we have available.

We, at PRA, are just looking at it from a more deterministic standpoint with our knowledge of the design, and just reconfirm that what we did in the past is, indeed, correct.

MR. EBERSOLE: This doesn't include a review of the BWR ATWS, does it, which is a reactivity ATWS? This is not in the ATWS scope?

MR. SHARON: No. We were not really considering to do a complete reexamination of the ATWS, okay.

MR. EBERSOLE: I am talking only about the BWR ATWS, the configuration where they get the excursion when they have a transient. Okay.

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DR. MARK: From something I read, I was very impressed with the marvelous idea that their control rods first put in graphite to displace water, as they moved. I don't think we have anywhere anything with that characteristic, but that, by itself, was a beauty.

MR. SHARON: Keep in mind that I think when these rods had the graphite follower on them, remember that these rods were pulled out well beyond their normal limits. These rods were only supposed to be pulled out, so they were still like 1.2 meters in the core itself. So the graphite, I think, would have been sitting somewhere in the core region. So when you did scram, it would not be a positive insertion, whereas, in order to compensate for that xenon that was building up in that core, they pulled these rods well beyond the administrative limits. They were almost completely out of the core. So now when they did scram, that graphite had a movement to the core region before it moved out, and that is what caused that positive scram, as they called it, where you got the positive insertion before you got the negative.

DR. MARK: I don't argue with you at all, but it was an absolutely beautiful feature that they managed to achieve.

DR. REMICK: In some nonpower reactors, there are rod followers. Anything from aluminum to fuel, following

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the rods.

I don't think, in our power reactors, we have that.

DR. SHEWMON: These things you all call rod followers are rod preceders, literally. They come into the core first.

DR. REMICK: As the absorber goes out, you are adding something in, and that is how you look at it.

DR. OKRENT: By the way, just a small point, but when you say "positive void coefficient," that is not quite rigorously true, and your words were different. You may just want to think about you would present that.

MR. SHARON: Okay.

The next item, II, number 2, these are accidents at low power and shutdown. I am not going to dwell on this. What it is, is that we found several years during a review, I think it was of Wolf Creek, that one could possibly design basis events occurring at shutdown, where, in conjunctions with the tech specs that were being proposed, it resulted in consequences that were in excess of the original design base.

The one I would point as an example is the rod withdrawal accident. If you analyze the rod withdrawal, in which the worst case now is at startup, and they calculated the withdrawal of one rod bank and the tech specs said,

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when you are in startup, you either have -- some Westinghouse plants have to have two pumps running. Others have to have four pumps running.

The events were analyzed in the FSAR, assuming either two pumps or four pumps were running.

When you go to mode 4, which is hot shutdown, tech specs only required one pump to be running.

If you analyzed rod bank withdrawal with one pump running, the event got worse. It didn't turn into a core melt. You were just going from a condition where you didn't exceed D&B limit. You exceed the D&B limit.

What it did is, it pointed out to us, when you look at the tech specs, there may be tech specs in the shutdown modes that allow systems to be taken out of operation that may, indeed, be required or were required within the design basis to be functional. Yet the assumption always is that if I postulated the event at shutdown, it would be bounded by the event, when it is postulated to occur in power or at start up.

So we think it is appropriate, and we've done this review on McGuire. We went off, and we had a contractor, Iberando Scientech, take a look at the McGuire tech specs. I think they found something like half a dozen discrepancies, where the tech specs either precluded one being able to draw a conclusion that a certain event was

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bounded by the design base. So we think it might be appropriate for somebody, and I won't say who yet, whether it is the Staff or whether we ask the industry to go back and reconfirm that the tech specs are, indeed, consistent with the design base, as they presented it.

Another example was, I think, TMI, the proposed tech specs, to insure that there were at least two paths available for decay heat removal during shutdown.

One of their proposals was, we have a tech spec that says either you have on coolant loop running and one decay heat removal train available, or you have two decay heat removal trains available, or you have two loops available, two reactor coolant loops running.

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What happened is, under that tech spec that said you could have one reactor coolant loop running, you now had a situation where you had one pump that was running.

We said, if you postulate your design basis accidents in the shutdown mode with only one loop running, can you tell me that you bound all of your design basis events?

The answer was: We don't know.

So it's stuff like that we want to make sure is not being missed in the design of these plants.

MR. WARD: Brian, I guess I'm a little troubled there. I don't know how you're going to insist on or develop or encourage some systematic way of looking at those sorts of things. Even depending on tech specs, you know, you really need some layers of protection there.

Tech specs can be violated. Obviously, that's what happened at Chernobyl. Their equivalent of tech specs, several of them, were violated.

How do we have such high confidence that they'll never be violated in U.S. plants?

MR. SHARON: I think, if you go back to our conclusions and our assessment in Section 1, which is the Operations section, no, one cannot absolutely guarantee that an operator will never violate a tech spec.

We hope, and I think we discussed this at the

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subcommittee meetings, you can't regulate safety or you can't regulate the attitudes of people. We just can't write a regulation that says you will have a good attitude toward safety.

That's something that we just can't do. What you can do is you can try to provide the atmosphere and the incentives to make sure that these people are indeed sensitized to recognize that they can't just willy-nilly violate their tech specs if it's convenient to do so.

That's what we hope that we've done through the operating procedures, the training programs, and the like, for these operators, is to have a healthy respect and understanding of the safety of the plant and that the procedures, the rules and everything are there for their protection.

They're there for a reason. Not because the regulators want to be stinkers and provide a bunch of obstacles for the operators.

So the answer is, you know, you can't give a hundred percent guarantee but we're going to strive to give as much as we can.

DR. OKRENT: Brian, getting back to Jessie Ebersole's question on ATWS as a reactivity accident, I recognize there's a different kind of reactivity feedback, but I wonder if anyone has spent a little bit of time trying

to see if there are a set of circumstances that might make that first portion of the ATWS event significantly worse.

I'll mention something that may, in fact, be a very bad, a wrong direction. But, right now, I envisage the water comes in at a certain temperature. If the water came in colder, you might have to get to, in effect, more voids in order to be at the same average density, the same reactivity, more voids to collapse; maybe, therefore, greater reactivity to be gained in the first phase when you get an MSIV closure.

That might turn out to be a nonevent or a trivial event. But one might at least try to see if there are ways that could change the character of the ATWS from what it is as it's currently analyzed to see if it moves one into a temperature range for the fuel that you don't like, for example. Okay?

MR. EBERSOLE: As I look at it, the positive void coefficient applies to the case where you're operating with little or no voids and you get some more of them, you'll shut down.

But it's the reverse of the case where you always operated with voids and you might get rid of them fast. So it's just the inversion of the same problem that requires a reverse view.

And if I look at all those things in which I

might lose the voids faster, then I have heretofore recognized a system reverse would be the same problem.

DR. REMICK: But, Jessie, that certainly is analyzed in transients. Every time your MSIV slams closed and you collapse the void, you get a positive reactivity insertion.

DR. REMICK: Suppose you were running with colder water coming in at the bottom of the core and, therefore, had more voids to collapse?

MR. EBERSOLE: And a turbine trip also with loss of feedwater.

MR. SHARON: I wish Wayne Hodges were here. I don't know whether, you know, but my understanding is we have told the BWRs that they're not allowed to operate with an inlet temperature below some number to compensate for the loss of feedwater heating.

DR. OKRENT: You see, now we're getting to it again. The Russians were told they weren't supposed to do something. Right? Okay. It's one thing to count on a tech spec. It's another thing to see, even if they violate the tech spec, they can't really give you so severe an excursion you're worried. Okay?

MR. SHARON: Okay. The next item is the multiple unit protection. I guess the first thing is that, right now, we've taken a look at the control room design. A

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number of action items came out of the TMI accident regarding control room habitability under severe accidents. The issue was deferred into the general category I think it was Roman IIB8 of the action plan, which was severe accident rulemaking.

As you know, the severe accident rulemaking was superseded by the severe accident policy statement which we are implementing right now.

The efforts related to looking at control room habitability and design in regard to this realistic source terms if currently being addressed in a revision to the standard review plan, which I was told this morning is on my desk and I should sign today to get to the CRGR. That's a revision to Section VI.2.5.

VOICE: VI.5.5.

MR. SHARON: I'm sorry.

So that area is well in progress.

DR. REMICK: Can I understand something? Whatever it is that's in this new version of the standard review plan, with multi-unit plants as they're currently designed, if one got a release ala Chernobyl, would the other plants on the site be able to be habitable?

MR. SHARON: I think our conclusion was the control rooms would have high confidence to be habitable. They were not specifically designed, I don't believe, for

like a category one release.

DR. REMICK: I'm aware of that, but I'm asking the question and I don't know what you're asking in your new version of the SRP.

MR. SHARON: Glenn, do you want to say a few words about what's in there?

MR. SOFER: Glenn Sofer, NRC.

The SRP revision that we're talking about right now addresses simply the use of BWR suppression pools as part of the cleanup systems. We have not yet reached the question of looking at control room habitability under severe accidents.

That's to come next spring. So we haven't yet factored that in.

MR. SHARON: I apologize. I was getting confused between sections. That one is supposed to be the revision of 6.2.5?

MR. SOFER: I believe it's SRP 6.4, Brian.

MR. SHARON: Close enough. Okay. So we will be looking at that revision to the control room habitability using realistic source terms. And as Len said, that will be out in the spring.

We have also recommended it for future plants. We will be looking at this as part of the advanced BWR as well as the EPRI white water reactor requirements document.

We don't think they should be sharing systems that are part of a shutdown capability to prevent accidents of one unit affecting accidents at a second unit.

MR. EBERSOLE: Let me ask you a question about that. That implies condemnation of sharing at large. Most sharing is designed to save putting an extra system in a unit.

If you do that, you know, once you've put an adequate number of systems in the unit, I don't see anything wrong with them sharing the flexibility that happens to the others accessible to it.

MR. SHARON: Let me qualify this a little bit. I understand what your concern is. What we're seeing here is that if you're going to share equipment, make sure that an accident, for example, at one unit is not going to take out the second unit.

In severe accidents, obviously, there are some benefits to sharing.

MR. EBERSOLE: Just don't share to be cheap and not have enough facilities at the first unit anyway.

MR. SHARON: Yes.

MR. EBERSOLE: Most sharing is done for being chinzy. That's the wrong way.

MR. SHARON: You're saying if sharing is there, obviously, which enhances safety, we don't want to preclude

that.

DR. KERR: If that's the case, it seems to me that sharing is not the thing that you should be discussing. It's inadequate equipment, period.

MR. SHARON: This may be a poor choice of words, and we can certainly try and clarify that.

DR. OKRENT: Uncontrolled interconnection.

DR. MOELLER: Well, now, if the control room habitability system in a second unit was adequate to protect the operators from an accident in the first unit, can we also assume then that if the control room in the first unit has an equivalent habitability system, that it's protecting its operators?

Can we just assume that your review will assure that the operators in the control room of the unit having the accident are also protected?

MR. SHARON: Yes.

DR. OKRENT: If the accident didn't somehow damage the integrity of the control room itself.

DR. MOELLER: Correct.

MR. SHARON: The last time -- I just wanted to point out, which we had identified last time, was that if operators had to leave a control room to shut the plant down, which they have to do -- one has to go out of the control room and wrack in the motor operators for the

accumulator isolation valves which are locked open during operation -- Appendix R requires for most plants, I believe, that the RHR isolation valves be locked closed because of fire concerns; mainly, because they don't have the separation and that a fire could potentially cause both valves to open.

So these are locked close by removing power at the motor control center. Again, an operator would have to go out and restore power to the motor control center in order to open those valves.

So an operator does have to leave the control room in order to bring a plant to cold shutdown.

The concern was that, well, if I had an accident in an adjacent unit, would I make the environment inhabitable so that the operator could not go out and take these actions?

We took a hard look at this and there's a number of arguments that would indicate why this is not really a problem. This is why we eliminated it.

One is, even if there was some sort of a radioactive environment, these actions are not actions where an operator has to go out and stay there. The operator goes out, takes the action and goes back.

So these operators can suit up with breathing apparatus and the like, and go out and take these actions,

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presuming that there was a harsh environment.

Number two, we don't really think there's going to be a very harsh environment over in these areas in the other plant.

And, three, if you had an accident at one unit, unless you're talking about something that proceeded very quickly and failed containment very quickly and had a big release, but if you look at the more probable type of scenario where one has slow over-pressurization and a late failure, these operators are going to know that there's a problem. And they're going to go and shut down that other unit.

And they're going to be able to go out and take these actions before there really is any harsh environment. So all those factors coupled made us believe that we really didn't think that there was some definitive action that could be taken in the design that would make those areas more habitable.

For this reason, we have eliminated it from our list of areas that we want to study further at this time.

MR. EBERSOLE: In that connection, it may well be that in severe accidents, the containment is breached into the equipment room because of the design of the penetration system.

That produces a concentrated ATWS. You know, a

plume, which is a little hasty to go into.

Are you with me?

MR. SHARON: I understand. I think, from that standpoint, that may be something that would come out of the severer accident vulnerability studies.

We would hope, you know, that, in the plant walk-downs and the like, that this stuff would be observed. If there was a failure of the containment, a weak point.

MR. EBERSOLE: I think most of them are that way.

MR. SHARON: That it would fail into a place.

MR. EBERSOLE: In other words, it's not an atmospheric event between the equipment room and the containment.

MR. SHARON: That may be an area we can single out if we see that.

DR. KERR: If I understood what you were talking about earlier, you were not talking about operators doing something that would damage the plant. You were talking about what operators would do in the adjacent plant, weren't you?

MR. SHARON: If you've already melted the core, you're not really worried about trying to get RHR going, I don't think, or trying to worry about an accumulator isolation valve that's open.

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Okay, it's in the undamaged units. So the real question is:

Can the containment in a damaged unit fail such that it could perhaps produce a harsh environment into the areas that one needs to go in the undamaged area.

MR. MICHELSON: Or that equipment needs to operate in the undamaged unit to prevent damage to the other unit.

I doubt that you've chased this sort of thing. I asked a few of these questions relative to generic issue 61, which has a potential containment failure, whether or not the environmental effects of the containment failure have been included in the examination.

And the answer was: No. For generic issue 61, I was surprised. They had a strange-looking probability of melt-down, even if you get containment failure, like only one chance in 10.

I asked: Does that include environmental effects?

The answer was: No.

MR. SHARON: The last area under design is Fires. The concern here was again at Chernobyl. The firefighters were being asked to fight fires in the presence of radiation. A number of things they did. One was they took off their protective clothing, which further exposed

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them to radiation. One of the things we want to make sure is that we have adequate provisions at nuclear plants to cope with fires.

We've taken a look at the firefighting measures as provided by Appendix R, and adjusting other firefighting measures that are put into the plant design.

We think that the operators have the proper protective equipment, the self-contained breathing equipment, turncoats, and so forth, to fight these fires. It's available. They have trained fire brigades.

These fire brigades have a health physics type on them which advises them regarding radiation if they're fighting any sort of a fire that has radiation associated with it.

The design is such that there are fire-retardent materials on the roofs of nuclear plants in the U.S. They use standpipes for hooking up and getting water up to higher elevations.

One of the concerns at Chernobyl was that they had no way of getting firefighting equipment to the roofs of the building other than by ladders.

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I don't know whether the Chernobyl plant or others have standard stand pipes for being able to hook up fire hoses on the roofs, there are access locations to get to the roofs of the buildings, and the like.

And I think the biggest thing we have to remember -- and Jesse pointed this out before -- and that is this was a unique situation where I had 30 fires flaring up all at once in various parts around the plant because of hot fuel that was spewed out. So I had a much, much higher radioactive source term in conjunction with the fire than I could probably ever postulate in a U.S. plant because we just don't see how one is going to get this kind of an accident that is going to, A, spew the core around and, B, spew it around in such a manner that it is going to start a lot of fires.

DR. MARK: You did refer -- and it has been referred to before -- to protective clothing. Now, there is no protective clothing that does a damn thing about gamma rays.

MR. SHARON: That is correct. Gammas are not going to be stopped by any kind of protective clothing. Just the betas.

DR. MARK: It was beta burns that did them in?

MR. SHARON: I apologize, Frank Cargill is not here.

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I asked him about this. He told me really it was the gamma dose for some of them that really caused their deaths.

MR. WARD: What is a beta source that isn't also a hard gamma source?

I have trouble understanding that.

DR. KERR: At least memory would indicate that some of the burns that these people had were not just thermal burns.

MR. SHARON: That is right. There were a lot of beta burns. That is what I understand. It is because they took their clothing off it was so hot, and without having the protective clothing on to stop the betas....

DR. KERR: There still is not much to do with a gamma dose.

DR. MARK: I was just curious about this because there must have been a ferocious gamma dose available for which protective clothing would mean nothing.

MR. SHARON: That is correct.

DR. KERR: One of the problems they had, I think, was treating the skin burns, particularly for people who had almost completely....

DR. MARK: Once in the hospital, this would be tremendous.

MR. WARD: Did you understand why there was such

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a high, apparently, beta field?

DR. KERR: Sure.

DR. REMICK: The smoke must have contained a lot of particulate matter.

DR. MOELLER: I presume this was skin contamination that had the heavy beta dose.

MR. SHARON: That concludes the section on the design implications.

The next area is III on the list. This is the area of the beyond design basis containment and venting.

(Slide.)

MR. SHARON: Obviously, Chernobyl didn't have a containment as we know it over here. I don't want to say what they had. I am not sure. I would call it more of a confinement type of arrangement.

It had no pressure retaining capability. It did not have any sort of a spray system or venting system as far as I understand, and because of that it is very difficult to draw any sort of an analogy between containments around U.S. reactors and Chernobyl.

However, it did point, I think very rightly so, to the benefits of a containment. We have taken a look. We think that if there was a large, dry containment around Chernobyl, it would have withstood the explosive force and not released anything.

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I think what it has done is it has just reemphasized the need to look at containment performance in our evaluation of severe accidents.

We are doing that right now. As you know, just this afternoon isn't there a discussion on the containment performance of the Mark I's by Mr. Bernero?

There is an effort underway within Research and NRR on the containment performance objective with regard to the safety goals and also with our severe accident policy implementation program.

We would be looking at filtered venting for some plants as a mitigation strategy to perhaps reduce vulnerabilities.

So with regard to the implications from Chernobyl on containment, I think I would conclude by saying it reinforced our need to take a hard look at containment performance during severe accidents, and we will be doing that.

The next area is under emergency planning.

We have taken a look at what the Soviets did and what they had in the way of emergency plans following Chernobyl.

They distributed KI to the population. We were told at Vienna that they had an evacuation plan, which was quickly assessed and found not to be adequate, and they had

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to devise a new one on the spot.

I think we later learned, or at least have inferred that that plan that they had was really a civil defense type of plan and was not specific to an evacuation plan for that reactor.

We have taken a hard look at the implications with respect to the source term that Chernobyl had and the type of failure.

Again, remember, it was a mechanical dispersal of the core. It literally blew the top off the reactor building and spewed everything straight up in the air.

The heat of the graphite, the air flow and the like caused -- I think, Jesse, you were pointing this out -- produced such a massive heat source and a thernal source for a plume to loft all of these particulates into the upper atmosphere.

This is one of the reasons that this stuff was able to get over to the Scandinavian countries as fast as it did.

The Soviets told us in Vienna that they burned something on the order of 25 percent of the graphite in the core.

So when you really look at it, Chernobyl had a really unique source term.

MR. EBERSOLE: Would it have been worse if they

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didn't have the fire?

MR. SHARON: In terms of like a meltdown?

MR. EBERSOLE: If they had their explosion but it never caught on fire?

MR. SHARON: That I don't know, quite honestly. I imagine it would have dispersed it a lot differently had they not had the heat source. But we really don't know.

They were asked at Vienna how much of the graphite did indeed burn. They estimated 25 percent, but how good that estimate is I don't know.

As you know, prior to Vienna there was a lot of controversy about graphite burning. There were experiments done out, I believe, around Hanford, where DOE had some contractors put an acetylene torch onto a block of graphite. They sat there, they held it, and there was swelling, I guess, and a depression in the graphite block. But it cooled so quickly once they took the flame away that it didn't burn.

MR. EBERSOLE: There has been a great to-do as to whether we got the graphite or not. I am not at all sure that if it didn't burn it might not have just settled around the plant and we might not have had a worse accident.

MR. SHARON: I think the fires were dispersed, and they said they were out by 5:00 o'clock in the morning. Yet there were still releases going on until a week later,

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which led me to believe or would lead me to believe that the heat source was coming from the core region itself, and it really didn't stop until they started dumping all that stuff on it -- lead, dolomite, and sand.

MR. WARD: The graphite fire wasn't out by 5:00 in the morning?

MR. SHARON: No.

MR. WARD: Those were the incidental fires?

MR. SHARON: Like the tar paper on the roofs and stuff.

So, anyway, with the uniqueness of the source term and the fact that they did not -- if they had an evacuation plan specific to Chernobyl, they certainly didn't tell us about it, and right now we don't know if one existed or not.

In any event, in looking at it, we concluded that we think that the 10-mile plume EPZ continues to be adequate. Then there is the 50-mile EPZ for the ejection pathway, which we also think is adequate.

Our emergency plans do not preclude taking actions outside of that 10-mile zone if they are deemed necessary. It is more of a guide than some sort of an absolute rule.

As you know, there is a number of efforts going on regarding EPZs and their adequacy or whether there is a

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basis to change them based on new source term information.

There is a lot of work going on in Research. There's some new regulations being drafted, I understand, in this area.

NUREG-1150 will factor into this, and I think our conclusion is that we need to take the Chernobyl accident into account and study it to infer what information we can when we are indeed looking at the EPZs and any potential changes to them.

DR. MARK: Do you have a decent estimate of the maximum dose at a 10-mile distance?

MR. SHARON: I think we do, but I don't know it. I would have to go back. I think in our presentation that we made, I believe September 12th, to the full committee there was a table which listed doses for various communities at certain distances from Chernobyl.

Maybe if the ACRS staff could find that presentation, that table would probably give you what you want.

DR. KERR: It certainly wasn't over 100 rems, was it?

MR. SHARON: No.

DR. MARK: Which bears very directly on your comments just now of how our EPZs look because if that dose was, as I believe, more like 10 rem --

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DR. OKRENT: It was bigger. There were numbers more than 50 reported.

DR. KERR: Not at 10 miles.

MR. WARD: As I recall, they told us the average person in the Town of Pripyat got 3, but there were reported doses up to 50.

DR. OKRENT: In any event, the unusual nature of the first part of the release, going up very high and going past nearby people when you have a lot of the short-lived activity, I think had that been released at ground level things would have been quite different.

DR. MARK: That is right.

DR. MOELLER: A couple of comments.

One thing, of course, the accident clearly showed was movement of the contamination or the radioactive material extended over wide areas. This brings me to the point that for that reason it emphasizes that it is important for different countries to have compatible action guide levels, and it would seem to me that one of the lessons to be learned here is to look at Canada and the U.S., which do not have comparable protective action guides.

So if we had an accident anywhere along the Canadian border that caused material to move from the U.S. to there, they might be taking action under one level

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and we might be taking it under another.

And at our Wingspread meeting the Europeans pointed out to us that one of the major problems was the fact that instead of the various countries observing what they had agreed on as protective action guides, they set ad hoc lower, reduced action guides, and I think confusion such as different action guides for Canada and the U.S. would quite likely lead rapidly to this kind of chaos.

So to me that is an important aspect.

Another item was that they did use KI, yet here in the United States we allow each state separately, individually to make the decision on what action level do you administer KI or do you use it at all. Again, that is inviting chaos.

We need to have a national policy, I would think, on KI, or at least NRC ought to encourage us to have a national policy.

Another interesting thing to me from Chernobyl -- and maybe it is obvious -- was that ingestion did not prove to be of any consequence whatsoever as far as any acute effects are concerned, and we probably knew that. But I think the fact that the accident clearly pointed it out is rather important.

DR. REMICK: Dade, when you say "ingestion," you mean --

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DR. MOELLER: Eating or drinking.

Breathing might have been inhalation, although I am not sure that inhalation -- we would have to check that, but I am not sure that inhalation --

DR. REMICK: I think it did on some of the firefighters.

DR. OKRENT: Ingestion is a problem.

DR. MOELLER: Long-range, absolutely. But I think there is a lesson there, and then, lastly, in reading your material, of course you pointed out that in the USSR the military played a major role in helping, I gather, with the evacuation even and other protective measures, or certainly in decontamination.

I would imagine that our military could probably be useful and beneficial, and I would think, therefore, that on a national scale when we are thinking about emergency preparedness we ought to incorporate DOD into our thinking and find out what they might be able to do with us or for us.

MR. SHARON: I think Shelley Schwartz from I&E would like to make some comments.

DR. SCHWARTZ: On your first point, on the conscious need to compare action guides, that is an area that I don't think really fits into the post-Chernobyl look. It is one that we were working on and discussing with

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Canada pre-Chernobyl for the plants close to the border. So we understand what kind of actions would be taken.

In fact, I understand that the Fermi reactor has good relations with their counterparts across the border, and they have been working --

DR. MOELLER: To try to get uniform measures.

DR. SCHWARTZ: It will take a federal effort to work with their counterparts in Canada to decide with one where we should have the same PAGs or to at least understand the differences.

I am not sure in the scheme of things if we will ever drive Canada or Canada would drive us to assuming that we are going to have the same PAGs, but it is an issue, and we are continuing our discussions with them.

DR. MOELLER: All of this, as I keep playing a broken record, ties into the revision of 10 CFR 20 and ICRP 26.

DR. KERR: Do the protective action guidelines in Canada say that they would take action on a higher level or a lower level?

DR. SCHWARTZ: I don't know what those numbers are.

With respect to the policy on KI, the federal government does have a policy on KI. It has allowed the states to make their own decision with respect to

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stockpiling predistribution of KI.

The federal government provided the information to the state and local governments and a cost/benefit analysis of stockpiling with respect to KI and came down on the side that it was not a federal requirement that it be stockpiled.

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There are some six states now that have pre plant. Two come to mind now. One is Tennessee, and they do predistribute, and I understand Alabama does stockpile.

DR. MOELLER: Yes. We had a report from the Director of Emergency Planning in Alabama. If an accident occurs, the first thing is, you drive into the local town and go by the city hall, pick up your KI bills. Then you go back and evacuate.

(Laughter.)

DR. MOELLER: Do we have -- and obviously, we do, from what you said, have a table for each state and where it stands on its KI policies?

DR. SCHWARTZ: I don't have that table, but the Federal Emergency Management monitors the off-site consequences, the off-site planning, and we can certainly ask them to provide us with such a table.

DR. KERR: I think that requires a little bit of explanation. I don't know whether you have some relative in Alabama or they move more slowly in Alabama than in other states, and people have the time to go and get the KI pills and go back and evacuate before the accident becomes more serious.

DR. SCHWARTZ: The point you make, Dr. Moeller, is the point we are all concerned about. Should there be distribution or stockpiling. Particularly stockpiling with

a shelf life. How long would it be good, and then how you distribute it after an accident?

Tennessee made the decision that they would predistribute to the folks around the facility, and I take it at Sequoyah, they've gone around and done it twice now over the years to make sure they had it.

It was their decison.

DR. MOELLER: In addition to whether you agree to distribute or stockpile, there is the question at what projected dose should it be administered. To me, that would be helpful, if NRC or FEMA hesitate to do something like this, the NRC does provide supporting funds, say, in the NCRP. You might ask them to independently assess, evaluate and make some recommendations. That would be a way, you know, to get it done.

DR. SCHWARTZ: That is a good suggestion, because the medical community, as you know, is discussing it on both sides. I think it is an excellent suggestion.

DR. REMICK: As far as stockpiling and distributing relate to the general public, the NRC does have a policy for plant workers.

DR. SCHWARTZ: Thank you. There is a policy to assure that plant personnel can have stock piles for their use, in case of an accident.

The last point I would like to make, if I may is,

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as far as the military, we ought to fathom the military capability, in case we have this type of thing in the United States. I contend that we essentially do have that kind of tie-in through the Federal Radiological Emergency Planning program, where, in the event of an accident, they would put together what we call a federal response center, where all the agencies who are responsible for radiological response will be there and, in fact, even before this, I remember, at one point in time -- and this was back at the TMI event, when there was need for military transport to bring some people to the site and that happened, I believe the resources there and the question as to whether we need to detail that capability is still an open question. But certainly, the transportation and personnel and manpower aspects are available at the federal level.

DR. MOELLER: Well, to know what that might do to assess an evacuation and to know what they might do in washing down streets or things.

DR. SCHWARTZ: Sure, when the governor gets involved, the first thing he is going to do is, he is going to call out his own national guard, which he uses in all those types of emergencies, and bring them into play as well.

DR. MOELLER: Thank you.

DR. SCHWARTZ: Thank you.

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MR. WARD: Dade, on the subject of KI distribution, this question came up at the subcommittee meeting. The Soviets apparently distributed, I don't know, 100,000, a couple of hundred thousand doses of KI. Their contention was that there were no adverse reactions.

Is that reasonable?

DR. MOELLER: Well, I presume it was one of our first major mass tests of that. The medical profession does tell us that what is it, 1 percent or something of the population will have an adverse reaction, and then, though I asked what is the adverse reaction, you know, I asked several M.D.s, and if my memory is correct, it is not all that bad. They might be ill for a short period of time.

DR. KERR: My impression was that the Russians didn't say they didn't have any adverse reactions. They said they didn't have any toxic reactions. Maybe that is my memory of the language.

DR. SCHWARTZ: I don't mean to be argumentative, but I think the words that I heard were no adverse reactions, but I will go back and check.

DR. LEWIS: I think that 1 percent number was people who had allergic reactions, and then there is a smaller percentage who may have serious ones. I don't remember the numbers, but there are small numbers.

DR. SHARON: The last area is under the general

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heading, Severe Accident Phenomena.

Again, because of the uniqueness of the accident itself, there was a mechanical dispersion, not a meltdown, which we calculate over here for severe accidents. I think the only conclusions that we drew is that, one, a, we have to recognize that this plan did have a mechanical dispersal mechanism, and with regard to analyzing severe accidents here, I think we just have to always keep in mind that if one does add enough energy deposition in the fuel, that one can, indeed, get a mechanical dispersal mechanism.

We shouldn't lose sight of that.

DR. OKRENT: There are other ways of getting mechanical dispersal mechanisms for lightwater reactors, besides a reactivity excursion.

MR. SHARON: Through steam explosions, for example.

DR. OKRENT: If the containment goes first, then you get not too big a steam explosion. It is a dispersal mechanism.

MR. SHARON: I think that is what we are really trying to say, is that one has to recognize that there are mechanical dispersal mechanisms.

The second one is that we noted that the chemical stripping effusion products from fuel particle surfaces did occur via oxidation of the fuel to U308. I think it is just

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something we have to keep in mind when we go through the source term evaluation.

The last area that I will talk about, and this is a new one which we added --

(Slide.)

-- is the implication of Chernobyl moderated reactors in the U.S. There's a number of graphite moderated reactors in the U.S. either in operation or contemplated. The Fort St. Vrain HTGR out in Colorado is the only operating commerical gas-cooled reactor, graphite moderated gas-cooled reactor in the United States.

There is the DOE reactor in Hanford, which I am not going to talk about, because we have not evaluated that. DOE is doing that. It is their responsibility.

I would point out that research programs that DOE is sponsoring on graphite reactors that might be applicable to the DOE reactors, we are certainly trying to take that data and use it as best we can. In other words, we are trying to benefit from their research on their graphite-moderated reactors, but we have not specifically looked at any implications of Chernobyl on their reactors.

The only other one that is, I guess, of some interest is that DOE, in their advanced reactor program, is proposing a modular HTGR as a concept; however, that is in such early stages of design, that we really didn't want to

go in and do a complete impact assessment at this time on that reactor.

So I am going to talk mostly about the Fort St. Vrain.

We took a look at Fort St. Vrain and its design.

Number one, we could not come up with any design weaknesses that had a nexus or basically derived from the Chernobyl accident, albeit it they both use graphite as a moderator. I think that is about where the similarity ends.

As you know, Fort St. Vrain uses inert helium as a coolant. They don't have the oxide core. They have a ceramic core. The fuel particles are encased in -- what is it, Peter, a silicon?

MR. WILLIAMS: Silicon carbide is one of the major barriers. There are pyrolitic graphic layers also.

MR. SHARON: It doesn't use water, so we don't get the steam.

Dr. Okrent?

DR. OKRENT: It has a steam generator, and if you had a gross rupture of the steam generator, you could have water ingress into the core pressurization, hydrogen generation, a variety of things that are not on your list.

MR. SHARON: We fully recognize one can get water ingress into these bores and add reactivity by virtue of

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adding water.

This is an undermoderated reactor. Chernobyl was an overmoderated reactor, so that adding water will increase the reactivity.

DR. OKRENT: It can do more than that, but in any event, the reactor happens not to have a containment. If you don't call the PCRV a containment, and I don't think you can.

MR. SHARON: I understand that was a subject of debate some years ago.

I would just point out, it has a pressurized concrete vessel around it. Some people consider that to perform the containment function. There's a steel liner inside of the vessel, and again, the access to the vessel itself -- I shouldn't say the vessel, but the access hatches, there's two places, one on the bottom and one on the top.

MR. WILLIAMS: They call them penetration. Penetration closures, and as I am sure you know, they are double penetration closures. These are monitored, constantly monitored, I think you are well aware.

DR. OKRENT: I am well aware. I was there when Fort St. Vrain was opened.

MR. SHARON: Anyway, we looked at the potential for fire in the core. In order to get a fire, one would

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have to have both of these penetrations fail, so that one could get the air in and get the circulation to keep the air flow in and out from the bottom to the top.

It was looked at. The Fort St. Vrain people said that in the event that they had the unlikely failure of both penetrations, so that they actually got that condition, they could flood the lower compartment up, so that they could seal off that lower penetration and, therefore, stop any air going into the core, so they could turn off the fire, basically, by starving it of air, if they had to.

We did not see any new concerns relative to the severe accident phenomena that might be associated with an HTGR; however, I think it did emphasize and point out to us a couple of things. One is, that Fort St. Vrain doesn't have a PRA, and we certainly think one would be desirable, particularly if the plant is going to continue to operate.

I know there is some question right now. Mark, do you think it would make it safer?

MR. SHARON: No, I think it would help us understand the relative level of risk. I would point out that Fort St. Vrain will come under the Commission's severe accident policy implementation program. We anticipate that they will receive a letter saying that they have to do plant specific vulnerability examinations like everybody else. They would obviously not be able to use the methods

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developed by the IDCOR people, so they would have to do something that was applicable to their plant.

DR. SIESS: I thought we had done something similar to a PRA some years ago. Accident progression initiation or something else.

MR. WILLIAMS: Peter Williams, NRC Staff.

There have been a number of PRA studies. There have also been some very limited PRA studies of Fort St. Vrain, both on design basis accidents, pressurization accidents and the core heat-up accident, have been analyzed by PRA. There is a good background for performing a PRA on gas-cooled reactors, but there really has been no formal one for Fort St. Vrain.

MR. SHARON: Lastly, we are interested in graphic thermal stress experiments, which would be helpful in understanding the performance of the graphite blocks under strong thermal stresses. One can get very high thermal gradients in these cores, because the graphite has such a high heat capacity. The coolant cools down very quickly. The graphite stays at high temperature, and you get strong gradients across the graphite, and I suppose, if these got strong enough, you could actually induce breaking up of the graphite.

Then the question is, what happens the? I believe, is it DOE that is doing some of this, Peter?

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MR. WILLIAMS: Some years ago, as a result of some calculated thermal stresses in the course of the work plans, our research organization sponsored a study at Los Alamos on graphite thermal stress. They have done work on the type of material that makes up the graphite posts. The consideration is to extend this study into the PGX graphite, which is material that is used in the core support fuel.

MR. SHARON: This is our other conclusion, that we would certainly encourage these experiments to go forward.

That concludes my presentation on the implications. As I said, we are right now in sort of a clean-up mode with the report.

We want to just clean it up, make sure that we've dotted all the "i's" and crossed all the "t's" on it. We would certainly look forward to getting a letter from the ACRS. Favorable would be better than not favorable.

And again, we would like to get this package together, so it can be published sometime in January, and we would like to go to the Commission with these implications and get their blessings on them right now for the week of January 19th.

If there's any other questions, I'll be glad to try and answer them or Staff here can.

DR. REMICK: There were several questions that

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came up in the subcommittee meeting.

One, Dr. Lewis raised the question of whether, under your discussion of tests and a comparison of the U.S. of unreviewed tests, whether there was consideration of surveillance testing in that section, and whether surveillance tests were perhaps conducted too frequently.

I was wondering if the Staff has made any consideration of that.

The other topic was one, the wording emphasized, the licensed operators. And there was discussion of, aren't you really talking about plant personnel beyond licensed operators.

Has the Staff given any additional consideration to that?

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MR. SHARON: I will apologize because I was out of the office all day yesterday. So I haven't had a chance.

My guess is that there has been nothing formally done, but we have made note of it, and I think it is something we will want to consider and try to include, as I said, as part of the final report writeup, to take into account the concerns that were raised by the ACRS subcommittee as well as the full committee and work them into the writeup.

These two areas would certainly be two that we would want to consider.

One, I will just repeat, was the surveillance testing. There was a question raised by Dr. Lewis. They were running a test at Chernobyl, and there is a concern that sometimes we run tests just for the sake of running tests and we may do more harm than good.

I think we acknowledge that was certainly a legitimate observation.

As you know from the diesel testing and the people who have complained about the fact that when you require a fast start test it may do more harm than good, I think it is an area that we have to take a look at in terms of what is the optimum type of testing from a safey standpoint.

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It is a difficult question, but I don't think it is one we should just put aside and say it is too tough to answer.

DR. OKRENT: I was under the impression from Chernobyl that they were not doing surveillance testing, that they were doing something very different from surveillance testing.

MR. SHARON: They were doing what typically, I think, we would have required as a startup test. In other words, they were trying to demonstrate that their safety system worked as designed.

DR. OKRENT: I will buy that, but not surveillance testing.

MR. SHARON: No. It just brought to light that they were doing a test because they were required to do a test and because if they were doing a test it brought about an accident.

Now, that is not saying that that was bad. It is certainly a matter -- it raises an issue that I think we would want to explore a little bit more and say, as an implication should we go off and study this or do some more work in this area or their ongoing programs, for example.

DR. REMICK: I might have mischaracterized what Dr. Lewis was talking about by calling it surveillance testing. I am not sure you limited it to surveillance

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testing when you were talking about unnecessary testing.

DR. LEWIS: No. We agreed on that yesterday. Just the general question of they were indeed not doing surveillance testing; they were doing a demonstration test to in a sense relieve themselves from regulatory requirements.

That is what they were doing. They were trying to evade a regulatory requirement for fast start of their diesels.

But it just raised in my mind the whole question because there exists a theory of reliability that you can exploit to some extent to give you reasonable testing intervals, and my question was not quantitative, but because of the fact that I have seen so many things happen as a result of surveillance testing, then I wanted to know how long it has been since it has been revisited.

DR. OKRENT: I have no problem with looking at surveillance testing, but at the beginning we heard that they weren't going to try to look for things that people were interested in that didn't have some connection with Chernobyl.

In fact, it was not even clear to me that they were doing a test to avoid a requirement. I didn't think their diesels could start that fast.

DR. KERR: I don't think ours can either.

DR. OKRENT: That may be.

MR. SHARON: No, they weren't. They did not have a requirement for the diesels to start fast. They had a requirement, though, that they had to keep the core covered and not go critical.

DR. OKRENT: Which they had trouble meeting?

MR. SHARON: As a matter of fact, the first test they ran showed that their system didn't work as designed, and they performed a modification, and that was what this test was about, to demonstrate the modification and fix the problem.

DR. MARK: Look, this is a question out of total ignorance.

I believe the N reactor has water and graphite. If the only immobile element is the water, if the water is voided, the reactivity at N reactor goes down, is that right?

DR. OKRENT: Correct.

DR. LEWIS: So I am assured.

DR. MARK: And that is masked by using the word "under-moderated"; whereas, Chernobyl was over-moderated. When the water got out, reactivity went up.

I think there is a more comprehensible way of saying that than using the words "under-moderated" and "over-moderated," that we have graphite at Fort St. Vrain.

There, if the coolant, the mobile element, goes out, nothing happens.

DR. OKRENT: A few cents of reactivity.

DR. MARK: Very few, and there is a really integral, crucial difference between us and Chernobyl, and it should, in my view, come out more clearly if said that way.

DR. LEWIS: I personally believe the lessons of Chernobyl are really not hardware lessons. They are management lessons.

DR. MARK: There's lots of lessons. I don't mean there is no relevance, but there is a total black and white difference between Chernobyl and the reactors we are talking about that have graphite in them.

DR. OKRENT: I wonder if I could ask the members if they have additional points to be covered in a possible letter. There is no decision on whether to send a letter on Chernobyl. I guess we have to.

If they have additional points on a letter -- for example, the current one doesn't mention Fort St. Vrain. Do you think something should be there in that regard?

I would appreciate either receiving a paragraph -- if not, at least a cryptic statement -- telling me what should be in the paragraph in your opinion, so I can try to have sometning that at least includes for review the points

that members....

We are out of time now.

DR. REMICK: My only point on the letter is I think we want to keep it.

If the staff has omitted something or said something incorrectly or not clearly, we ought to address that. But I am not sure we should just put in statements and not know whether the staff has addressed that adequately or not.

I guess I am feeling that in drafts there are statements which the staff already has made in their document. I am not sure we need to address it. We should be able to look at it from that standpoint.

There ought to be some correlation between what we said and what staff said.

DR. OKRENT: There might be.

Anyway, we are beyond your allotted time, Mr. Chairman, and we normally had left 45 minutes for discussion.

MR. WARD: I think the discussion occurred as we went along.

DR. OKRENT: Anyway, I am going to ask again that members get me either paragraphs or identify specific items that they would like to see paragraphs on by 5:00 o'clock today.

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MR. WARD: Let's come back at 1:50.

(Whereupon, at 12:50 p.m., the committee was recessed for lunch, to reconvene at 1:50 p.m., this same day.)

AFTERNOON SESSION

(1:55 p.m.)

MR. WARD: Our next topic is discussion of containment performance.

Dr. Mark.

DR. MARK: This is hardly containment performance in the broadest sense. I believe what we will talk about are the concerns that have been raised in connection with the Mark I boiling water reactor containment.

I don't think we will reach any other item on this.

We had a meeting on Tuesday. I have forgotten just who was there. There were three of us -- Bill Kerr, Charlie Wylie, and who else?

MR. BERNERO: Ebersole and Michelson.

DR. MARK: We had a meeting of the subcommittee, which heard mainly from Bob Bernero on his proposal for a radical immediate fix on BWR Mark 1 systems.

We had in front of us notes from Dave Okrent and Commissioner Asselstine supporting the idea that something should be done.

I am sorry, did I misrepresent you, Dave?

DR. OKRENT: Correct. I said something should be done.

I was not specifically supporting an A through E

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or 1 through 6.

DR. MARK: Anyway, we had this meeting and we will have a presentation from Bernero this afternoon giving the gist, I believe, of what he has felt to be clear, and we are being asked to make some comments either for his proposals or against them in the course of this meeting because his suggestions are going to reach CRGR in the course of 10 days and the Commission is going to make a decision on them 10 days after that.

MR. BERNERO: And presumably so am I.

DR. MARK: That is, I think, where we stand. What Bob is proposing he will tell us. He will give us some notions of his estimates of the need for this, the effects of this, and the cost of this because what he is proposing will have some cost and will be based on some need.

I think at that point, Mr. Chairman, unless you have something urgent, I will turn the floor over to him.

We do have here Mr. Pickens, I think, of the BWR OG, the boiling water reactor owners group, who will have some things to say which are not, I believe, totally orthogonal to what Bernero is saying.

Please, Bob.

(Slide.)

MR. BERNERO: All right, Mr. Chairman, the topic here is a proposal via the form of a generic letter for some

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severe accident issue corrections or problem corrections in one type of U.S. containment.

The Mark I containment is singled out in this action in the draft generic letter. You have seen in its analysis it indicates that the other boiling water reactor pressure suppression containments lend themselves to the same treatment, albeit with slightly different requirements.

The same can be said for other containments as well, PWR containments, so that this model of action that I propose I would also encourage people to think in terms of applying it to other containments as well as the Mark I.

The Mark I has been selected out of our group because of its priority. It is the largest in population, 24 containments, all licensed, and they are often singled out as the most difficult to analyze or to accept in a severe accident phase.

Just to refresh your memory, you recall that the NRC put out the severe accident policy August 8th of 1985. After a good deal of controversy, the Commission did say present reactors are safe enough, but -- and the "but" -- the mortgage for existing reactors is that as long as you know you don't have any outliers, significant vulnerabilities to severe accidents that might be plant specific in character. And the Commission did also indicate

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its desire to consider the balance of prevention and mitigation, the concept of defense in depth in dealing with the search for outliers, and special consideration of containment performance was identified, whatever that meant.

(Slide.)

The search for outliers, we are really looking for a significant vulnerability. This is not really an objective to quantify inliers; in other words, to come up with a precise description of the accident frequency distribution in a reactor.

The objective is more in terms of finding something that is outside the pale that is a significant contributor to risk.

The expected theme that the severe accident policy statement shows is that unless you have already done it through the form of an extensive PRA, or something like that, unless it is already done there should be a plant specific evaluation to identify the outliers and to backfit as appropriate.

The Commission takes pain to say consistent with the backfit policy and rule, where now the policy statement goes on to say where the technical issue goes beyond current regulatory requirements generic rulemaking is preferred.

But the policy statement recognizes alternate

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instruments, such as bulletins, orders, or generic letters.

It is rather interesting that the statement was made -- where it goes beyond current regulatory requirements --

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-- because if you look with respect to containment at the two key design criteria for containment, Criterion 16 on containment design and Criterion 50 on containment design basis, they have within them the words that indicate that severe accident performance, core melt performance of a containment is indeed beyond the design basis, an essentially leak-tight barrier against the uncontrolled release of radioactivity for as long as postulated accident conditions require.

It is a clear tie and has been over the years a clear tie back to the design basis accident, general design Criterion 50.

The criterion goes on -- of course, it is much longer than this, but the key words are conditions that result from degradation but not total failure of emergency core cooling functioning.

Again, the design basis accident postulates certain degraded conditions, single failures, things of that sort, but it does not enter the land where no cooling at all, where core melt prevails.

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DR. MARK: Bob, a comment. If the Commission issues a rulemaking -- which of course takes forever -- then that is law. That has to be adhered to.

If the Commission forwards a generic letter, is that or is that not law?

MR. BERNERO: It doesn't have the same stature as the Commission regulation, which has the force of law. Nonetheless, it is a requirement imposed by staff, not the Commission.

The Commission does not issue generic letters. Staff does, and the staff frequently resorts to this instrument to obtain detailed implementation of things -- hydrogen control, details of containment atmosphere dilution systems, things of detailed interpretation.

DR. MARK: Supposing I am a utility, and I feel very negative to you, Staff, and you issue a generic letter and I say some unutterable word, what then?

MR. BERNERO: First of all, I don't know if anyone has ever done it, but if they did, the staff can issue an order. The staff has the authority to order you to do something, and then you are citable, and that is enforcible. That is clearly enforcible.

DR. MARK: That sounds to me like law.

MR. BERNERO: I believe it is. I can't really tell you. I know a generic letter is citable and

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enforcible. People do it. But whether it has the force of law, that phrase I would have to defer to legal counsel.

DR. REMICK: I am not sure it has the force of law, but I think what you are referring to is if staff is in that situation they can issue -- the Commission could issue a show cause letter, saying you show us cause why we shouldn't shut you down because you haven't done this, and that gives the licensee an opportunity to come back and argue his point, where he would request a hearing.

DR. MARK: But it would be settled finally by the Commission.

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MR. BERNERO: The Commission doesn't have to. If they come back and don't have a good reason for not implementing the letter, then the license can be amended to require it. There can be order. There are legal instruments that are available at the staff level.

DR. MARK: I really don't want to take this very seriously, but you are using the route or talking here about the route of a generic letter rather than a rulemaking, in part at least because it is something that would be seen to happen in the next year or so, whereas the other could not.

MR. BERNERO: One is time, the other is character. The character of the action where one speaks of specific pipe sizes, event sizes, power supplies lends itself far better to a generic letter than the statesmanlike proceedings of a rulemaking, although sometimes we have had very prescriptive rules.

The important point I was trying to make is we are beyond the regulations.

(Slide.)

In the generic letter draft which you have, there is a hard to read table. I don't expect you to read this on the board, but I want to single it out.

It is a listing of the PRA results and sample individual plant evaluation results. That is what the IPE here stands for.

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The results we have of the analysis to date of boiling water reactors and a key parameter -- and this is a very good point that came out in Tuesday's meeting with the subcommittee -- the last column -- we have here core damage frequency in this column here, but the last column is containment conditional failure probability.

There is a sloppiness of language that has grown, and we are struggling with it. If you analyze in a risk analysis a single accident sequence, TMLB-prime, station blackout, or an ATWS of some type or other, you do a containment event tree and analyze the outcome, the probability of different outcomes, and from that containment event tree analysis you can derive a conditional probability.

Given accident sequence X, what is the conditional probability of containment failure?

First of all, you have got to be careful. Containment failure shouldn't really be loss of leak tightness because a successfully vented, filtered pressure relief is not a failure, or I wouldn't define it as a failure.

Failure ought to be a large release or a relatively large release that the containment didn't provide substantial mitigation.

So that if you look at containment failure as a

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relatively large release, for a given accident sequence you have a conditional probability.

For a given plant you have a family of accident sequences, each with its own probability, and if you take a weighted average of those conditional containment failure probabilities, you have something you might call the fraction of core melts for that plant that can be expected to result in a large release, and that is really what we are talking about, the overall success or failure rate of containment.

MR. WARD: Is that what that last column is?

MR. BERNERO: That is what we are trying to make it be.

DR. MARK: Would you define a large release in terms of the number of curies or the number of rems at the plant boundary?

MR. BERNERO: No. That is going to make it very difficult. Of course, there is a lot of debate and source term space about how large is large. What do you get if you have an unmitigated release?

If we would distinguish in a qualitative way and say a filtered vent release is evidently the noble gas activity and a large release ought to be anything notably in excess of that -- and there is a curve I am going to show you in a moment -- that noble gas activity alone is a

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nontrivial release.

Therefore, for purposes here we are talking about a relatively unmitigated release that involves relatively large quantities of aerosols.

Yes, sir.

DR. SHEWMON: If I put zero up there as a probability, you would doubt my veracity. Anybody who puts 1.0 up there, I kind of doubt, too.

MR. BERNERO: I was about to get to that one. That is actual.

There are two plants in here that are a problem. This is Limerick. This is GESSAR II. It really was evaluated in both cases, but the meaning is incorrect if you say 1.0.

The 1.0 in that particular entry actually represents containment failure by just relieving pressure, just as WASH-1400 actually had -- if you went through here, that same 1.0 could have been put in -- WASH-1400 had pressure relief in all accident sequences but not all of them were unmitigated releases.

WASH-1400, if you entered an inferred number -- and the text of the letter does that -- it is that infamous 90 percent failure probability that Harold Denton used.

In this particular case, if you go into the hearing record for Limerick, that number is on the order of

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50 percent. The containment always bursts in the analysis, but roughly half the time the burst was a filtered release and half the time it was not.

So the proper entry here for -- whether I will call it the fraction resulting in large release or this improperly identified conditional probability, the proper entry here is 50 percent or less than 50 percent.

The proper entry here is 90 percent, and the proper entry for GESSAR II -- you may recall that safety evaluation report. The containment bursts in the wet well all the time, and the unfiltered outcome was a tiny fraction. In fact, it would be dominated by what is the likelihood of pool bypass. It is a very small number, on the order of, say, 1 percent.

Therefore, if in a systematic, this column needs some correction to put it all on the basis of the fraction resulting in large release. In that case it ranges from 90 percent down to 1 percent, say, on a Mark III containment because the Mark III has a dry well that is vastly more strong than the wet well.

DR. MARK: Which is the 90 percent?

MR. BERNERO: 90 percent would be WASH-1400, the reactor safety study, because in the BWR the dominant releases were not filtered releases.

DR. SHEWMON: And at Limerick that I asked about,

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that is translated .5?

MR. BERNERO: It is 50/50 or slightly less than 50/50, yes, sir.

DR. OKRENT: It is my recollection that for GESSAR there remained an open question concerning the possibility of failure of the pedestal, and therefore a loss of integrity of the dry well and hence --

MR. BERNERO: A bypass.

DR. OKRENT: A partial bypass because some of the fission products might have gotten into the pool, but a nontrivial fraction would be available to get through another path.

I have not seen anything put out by the staff that says that this failure mode can now be neglected.

MR. BERNERO: One of the difficulties we have not evaluated -- in fact, you could say that for almost every one of those -- the fraction resulting in a large release is not an explicit product of any PRA. One just doesn't calculate it, and in GESSAR II it wasn't calculated.

The matter was left, but the fraction which would have resulted in a large release was that whatever the population of those sequences that result in a dry well pool bypass -- pedestal collapse, the vacuum breaker, or ventilation pan path collapse -- whatever event would result in bypassing the pool and the filtration mechanism, and it

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was never calculated.

DR. OKRENT: But you gave 1 percent?

MR. BERNERO: Yes, that is my interpretation, and you have apparently suggested that the uncertainty is greater than that and it might be -- I don't know. You didn't name a number -- 10 percent?

DR. OKRENT: You have 1 percent, if I understood it correctly, as your central estimate. I am not aware of its basis.

MR. BERNERO: It was not explicitly evaluated. I would infer it from the text, just as one infers 90 percent from the reactor safety study.

DR. OKRENT: Well, what I am getting at is that if most core melts were to lead to failure of the pedestal and most of those led to failure of the dry well --

MR. BERNERO: Then it would be a high fraction, indeed.

DR. OKRENT: At least it seems to me it would be unlikely to be 1 percent.

MR. BERNERO: And if most core melts led to a very energetic direct heating and consequent failure of dry well and containment, then it would indeed be a high number.

You know, really what you are saying is there is an uncertainty, and one can choose to deal with that as a

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dominant uncertainty or as a low uncertainty.

When I give something on the order of 1 percent, I am saying my interpretation of that record is it is not a dominant uncertainty.

(Slide.)

The key results I would just summarize for BWR containment.

Again, it is in the context of this fraction resulting in an early release for the distribution of accident sequences that that particular plant has.

The reactor safety study, Peach Bottom, 90 percent is the approximate value of the early or large release for the family of core melts.

IDCOR studied the same plant, the Peach Bottom plant. Again, it was not explicit, but upon request earlier this year Philadelphia Electric actually looked at the results and said what is the approximate fraction, and they came up with a number on the order of 20 percent.

Vermont Yankee, in a 60-day containment study, tried to infer -- first of all, from Peach Bottom accident distributions they translated it to Vermont Yankee by changes or differences in plant design, and they got a family of accident sequences that was at least tentatively appropriate to Vermont Yankee. Then they did containment event trees, and they came up with 7 percent.

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Now, they are slightly smaller. I distributed to the committee a table of data for all the BWRs, and in that table of data -- you can look -- the containments don't vary that much, the Mark I's, in size, although the reactors inside the containments vary over a factor of 2 in size.

Vermont Yankee is one of those half-sized reactors in a 90 percent sized Mark I containment. So this is a surprisingly close outcome in round numbers, 10 percent.

Now, NUREG-1150 is not available to us yet, but we have had plenty of warning that the uncertainties are such that will come back to something like the reactor safety study, at one end of it at least.

So we expect the outcome of NUREG-1150 to be somewhere in the range described by these three earlier things, and why is that so? What are the features of that containment?

(Slide.)

Here is an elevation view. It is slightly different from the view you have in your packet, but it is a simple line diagram.

DR. MOELLER: Excuse me. To say it will be in the range is to say that it will be between 7 and 90 percent?

MR. BERNERO: Yes, 10 percent to 90 percent.

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What they publish now for Surry -- well, they haven't published it, but at least we have been reviewing it and it has been briefed to the Commission -- that the early release fraction is somewhere between 1 percent and 60 percent.

So 10 percent to 90 percent for the Mark I would not be surprising at all.

But if we look at this containment -- I will boost the picture up a little bit -- why should this containment be so vulnerable?

If you look at it, the first thing you see is it is very small. For the reactor within it, it is a very small, three volume.

What is means is that if you have a large scale core melt -- remember the sequence of events. When you roast the core, the early phases of a core melt, you drive off the gaseous and volatile activity as the fuel disintegrates, and the relief valves conduct these volatile materials -- the gases, the cesium, the iodine, and the cesium iodide if that be it. If they are conducted down into the suppression pool, where the relief valve tailpipes go, they go right down into the water.

Now, obviously, the noble gases are not scrubbed. They are obviously not sufficiently soluble. They will just cycle around. But the cesium, the cesium

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iodide, they will be captured in the pool.

But if there is large scale pool melt, it has to dissolve through the control rod assemblies, which are in the bottom of the BWR, and then work its way down onto the floor, the lower head area.

Now, this area is open to the surrounding containment, but notice you have a very narrow island of space, not an island -- it is an annulus or disc of space. The dry well wall is very close to the lower head area. The downcomer vents to take the steam away are close by.

And the result is if you have a large scale core melt, the debris, being so hot, has the ability to directly attack corrosive attack of the wall. It has the ability to have radiant heat transferred to the wall very intensely an a convective heat transfer. And of course, while it is in contact with the concrete, you can have vigorous core-concrete interaction, and that core-concrete interaction can generate a lot more hydrogen, CO-2, and entrain aerosols while it is at it.

Yes, sir.

DR. SHEWMON: As I recall, GE takes a different view of the vulnerability of this.

Is the difference of opinion between them and the staff whether the operator will be able to get water into this or what?

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MR. BERNERO: Remember, we are speaking in conditional probability space. I think everyone would agree -- and the results on that big, complex table do tend to support it -- that all else being equal, you are most likely better able to keep from melting the core in a BWR than in a PWR.

They have got more water systems and more adaptability, and if you can't work at high pressure you relieve it down to low pressure, and it is pretty well established that it is a more flexible system.

It starts out being two-phased, and you don't mystify the operator with double bubbles and the things like that that you have in a PWR. So its likelihood of core melt is probably somewhat lower.

Nonetheless, if you do have a large scale core melt and accept the possibility of melting through all of the hardware in the lower end, which is going to tend to make a more dilute corium -- it has more inert metal in it -- we are speaking solely of the conditional probability. The situation, never mind how it happened, you are there.

You have got large scale core melt on the floor, and that is the vulnerability we are speaking of.

MR. WYLIE: But your sprays are pouring water on that all the time?

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MR. BERNERO: Not yet.

The situation in the plant is that you have this chance of generating a lot more hydrogen besides the hydrogen you have generated in the core degradation up inside. You have got a chance to have all these extra aerosols, inert materials of all kinds, and there are some things already in place that help to mitigate.

One, we have already inerted these containments. So we have taken the edge off the hydrogen threat, the hydrogen combustion threat. By inerting, the hydrogen now becomes an inert gas, just another inert gas.

We also have in these containments -- right about this level here in the typical plant, one on top of the other, are two spray ring headers. They are fed -- on the typical plant, it is an alternate mode of operation of the RHR system -- RHR is valves -- such that you can squirt into the reactor or into these spray headers and get a rather high flow, fully developed spray pattern all through this space here.

So that is goodness, but although a safety grade system, a redundant system, you know, two independent trains, and all that, it's an alternate mode with little priority, because the priority is, of course, cool the core. And this containment has this fragility, due to being very small and having molten core material right here.

In addition, even if you are cooling it, here it depends on the model. How much inert gas are you dealing with, how much hydrogen is generated at each state of the reaction? How much CO2 is generated by core-concrete interaction. Depending on the amount of inert gas generated and the inert average pressure and temperature which will drive the pressure, you can reach overpressure of the containment rather easily.

The design pressure of the typical Mark I containment is on the order of 50 to 60 psi, but the volume is a factor of 8 or 9 less than a large dry containment. So the inert gas can build up to burst pressure rather soon. If you build up to burst pressure, analysts have wrestled with this for sometime, at what point would it burst? Conventional wisdom up to now has said, it is going to break at the knuckle right about here. Somewhere around twice design pressure. People looking more at that say no, the biological field is going to back it up. It is probably going to break down here somewhere at a somewhat higher

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pressure perhaps as high as three times design pressure, but it still is very likely to reach a bursting pressure, unless you do something about it.

DR. REMICK: There was something I missed.

You answered Charlie on his question on why you wouldn't have water from the containment spray, but you must be assuming here that because you had a core melt that there's no chance of HPCI, LPCI, RCIC or core spray putting water into the core spray down into the cavity.

MR. BERNERO: That may be why you have a core melt. If you had an RHR LPCI system, you wouldn't have a core melt. So it is an academic question. It is the very reason why I question it, and remember, someone remarked that the spray is not out here, it is not down here.

There is one or two doorways in those things, usually, but keeping that in mind, let's look at a summary. Now vulnerabilities, some of which are either fixed or partially fixed --

(Slide.)

-- you've got a small volume, exposing rapid overpressure vulnerability and especially vulnerable to hydrogen burn, but as I said, they are already inerted, although you have a window, the tech spec window of 24 hours at the beginning and at the end of the cycle, where you don't have to be inerted, it is a small drywell floor. The

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lower head area is close to the drywell wall. There is that potential for direct debris attack, and of course, direct heating by radiation or convection.

A characteristic of the containment is passively, the way WASH-1400 evaluated it. Look at the thing and watch. It has relatively little capability to cope with the core melt, but actively, because, again over the many diverse systems and alternatives, there are options for active response that open up avenues of improvement.

What we say in the generic letter is, there is a five-element approach to the improvement of this containment that embraces control of hydrogen, whether reaffirming what we have or changing control of the sprays in the drywell. An extremely important function, the pressure relief, in order to avoid catastrophic failure, control of core debris, and lastly, the procedures and training for this active response.

Now one thing I want to put up, because it is significant --

(Slide.)

Earlier on I said that the success state is a filtered release of the noble gas. I invite your attention to the is figure in the letter.

This is all noble gas released. This assumes all of it coming out after a one-hour hold up in decay before

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release.

Remember, the noble gases have three half lives. If you speak of noble gas activity in a power reactor, there are three half live regimes of interest, and the easier way is to start at the tail end of the curve and work back uphill. Everyone knows krypton-85 has a ten-year half life, and we had a building full of it at TMI for a year and had ample discussion of it. It is a very small dose, a very, very small dose.

When you back up on the curve, there is a point within a month or two of shutdown, where the half life is not krypton-85, but xenon-133, 5.2 days. That is the dominant activity that was in the TMI building for the days, for the weeks after the accident, why people couldn't go in there, you know, and freely walk around. It is a 5.2 day half life.

Then when you get up to within hours of shutdown, you go into a steeper half life curve, which is a whole family of short-lived activities. Basically, you have a fairly significant decay from zero to one hour, and you are making the transition from one hour to maybe eight or twelve hours, before you really get to the xenon-133 period or half life regime.

We chose, since it takes about an hour to melt the core, if you want to do a fast core melt, it takes about

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an hour. We chose one hour as the lag time, and we took two elevations of release, a 10-meter release and a 100-meter release.

What is the off-site dose to a member of the public or a member of the plant staff for that matter, if you had such a release?

The solid line here is for elevated release at 100 meters with expected meteorology. This is 50 percentile meteorology. If you flip to rare or bad meteorology, here is the curve, this is the 95 percentile, or if you invert it, the 5 percentile meteorology.

Notice the dose. 20-rem whole body, pretty much for the first two miles, and over here, the 96 percentile is on the order of 100-rem whole body. You can up into the clinically detectable effects, the radiation injury range.

On the other hand, if you make it a ground level release, a nominal 10-meter, as if it blew out a side panel in the building, then the ground level release in the first two levels -- here is the expected meteorology, and here is the 95th percentile, you can get up into lethal doses.

So keeping that mind, the noble gas release should be one, something you don't routinely adopt as an alternative, and it should be released at a high point. With that frame of reference of off-site dose, it should lead one to say, you don't want to have that stuff bursting inside

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the reactor building. You don't want the operators immersed in it.

DR. KERR: Let me make sure I understand. That is based on the assumption or the release occurs one hour after shutdown?

MR. BERNERO: Yes. In other words, the noble gas has decayed one hour after shutdown, and the person out there now sees the whole puff.

DR. KERR: Do you wait 12 hours and it goes down by, you say, perhaps a factor of 30?

MR. BERNERO: No. In fact, I was asking a member of Staff today, we've got a whole bunch of calculations.

DR. KERR: I am just looking at number one up there.

MR. BERNERO: It can be as much, but what I'd like to do is have a sensitivity that goes one hour or two hours, five hours. You know, something like that, to give a little bit better bracket than that. You know, there is something appreciable, and it is going to vary by sequence, but we backed up to the one hour, as the more conservative one.

DR. MOELLER: These are noble gases only?

MR. BERNERO: Just noble gases.

So this assumes a scrubbed release. The French, as you know, have that sand filter with a DF of 7 to 10 or

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something like that.

So in their calculation, they would get the noble gas. You know, they might be out at 12 hours and get 30 times less than that, plus whatever residual particulate would be in it, depending on the efficacy of the filter.

So I am missing a slide here.

I wanted to summarize -- excuse me for minute. I took one slide out that I shouldn't have taken out.

You should have it. It is containment improvement strategy. I skipped it. I mistakenly took it out of the pile.

(Slide.)

The containment improvement strategy goes like this:

First of all -- there are far more words, of course, in the letter.

The first thing is, let's look at hydrogen. We obviously want to prevent that combustion. We have inerting now, and the issue evolves into, do we have to do something besides inerting? Do we have to eliminate that tech spec. Do we adjust that tech spec from 24 hours to 12 hours? Well, in one sample case -- this is Vermont Yankee -- there was a study made of the logbooks for three years in a typical BWR Mark I, and I would assert that that is typical. How long did they operate on inerted? It works

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out to be 1.1 percent of the time.

The Owners Group is doing a survey of other plants, but it appears that we are working on a very small fraciton of one contributor to risk, and therefore, at this time, the letter says, it isn't worth altering that tech spec. It merely reaffirms present requirements, and so what it says is, by inerting, the hydrogen control remains the same.

Now the drywell spray was designed to fit the pumps available to feed it. The pumps in question are the RHR LPCI pumps, which, in a boiling water reactor, are about the largest things they have in the reactor side of the house.

They are very high flow pumps, and depending on how they size the manifold and spray nozzles, they typically provide so much water, that there operating procedures are filled with precautions. Don't overdo it. Don't overspray and collapse the darn thing.

If you have a system designed for feed by a very, very large pump, and that pump doesn't work and going back to an early thought, maybe that is why you have the core melt, what available pumps might be connected as backup supplies?

The difficulty is, that the system is designed only for a 10,000 gpm pump. Where are yo going to find a

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10,000 gpm backup?

So the first requirement here is cut the number of nozzles, roughly, in order of magnitude, in order to enable smaller pumps that are available in the plants, smaller systems, to produce and feed water, such that they will produce the spray in the containment, not dribble water in, which they would do in a system designed as it is now, but produce a developed spray pattern, and that you would have backups, and it does two things for you. It gives you different ways to get the spray pattern, and it also maintains your control geometry.

You don't have some very large flow rate trying to flood up the torus and shifting into a flooded geometry. It extends your water supplies and still leaves you with controlled geometry.

Keep in mind, even a 1/10 flow rate, a 1/10 RHR flow rate, can flood the drywell in something on the order of 15 minutes.

(Slide.)

The drywell, remember, is only about a foot deep right there. You can put about a foot of water there, and anything over that will just flow down the vent into the torus.

DR. MOELLER: How does this affect any cleaning?

MR. BERNERO: Now, wait, if you do that, and

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you flow down, you ask yourself what did I lose? Am I getting something for nothing here? It turns out that there was no design basis for that flow, other than the fact that those big pumps were there. So if you have a spray pattern, you can still get scrubbing. If you look at the decay heat mode, you still have plenty of capacity there.

It turns out, and this is something you have to look at in each plant, about what the right flow rate is. It turns out that you apparently do get something for nothing. There was a surfeit of water in the first place. So with that comes a reliability requirement.

Now that you have that lower flow, you have to have two backup supplies, and the two backup supplies described are one, maybe alternating current power, conventional AC buses, and the other shall be good in a station blackout.

The typical plant has a diesel-driven or separate engine-driven fire main system in plant after plant. I have walked through the plants, and you can stand there with one hand on the spray header leading to the reactor building, stand there and look at a fire main 20 feet away or 15 feet away.

So the typical plant responds not with a wholly new system, but with existing connections or slight adaptations. In order to connect something like RHR

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service water, conventional service water, fire main water, systems of that sort, to the spray and provide a reliable backup supply for drywell spray, and of course, you get the benefits. You start out with the pool of water on the floor. You have a steady spray coming down to cool the -- and prevent direct heat transfers and the spread of aerosols.

DR. OKRENT: Do we know, unequivocally, that adding water into the drywell cannot lead to really adverse interactions between water and core debris?

MR. BERNERO: I am not sure I am willing to say unequivocally.

Let me say this. The primary issue would be one of steam explosion, a vigorous or violent reaction between molten core material dropping into a pool of water and several factors indicate to me that this is not a significnt uncertainty in the boiling water reactor.

One, what I would call the consensus or the collective wisdom, the Steam Explosion Review Group results, indicate for the pressurized water reactor geometry, the steam explosion is not a dominant uncertainty. There is still debate about it, admittedly, but keep in mind, the pressurized water reactor is, at least in simple form, a 12-foot, on a side cube sitting over a bowl full of water.

That is a geometry of a PWR.

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The geometry of the BWR, the bowl is full or hardware.

And given that in the BWR, the large-scale core melt was designed for that hardware in order to have a bulk of corium reach the lower head area, the corium is, first of all, more dilute. It is necessarily more dilute, and as such, I think it is as a somewhat lower temperature.

Secondly, it is falling into a very limited pool of water.

The geometry here limits you to about one foot. Some plants are as little as seven inches, some perhaps as much as 15 or 16 inches, but that is it.

So to get a large amount of corium to disperse effectively and immediately in a large amount of water and thus generate the steam explosion, I am not sure I can say, unequivocally, but I think the proponderance of evidence is that it is not detrimental, that it is favorable for cooling and crunching corium material, as it comes out and as it tries to progress across the floor.

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DR. OKRENT: Again, you may be correct but let me just ask is there any mode of a significantly adverse steam explosion outside of the support structure into which the molten fuel first falls?

In other words --

MR. BERNERO: If you reduce that flow rate, it's virtually impossible to get more than a foot of water. You'd have to flood the whole containment.

DR. OKRENT: I don't have a feel at the moment for whether a foot of water interacting with some plausible amount of molten or quasi-molten material can do damaging effects to the dry well.

Maybe somebody has done a bounding calculation and shown that it doesn't matter if a steam explosion occurs. It's my impression that steam explosions of modest size can occur with a fairly high degree of likelihood of you have water and the molten material around.

MR. BERNERO: No doubt.

DR. OKRENT: So it becomes a question of not being able to get something sufficiently damaging, which is why I phrased the question with those qualifying words.

MR. BERNERO: It is certainly. Anytime you would take material at approximately 5,000 degrees Fahrenheit and drop it in blobs or globules into water that's at 100 degrees Fahrenheit, it's going to be rather exciting.

You're going to get very vigorous exchanges and bubbling and spattering and bursting. And I cannot say that that is clearly harmless to the containment.

The only thing I can say is it falls, once again, going back to the picture.

(Slide.)

The initial dropping is into a relatively enshrouded area that doesn't have spray on it, but merely has whatever water can flow into it. If there is vigorous spattering or knocking around, not vigorous in terms of a steam explosion in a reactor vessel that would tear the top off, but knocking things around, it is already mechanically shielded somewhat.

And then, if it comes out, depending on where the doorways are, they vary from plant to plant, the recirculation loop is out there, that's what that pump is, you have some hardware. In fact, it's a very crowded area for recirc loops and piping, and things like that, before you hit the wall.

One can only use engineering judgment. I know of no calculation. And we certainly didn't attempt any that would say:

This is the kinetic energy of a maximum credible particle that could hit the wall of the dry well. We just didn't do it.

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DR. OKRENT: It just seems to me it would be useful if one could be assured that, in all of the Mach I's, there was no surprise waiting for one, some kind of a steam explosion that in fact leads to a rupture of the dry well.

It doesn't strike me as something that one couldn't do, at least initially, some order of magnitude estimates to see if the potential is even there.

If the potential is there, one would then perhaps have to look more closely to see if it can be.

MR. BERNERO: I really think we're touching on a difficulty that has haunted Mach I analyses for years. If you go into the very elegant, long-range, massive program analyses, the source term analyses, for years, people have said the failure is right there at the knuckle.

And then of course it's analyzed as if it relieves into the reactor building. Mechanistically, it is so hard to model the hardware that's in the way, the fact that the dry well shell is that far away from a massive reinforced concrete biological shield. There is no free path to the ambient.

Two of the Mach I's are reinforced concrete vessels with a steel liner. The rest of them are freestanding steel vessels with this big biological shield around it.

In any case, if you were to have a truly

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bypassing failure due to debris ejection by steam explosion in the lower cavity, it not only has to miss the hardware, it has to go through.

Merely perforating this is not a significant failure, although I must admit it's the fallback position. It gives you noble gases in the reactor building, which is not desirable but you still get an awful lot of filtration and attenuation.

It's not a free path to the outside.

MR. ETHERINGTON: When you speak of a steam explosion, you're talking about shock or static pressure?

MR. BERNERO: In this particular instance, I would be talking more about the debris than I would about the pressure effects, because the pressure effects -- a pressure suppression containment is much more resilient unless you get a very, very rapid pressure pulse that can't be dissipated.

After all, this thing was designed to absorb a guillotine rupture of a very large pipe and to respond quickly to that. I would be more concerned about the debris ejection than the other.

DR. REMICK: Bob, a question. On the pumps that you're talking about, the small pumps, do you envision those being in the RHR quandrants, or would these be out in the engineering building outside the dry well?

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MR. BERNERO: They are where they are. They're existing pumps. I personally walked down. I think I've walked down 21 Mach I's. But you go and you look. In Vermont Yankee, they have a 1,200-gallon a minute diesel-driven fire pump that sits in a separate pumphouse. It's like a blockhouse. It's on the shore of the Connecticut River, which is their heat sink.

It's diesel-driven and it's already piped into the spray header. It's just a matter of opening the valves. There are no new pumps identified here. It's using existing systems and really changing valves or changing spool pieces and flanges.

DR. REMICK: In some cases, an interconnection.

MR. BERNERO: Oddly enough, Vermont Yankee must have 10 pumps connected, alternate sources. Why nobody seems to know. They just belt with suspenders when they designed the plant. It's a matter of selecting what they would use rather than building or installing.

DR. KERR: Would you talk about a source that should supply water given a station blackout? Does station blackout include the demise of this diesel-driven fire pump.

MR. BERNERO: No. We have a problem in this agency when we talk about station blackouts because from plant to plant, its meaning differs. The classical PWR,

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which is the classical model people have in mind when they speak of it, has division one, division two diesel generators and off site power.

Given less of off site power, diesel 1 DG fails, diesel 2 DG fails, we have station blackout. All I have is one turbine-driven aux feed pump. That's the mental image of station blackout that all of us have when you say A44.

But, now you look at a boiling water reactor. You've got in the old ones division one, division two and two turbine-driven systems, RCIC and HPSI.

In the later model BWR's, you have division one, division two and division three, a third diverse diesel generator plus a RCIC.

DR. KERR: That's the reason I asked.

MR. BERNERO: So, station blackout, I think, really has to mean the loss of vital buses A and B and off site power. I mean, loss of vital buses A and B due to loss of off site power.

DR. KERR: A dedicated diesel.

MR. BERNERO: Dedicated diesels.

DR. KERR: It would do all that with station blackout.

MR. BERNERO: When you get to Shoreham, they must have about 12 diesels up there now, plus a gas turbine. It's very difficult to define a blackout for that site.

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So what I am referring to is really a source of motive power that's independent of the normal safety buses, vital buses A and B.

MR. EBERSOLE: Bob, I think you have to add though: And independent of environmental conditions or requirements depending on electrical power of which HPSI and RCIC are not one.

MR. BERNERO: They have a limited time, that's right.

MR. EBERSOLE: By the way, I take it you're isolating this notion from the notion that earthquake events led to your problem?

MR. BERNERO: That's covered elsewhere.

DR. OKRENT: One more question. You've ruled out then that a steam explosion inside the region below the pedestal could damage selectively one region of the pedestal so that the pedestal collapsed.

MR. BERNERO: The residual uncertainties that are not explicitly recognized but are obvious are that steam explosions causing catastrophic failure or early release either in vessel or ex-vessel, direct heating sufficient to overpower the condensing vetns -- you know, the pressure suppression capability -- is a residual uncertainty deemed not significant.

Later on, I'll talk about the seismic one.

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They're not included. Perhaps they should be explicitly covered as why they are excluded.

DR. OKRENT: Your answer was it's your opinion this is a sufficiently low probability, or you just assumed it away, or it hasn't been analyzed?

MR. BERNERO: Former is the answer.

MR. EBERSOLE: Bob, before you get to this disastrous temperature condition that melts the core, you've achieved environmental conditions that have long since disabled the low pressure venting system, the SRV solenoids.

So you have closed up, haven't you? Now, you may have high temperature/high pressure superheated steam.

Am I correct?

MR. BERNERO: It depends. One of the factors you consider in selecting a venting pressure venting the containment, there are three factors that you should consider in choosing a pressure at which to vent the containment. I'll talk about that in a moment.

One is how much can the containment assuredly stand in the way of pressure?

At what pressure can you assuredly operate a vent?

Thirdly, have you considered the back-pressure reclosure of the SRV's?

MR. EBERSOLE: I wasn't talking about the

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containment event.

MR. BERNERO: You were talking about the SRV's being forced back shut by high pressure in the dry well.

MR. EBERSOLE: I'm talking about the fact that the SRV's are held open by the active energization of the solenoid valves. By the time you've got enough temperature to melt the core, those valves have died long ago.

And I suspect you have a primary vessel pressure, which is right up to the disabled system pressure, super-heated steam. And when all this goop comes out of the bottom and you have the control rod drives, it's going to come out with some force, which is rot a very placid injection method.

MR. BERNERO: I'm not saying it's placid.

MR. EBERSOLE: It's not just called a pour out. It's going to come out with a shot.

MR. BERNERO: I'm afraid our engineering intuition differs. I do not believe that you can design tons of metal in such a discrete way as to come out like a shot, even on a PWR.

MR. EBERSOLE: It comes out a little at a time.

MR. BERNERO: Yes. And the pressure comes down like gangbusters.

MR. EBERSOLE: So it's going to come out in a high pressure jet.

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MR. BERNERO: I just do not believe that. But let me please...

(Slide.)

...the rest of the strategy is that having provided the dry well spray, that there is wet well pressure relief to the stack. If you read the text of the letter, it says that, too, must be operable in a station blackout. A full spectrum of accident sequences.

And the argument is, basically, that, first of all, this is a procedure of last resort. You don't want to really release that gas unless you have to. And no one knows for sure what the distribution of accident sequences is.

At Peach Bottom, we have research with NUREG 1150 waxing eloquent about the cumulative core melt frequency is one times 10 to the minus 5 at its all station blackout.

IDCOR looks at the same plant with equally competent people and they say it's 1 times 10 to the minus 5, and it's all ATWS.

Right there, I've got 2 times 10 to the minus 5. I won't throw either of them out. In fact, I assert that prudence demands that you look at the spectrum of accident sequences and be prepared to deal with the spectrum.

Be prepared to deal with loss of injection, loss of containment heat removal blackout, whatever it is. So

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the pressure relief, this vent, in order to get the pool scrubbing and an elevated release requires a hard vent, hard enough to stand the pressure you chose to vent going to the stack.

Now, the older plants have that 100 meter stack out in the yard. The newer plants use a stack on the building somewhere. It's not quite 100 meters tall. But get that high release and have the hard duct and select the pressure.

And the generic letter says: Select the pressure somewhere between design pressure and 50 percent over-pressure. No more. We don't want to count on that margin more than 50 percent. Yes, sir...

DR. REMICK: Beyond the pool scrubbing and the torus, you're not going through the standby gas treatment or anything like that?

MR. BERNERO: It wouldn't take water vapor anyway. It would just plug on you. There are procedures in many plants where, if it's only a little bit of pressure, you try -- by gas. Or for dry venting, you know. Clean venting.

But, now the debris confinement --

DR. OKRENT: Excuse me a minute. Before we leave that bullet, when the French choose to provide a vent, they provide the sand and gravel filter.

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Why is it not a good measure here if you want to consider a backup to the pool?

MR. BERNERO: I'm glad you asked. I was about to flip the drawing back up. And I owe you a dollar for that one.

If you look and recall now, there are two things I want to illustrate on the picture. One is are we filtering the exhaust, the vent, properly? And are we controlling debris properly?

First of all, remember that the volatile activity comes out first and has forced the bubble through the pool via the relief valve lines. And then, with core on the floor, lanthanum oxide, or whatever aerosols that are generated by that reaction, are going to come out.

And you want to have them bubble through also because you're venting the vapor space here.

The question comes up: What about pool bypass? There are vacuum breaker lines. You can have a bypass leak through a perforation in the liner. If a little bit of debris got there, remember, we had the problem with Oyster Creek, if you had a premature crack, or something like that. That is one of the reasons why the presence of a continuous spray in the dry well is so important.

Now, admittedly, those are young aerosols, right off the core concrete. But you have a cooling scrubbing

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spray present continuously, with a high level of confidence right in the dry well.

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So you have in effect two consecutive filters. So even a bypass goes through the spray, plus whatever attenuation it might get by working its way down the gap or in the building.

DR. OKRENT: Would you remind me, the spray is recirculating?

MR. BERNERO: It could be. It depends on what system is supplying it. It is most likely in this case an independent source of cold water.

DR. OKRENT: Where is the water accumulating?

MR. BERNERO: It takes over a million gallons of water to fill that torus, over and above the level it has now.

DR. OKRENT: How long is the spray going to be on?

MR. BERNERO: Continuously.

DR. OKRENT: For how long?

MR. BERNERO: Hours.

DR. OKRENT: At some point you will fill up the torus, I assume?

MR. BERNERO: At 500 gallons a minute or 800 gallons a minute. You have about one and a quarter million gallons to go to fill this space. That is a long time. If that accident isn't over by then, you are in deep soup.

DR. KERR: You are also losing maybe 500 gallons

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a minute due to evaporation.

MR. BERNERO: Well, it depends. Well, no, by the way, there is a technical error in the letter. It says that 1 percent decay heat is 10 minutes. It is really two to three hours. 1 percent decay heat prevails at about two and a half hours after shutdown, and you are down at that level rather quickly, down to the 1 percent level.

You are losing a little through evaporation. I am taking no account of that. I am just adding fresh water from the cooling tower basin, from the Connecticut River, or whatever the source is.

DR. KERR: But you still lose 4- or 500 gallons a minute, an appreciable amount.

MR. BERNERO: It is a very long time. That was one of the reasons for reducing the flow because if you achieve anything like LPCI flow, you are going to much more rapidly get into questions of do you really want to flood, and I don't think so.

DR. OKRENT: Let me ask, would you accept a recirculating mode?

MR. BERNERO: Yes. It is built right into the procedures. If you have got the heat exchanger, you recirculate and remove heat that way.

DR. OKRENT: It is my impression that there were concerns about recirculation at Three Mile Island.

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MR. BERNERO: Oh, yes, we aren't there yet. That is the next degree.

Core debris comes in two forms, and people forget that. Core debris comes as that mysterious material we call corium, which is metal and uranium oxide and that kind of stuff, the stuff that would fall to the floor and be congealed, we hope, by water, but also that water there is now core debris. It is one-tenth or one-twentieth of a curie per millimeter.

Remember, TMI, the reactor coolant sample that we got a day after the accident was 1 curie per millimeter gross beta gamma. Now, a suppression pool reactor coolant system in a BWR, multiply that by 20 -- or rather divide it by 20. So you are talking about one-tenth or one-twentieth of a curie per millimeter.

That is core debris, and it shouldn't be run all over the place. But keep in mind that in the typical Mark I configuration the RHR system and its heat exchanger are arranged close by in the RHR pump, usually in the corner room, and the heat exchanger not too far from it, so that if you did recirculate that water it would be in a very controlled area and its leakage would fall down into the corner room.

And therefore, the debris control says make sure that the water in the torus, which is core debris, can't go

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anywhere offsite, outside, into the turbine hall, or into some strange drainage system, even if the torus fails.

In other words, you form a hydraulic boundary -- the floor of the torus room, the floor of the corner rooms -- which are accessible, and that is debris control for the water, and then debris control for the corium is by the spray and by the geometry which, if you get out, really would go down into the torus room, and there is a pool of water to quench whatever might reach there.

Yes, Forrest.

DR. REMICK: I am trying to understand Dave's earlier question.

Suppose you did run your small pumps at 800 gallons a minute and you do fill up the million gallons. It would just go back into the dry well, wouldn't it, and there would be nothing wrong with filling up the dry well?

MR. BERNERO: In fact, the emergency procedures as presently drafted even encourage flooding this thing when you start getting into questions.

Now, if you want to relieve pressure, if you still have to relieve pressure, you are going to vent the dry well, and what kind of filtration are you getting? Is the submersion here enough? Is there a transition zone where it wouldn't be enough?

Really, it is sort of a marriage of convenience,

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reducing the spray flow rate. To make more systems available as backups pushes this problem aside because it takes so long to fill the torus.

The vapor space of the torus is enormous, and it takes a long, long time to get to that position. Therefore, you are way down in the tail-end. You are almost in the cold shutdown.

DR. OKRENT: I believe you said about 800 gallons a minute?

MR. BERNERO: It varies with the plant. What they actually deliver through the sprays you have to go dig out of some analysis. The pump that furnishes it is capable of 10,000 gpm.

DR. OKRENT: At 800 gallons a minute, you are about, let's say, 5000 an hour, or a round number, 100,000 a day, 10 days, I sort of came out with.

MR. BERNERO: Yes. That is the cold shutdown of a core melt.

DR. OKRENT: That is not the end of decay heat generation; it is something to follow?

MR. BERNERO: It is something to follow, but it is a far slower damage control situation, a far less violent situation than you have at the beginning.

Yes.

MR. MICHELSON: At the subcommittee meeting, I

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think you indicated you would find out what level indication is available on the torus.

Did you find that out?

MR. BERNERO: No, I haven't had a chance to do that. There are some. I have actually read in plant procedures where they read as part of the procedures.

MR. MICHELSON: I know there's some. The question is the range.

MR. BERNERO: The ones I have seen do measure wet well level and even water in the dry well.

MR. MICHELSON: There is some question about how rapidly it would be permissible to be flooding the containment with the larger pumps because as you start the flooding you lose the vapor suppression and the normal capabilities. It may go down to unstable operations. Once you fill that downcomer header partly with water, you may have unstable condensing.

DR. REMICK: Except he is spraying here from other pumps.

MR. MICHELSON: That is later on. The question, though, is related to the scenario early on and not later on. He is not going to connect those fire pumps up for some time.

MR. BERNERO: The fire pumps should be connected immediately, or they would be preconnected.

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MR. MICHELSON: I thought there was some question about at what point in time you bring them into play.

MR. BERNERO: When the temperature in the dry well starts to go up. They should be remote manual or by simple manual procedure.

Most of the owners I have talked to are talking the remote manual -- you know, push a button.

MR. MICHELSON: The question, though, is at what point in time, because I don't know how hot the water is, I don't know how long they have vented, I don't know what got the core into the condition where it finally did melt. The torus may already be at 200 degrees at that time.

We don't know what is happening. I haven't seen any scenario discussions.

DR. MARK: Bob, would you be able to wind this up in around 10 or 15 minutes?

MR. BERNERO: Very much so.

DR. OKRENT: And you are going to tell us something about where seismic fits into this, did you say?

MR. BERNERO: I was going to say, yes. You keep asking me just as I get to the slide.

DR. OKRENT: I like to be a straight man.

(Slide.)

MR. BERNERO: Quality and design standards. These words are extracted directly from the

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generic letter. They are the conditions of these requirements.

The first one is should these be Seismic Category 1 or Class 1E, push buttons, or that kind of thing?

These requirements are intended to be optimized use of existing equipment, and therefore, of themselves they do not bring in new quality standards.

Nevertheless, you know the Seismic Category 2 over Seismic Category 1 issue. You don't put flimsy iron beams above Seismic Category 1 pipes, or something like that. You don't connect non-Class 1E systems to Class 1E electrical systems without appropriate isolation, so that any adaptations or alterations are not going to be allowed to compromise the quality of existing systems or equipment.

But going beyond, to add quality unique to whatever these adaptations are, is not considered appropriate because they are really optimized use of existing equipment.

So if you ask the question, what does this say about seismic risk, how can one look and say I want confidence, that that confidence doesn't dominate or bypass the containment in any way?

I just had occasion to discuss this with Bob Kennedy the day before yesterday at this conference in

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Raleigh, North Carolina, and the discussion going on seismic margins, you know, if you can't quantify the seismic risk, perhaps you can enter the tunnel from the other end and say up to what point can I have high confidence that there is a low probability of seismic failure?

For systems you can go rather high, into perhaps .3 of a G or .4 of a G. You can get up to some fairly large energetic earthquakes and still give an appropriate walkdown, being assured that the systems are okay.

Containments, if they are reinforced concrete containments -- such as a BWR Mark II, or a large dry containment, or the BWR Mark I, the principal dry well structure of it -- those containments have inherent capabilities that probably make them equal to or better than the systems.

The Mark I containment, it is reinforced concrete, all right, the biological shield, but you have got that vessel sitting inside that biological shield and kind of rattling it, and you have an independent coupled torus with all events.

There may be a seismic failure of that containment that you would encounter with the Mark I long before you would encounter it with a Mark II containment because of what I will call that apparent vulnerability.

The approach here does not provide any

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mechanism to deal with that for the simple reason that there is no obvious mechanism to deal with it that is available by optimal use of existing equipment short of literally redesigning the containment and backfitting it in some really enormous way.

If you look at the containment, there is nothing that we can identify that would significantly the seismic performance of this containment, and by that I mean the passive resistance to seismic excitation, the coupling between the dry well and the wet well. We can identify nothing, and therefore nothing is provided.

DR. OKRENT: Well, what you say under implementation is that given the implementation of the generic improvements of Mark I containments, there is no need for an individual plant evaluation for containment performance.

And as I read this, this could write off any need to look at the containment capability, given an earthquake. It could be read that way.

As of now, I have not seen any written study of the Mark I's. Because I am sure they are not all the same with regard to seismic, they have to be located at a variety of sites, et cetera, et cetera, I have no feel for what kind of failures may occur that may lead to early containment failure or not.

I, for one, am not willing to just say, throw up my hands and say it may be bad and there is nothing we can do about it without looking.

This is what was said in 1966 about the whole core melt thing, you may remember, and for literally then maybe almost 15 years it was the situation that prevailed. People said, well, containments are going to go given core melt, until suddenly there was the reversal.

So I, for one, need to see a good look from the seismic point of view.

One can talk here, if you wish, about an opinion based on what one now knows.

DR. KERR: Dave, are you assuming that these containments have not been designed for seismic?

DR. OKRENT: They have been designed for an SSE, but everything here is beyond design basis. You don't get to core melt.

DR. KERR: Is that the context from which you are coming?

DR. OKRENT: That is the context.

MR. BERNERO: I can't give you an answer other than the one I gave you.

DR. OKRENT: But you are writing off possibly, with the words you have got there, any further look from what I will call the external event. There may be some

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other external events that are of interest. But certainly seismic strikes me as one that needs to be looked at, and it has been too long, in fact, that we haven't had the benefit of a good evaluation of the Mark I's from the seismic point of view.

MR. BERNERO: I would just point out on the implementation Dave picked up on, this closes the individual plant evaluation for containment performance or is intended to, and the time, nine months, means that it would be implemented by anything like a range of nine months from the effective date to perhaps as much as two years.

If someone has a long fuel cycle and enters the refueling eight months after the effective date, we are going to get one more fuel cycle plus eight months, and you know it might be as much as two years.

But that was chosen in order to have an orderly implementation without competing risks of rushed and sloppy work.

There are some words in the severe accident policy statement --

MR. MICHELSON: Before we get to that, could you clear up a point for me?

Having decided to put this system in and now having it available, what is the decisionmaking point for the operator? When does he decide to turn it on?

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MR. BERNERO: Symptoms. He follows the symptoms in the reactor coolant system and the containment, the containment pressure and the containment temperature.

MR. MICHELSON: And what numbers do you have in mind? Design, twice design?

MR. BERNERO: The pressure of interest -- this is for the venting, of course -- the pressure of interest is either as low as design pressure or as high as 1.5 times design pressure for venting. That is in the letter, and the temperature is plant specific, but it is usually --

MR. MICHELSON: That is going to be the point at which you would first start this auxiliary spray?

MR. BERNERO: Yes. His procedures tell him under this symptom at this temperature start spray.

MR. MICHELSON: Up to that point he is following his conventional mitigating steps?

MR. BERNERO: I might add something here that is rather significant. There is already an evolution among owners, BWR owners, Vermont Yankee the first one. To containment vent during an event -- this is the noble gas release -- is not something they will permit their operators to do without authority.

Vermont Yankee has already said publicly they intend to finish their procedures now the way the French do it. The French operator has an EOP to go into an incident

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procedure and accident procedure or a loss of function procedure, but he is not authorized to vent the containment.

The technical support center and above must authorize that, and that is exactly the tone that Vermont Yankee has taken publicly already.

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We may very well end up that way. And that decision, since it's an evolutionary thing some hours into the sequence, would be a reserved decision.

DR. OKRENT: When you're talking about things that are not designed for class one conditions, I recall vaguely a study funded by the NRC and done by some group in California using something called a chilled service system which, if I remember correctly, would also get at least some fraction of the noble gases.

Maybe I'm wrong.

MR. BERNERO: Some cryogenic, maybe.

DR. OKRENT: Yes, and for which their estimate was $2.5 million. In other words, venting is here through something like that and high, but in this case, not taking the full noble gas load as a release and being much more confident on other things.

MR. BERNERO: I'll show you a cost benefit thing in a moment that I think is a better vehicle to address that. Let me take that up.

MR. EBERSOLE: You said at Vermont Yankee they can't vent unless they go through --

MR. BERNERO: The decision has to come from the boss.

MR. EBERSOLE: Is that strictly post-core damage failure?

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MR. BERNERO: Oh, yes. Dirty venting. Clean venting, they have a procedure.

MR. EBERSOLE: Okay.

MR. BERNERO: But if it's high activity level in the containment, they must get authorization.

(Slide.)

The generic letter postulates two mechanisms of justification. Why is this justified?

First is that it is needed for safety, needed for safety in the sense that it is not a tolerable range of debate. If the fraction of core melts resulting in large release is arguably somewhere in the range of 10 percent to 90 percent, that's just not good enough.

And at the inconclusion of these enhancements, you know, these more reliable sprays, and venting and so forth, could at least shift that range of debate into the next order of magnitude below. It's not clear what kind of improvement. We don't have any good, well-developed quantification of it.

But, that shifting the range of debate to some greater, or I should say lesser fraction resulting in a large release, or greater confidence of mitigation, and that it can be justified, this is potentially interpretable as Commission policy.

It is July 16, 1986, the Commission's response to

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Chairman -- or Congressman Markey, or whatever the House Committee is that he chairs, and the question was about a BWR with Mach I containment.

He referred to Harold Denton's speech citing a 90 percent chance of failure. Again, that's a 90 percent fraction of core melts resulting in a large release -- is that acceptable?

And this is the Commission's answer. I reproduce only the first part of it, and it says:

"The likelihood of core melt should be very low and, in addition, there should be substantial assurance that the containment will mitigate the consequences."

Then it went on to refer to other questions, and things like that. So one can argue on those grounds you can't stand up with a straight face and tell me 10 percent to 90 percent is substantial assurance that the containment will mitigate.

And perhaps we can feel that assurance if the debate is between 1 percent and 10 percent. That's one ground.

The other ground, consistent with the severe accident policy statement, is cost benefit analysis.

(Slide.)

And you can get in a great debate. The owners group kindly provided some costs for the envisioned changes.

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The range of costs they gave was $.7 to 2.2 million dollars per plant. Obviously, I prefer to look at that as, let's say, $3 million a plant, rounded upward a little bit.

If you went out to implement it, you might run into a hardware problem you didn't foresee. And it's very easy to incur an additional half million dollars or so expense.

Also, we're in a context where emergency procedure guideline, revision and implementation is free. It's not really free. It's quite costly. But those are separate requirements. They're like overhead, you've got to do them anyway.

And that requirement is when we're through with the emergency procedure guideline, Rev. 4, we should be there with appropriate accident management and we're very close now.

So that cost is not really counted in here. You're only really counting the cost of spray enhancement and the cost of venting improvement. We did a base calculation and a series of others. I'm using the old terminology here: frequency of core melt, conditional containment failure probability before the enhancements, and to the enhancements, the averted loss per year monotized. And then a present value of averted loss calculated two

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ways.

DR. REMICK: What's the averted loss there?

MR. BERNERO: The averted loss was taken as the paper says. If you go to the Strip Report, remember the Sandia siting study and the site by site analysis of citing source term releases and their economic costs, you can identify for a large release -- the numbers vary from site to site -- but a good representative value is 10 to the 7th person rem per large release as a typical value.

I used that and multiplied it by a thousand dollars a person rem. Then, of course, it's simple arithmetic.

Frequency of core melt held constant times delta failure probability times 10 to the 10th dollars. And that gets you an averted loss per year. You know, it's a frequency of averted loss.

And to get a present value, you have a philosophical question. You're monotizing human life or human health. Should you have a discount rate associated with such monotization.

DR. LEWIS: Yes.

MR. BERNERO: I say yes also. But, nonetheless, the argument is made occasionally, so I do it two ways. I multiply by 8, which is a 12 percent discount rate, present worth at 12 percent. Then I multiply by 30 the remaining

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life of the plant, 30 years, with no discount rate. So you can take your pick.

DR. REMICK: Let me point out you're not discounting deaths, you're discounting the dollars, which makes sense.

MR. BERNERO: No, I'm discounting the monotized value of health effects. Now I have not considered economic effects, but there is a reference in the letter. Economic effects can swamp this calculation. They can easily be 10 times greater than the monotized health effects.

So the base calculation uses a cautious 10 to the minus 4 per year frequency of core melt and says: Let's sit like Solomon. If NUREG 1150 comes out with 90 percent and IDCOR comes out with 10 percent, we'll call it 50 percent.

And let's say it's worth a decade. Actually, it's not terribly sensitive to this number, as you can see yourself. Going down a full decade isn't necessary. The averted loss per year, you say, well, the $3 million a plant or a little less, it's worth it.

Lower frequency of core melt. You drop down to be debatable. Certainly, debatable. Slightly below being worth it. Less change in the containment.

If, instead of dropping down by a factor of 10, you drop only by a factor of 5, at one significant figure, you can't even see it. If you had a better containment to

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start with, what if Peach Bottom, IDCOR was right to begin with? You'd come down still in the debatable range.

If you use an optimistic calculation -- by that, I mean a low core melt frequency of the better end for the containment to start, you come out slightly less than worthwhile.

And of course, if you use the pessimistic calculation of a high core melt frequency, the 90 percent to start with, then come down, it's clearly worth while.

The judgment that's made is it's a justifiable backfit under the backfit policy. Therefore, go ahead and do it, and there is a sufficient exploration of uncertainty here to develop confidence in that choice.

If you would go to something else, Dave, if you would go to a cryogenic capture of the noble gas activity, what you have to do now is filter out the person rem of noble gas and justify that additional cost. And it just isn't worth it.

You see, what we're justifying here is a cost in the same order of magnitude as the cost of an additional cryogenic filter. It just simply isn't worth it to throw any more bells and whistles at this.

DR. OKRENT: I think if you go through the procedure that you have just done, that's the result you'll get. However, were such an accident to occur and let's say

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you had implemented the things that you're recommending, and there had nevertheless been this full release essentially of noble gases, and the question at the Presidential Commission was asked: Was there nothing you could do for the noble gases?

One said, well, if we spent another, let's say, $2.5 million, we might have cut those down by 90 percent.

And you say, This is a plant that would have cost $2 billion to replace at the time it would have cost $2 billion to build that chill filter thing. And you'd say, Yes.

Now, tell me, what is this cost benefit you're talking about? Because I really don't understand.

MR. BERNERO: Dave, that argument was the very same line of logic that had us debating like a bunch of antic fools for a year, about 50,000 curies of krypton 85 in the TMI building. We wouldn't even open an airlock for a measurement because if 40 curies of krypton 85, we're talking with the noble gases about a potential one hour after shutdown of high dose, but doses that are down in a range with a high release, clinically detectable effects or lower are substantially lower than that.

And you would be integrating person rem at the very, very low end of exposure, which I don't think is a responsible use of national resources. And that's why I

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don't do it.

DR. OKRENT: I'm just saying there may well be a public perception that's very different.

MR. BERNERO: That was the same public perception that said that we should vent the krypton 85 at TMI on a 2,000 foot hose attached to a tethered balloon. And that's where we have a witch doctor, that's not science.

(Laughter.)

DR. OKRENT: I'm sorry. I don't want to mix the small doses.

MR. BERNERO: But, even these doses for that short time.

DR. OKRENT: Secondly, at the moment, not having looked at the possible advantages, let's say, of something like a chilled filter, I'm not an advocate of a chilled filter. It's just something I recall was paid for by an NRC study.

But it may be that if and when you look at earthquakes, you will find it has an advantage there. I don't know until one looks.

So I'm reluctant to act as if one has enough knowledge to close the door.

MR. BERNERO: Dave, you just referred to the frustrating discussion of core melt. Since 20 years ago, 1966, you go back farther than I do. I reached my

1 DAV/bc

threshold of frustration quite a while ago.

DR. MARK: Bob, I think you've gone through the picture for us.

MR. BERNERO: I just was putting up the schedule.

(Slide.)

DR. MARK: You have a mind to send a letter up to CRGR and subsequently to the Commission.

MR. BERNERO: The CRGR is actually December 22nd. There's a slight adjustment. And presuming generally digestible comments from you and from the CRGR, we would prepare an information paper for the Commission and just tell them this is what we intend to publish for comment, unless you object; because, remember, this would be a staff action, not a Commission action.

It would sort of be like tacit consent on their part.

DR. MARK: You really would like to have us say go ahead, or slow down, or what?

MR. BERNERO: Or whatever. Constructive criticism. If you don't agree, I'll take your names and I'll get you.

DR. MARK: I think I would like to cut this off here.

DR. OKRENT: Can I ask just one question?

1 DAV/bc

DR. MARK: As long as it's short.

DR. OKRENT: It's very short. It's for a future answer. You're familiar with the paper at the ANS meeting by Chu and Stoyanov about a new scenario for intersystem LOCAs in BWRs at the last ANS meeting?

MR. BERNERO: No, I'm not familiar with that. I will look into it.

DR. OKRENT: At some future time, let me know of its import, if any.

DR. MARK: I want to call on Mr. Pickens, but before that, Mr. Remick has something he wishes to tell us.

DR. REMICK: Bob, this is intended to be a constructive comment. Your averted loss present value. I spent a lot of time when you were talking trying to figure that out. Why is it a ratio? Really, I think it would be much clearer if you had averted loss present value method A, method B.

MR. BERNERO: Okay. I didn't intend it as a ratio. I intended it as true values.

DR. MARK: Thank you, Bob.

Mr. Pickens, you have skipped an important meeting, I believe, in order to be here.

MR. PICKENS: They're finished, so I have that latest input. I'll come up here.

DR. MARK: Can you tell us what you need to in

1 DAV/bc

10 minutes?

MR. PICKENS: Yes, I can cover it quickly. I brought 40 copies of the one slide I'm going to be throwing up, pass it around. We can put some in the back of the room.

I just came from a NUMARC working group meeting. I didn't have to miss any of it today; I managed to get through in time.

What I wanted to open with was just some quick comments about the utility perspective on what the current program is and why.

Indeed, NUMARC and the BWR owners group have taken on a parallel program to Bob Bernero's, and then give you a couple of quick comments on some of the aspects of Bob's presentation.

The statement we recently made in a presentation to the Commission in a briefing which they asked for was that utilities do not lack confidence in the capability of the Mach I containment to meet the challenges of a severe accident.

I think this is confirmed, indeed, by the Commissioners themselves in the severe accident policy statement, where they stated that based on the current information, that they felt the plant was safe.

They did identify in the severe accident policy

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that if new information or issues were brought forward, they should be considered an appropriate action should be taken.

We agree with that. I guess that we would have to construe that right now though we don't see new information being brought forward. We see a refinement of some old information that was available at the time that that judgment was made on plants being safe.

And, indeed, the refinements say maybe it didn't go down from whatever that number was in WASH-1400 as low as we would have liked it, or that, indeed, it is going down.

The reason then that we have decided to take on a program and are participating in this process, and we've been trying to work with Bob and work with this committee, the Commissioners, and things, is that, in our opinion, there are uncertainties in the minds of some which don't allow them to state unequivocally that the same conclusion which we have reached -- which is that we have confidence in the Mach I containment in severe accidents when the challenges are there.

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What we would like to do in this program is to help bring them to the point of being able to make the same conclusion which we make. In order to do that, we've identified the program, the slide which we've handed out.

(Slide.)

The overall objective of the program we've put together is to evaluate containment integrity and evaluate potential imnprovements to minimize off-site releases for severe accident conditions beyond the design basis accident within an appropriate cost benefit goal.

I mentioned when I was before the subcommittee on Tuesday, they asked me for some quick comments. We have a lot of people that we are responsible to in terms of being utilities -- ratepayers, stockholders, public utility commissions, and we have to demonstate that we are effectively spending the money which we are investing into these plants.

I think the cost-benefit goal is appropriate, and I have to say, I think Bob has been trying to put together an appropriate benefit analysis to be put through.

We do have some comments on it, on whether or not we think it is appropriate. We haven't gone through the complete process. We don't have it ready to put before you today, but we do think it needs to be done in an appropriate cost-benefit goal.

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The next step in our process is to identify the challenges to containment. This list that you see before here on the seven items is a partial list. You can see the asterisk which says, other issues to be added, and indeed, on the subject of, say, steam explosions, there's been a suggestion by some members that we should put forward to make this a comprehensive document, all challenges to the containment, even those which we have under IDCOR and other industry efforts identify and feel have closed, so that this will be a comprehensive document of challenges to the Mark I containment and how we feel they have been dealt with. And if there are improvements that need to be made from the challenges which are identified under step 2, we will then go to all of the work that's been done on severe accident sequences.

What we plan on doing is, we plan on using the IDCOR work. We plan on using any plant-specific PRAs. We would like to go out and talk to the people at the various National Labs, find out what their concerns have been. What we want to put together is a complete set, whether we agree with them or not, a complete set of the most severe dominant, severe accident sequences, so we can really see what we are talking about, in terms of making everybody feel comfortable, once and for all, that we are going to close the issue of Mark I's capabilities under severe accidents.

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We will then assess the plant's abilities to meet those challenges, identify the vulnerabilities and then propose alternatives to address the vulnerabilities.

The alternatives might turn out to be identical little ones that Bob Bernero has presented. Some of the work we have seen has identified the drywell sprays as being critical, as they have identified venting as being critical. We might not identify any fixes. We are trying to go into this with a very open mind, at this point, saying we might confirm, Bob, we might not. We might come up with some, we might come up with more. Then we will go through and evaluate the alternatives.

Now Bob referenced earlier the cost estimates which have been provided, the 700,000, the 2.2 million, and indeed, those were put together by utilities as part of the process of the IDCOR Owners' Group work in responding to Bob's request on June 16th, when we went in.

I would have to qualify those, that those were order of magnitude type cost estimates. The question which I received on Tuesday on the subcommittee was, what do I think about those?

My gut feel is that they are perhaps a little bit low. The number that we felt comfortable with in that report is, it would cost something around the order of $5 million. That did include core debris barriers. If you

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took those out, it is going to drop down significantly, but I think that it still might be slightly higher than .7 to 2.2.

It is very difficult to use those numbers as absolute numbers. Evaluate the alternatives, using the accidents sequence, the benefit that we see, the risk reduction against the cost.

What we are trying to do is, working within an appropriate cost-benefit goal, realizing there is a NUMARC working group looking at safety goal, cost benefit and that type of thing, we are going to work to put our report in the unit such that when we finish, that group inside NUMARC can, with their current status and their current position get the results of our report, crank it through a cost benefit and then we can present to NUMARC, for their action, some proposal, initiative, whatever.

The Owners Group would present this to the NUMARC working group, and we would work our way back up through the process of NUMARC to identify whether there were any initiatives that the industry would then deem they would take on.

That is our basic program.

The only other couple of points that I wanted to go over, were perhaps the IDCOR number of 20 percent on the conditional containment failure probability. I think that

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there are those inside of IDCOR who feel that it could be identified as lower, and it could be going down as low as 5 percent. It is not a firm number at this point, I don't think. I think if it is moving anywhere, it is moving down.

NUREG 11.50 and the 90 percent. There was a Wall Street Journal back in July which came out, which said, yes, it was going to confirm the old WASH-1400 results. There was a lot of action taken on the part of the industry at that point, a lot of phone calls that went between the Commissioners. It comes down to, in our understanding, two key parameters. That is the ultimate pressure capability of the containment and how does the corium act?

I think that those two key parameters, depending on how the final decision is made to incorporate those into NUREG 11.50, will have a great deal to do with the conditional failure probability that it finally results in.

In fact, there is EPRI work going on in the area of corium phenomonology, and there is a program being sponsored underneath the Owners Group taking a look at the ultimate pressure capability of the Mark I containment. It is currently being used as 135 for the pressure capability. Bob identified that it might go much higher, in fact, indeed even up to three times. I think that some of the preliminary work that I have seen says, yes, it is going to

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go much higher, if those two key parameters are turned around, and you don't assume that the corium is going to quickly reach the liner. If you don't assume that you are going to reach that pressure of the 135 or whatever it was raised up to. It turns around the result drastically, in NUREG 11.50, to whereas, instead of reporting 90 percent, it might come out and report 10 percent or 30 percent or something much lower.

I thinkt that is key in considering what we are putting together here.

One comment on the IPE and the question of seismic capability of containment. The IPE is not structured right now to address the seismic capability of the containment.

DR. OKRENT: I am aware of that, but I am curious that the BWR Mark I Owners Group haven't, of their own initiative, long before this, tried to evaluate the seismic capability of the Mark I rather well. If I were such an owner, I know I would have been glad to pay what is a relatively modest amount of money, compared to what it costs for a day of downtime.

MR. PICKENS: With the amount of work that's gone into the Mark I containment programs, as such, over the years, I think that we feel very comfortable with the seismic capability of the Mark I containment, and we don't

1 DAVbw

see a need to be going into it.

And there is a Seismic Owners Group and there is a whole seismicity issue that is being addressed at another place, and the utilities are participating in that.

DR. OKRENT: There is a seismicity issue that is being examined, I agree, by EPRI, but we are still acting, as far as I know, in any published form, that a good evaluation or maybe even a rough evaluation of the seismic capability and failure modes for Mark I's, I don't know if they are all the same, or there are important differences.

The other thing I am curious about is, whether, in your evaluations, you will allow for things like we heard about at this meeting -- degradation a la Oyster Creek, degradation of some portion of the containment. In this case, it was found -- I don't know what degradation exists that we don't know of, and how this may affect failure pressure and failure place. Perhaps, significantly.

MR. PICKENS: If there were indications that pointed to degradation being a generic problem, and at this point, they point to its not being a generic problem for degradation of the drywell liner, we would certainly incorporate that.

DR. MARK: I think the laast question on which you didn't comment, Mr. Pickens, do you have a time scale in mind?

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MR. PICKENS: We discussed the schedule at the meeting this morning, and it is currently anticipated that we would have a final report on March 31st, and it is at that point that we would then be working through NUMARC to propose any industry initiatives that we are going to take.

DR. MARK: Is that a pretty firm date?

MR. PICKENS: Yes. We feel pretty confident in being able to meet the March 31st date.

DR. MARK: I think if you are willing, I would like to have us leave it there, except for the things we have to decide for ourselves.

DR. OKRENT: I would like to offer one comment to the last response.

It seems to me, when you are looking at things probabilistically, you don't wait until you have a clear indication that there is an important degradation effect, in other words, that the probability is 1 or 1/2 or so forth. it goes counter to the whole idea.

I will leave it at that.

DR. SIESS: The whole idea of what?

DR. OKRENT: Of estimating risk.

DR. MARK: We've had Mr. Bernero tell us the line of thought he's been building up and following. We have had a picture that NUMARC is actually applying itself to similar problems and hopes to have something firm, solid,

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put out in four months. And Bernero is talking of going ahead on a rather more rapid time scale, and we are being asked to say something on the subject, if we possibly can, this week.

I think we've really got the whole picture.

DR. OKRENT: Can I ask one or two things of the chairman, the subcommittee chairman?

Let's say we have the NUMARC report, where they have used methods, and so forth, that the Staff considers to be transparent, or will we then be in that situation, where they have used a code that the Staff has reviewed here and there and so forth.

You understand what I mean. So there is then another six-month period, or whatever.

DR. MARK: I hear ya. I don't know.

DR. OKRENT: I don't either.

Apparently, there will not be a seismic input from NUMARC. I heard nothing that indicate --

DR. MARK: There wasn't any seismic input from the Staff either.

DR. OKRENT: I fought them equally.

(Laughter.)

MR. EBERSOLE: Carson, let me ask you a question. The proposition has been made that we are going to have what is called a source of water that is going to

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go into a spray system which will spray.

The question is, if I've got water to put anywhere, I am going to put it on the core first, whether it is on the floor or not.

I am going to try to put it on where it ought to be, which is in the vessel. If it is gone to the floor, then there must be a hole for me to let it get in there also.

Is there, in parallel with this proposition, that I have that same sort of water excess to fuel to core, not before it gets on the floor, not after it gets on the floor, but before it gets on the floor?

DR. OKRENT: At what pressure, Jesse?

MR. EBERSOLE: That is a question that we ask, at what pressure are we going to assure ourselves that we are going to go to the primary vessel, so that the pressure in it is the same as in the drywell?

DR. MARK: Look. I think we are really at the spot where we don't know.

MR. EBERSOLE: Certainly, that would be a step to take prior to cooling it on the floor.

DR. MARK: We don't know, I believe, what answers deserve to be given.

MR. BERNERO: May I, Mr. Chairman? I have two answers to that question.

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One, this activity is consciously segmented or bining the problem to be containment only, and therefore, does not consider enhancements of the AES or the relieve valve system. Nonetheless, I will give you a very specific example. I mentioned earlier that Vermont Yankee plant has that fire pump.

The procedures set for Vermont Yankee, as they should logically do, give the operator the priority determination, that if you've got the water, and you've got it in that pipe, the first choice is to put it on the core, but if, for some reason, you are not able to put it on the core, then put it in containment.

MR. WARD: Okay.

DR. MARK: I turn it back to you, Mr. Chairman.

MR. WARD: We are going to attempt to draft a letter, I take it, tomorrow sometime?

DR. MARK: I am willing to try to do that, except, I don't know what it should say.

MR. WARD: Well, you've got 15 hours!

(Laughter.)

DR. MARK: I think it will take about that long!

(Laughter.)

MR. WARD: Bob's offered to help you.

(Laughter.)

DR. KERR: Mr. Chairman, I would like some

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indication --

DR. MARK: I hae an inclination to say that we should encourage Mr. Bernero to keep going and go ahead, but encourage the Commission not to jump on things, until they have had a chance to see NUMARC.

That would be my idea of what we might say. I am not sure that it is acceptable. I wonder what the committee would like to have it say.

DR. KERR: I think it should say something like that.

MR. WARD: Okay.

I think we've got a sense.

DR. OKRENT: He's got a sense of three opinions. I am not sure. I think he should try something.

MR. WARD: It is a matter of drafting a letter.

DR. OKRENT: Once it's on the table, the committee will agree or not. You know how they do.

MR. WARD: Yes.

DR. MARK: I believe that is the best we can do, Bob.

MR. WARD: Thank you very much.

Let's take a break for 10 minutes.

(Whereupon, at 3:50 p.m., the recorded portion of the ACRS general meeting was concluded, and the committee recessed, thereafter, to reconvene in unrecorded session.)

# CERTIFICATE OF OFFICIAL REPORTER

This is to certify that the attached proceedings before the UNITED STATES NUCLEAR REGULATORY COMMISSION in the matter of:

NAME OF PROCEEDING: ADVISORY COMMITTEE ON REACTOR SAFEGUARDS
320TH GENERAL MEETING

DOCKET NO.:

PLACE: WASHINGTON, D. C.

DATE: FRIDAY, DECEMBER 12, 1986

were held as herein appears, and that this is the original transcript thereof for the file of the United States Nuclear Regulatory Commission.

(sigt) David L. Hoffman

(TYPED)
DAVID L. HOFFMAN
Official Reporter
ACE-FEDERAL REPORTERS, INC.
Reporter's Affiliation

# POLICY STATEMENT

# ON

# DEFERRED PLANTS

# DEFINITIONS

DEFERRED PLANT

- CP IS IN EFFECT
- CONSTRUCTION CEASED OR REDUCED TO MAINTENANCE LEVEL
- LICENSEE HAS NOT ANNOUNCED TERMINATION

TERMINATED PLANT

- CP IS IN EFFECT
- LICENSEE HAS ANNOUNCED THAT CONSTRUCTION HAS BEEN TERMINATED PERMANENTLY

CANCELLED PLANT

- A PLANT WITHOUT A VALID CP

# DIRECTIVES ON DEFERRED/CANCELLED PLANTS

- PORTIONS OF THE 1985 POLICY AND PLANNING GUIDANCE ITEM IV.B, PLANNING GUIDANCE 2
- REQUEST FOR PROCEDURES TO REACTIVATE A PROJECT AFTER CONSTRUCTION AND LICENSING HAVE STOPPED
- COMMISSIONER ZECH'S REQUEST
- SEVERE ACCIDENT AND STANDARDIZATION POLICY STATEMENTS

## POLICY STATEMENT CONSIDERATIONS

- MAINTENANCE, PRESERVATION AND DOCUMENTATION REQUIREMENTS FOR DEFERRED PLANTS
- APPLICABILITY OF NEW REGULATORY STAFF POSITIONS FOR DEFERRED PLANTS BEING REACTIVATED
- PROCEDURES FOR REACTIVATING DEFERRED PLANTS
- IDENTIFICATION OF REGULATORY IMPROVEMENTS AND RESEARCH INITIATIVES
- POPULATION AND STATUS OF DEFERRED AND TERMINATED PLANTS

# SCOPE

- PROCEDURES DEVELOPED FOR DEFERRAL AND REACTIVATION OF DEFERRED PLANTS

- DOES NOT COVER REACTIVATION OF CANCELLED PLANTS

# ELEMENTS OF POLICY

* MAINTENANCE, PRESERVATION AND DOCUMENTATION REQUIREMENTS IDENTIFIED

  - APPLICABLE REGULATIONS - 10 CFR PARTS-21, 50.55, 50.71, 50 APPENDICES A&B

  - APPLICABLE GUIDES - R.G. 1.28, 1.37, 1.38, 1.58, 1.88 AND 1.116

  - INSPECTION PROCEDURES AS APPROPRIATE

ELEMENTS OF POLICY (CONTINUED)

° APPLICABILITY OF NEW REGULATIONS DURING DEFERRAL

- PLANT-SPECIFIC BACKFITS OF NEW STAFF POSITIONS WILL BE CONSIDERED IN ACCORDANCE WITH BACKFIT RULE - 10 CFR PART 50.109

- GENERIC BACKFITS WILL BE IMPLEMENTED EITHER THROUGH RULEMAKING OR GENERIC ISSUE RESOLUTION

- PROVISIONS OF OTHER POLICY STATEMENTS APPLICABLE TO PLANTS UNDER CONSTRUCTION WILL BE IMPLEMENTED

# PROPOSED REGULATORY IMPROVEMENTS

- REGULATORY CHANGES MAY BE NEEDED TO:
  - ESTABLISH THAT CP REMAINS IN EFFECT (EVEN IF IT EXPIRES) UNTIL NRC WITHDRAWS CP
  - CLARIFY LICENSEE OBLIGATIONS UPON PLANT TERMINATION
- INTERNAL NRC GUIDANCE ON SPECIFIC INFORMATION NEEDED FOR TERMINATION OF CP
- DETAILED GUIDANCE FOR INSPECTION OF DEFERRED PLANTS PRIOR TO REACTIVATION

# AIF PARTICIPATION

° STAFF MET WITH AIF, FEBRUARY 19, 1986

° AIF PROVIDED LETTER, MARCH 31, 1986 ON "REACTIVATION OF CONSTRUCTION PROJECTS"

- AGREES THAT POLICY STATEMENT IS NECESSARY

- POLICY SHOULD APPLY TO PLANTS WITH CPs

- PLANTS WITH WITHDRAWN CPs SHOULD BE HANDLED ON A CASE-BY-CASE BASIS

- 10 CFR PART 50.109, BACKFIT RULE, SHOULD BE USED TO IMPLEMENT NEW REQUIREMENTS IN EFFECT FOR PLANTS WITH CPs

- CURRENT PRESERVATION REQUIREMENTS ADEQUATE AS LONG AS RECORDS MAINTAINED.

° REVIEW AND COMMENTS ON FINAL DRAFT, JULY 2, 1986

# POLICY OVERVIEW

- NO SAFETY IMPLICATIONS

- ESSENTIALLY CONSOLIDATES EXISTING REQUIREMENTS

- COORDINATED WITH INDUSTRY

✓ this copy has both odd + even pages

# CHERNOBYL:
# WHERE DO WE GO FROM HERE?

## Proceedings of the Conference-by-Computer

**Sponsored by McGraw-Hill's Nuclear Publications**
**September 29-October 17**

Where should the international nuclear community turn in the wake of the Chernobyl-4 tragedy? What directions does Chernobyl illuminate for nuclear research, power reactor operations, and nuclear plant regulation? What can we learn from the world's worst nuclear power disaster about preventing accidents in the future?

To seek answers to the complex issues raised by Chernobyl, McGraw-Hill's Nuclear Publications sponsored an international Conference-by-Computer for three weeks from September 29 through October 17. Accessible by anyone with a personal computer and modem or other communications computer, and open 24 hours a day, the conference provided an international roundtable for members of the nuclear community the world over to interact and exchange ideas. The computer was open 24 hours a day so persons in all time zones could participate at their convenience, and more than 80 participants were registered in the conference.

The conference covered Chernobyl's impact in five major topic areas, and a distinguished list of experts contributed their thoughts to begin the international dialogue on each topic. The resulting proceedings may pose more questions than they provide answers, but they assuredly provide a thought-provoking portrait of the issues raised by Chernobyl.

# CHERNOBYL:

# WHERE DO WE GO FROM HERE?

## Index to Proceedings


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Section II.....Summary of the August 1986 IAEA Meeting on Chernobyl -- *by Ann MacLachlan, Nucleonics Week*

Section III.....Implications for Source Terms

Section IV.....Implications for Containments

Section V.....Implications for Human Health Protection

Section VI.....Implications for Accident Management

Section VII.....Financial Implications

| CODE | NAME |
|---|---|
| cwalter | CHARLES WALTER |
| danderson | DAVID ANDERSON<br>Canaton, Inc.<br>Vice President & Mgr. Ontario Operations<br>55 Queen Street, East, 9th Floor<br>Toronto, Canada M5C 1R5<br>Phone: (416) 366-9421<br>Telex #: 065-24582 |
| dbizzak | DAVE BIZZAK<br>Delian Corporation<br>One Monroeville Center, Suite 700<br>Monroeville, PA 15146<br>Phone: (412) 856-5700 |
| dbunch | DELBERT BUNCH<br>Deputy Assistant Secretary for<br>Reactor Deployment<br>Office of Nuclear Energy<br>Washington, D.C. 20545 |
| dmccloud | DENNIS MCCLOUD<br>Tennessee Valley Authority |
| donf | C.D. (DON) FLETCHER<br>E.G. & G. Idaho Inc.<br>Idaho National Engineering Laboratory<br>P.O. Box 1625,<br>Idaho Falls, Idaho 83415<br>U.S.A. |
| dpaseur | DON PASEUR<br>Tennessee Valley Authority |
| drose | DAVE ROSE<br>Argonne National Laboratory |
| dsamek | DON SAMEK<br>Donaldson, Lufkin & Jenrette |
| dtorgerson | DAVID TORGERSON<br>Atomic Energy of Canada Ltd. |
| dwenderoth | DICK WENDEROTH<br>Name: Richard G. Wenderoth (Dick)<br>Employer: Florida Power & Light (FP&L)<br>Address: 700 Universe Blvd.<br>P. O. Box 14000<br>Juno Beach, Florida 33408<br>Department: Nuclear Licensing<br>Interests: Sports & Travel<br>Telephone: 305-694-3595 |

| CODE | NAME |
|---|---|
| ebatchelder | ELEANOR BATCHELDER |
| ehicken | ENNO HICKEN<br>GRS |
| ethrom | EDWARD D. THROM<br>U.S. Nuclear Regulatory Commission<br>Division of Safety Review and Oversight<br>Mail Stop 244<br>Washington, DC 20555 |
| fvanuitert | FRED VAN UITERT<br>Ministry of Economic Affairs |
| ghaywood | GENE HAYWOOD<br>N. Perry RKE |
| ghuston | GAYLE HUSTON<br>Cleveland Electric Illuminating Co. |
| gmalcotsis | GEROGE MALCOTSIS<br>Name: DR. G. Malcotsis<br>Address: Motor Columbus, Ing.-Unternehmung AG<br>Parkstr. 27<br>CH-5400 Baden<br>Switzerland<br>Job Title: Vice-President |
| gmhuston | GAYLE HUSTON<br>CEI |
| halbrecht | HELMUT ALBRECHT<br>Institut fuer Radiochemie<br>Kernforschungszentrum Karlsruhe<br>Postfach 3640<br>D 7500 Karlsruhe FRG |
| hdeckers | HANS DECKERS<br>Studiecentrum voor Kernenergie |
| help | MICHAEL HOLMSTROM<br>ASDFASDF |
| hgloeckner | HANS GLOECKNER<br>Hochtemperaturreaktorbau-GMBH<br>Gottlieb-Daimlerstr.8<br>D-6800 Mannheim Germany<br>Phone: (Germany-W) (0) 621-451-242 |
| hlefebvre | HAZEL LEFEBVRE<br>Belgonucleaire |
| hutch | MICHAEL HUTCHERSON |

# LIST OF PARTICIPANTS

| CODE | NAME |
|---|---|
| abrachet | ALAIN BRACHET<br>Direction des Etudes et Recherches<br>Electricite De France<br>6 Quai Watier,<br>78041 CHATOU<br>CEDEX BP 49, France<br>Phone: (1)30 71 76 94 |
| ajm | ARTHUR MOORE<br>Senior Licensing Engineer<br>Washington Public Power Supply System<br>P.O. Box 1223<br>Elma, Washington 98541 |
| alhomme | ALAIN L HOMME<br>CEA IPSN France |
| amacl | ANN MACLACHLAN<br>European Editor, Nuclear Publications<br>McGraw-Hill Inc.<br>128, rue du Faubourg St. Honore<br>75008 Paris, France |
| ani | LARRY A. CROSS<br>Computer Operations Manager,<br>Nuclear Engineering Department<br>American Nuclear Insurers<br>270 Farmington Avenue<br>Farmington, CT 06032<br>Phone: (203) 677-7715 |
| aprichard | ANDREW PRICHARD |
| aseltz | ANN SELTZ<br>Technical support for conferencing.<br>McGraw-Hill, Inc.<br>1221 Avenue of the Americas<br>New York, NY 10020<br>Phone: 212-512-6898. |
| asharon | DR. ARIEL SHARON<br>Fauske & Associates, Inc.<br>16W070 West 83rd Street<br>Burr Ridge, IL 60521 |
| bcushman | BOB CUSHMAN<br>Niagara Mohawk Power Corp.<br>300 Erie Blvd. W.<br>Syracuse, NY 13202 |
| bdavis | BILL DAVIS<br>PNL |
| bgarner | BARRY GARNER<br>Nuclear Plant Engineering Department<br>Pennsylvania Power & Light Co.<br>1002 Hamilton St.<br>Allentown, PA 18101 |
| bgelbe | BOBO GELBE<br>GWDG |
| bkuczera | BERNHARD KUCZERA<br>Nuclear Safety Projekt<br>Kernforschungszentrum Karlsruhe<br>Postfach 3640<br>7500 Karlsruhe |
| bmann | BRIAN MANN<br>Nuclear Operations Department<br>Virginia Power<br>P.O. Box 26666<br>Richmond, VA 23261 |
| bruegger | BERTRAND RUEGGER<br>OECD NUCLEAR ENERGY AGENCY<br>(NEA),<br>38 Boulevard Suchet<br>F75016 Paris, France<br>Phone: 45 24 96 86<br>Telex #: 630 668 AENEA F<br>Fax #: 45 24 96 24 |
| callen | CARDIS ALLEN<br>U.S. NRC |
| cholmstroem | CONNY HOLMSTROEM<br>Studsvik energiteknik AB<br>S-611 82 NYKOEPING<br>Sweden<br>Phone: int+4615521923 |
| clear | JOHN WALMSLEY |
| clove | CAROL LOVE<br>Licensing Specialist<br>Carolina Power & Light Co.<br>OHS-5, P.O. Box 1551<br>Raleigh, NC 27606 |
| cthomasjr. | CHARLES THOMAS JR.<br>Yankee Atomic Electric Co.<br>1671 Worcester Road<br>Framingham, MA 01701<br>Phone: (617) 872-8100 ext.2645 |

| CODE | NAME |
|---|---|
| ksoda | KUNIHISA SODA<br>Japan Atomic Energy Research Institute<br>Tokaimura, Ibarakiken, Japan |
| kswinth | KEN SWINTH<br>PNL |
| kwoodard | KEITH WOODARD<br>Pickard, Lowe, and Garrick |
| lleach | LARRY LEACH<br>UNC Nuclear Industries<br>P.O. Box 490<br>Richland, WA<br>USA<br>509- 783-9989 (Home)<br>509- 376-5362 (Work)<br>We run N Reactor.<br>Graphite moderated.<br>Pressure tube.<br>4000 Mwt<br>Pressurized water.<br>Negative void coefficient.<br>Diverse fast (1.5s) Scram.<br>Confinement. |
| lois | LOIS WEBSTER<br>American Nuclear Society |
| lpl | LAWRENCE LEACH<br>UNC |
| lrobertson | LARRY ROBERTSON<br>FEMA<br>Emgergency Management Specialist<br>467 J. W. McCormack POCH<br>Boston, MA 02109, USA<br>Telepphone: (617) 223-9565 |
| lwebster | LOIS WEBSTER<br>American Nuclear Society |
| malexich | MILT ALEXICH<br>American Electric Power Co. |
| martyplys | MARTY PLYS<br>Fauske & Associates<br>16W070 West 83 rd St.<br>Burr Ridge Il. 60521 USA |
| mberman | DR. MARSHALL BERMAN<br>Sandia National Laboratories<br>Albuquerque, New Mexico, USA<br>505-844-1545 |
| mchiron | MAURICE CHIRON<br>USSI |
| mdps | MLAFUMA DPS<br>CEA |
| mgoldman | MORTON GOLDMAN<br>NUS Corp.<br>Sr. VP & Technical Director<br>910 Clopper Rd, Gaithersburg, MD 20878<br>301-258-1735 |
| mholmstrom | MICHAEL HOLMSTROM<br>NY Teknik-Stockholm-Sweden |
| mhutcherson | MICHAEL HUTCHERSON<br>Fauske and Associates |
| mkarasulu | MUZAFFER KARASULU<br>NYPA<br>Nuclear Generation Department<br>White Plains, NY 10601<br>Phone(914)681-6842. |
| mminns | MICHAEL MINNS<br>CEI |
| mnh | MICHAEL HUTCHERSON<br>Fauske and Associates |
| mpastor | MARIE-HELENE PASTOR<br>Atomic Energy of Canada, Limited |
| mpreat | MICHEL PREAT<br>TRACTEBEL |
| mryan | MARGARET RYAN<br>Chief Editor, Nucleonics Week |
| msuomela | MATTI SUOMELA<br>STUK<br>Finnish Center for Radiation and Nuclear Safety<br>P.O. Box 268<br>SF-00101 Helsinki<br>Finland |
| npelzer | NORBERT PELZER<br>Inst. f. Voelkerrecht |
| ogremm | OTTO GREMM<br>KWU |
| oparis | OSCAR PARIS<br>NRC<br>Technical Administrative Judge<br>(Environmental Scientist)<br>Atomic Safety & licensing Board Panel<br>U.S. Nuclear Regulatory Commission<br>Washington, D.C. 20555 |

| CODE | NAME |
|---|---|
| itripputi | IVO TRIPPUTI<br>ENEL |
| jbmartin | JERRY MARTIN<br>Battelle |
| jcharles | JEAN CHARLES<br>Framatome |
| jdevine | JACK DEVINE/W. MICHAELS<br>J. DeVine is an Electric Power Research Institute staff member and participated heavily in the TMI post accident cleanup programs. Jack is a conference paper author. W. Michaels is from S. Levy Inc. in Campbell, Calif. Wayne is a senior engineering consultant and is helping Jack monitor the conference for EPRI. Mr Michaels' background is primarily nuclear fuel performance and he is currently the data base administrator for numerous computerized research data bases, including several for EPRI and DOE on Chernobyl. Mr Michaels' experience with computers and nuclear power was also utilized on analysis of the TMI accident. Wayne can be reached by mail for JDEVINE or at SLI (408) 377-4870. |
| jgaunt | JOHN GAUNT<br>Principal Consultant International Energy Associates Limited, Washington D.C.<br>Previously Senior Representative U.K.A.E.A in the U.S.A. Counsellor, Nuclear Energy, British Embassy, Washington D.C.<br>Tel. (202)-342-6728 until October 3 then (703)-246-0444 |
| jgleave | JUDY GLEAVE<br>CEGB |
| jguy | JUDY GUY<br>CEGB |
| jhassberger | JIM HASSBERGER<br>LLNL |
| jjardi | J. JARDI |
| jjim | JULES JIM |
| jmaclellan | JAY A. MACLELLAN<br>Battelle Pacific Northwest Laboratories<br>P.O. Box 999<br>Richland, Washington 99352<br>509-375-2626<br>Batttelle is a primary contractor to the Department of Energy at the Hanford Site in Washington State. Battelie is also a contractor to the U.S. Nuclear Regulatory Commission for Emergency Preparedness at commercial power reactors in the U.S. I am involved in EP evaluations at power plants and standards development and performance testing of radiobioassay laboratories. |
| jmatuszek | JOHN MATUSZEK<br>NYS Health<br>John M. Matuszek<br>Wadsworth Center for Laboratories and Research<br>NYS Department of Health<br>Albany, New York 12201 |
| jpritchard | JANE PRITCHARD<br>CEGB |
| jrausa | JULIE RAUSA<br>Baltimore Gas and Electric |
| jscarborough | JACK SCARBOROUGH<br>MAC |
| jtrotter | JOHN TROTTER<br>NUS Corp. |
| jwalmsley | JOHN WALMSLEY<br>ESCOM<br>McGraw-Hill |
| kkish | KARL KISH<br>Centerior |
| kmaize | KEN MAIZE<br>UCS<br>Union of Concerned Scientists<br>1616 P St. NW<br>Washington DC 20036 |
| kodonnell | KEVIN O'DONNELL<br>GPU Nuclear |
| kshea | KATIE SHEA<br>Newman & Holtzinger |

| CODE | NAME |
|---|---|
| pellison | PHILLIP ELLISON<br>Fauske & Associates |
| pjones | PETER JONES<br>CEGB |
| pkarahalios | PARIS KARAHALIOS<br>Stone & Webster Engineering Corporation<br>245 Summer Street<br>Boston, Mass. 02107<br>Tel. (617) 589-1373 |
| pparshley | PAUL PARSHLEY<br>Donaldson, Lufkin & Jenrette<br>140 Broadway<br>New York, N.Y. 10005 |
| ppirila | PEKKA PIRILA<br>VTT Finland<br>Technical Research Centre of Finland<br>Nuclear Engineering Laboratory<br>P.O. Box 169<br>00181 HELSINKI<br>Finland |
| pschmitz | PAUL SCHMITZ<br>Bechtel Western Power Corporation,<br>Norwalk California,<br>(213) 807-3809 |
| psviatoslavsky | PETER SVIATOSLAVSKY<br>Wisconsin Public Service Corp.<br>Owner/Operator of Kewaunee Nuclear Plant<br>600 n. Adams St.<br>P.O. Box 19002<br>Green Bay WI. 54307-9002 |
| pthome | PAUL THOME<br>Field Automation |
| ravery | ROBERT AVERY |
| rcopp | ROBERT COPP<br>INEL<br>Robert D. Copp<br>Manager, DOE Advanced Reactor<br>Severe Accident Program<br>EG&G Idaho, Inc.<br>PO Box 1625<br>Idaho Falls, Idaho, USA 83415 |
| rfenerty | RETTA FENERTY<br>LP&L |
| rhobbins | RICHARD HOBBINS<br>EG&G IDAHO |
| rllovet | RICARDO LLOVET<br>WNESP |
| rwight | ROY WIGHT<br>Ill. Department Nuclear Safety |
| saicalbert | THOMAS ALBERT<br>SAIC |
| shall | STEVEN HALL<br>SRD/UKAEA |
| ssholly | STEVE SHOLLY<br>MHB Technical Associates<br>1723 Hamilton Avenue<br>Suite K<br>San Jose, California 95125 |
| sstiefel | SILKE STIEFEL<br>Institut fuer Reaktobauelemente<br>Kernforschungszentrum Karlsruhe<br>Postfach 3640<br>D-7500 Karlsruhe FRG |
| tboulette | TOM BOULETTE<br>Maine Yankee |
| tmoore | TAYLOR MOORE<br>Electric Power Research Institute<br>Senior Feature Writer, EPRI Journal<br>3412 Hillview Avenue, P.O. Box 10412<br>Palo Alto, CA 94303 USA<br>Tel. (415) 855-2267 |
| vhuguette | VERSELE HUGUETTE<br>Institut Hygiene Epidemiologie |
| wgraves | WILLIAM GRAVES<br>E.I. du Pont de Nemours<br>Savannah River Laboratory<br>Aiken, SC 29808. |
| wrossbach | WERNER ROSSBACH<br>Interatom GmbH |
| xjardi | XAVIER JARDI<br>Empresarios Agrupados |
| zrevesz | ZSOLT REVESZ<br>Electrowatt Engineering Services, Ltd. |

graphite moderator—which stores large amounts of thermal energy—included the lack of strong emergency shutdown measures, such as a fast shutdown system. For example, the control rods would take some 20 seconds to drop fully into the core. The Soviet report repeatedly emphasizes that because of its high capacity (1,000 MW), the RBMK is used for baseload power operation and should only be shut down when absolutely necessary. Other sources say that it takes three to four weeks to get a RBMK-1000 back up to full power following shutdown. Thus, control systems are arranged in steps according to the severity of reactor disorders, with provisions for power reduction to 60%, 50%, and 25% before the ultimate descent to full shutdown. The concentration of RBMK stations in the heavily populated western USSR suggests that their continued operation was basic to the region's functioning.

Bryan Edmonson of the Central Electricity Generating Board, who chaired the working group on the accident sequence leading up to core disruption, said the accident suggested two points for further attention. First is the importance of placing complete responsibility for safe operation in the hands of a senior officer on site and in the context of a "nuclear safety culture" that envelopes all operations—this is what he called "the principal lesson from the Chernobyl tragedy." Second, the accident underlines the importance of training, education, and development of operator aids. "The (Chernobyl) control room staff were swamped with information," Edmonson said. The Russians have emphasized their desire to upgrade their simulator programs to better model accident situations. (An RBMK simulator at Smolensk apparently is used chiefly, if not exclusively, to simulate normal operations.)

Some western experts observed that the Soviets "appear to have done nothing since Three Mile Island" in the area of man-machine interface, at least on RBMKs, in contrast to the wholescale makeovers of control rooms and other similar initiatives that have been taken in the West. Several of the western experts said they were ready to share experience with the Russians in that field and to help design simulator programs if asked. French representatives confessed to be somewhat puzzled at the Russians' sudden discovery of the importance of man-machine interface, since through bilateral contacts they should have been quite aware of what France and other countries were doing in that domain.

## How did the accident actually happen?

Stated simply, the accident sequence was as follows. The turbine-generator rundown test was supposed to be done at between 700 MW(t) and 1,000 MW(t), but was delayed for several hours because the grid needed extra power from Chernobyl-4. In the meantime, the operators had decided to disconnect the emergency core cooling system (ECCS) pumps so that the test conditions would best approximate emergency conditions. By 1:00 a.m. April 26, all control and safety rods had been with drawn from the core so that power could be raised to the 700 MW(t) level required for the test, compensating for the xenon poisoning that had built up over nine hours of unscheduled operation at low power the evening before and an operator error—failing to properly synchronize the local automatic control rod system and the global automatic control rod system as the second took over from the first at low power—which had allowed the reactor to drop to only 30 MW(t) around midnight. The operator could get the power up to only about 200 MW(t).

The ECCS had been switched off since 2 p.m. the previous day, although operation without it violated "a most sacred rule." All eight main coolant pumps were operating at regimes far higher than normal although the reactor was at only 200 MW(t) (versus design capacity of 3,200 MW(t), increasing coolant flow through the core to several times normal levels. Steam pressure and water level in the steam drum separators began to drop as the coolant was saturated, and the operators disconnected further trip signals associated with those parameters so the reactor would not trip before the test could be performed. The automatic control system pulled rods up with the drop in steam volume.

**The experiment proper began at 1:23:04, when the staff closed the emergency regulating valves** on the second turbine generator (No. 8). When the pumps began to run down with the TG, flow was reduced, the coolant boiled and the reactor power began to rise slowly. Two attempts, at 1:23:40 and a few seconds later, to activate the scram system (which in fact took 6 seconds to "bite" at all) were in vain. Legasov said that even the reason for pushing the AZ-5 (emergency protection level 5) scram button was ambiguous, according to accounts given by the operators "while they were still alive." Perhaps, they said, it was not because the shift supervisor saw the power rising at all, but because "he was sure the tests were going all right" and thus the reactor could safely be stopped.

In any case, at that point, "the reactor was free to do what it wished," according to A.K. Kalugin, of the Kurchatov Atomic Energy Institute. What it did, in fact, was to go prompt critical, reaching 1.5 beta (percentage of prompt neutrons) in less than two seconds. Its power shot up more than 100 times the initial 200 MW(t) in less than a second to reach some 300 calories per gram of fuel, at which point, it is postulated, the fuel disintegrated—in effect, exploded. In this respect, the accident can be said to have been detonated by the explosion of the reactor's overheated nuclear fuel. But the participants in Vienna agreed that the

# PROLOGUE

*This article, which appeared in the Sept. 4 issue of Nucleonics Week, portrays the state of knowledge about Chernobyl after the special International Atomic Energy Agency meeting in Vienna August 25 - 29, 1986.*

## WHAT THE EXPERTS LEARNED FROM CHERNOBYL POST-ACCIDENT REVIEW

*The Chernobyl post-accident review meeting in Vienna was divided into four working groups on the following topics: the accident sequence leading up to core disruption, the sequence post-core disruption, decontamination and evacuation efforts, and medical and biological aspects. The following report by Nucleonics Week's Ann MacLachlan presents digests of information disclosed about each topic, culled from chairmen's summaries, press conferences, and individual interviews, as well as daily summary reports prepared for the U.K. government by John Gittus, head of the Safety and Reliability Directorate of the U.K. Atomic Energy Authority.*

### Why did the accident at Chernobyl happen at all?

The Soviets have, with considerable justification, pinned the cause of the accident on an incredible confluence of "deliberate and willful" violations of operating rules and procedures by the unit's operators. However, Valery Legasov, leader of the Soviet technical team and himself untainted by association with the RBMK reactor type (Legasov is a physical chemist interested in high-temperature and molten salt reactors), told the Vienna meeting that the mistakes had begun with the RBMK designers themselves. Although the drawbacks of the RBMK design were "evident from the beginning," the designers made "a tremendous psychological mistake" in not foreseeing the possibility of such gross operator errors.

Moreover, when the RBMK was designed 25 years ago, automated control technology was considered less reliable than operators, and the units have evidently not been backfitted with more modern control systems. "This is our claim against the designers: now, with hindsight we can see that (the accident) could have easily been prevented using technical means (i.e., engineered safety features)," Legasov said. Dismissal of the chief designer of the RBMK, Ivan Emilyanov, was announced by the Soviet Politburo in July (NW, 24 July, 3).

The immediate genesis of the accident is by now well known. Station management of Chernobyl—incidentally, the best-running RBMK station up to the accident—had contracted with the firm responsible for the unit's electrical system, Dom Tech Energo, for tests of turbine-generator rundown. They wanted to see how long the feedwater pumps could be powered from the still-spinning generator rotor in the event of a loss of offsite power and before the unit's own backup diesels could be switched in. This would have needed to be about 30-40 seconds, as the backup diesels apparently are not the fast-starting kind, possibly for economic reasons.

**Legasov said although there had been "a lot of discussion in the Soviet Union on the justification** for this test in general," the turbine-generator rundown calculations had been incorporated in the RBMK design, and presumably in the safety analysis reports as well. During initial tests of the rundown regime in 1982 and 1984, it was found that the voltage on the generator busbar had dropped off long before the rotor's mechanical energy was exhausted. The Chernobyl-4 team was to test a new impulse block designed to keep the voltage up for the required period of time. Whether this was ordered by safety authorities in Moscow or was an initiative taken by the station itself was not clear and, in fact, a specific question to that effect was not answered by the Soviets.

The latter hypothesis would seem to be strengthened by Legasov's remark that the experiment had not been properly designed nor properly cleared with authorities. It was, in fact, not even regarded as safety-significant by the station management. One of the points still unclear after the Vienna meeting is the underlying motivation for the operators' kamikaze behavior: up to the very last minute, they drove to complete the turbine-generator test, even when the reactor was dangerously unstable and when they had disconnected all safety and protection systems. Much has been made of the operators' apparent ignorance of the dangers of what had been a smoothly running power plant. But Soviet scientists, led by A. Abagyan from the Institute of Nuclear Power Plants (described by Abagyan as the Soviet equivalent of the U.S. Institute of Nuclear Power Operations), also emphasized that the accident occurred at a very bad time psychologically. It was at the end of the work week and early in the morning. It is also worth noting, as Gittus does in his report, that it was the right before the May 1 national holiday. The operators knew that if they did not complete the test that night, it would have to await the next shutdown the following year.

The operators were so determined to complete the test that late in the pre-accident sequence, they attempted to throttle the reactor back up to 20% power—rather than letting it run down as the experiment program called for—so the test could be repeated in case it failed the first time. A French expert said that on the Electricite de France system—based on a standardized reactor series as in the USSR—if such a test could not be done at one unit, it would simply be transferred to another. But perhaps the Chernobyl operators were aiming for some kind of award or distinction to be noted in the station's annual report or for personal recognition for completing the test.

**Design features that contributed to the accident, besides the positive void coefficient and use of**

limited quantity in the steam separators.

Taking into account the possibility of a core meltdown following the explosion, the Soviets developed a special refractory concrete found efficient up to 2000 degrees C for use as a core underpinning, but it proved unnecessary. About 25% of total graphite, or about 250 metric tons, was estimated to have been consumed in the graphite fire. The rest of the graphite blocks are probably in the outer space surrounding the reactor.

V.F. Demin of Kurchatov, repeated the earlier Soviet estimate of 50-million curies for noble gases and 50-million curies of other radionuclides, with a 50% error margin. These numbers are based on measurements taken on May 6. Western scientists say this corroborates with their estimates, made in May on the basis of fallout over western and northern Europe, that 20% or more of the core radionuclide inventory was released. One expert estimates that 20% of the cesium inventory and 40% of the iodine may have been released, including dropping of 12-million curies of radionuclides corresponding to the composition of fragmented fuel on the first day (April 26) in the "near-in zone" assumed to be in the 30-km radius around the power plant.

## Emergency measures taken following accident.

The Soviet experts also outlined the immediate firefighting efforts, with three units deployed to the accident within a matter of minutes. Thanks to their efforts, the fire was extinguished by 5 a.m., mainly using water. Priority was given to limiting the spread of the fire to the adjacent Unit 3, whose turbine building roof apparently had caught fire. They also described the well-publicized and unprecedented program to drop a total of 5,000 metric tons of materials on top of the reactor from helicopters beginning April 27. First came 40 metric tons of boron carbide to stop any remaining chain reaction, then 800 metric tons of dolomite (limestone) to starve the graphite fire of oxygen. There followed 2,400 metric tons of lead for shielding and to carry excess core heat to the bottom of the debris, and finally sand and clay as filtering agents.

However, Legasov noted, this cover had the unsettling effect of increasing the temperature of the core by reducing air flow, and after the dumping was stopped on May 2 because designers of the dropping scheme feared the reactor vault would yield under the weight of the materials, releases began to rise steadily, reaching 8-million curies on May 5. It was at this point that the Soviets decided to pump liquid nitrogen into the reactor from underneath, and this, combined with natural convection of air, led to an eventual drop in temperature and decrease in releases.

Legasov noted that taking measurements had not been easy, especially right following the accident. The high radiation fields—from tens to thousands of roentgens/hour—rendered semiconductor instruments from the U.S., Sweden, and the USSR unreliable for temperature measurements. "The more complex the system," he said, "the less reliable the results in the given radiation circumstances." The Russians used simple devices such as thermionic valve amplifiers and materials of known melting point.

As for the "sarcophagus" planned to cover unit 4, the Russians have chosen an open circuit system over a closed circuit because it facilitates measurements. It is not clear how much of the planned entombment will be finished by this fall. One expert thought it would be the outer wall only, to protect workers coming back to restart Chernobyl-1 and -2 and perhaps unit 3, with the rest of the sarcophagus work taking much longer.

**In his opening remarks, Legasov addressed the subject of the initial administrative response, in**cluding how evacuation decisions had been made. He confirmed that the plant management had promptly informed Moscow—western participants said it had been Abagyan himself—that a full- fledged emergency was underway "a few minutes after the fire began." Abagyan and his team left immediately for the plant site. Unfortunately, Legasov continued, in subsequent communications the plant management "somewhat distorted" their original alarm, "informing us that they could control the already inexistent reactor. They did not adequately understand the situation." When Abagyan and his team arrived after a few hours, they immediately assessed the scale of the disaster, informed the government, and by 8 p.m. on April 26, a governmental response committee had been set up and Legasov himself was on site.

The decision to evacuate the town of Pripyat, only a few kilometers away from the plant, was taken at 9 p.m. even though the first wave of releases had miraculously missed the town. But because of the way radiation had spread around the plant, Legasov explained, "not a single one of our previously prepared emergency plans could be implemented." He said he believed that if the evacuation had begun that night, the loss in terms of human life would have been greater than it was (In fact, no members of the general population have died or suffered acute radiation sickness, according to the Soviets). In effect, the roads were more dangerous than stone houses. The evacuation was organized during the night—by which time the radiation situation was "worsening" around Pripyat—potassium iodide pills had been distributed by

destructive physical phenomenon was a steam explosion.

The small particles of fuel, at 3,000 degrees C, converted the remaining water droplets in the channel to steam in a classic fuel-coolant interaction, which blew off the channel plug and displaced the roof slab. This, in turn, ruptured pressure tubes in all 1,660 fuel channels, exposing them to the air in the reactor building. This was accompanied by "various chemical and exothermic reactions" such as steam-zirconium and steam-graphite, Legasov said.

The Russians, in fact, do not have measurements for the actual accident sequence, since operators had directed the plant's computer to record in priority those parameters linked to the TG test. What they presented in Vienna is a mathematical model of what they think happened, which they now plan to verify with further experiments. A graph shown by Legasov on the first day showed a first, small peak in reactor power, followed by a trough due to an initial reactivity-dampening Doppler effect, then an enormous surge at 1:23:45 as the power shot up off the chart.

**Witnesses were quoted as saying they heard a second explosion three to four seconds after the** first, at 1:23:48 according to the Soviet model, in what "seems to be close to a volumetric explosion" according to Legasov. The Russians assume the second explosion came from detonation of hydrogen or carbon monoxide, although some western experts think it could have been a reactivity excursion and fuel-coolant interaction in another fuel channel. In any case, for the Soviets, the nature of the second explosion is "not their first priority," according to Pierre Tanguy of Electricite de France, the IAEA expert in charge of the accident description.

In his concluding remarks, Tanguy stressed that power excursions are "nothing new for safety analysis." In fact, past power excursions have been on the order of 1 to 10 milliseconds, whereas at Chernobyl the time constant appears to have been much slower, more like a second, although this still must be verified through further experiments. The time constant in a nuclear explosive device is in units of nanoseconds, no greater than one-billionth of a second, he recalled. What happened at Chernobyl "has nothing to do with a nuclear bomb," Tanguy said.

The French expert noted that "the last minutes of that explosion are very interesting for reactor physicists and thermohydraulic specialists." He expressed hope that there would be the opportunity for the international community to use the Soviet data to validate computer models and their underlying assumptions, which could be applicable to reactor designs everywhere. Parametric studies, in which the accident is simulated with parameters changed one by one, also could best answer the question, raised several times last week, of whether the backfits and other measures announced by the Soviets to increase RBMK safety are in fact sufficient. Such studies, he said, are necessary to understand reactivity accidents and to analyze the probability and consequences of severe accidents.

Tanguy quoted Kalugin as saying the explosion involved about 30% of channel volume, in the lower part of the core. There was "some kind of homogenization and a corresponding increase in multiplication" (reactivity). Graphite blocks were ejected from the core. The first explosion was calculated to have generated "tens of atmospheres" of pressure, while the reactor roof slab was designed to resist only two atmospheres. French specialists, working back from the energy needed to rupture all the fuel channels, have estimated the amount of energy released in the first explosion at 200 megawatts per second (200 megajoules).

Tanguy said that what happened to the fuel loading machine is "immaterial" and had no bearing on the accident. Similarly, the Soviets say that the spent fuel pool—as well as all piping and components contained in the leaktight confinement compartments and onsite gas and fuel oil storage tanks—were undamaged in the explosions. "Nothing happened to the spent fuel pool," Tanguy said the Soviets had told the delegates. The water was still in place, as were the approximately 100 spent fuel elements in storage at the time of the accident. (The Soviets reported that 75% of the Chernobyl-4 fuel in-core at that time was still first-load fuel).

**The "first priority" for the Soviets, Tanguy said, is locating the dispersed fuel, so that plans can** be carried out for entombment of the reactor. Abagyan reported provisional figures estimating that 3.5% to 4% of the fuel (about seven metric tons) was released from the reactor, with 0.3% to 0.4% dispersed on site (in aerosol particles from one to several microns in diameter), 1.5% in a 20-km zone around the site, and 2% "elsewhere." The Soviets confirmed that the uranium fuel had undergone oxidation during the accident and may have reacted with the graphite to form carbides. They said they would make available a great deal of data on this subject.

Visual examination confirmed that only about 10% of the graphite had been ejected from the reactor building, and it did not contain fragmented fuel. The Soviets have found no fuel in the turbine hall, Tanguy reported. They believe most of it remained in the bottom of the reactor well and adjacent piping, with a

tims of severe radiation. "They clearly had a well-drilled system," commented one western medical expert. "It was very impressive." He said there might be a link with civil defense organization, but the most accepted view is that the Soviets have previous experience with emergency treatment of radiation victims.

Ilyin himself said that in retrospect, he was astonished at the scale of the response and its quick organization: hundreds of Soviet institutes supplied specialists, millions of dosimetric measurements were taken, and several hundred thousand people were medically examined, including blood tests, by May 10.

In the initial response, two emergency surgical teams were on the way within minutes and four teams within an hour. The first 29 victims of fire and radiation were admitted to regional hospitals by 2:10 a.m., barely a half hour after the accident began. Potassium iodide tablets were distributed to all site workers and patients by 3 a.m. A special multi-disciplinary medical team was alerted in Moscow at 6:40 a.m. and was en route for Kiev by 11 a.m. Iodide tablets were being distributed by medical staff and Komsomol teams by 8 p.m.

Activity in the accident plume reached dose rates of 1,000 millirem per hour on April 27 and 500 mrem/hour on April 28 at a height of 200 meters and a distance of 5 to 10 km from the reactor site. Dose rates of one mrem/hour were measured from aircraft at a height of 1,200 meters about 30 km northwest of the site.

**In terms of composition, the first release was very similar to the fuel inventory of the unit but was** enriched in volatile nuclides such as iodine and cesium. Further away from the site, volatile nuclides were found to dominate completely. However, fuel particles at various distances were found to contain single nuclides, a phenomenon "difficult to understand," according to Dan Beninson, of the Argentine Comision Nacional de Energia Atomica's licensing branch and chairman of the International Commission on Radiological Protection, who led the discussion on radiological aspects.

The initial release contained a large amount of beta-emitters and caused problems of inhalation, leading to severe radiation damage to mouth and nose, Guskova told the experts. Once the most severely irradiated were given bone marrow transplants—all but three have died since, mostly from herpetic infections—there was a second wave of major problems three weeks later due to the recurrence of beta radiation burns: "even decontamination wasn't enough to get (the radioactive dust) all off," a U.K. expert commented. As the personnel had not been wearing high-dose film badges, the victims were divided into dose categories on the basis of what the U.K. specialist called "the vampire syndrome"—lymphocyte counts in massive blood samples drawn in the first 24 hours after admission.

At the time of the accident, there were 176 operational staff on site as well as 268 construction site workers. Of 300 people checked for radiation sickness in the early days, 203 were found to be suffering from it. There were 22 victims in the Category IV (most severely irradiated) level who had received between 600 and 1,600 rads; all but one are dead. Of the 23 victims who received 400 to 600 rads, seven are now dead, while of the 158 victims who received between 100 and 400 rads, only one has died so far.

One of those working at the plant on the night of April 25-26 was not found later; it is assumed he was working on top of the reactor and was buried in the wreckage and/or irradiated to immediate death. Interestingly, Abagyan of the Nuclear Power Plants Institute said that the three operators present in the control room the night of April 25 survived and are "in fine shape" at present, although they have been punished for "severe violations" of regulations (he did not know the nature of the punishment). The four or so people in the turbine hall at the time of the experiment—presumably employees of the electrical firm who were involved in the experiment—did not survive, he indicated.

Ilyin said the accident had emphasized the importance of skin dose in determining the prognosis of accident victims: 20% of the victims had lost 80% to 90% of their skin, mostly from beta radiation. However, Na-24 tests had proved that there was no neutron irradiation involved.

The U.K. specialist said that the Russians had used "good standard medicine, simple hygiene, very basic broad-spectrum antibiotics—no high-tech" methods. It seemed to work, as in his opinion the medical team saved all those who were salvable. All those who received doses under 4 gray (400 rads) are alive today and on the way to recovery, he reported.

**The Vienna participants spent a good deal of time discussing the problem of followup of the** Chernobyl victims and the general population in the European part of the Soviet Union. Beninson said it was agreed that "all irradiated groups that can be studied should be studied." But as for non-stochastic effects in the general population, he noted that past studies had sometimes relied on elaborate registries that had not always proved cost-beneficial. This was one of the most urgent areas for international cooperation, participants agreed. "This is the experiment" on the biological effects of non-ionizing radiation, said one medical specialist. "In 20 to 30 years' time we're going to know whether the linear dose hypothesis" is correct, "at least for leukemia and maybe for lung cancer."

As for the estimates of long-term radiological consequences, the Soviets acknowledge that their models are

volunteers at 8 p.m., and by 11 a.m. April 27, all was ready for the 45,000 inhabitants to be taken away in 1,100 buses. The town was totally evacuated in 2 1/2 hours, the Russians told the Vienna meeting.

Another 90,000 were evacuated in the next 10 days from the surrounding areas including the town of Chernobyl 15 km away, so that altogether about 135,000 people were evacuated from a 30-km radius area. U.K. experts estimate that groups of the 90,000 later evacuees—basically a rural population that spends much time outdoors—received external doses of 30 to 50 rads, and 10 times that to the thyroid from inhalation, assuming they had followed instructions not to drink milk from the area and to take iodine tablets. These doses would be twice as high as those to the Pripyat inhabitants evacuated April 27.

The Russians indicated that their criteria require action when predicted whole-body doses exceed 75 rem. A "whole set of problems" arose at Chernobyl, including where to take the evacuees, how to decontaminate them and change their clothing, and what to do with the tens of thousands of cattle and other livestock. The successful evacuation experience had shown, said East Germany's Harald Rabold, who led the discussions on decontamination work, that "only a strong centralization of authority" could make possible such an operation. It was clear from the descriptions that the military played a key role in the evacuation and further decontamination work.

At the plant, loose cesium contamination proved a predominant problem. Decontaminant sprays were used widely, with vacuum cleaners picking up loose particles. Some quick-drying films were applied to walls, which pick up active particles and can be then peeled off; solid wastes are being stored in the waste repository for Unit 5, which had already been completed. Thanks to the national hydro- meteorology service, which constantly dusted the clouds, rain was postponed until the end of May, although this created problems on the ground as the radioactive dust resettled and whole areas had to be decontaminated again. The video shown by the Soviets in Vienna has a lot of footage on the decontamination effort as well as on the emergency headquarters directing the operation. The decontamination team involved about 1,000 people in protective clothing, with all sorts of equipment including concrete mixers.

**Legasov gave a report August 25 on the state of the reactor and plant four months after the accident.** According to Legasov, the release of activity from the reactor vault has ceased, with only aerosols escaping at the rate of some tenths of curies per day; temperatures above 200 to 300 degrees C have not been found at any point; and air and water temperatures do not exceed 45 degrees C and are stable. A diagnostic system based on buoys lowered into the reactor allow determination of the radiation levels and isotopic content of releases.

Legasov noted that "one of our priority tasks was to shut down units 1, 2, and 3." It was not fully explained why the third unit, just next to the damaged one, was not shut down until 5 p.m. April 26, and the first and second units not for 24 hours. Western specialists suggested that power from these units was crucial to the evacuation and fire-fighting efforts. Now, Legasov said, it has been determined that units 1 and 2 equipment is totally in working order, so the interior of those buildings is being decontaminated with a view to restarting the units by the end of this year. He did not explain how the operation would be organized, although he acknowledged that the "main problem" in restart is finding a new area to house the staff. "The town of Pripyat is not inhabitable for the foreseeable future," he said. Review of unit 3 is now underway, and when it is completed, "we will draw a conclusion" on restart of that unit, Legasov said.

The losses from the accident in the European part of the Soviet Union comprise 203 seriously injured, 31 dead (all these from the plant staff and the fire-fighting teams), collective doses of 9-million manrem to a population of 75-million in 1986 and 29-million manrem over their lifetime. The population in the evacuation zone (135,000) is estimated to receive 1.6-million manrem in 1986. The figures on internal dose, largely through cesium- 137, are based on planning levels rather than realistic ones and are believed by non-Soviet experts to be too high by a factor of 10.

In the future, the area around the power plant will be divided into three economic zones according to level of contamination, Legasov said. First is a "complete isolation" zone, where no work (except presumably operation of the other Chernobyl units) can be resumed in the near future. Second is the evacuation zone (assumed to be in a radius from three km to 30 km from the plant) where, once it is decontaminated, partial economic and industrial activity can be resumed, with livestock remaining a question mark. The third is a "monitoring zone," which is assumed to pose no danger for human health but is maintained under constant surveillance because of the possibility of wind direction changes.

## What have been and will be the accident's radiological effects?

Participants in the Vienna meeting were unanimous in their admiration for the Soviet emergency medical response to Chernobyl, which was outlined in great detail by Leonid Ilyin, director of the Institute of Biophysics and chairman of the USSR National Radiological Protection Board and by Dr. A.K. Guskova, also of the Institute of Biophysics and director of Hospital No. 6 in Moscow known for its treatment of vic-

described above.

The fuel dispersion was reported to involve 3.5-4% of the fuel inventory, i.e. about 7 metric tons. The resulting small fuel particles contained fission products and transuranium elements with concentrations corresponding to the fuel burn-up. It is quite clear, therefore, that roughly the same percentage (3-4%) of all low volatile elements (Mo, Ba, Sr, Ru, Zr, Ce, Np, Pu, Cm,...) has been released. Since the percentage corresponds very well to the amount of dispersed fuel it can be concluded that fuel dispersion was the principal release mechanism for all low volatile elements.

In addition, the dispersed fuel particles may have served as effective carriers for highly volatile fission products like I, Cs, and Te which were released by vaporization from less heavily damaged parts of the core and subsequently condensed on these particles. (The integral surface of the aerosol particles is considerably larger than the surface of all solid structures in the reactor hall.)

## 4. CONCLUSIONS

Since a transient overpower accident caused by prompt criticality can be excluded for western LWRs, the most severe accident to be considered here is a coremelt accident (CMA). However, initiating events as well as sequences and source terms of coremelt accidents are very different from the Chernobyl accident.

A CMA is expected to occur after a loss of coolant accident, if all emergency core cooling systems would fail and the fuel would melt due to missing removal of the radioactive decay heat. The main characteristics of this type of accident have been investigated during the past 12 years, and source terms for a variety of accident sequences, especially for PWRs, have been evaluated. In these cases, the main release mechanism will be vaporization from the liquid coremelt at temperatures in the range of 2200- 2500 C. The low volatile fission products and transuranium elements will contribute very little to the integral source term while the main radiological risk will arise from the released nuclides of the elements I,Cs, Te, and Sb. In the case of an intact reactor building, the radiological source term will result only from leakages containing Kr, Xe, gaseous iodine, and very small amounts of aerosol particles - with the consequence that no long term contamination of the environment will occur.

In our opinion, future efforts on source term related questions should include the following aspects:

-- Completion and refinement of source term analyses for coremelt accidents, in particular; further investigations on the formation of molecular and organic iodine, on the transport behavior of fission products in BWRs after release from the fuel, on leakage rates of aerosols and gases from reactor buildings,...

-- Intensified attempts to make the results of risk studies transparent for the public in order to regain acceptability of nuclear power after the Chernobyl accident.

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source.terms/intro #2, from sysop, 5235 chars, Fri Sep 26 14:41:13 1986

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TITLE: Paper 2
Chernobyl and LWR/PHWR Source Terms

David F. Torgerson
Atomic Energy of Canada Research Company
Pinawa, Manitoba, Canada ROE 1LO

When discussing the relevance of Chernobyl to LWR/PHWR source term technology, it is necessary to enunciate clearly the essential differences between the Chernobyl and LWR/PHWR accidents. For convenience, the Chernobyl source term can be divided into three parts: 1) the initial explosive release, 2) releases occurring during core covering, including those occurring during graphite combustion, and 3) releases occurring after the core was covered.

### Explosive Release

The explosive release was characterized by the direct expulsion of radioactivity to the environment. I believe that such a release is unlikely in LWR/PHWRs for the following reasons. While all reactors have scenarios whereby positive reactivity can be inserted into the core, Western reactors have powerful emergency shut-off systems that are designed to prevent prompt criticality accidents that are characteristic of a particular reactor type. Also, the presence of large, strong containment buildings would attenuate any releases. Even if the containment building were impaired by the accident, our current understanding of aerosol physics and chemistry suggests that there would still be considerable attenuation of the releases, depending on the nature of containment failure.

### Releases During Core Covering and Graphite Combustion

The exposure of the Chernobyl core to the atmosphere also had a large effect on the releases. First, at least part of the core must have been exposed to hot, oxidizing conditions. (In fact, conditions in the core may have been both oxidizing and reducing depending on airflow and on graphite chemistry.) These conditions may have resulted in the oxidation of UO2, which would break up the fuel matrix and, thus, would expose fission products to the environment. In addition, graphite can react with UO2 at temperatures greater than 1500RC which would also affect the fuel matrix. Finally, the burning of the graphite produced a high density of aerosol particles that adsorbed radioactivity, and that were transported to the environment by the thermal plume. Such releases are unlikely for

still insufficient, especially to predict internal doses through the food chain. The special quality of the Ukrainian soil, which is rather low in clay and humus content, may cause an uptake into the soil of cesium-134 and -137 in the area around Chernobyl several times what one would expect in western European soil, according to Soviet calculations based on data and models developed to study weapons fallout.

Some 1,000 whole-body examinations performed so far of exposed people have shown agreement between observed and calculated cesium levels in only about 3% of cases, while the remaining 97% average about 10 times lower than expected. This would indicate that the expected dose of 210- million manrem to the larger population from internal contamination over the next 70 years, which the Russians say is a "maximizing" calculation used for planning purposes, is too high by a factor of at least 10, as Beninson himself pointed out at the conference. This, in turn, would lower the Soviet estimate of cancer fatalities in the Soviet Union from Chernobyl by an order of magitude, from some 25,000 to several thousand.

Although the point was not stressed much during last week's meeting, the Soviet report revealed that environmental damage was expected in the immediate vicinity of the power plant. Notably, hydrobionts (water plants) in the cooling pond had received internal doses of up to 10 rad/hr. Studies of the effects of the accident on living organisms and ecosystems are continuing, the Soviets said.—*Ann MacLachlan, Vienna*

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# THE SOURCE TERM IMPLICATIONS

source.terms/intro #1, from sysop, 7105 chars, Fri Sep 26 14:39:58 1986
There is/are comment(s) on this message.

TITLE: Paper 1

Source Term Implications of Chernobyl
H. Albrecht, B. Kuczera
Karlsruhe Nuclear Safety Center

## 1. INTRODUCTION

The Chernobyl accident which occurred on April 26, 1986, 1:23 h, can be characterized as a prompt critical power excursion driven by positive void reactivity feedback mechanisms. During the transient, the reactor power has met a peak vaue being 1-2 orders of magnitude above nominal conditions. The period of the excursion was estimated to be a few seconds, the transient energy release into the fuel may have exceeded 300 cal/g. Starting from the steady state temperature level, the sudden energy deposition may have caused fuel temperatures of 3000 C and higher, i.e. parts of the core will have reached melt conditions, probably combined with some fuel vaporization. The associated pressure build-up will have ruptured the cladding tubes whereby finely fragmented hot fuel was ejected into the coolant channels. Violant fuel/coolant interactions then caused a strong overpressurization of the fuel channels with the consequence of massive core disruption and, subsequently, of the reactor building. From that moment on, a direct connection between the core region and the environment was available through which core material including fuel particles and fission products were ejected.

## 2. RADIOACTIVE SOURCE TERM

From Soviet information, the following picture can be derived:

Major activity release took place during the first 10 days of the accident, i.e. from April 26 to May 5. It was stopped by covering the destroyed reactor with boron, dolomite, sand, etc. via helicopters. Due to thermal buoyancy and chimney effects a substantial fraction of the aerosol and gas release was transported into high atmospheric regions (1500 m and even higher). The source term which was calculated by the Soviets and published at the Vienna Conference in August 1986 is summarized in the following table:

AMOUNT OF ACTIVITY RELEASED BY MAY 6, 1986

| ISOTOPES | [%] |
|---|---|
| Xe-133 | possibly up to 100 |
| K4-83/85m | possibly up to 100 |
| I-131 | 20 |
| Te-132 | 15 |
| Cs-134/137 | 10/13 |
| Mo-99 | 2.3 |
| Zr-95 | 3.2 |
| Ru-103/106 | 2.9 |
| Ba-140 | 5.6 |
| Sr-89/90 | 4.0 |
| Np-239 | 3.2 |
| Pu-238/39/40/41/42 | 3.0 |
| Cm-242 | 3.0 |

Table 1: Activity release of the main radionuclides, calculated for May 6, 1986 from radiometric measurements; evaluation error +/- 50%.

The radioactive fallout in the European territory of the USSR was estimated to be approxiamtely 50 MCl or 3.5% of the radioactive core inventory on May 6. These numbers do not include the inert gases (Kr, Xe) and the amount of radioactivity transported across the borders of the USSR; both contributions to the overall source term are assumed to be in the same order of magnitude as the 50 MCl mentioned above.

## 3. COMMENTS ON THE SOURCE TERM

The composition of the source term can be understood by assuming two different release mechanisms: a) Fuel dispersion as a consequence of the nuclear excursion and the subsequent explosion; release due to this mechanism occurred as a large puff during the following days when the finely dispersed fuel being scattered over the core region and the reactor building was blown out by a flow of hot air and graphite combustion products; b) evaporation due to increased fuel temperature; according to Soviet estimates, the temperature decreased very shortly after the excursion to vaues below 2000 C; the evaporation release was almost completely stopped on May 6, when the core region was completely covered as

of August 86.

In this respect, the main question to discuss seems to be the following:

3.1. WHAT IS SURPRISING AS REGARDS THE CHERNOBYL SOURCE TERM?

Referring only to what we knew before the IAEA conference, one can say that we were first surprised by four points:

a) the high level of the source term,

b) its high relative content in low volatile fission products and in transuranics (such as Np-239) compared to NUREG-772 relevant data,

c) the distribution of iodine between its different chemical/physical forms (only a few available measurements reveal complete, but they all indicate a high percentage in volatile forms),

d) the necessary rise of the radioactive plume to a very high elevation to explain the quick and long distance transfer.

Of course, the information given later by the Soviets in the frame of the Vienna conference helps us to understand the four points above, but it raises at the same time new topics deserving reflection:

e) the type of the accident (it has been underlined that what happened in Chernobyl--i.e., high release without warning--is a type of accident usually classified in the residual risk--i.e., physically inconceivable or with a very low probability--in the western P.S.A.'s),

f) the actual release history (i.e., the variation of the release rate v. time during the days following the accident, even after the dumping of materials on the disrupted core).

3.2. WHAT ISSUES NEED A DEEP INSIGHT TO CONFIRM OR REFUTE THE PRESENT WESTERN SAFETY ANALYSIS CONCERNING SEVERE ACCIDENTS IN NUCLEAR PLANTS?

All the above points have of course to be explained as best as possible, firstly by the Soviets. But it is necessary to sort out now the points which could call into question our safety analysis concerning severe accidents, if any.

At the IAEA conference in Vienna, the general feeling of the western experts was that the Chernobyl accident was not in general challenging the safety analysis concerning severe accidents. Nevertheless, it is clear that every nuclear-developed country is going to re-examine or to deepen some parts of its philosophy, with the objective of establishing it more thoroughly.

In this spirit, it seems that the following points have mainly to be looked at:

a) probability of occurrence of accidental sequences of the Chernobyl type--i.e., accidents leading to high radioactive release without a warning time. (Clearly this is not included in the source-term topic and not entirely either in the other topics at the conference, such as ``implications for containments," but this subject has to be discussed somewhere);

b)chemical/physical species and relative amounts of radioactive products in the source term (points b and c of 3.1.);

c) plume rise on the site, in relation to the energy released and the meteorological data;

d) future of the radioactive products trapped in the plant (radioactive products are still escaping from the Chernobyl plant, but the discussion must not be restricted to this particular point; e.g., have we correctly taken into account all events or phenomena governing the long-term behavior of the radioactive products inside the plant, such as interaction of contaminated water with corium?)

3.3. WHAT IS IN AGREEMENT WITH OUT CURRENT ANALYSIS?

If one excludes the rise of the plume above the site, it seems that the western organizations which have tried, starting from the meteorological data and the radioactive measurements made in western Europe, to compute the source term leaving the Chernobyl plant, made ``a good guess," taking into account the incertitudes on the data and the models. So this seems to confirm the general adequacy of the models used to compute the dispersion into the environment.

But it must not be concealed that difficulties have been met in performing these computations, mainly due to:

--paucity of radioactive measurements (e.g., a very few gave the distribution of iodine in its possible chemical forms),

--insufficiencies in the meteorological data (e.g., rain or not rain?),-unusually long-distance transfer.

So, even if one can be generally satisfied with the models, improvements have to be made concerning the data acquisition mainly.

4. LAST WORD

It was tried in this paper to ask the main questions concerning the Chernobyl source term and to begin discussion on them without going too far in drawing conclusions.

Thus, the discussion during the conference-by-computer could now proceed as follows:

a) complete the list of questions to be examined,

b) give answers to them if possible, or at least make comments, and

c) make a synthesis on what we think:

--we have learnt from this accident,

--we have to study further, and

--about the adequacy of our current approach concerning source-term research and the analysis for severe accidents in nuclear plants.

LWR/PHWRs. For example, in-vessel releases are characterized by non-oxidizing conditions, and iodine and cesium would rapidly form CsI, which would be less volatile than the I2 that is formed under oxidizing conditions. Also, the fuel would not oxidize to the point where fuel decomposition would occur. Even under in-vessel fuel-melting conditions for LWRs, most of the low-volatile species would remain in the melt.

During ex-vessel releases, dense aerosols may be produced by the core/concrete interaction. However, there are two important attenuation mechanisms in LWRs that were not operating at Chernobyl. First, as mentioned previously, even an impaired containment building would attenuate aerosol emission to the environment. Perhaps more importantly, both LWR and PHWR accidents are characterized by extremely wet conditions in containment. It has been demonstrated in many experimental studies, and from the analysis of reactor accidents, that wet conditions significantly attentuate the source term. For example, in LWRs, aerosols from the core/concrete interaction would likely be released to containment through several feet of water. The water, depending on its depth, would effectively scrub out a good portion of the aerosols. No such attentuation took place at Chernobyl.

Releases After Core Covered

The final release at Chernobyl took place after the graphite fire was out, and when the core was covered with sand, boron carbide, and dolomite. The higher temperatures, due to the reduction of radiative heat transfer from the covered core, increased the releases from the core. In particular, there was an initially high release of iodine, and this may have been due to hot/oxidizing conditions in the core, leading to decomposition of CsI. The releases were apparently controlled by pumping nitrogen under the core to prevent further oxidation and to cool the core. Again, I expect that the presence of water in a LWR/PHWR accident would effectively attenuate long-term releases.

My general conclusion is, then, that the Chernobyl source term may not be directly applicable to LWRs/PHWRs. However, the Chernobyl releases may be able to be at least qualitatively explained by the advances in source term technology that have been achieved over the past 10 years. Also, if indeed there was no fuel melting (as the Soviets claim), then the accident has also confirmed that fuel oxidation by air (or perhaps UO2 reduction by graphite at high temperatures) can also lead to high releases of fission products. In any event, while there are currently many puzzling aspects of the Chernobyl source term, it is highly unlikely that the Chernobyl accident will lead to the discovery of any key areas of LWR/PHWR source term technology that have been overlooked.

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source.terms/intro #3, from sysop, 6970 chars, Fri Sep 26 14:42:35 1986

There is/are comment(s) on this message.

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TITLE: Paper 3

# THE SOURCE TERM IMPLICATIONS OF CHERNOBYL

by

ALAIN L'HOMME

Head of Service of Analysis of Sites, Accidents & Containment (SASC)

Institute of Health Physics & Nuclear Safety (IPSN)

Atomic Energy Commission (CEA)

Nuclear Research Center, FONTENAY-AUX-ROSES (CEN/FAR), FRANCE

## 1. FOREWORD

The only objective of this paper is to launch the discussion between the participants in this conference-by-computer about the following topic: the CHERNOBYL source-term implications. Consequently, the paper tries to raise the main questions without necessarily developing a full response to them.

## 2. PARAMETERS CHARACTERIZING THE SOURCE TERM

In this paper, the source term is understood as being the radioactive releases leaving the outer containment of a nuclear plant. This source term is fully characterized by the three following parameters:

--the levels of the radioactive releases for the different isotopes,

--the kinetics of the releases,

--the chemical/physical forms of the different elements.

It is clear that problems upstream and downstream of the source-term problems will also be addressed during the conference, interfacing with other topics of the conference, mainly:

--upstream: events and phenomena happening inside the plant, governing the parameters of the source term, and

--downstream: dispersion of the radioactive releases into the environment.

## 3. MAIN QUESTIONS ABOUT THE CHERNOBYL SOURCE TERM

Of course the reasoning that leads to these questions starts fom the confrontation of:

--the conclusions of the safety analysis concerning severe accidents in nuclear plants drawn in the world before the CHERNOBYL accident, and

--the analysis of the data collected since April 86 on the CHERNOBYL accident, either by the measurements made on the dispersion of the fission products in the western world, or by the information given by the Soviets, in particular during the IAEA conference at Vienna at the end

EPRI (Electric Power Research Institute), of Palo Alto, California, has compiled a Chernobyl bibliography. This bibliography includes references to some 450 articles, journals and other publications which deal with the Chernobyl AES and the RBMK type reactor. This bibliography represents a pre-accident compilation of material. Articles dating from 1970 through 1986 appear in the bibliography.

The coordination of this effort was handled by S. Levy Incorporated (SLI), of Campbell, California. SLI has computerized this bibliography and has provided full searching capability for all the entries.

The entries in the bibliography were obtained from EPRI sources and contacts. In addition, SLI used its resources to collect information from six large databases, managed by the DOE, among others.

The actual information contained in this bibliography includes:
An abstract of the article; authors; sources; publish date; work date; title; original language; availability of an english translation; and other information. All this data may be searched for any word or phrase the user wants to find.

SLI has also computerized the International Atomic Energy Agency (IAEA) Report prepared by the USSR on the Chernobyl AES presented at the IAEA Expert Conference in Vienna, August 1986. This report is also fully searchable for any word or phrase.

EPRI, with S. Levy Incorporated, would like the Conference participants to be aware of this bibliography and report and its availability. For any further information or questions on this information use the mail system to contact. jdevine.

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source.terms/intro #9, from pkarahalios, 1298 chars, Tue Sep 30 13:43:28 1986
This is a comment to message 4.

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From R. Gardner of Stone & Webster

The effects of thermal plume energy in reducing doses at short distances is not a "surprise" and would be applicable to LWRs.

Although most generic analyses of offsite consequences are based on a cold plume, this is in fact a conservative simplifying assumption.

Source term analyses show that releases from late containment failure sequences are small and that it is only with the very unlikely case of early containment failure that releases of any offsite significance can be derived. In early failure, the release would be typically accompanied by steam and hot hydrogen from the containment atmoshere. Stone & Webster has calculated the plume energy to be in the order of 1.0E+7 cal/sec (about 42 MW), consistent with 1.2E+7 cal/sec attributed to the PWR-2 release in WASH-1400.

As reported by Karahalios & Gardner of S&W at the CSNI Aerosol Specialists Meeting, Karlsruhe, FRG, September 1984, these energies would produce a dose reduction of about an order of magnitude in the first few miles, as compared to a cold release, with small increases at larger distances.

Copies of the paper are available on request - Stone & Webster Engineering Corporation, 245 Summer St., Boston, MA 02107.

R. Gardner

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source.terms/intro #10, from pkarahalios, 2692 chars, Tue Sep 30 14:27:25 1986
This is a comment to message 3.

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From E.A. Warman of Stone and Webster

With respect to the rate of release, i.e. the actual release history, it is significant to pint out that 76% of the "release", as measured by ground deposition in the Soviet Union, occured after the first day. It could thus be concluded that more than three-fourths of the release from Chernobyl was associated with the graphite fire. (Note that the graphite fire was stated to have started approximately ten hours after the start of the accident according to the oral presentation by the Soviet delegation in Vienna.) All other differences between RBMKs and LWRs aside, since LWRs have no graphite the three-fourths of the release associated with the uncontained graphite fire has no relevance to LWR source terms. That is not to say that the remaining one-fourth of the uncontained release from the autocatalytic core dispersal accident at Chernobyl has much relevance to LWRs either. It simply means that the technical conclusions as tp the lack of relevance to the LWRs of the remaining one-fourth of the Chernobyl source term require addressing other issues, such as the inability of creating autocatalytic core dispersal in an LWR and the effectiveness of containments in western nuclear power plants.

I disagree with the author's characterization that the Vienna conference raises "new topics deserving reflection" with respect to the statements wich follow under the heading "e. the type of the accident". The conference indeed raised some new topics, but the topics in item e are not among them in my opinion. For example, the Chernobyl accident is far from "physically inconceivable" for the RBMK design and, as noted above, Chernobyl was not a high release without warning, as three-fourths of the

source.terms/intro #4, from jscarborough, 335 chars, Mon Sep 29 13:40:38 1986
There is/are comment(s) on this message.

TITLE: dispersion of fission products by thermal plume
Does not the very significant thermal energy of the plume, which reportedly carried finely dispersed fuel and graphite out of the reactor into the environment, also have to be considered a "surprise" and included as a new transport mechanism in future risk assessment studies?

source.terms/intro #5, from fvanuitert, 3064 chars, Tue Sep 30 07:20:50 1986
There is/are comment(s) on this message.

TITLE: can the source-term be reduced for existing plants?

G.C. van Uitert
P.O.-box 20101
Ministry of Economic Affairs
The Hague, The Netherlands

1. INTRODUCTION

A number of barriers exist between the radioactive inventory of the reactor core and the enviroment, viz.

a. the fuel itself
b. the cladding
c. the primary circuit
d. the walls of the drywell
e. the inner containment
f. the outer containment

It is difficult to improve the last four in case of existing LWR's (except perhaps by applying a 'FILTRA' containment filter).

What is the situation regarding the first barrier, the fuel itself?

2. SHOULD THE FIRST BARRIER BE IMPROVED?

A hugh effort has gone into developing nuclear fuel. Most of the research was directed at developing uraniumdioxide pellet fuel. As a result fuel elements now have very low leakage rates and because of that the primary circuits remain clean. Nevertheless this research has to continue. Peacefull use of nuclear energy would benefit greatly if a fuel can be developed which does not even release radioactive fission products during core melt conditions.

3. SOME SUGGESTIONS FOR FUEL IMPROVEMENT

A fuel has been developed for the HTGR which has a high retention compared with LWR-fuel. This HTGR-fuel is based on coated particles (i.e. BISO- and TRISO coated particle fuel). Table 1 shows the improved behaviour of coated particle fuel compared to LWR-fuel.

Table 1: Cumulative fractions of released fission products 720 hours after a core overheating accident (according to General Atomic)

| | LWR | HTGR |
|---|---|---|
| Kr-Xe | 1.0 | .17 |
| I | 1.0 | .006 |
| Cs | 1.0 | .0009 |
| Te | 1.0 | .003 |
| Sr | .11 | .00002 |

This raises the question whether coated particles might be applicable in LWR-fuel elements. As far as I can see there are no objections. In the Netherlands some development has been done on sphere pach fuel. Uncoated uranium oxide fuel (with a density of about 88 %) was put in fuel pins. It was then irradiated in the Dodewaard NPP. The fuel behaviour was quite satisfactory even at a burnup of about 30 MWd/kg. As the size of the coated particles is about the same as the uncoated particle size, probably many of the research results are applicable. Probably even better fuel behaviour can be attained by mixing the coated particles with metal particles. This would increase the heat transport coefficient, by which the temperature in the fuel becomes more homogenous. The heat transfer coefficient is also improved.

Through these phenomena the fuel temperature at accident conditions will be lower. Fewer radioactive fission products are probably released. By increasing NPP's safety aspects the confidence of the population in nuclear energy can be restored.

WHO DOES RESPOND?

source.terms/intro #6, from fvanuitert, 88 chars, Tue Sep 30 07:30:05 1986
This is a comment to message 5.

NOTE from the author of message nr. 5.
Read 'sphere pack' in stead of 'sphere pach'.

source.terms/intro #7, from rcopp, 168 chars, Tue Sep 30 10:20:41 1986

TITLE: comment to paper #1
what evidence exists that the fragmented hot fuel initiated violent fuel/cot interactions which subsequently caused pressure tube failures?.

source.terms/intro #8, from jdevine, 1666 chars, Tue Sep 30 12:01:04 1986

TITLE: Chernobyl Technical Reference
To all Conference Participants:

Fuel dispersion as a consequence of the nuclear excursion and the subsequent explosion; release due to this mechanism occured as a large puff during the first phase of the accident and also during the following days when the finely dispersed fuel being scattered over the core region and the reactor building was blown out by a flow of hot air and graphite combustion products.

==========

source.terms/intro #13, from cthomasjr., 286 chars, Tue Sep 30 17:15:09 1986
This is a comment to message 11.
---

I WOULD GUESS THAT RESPONSES TO YOUR LAST PARAGRAPH REQUESTING CORE INVENTORY DATA ,WOULD SHOW THAT RELEASES OF THE VOLATILE FISSION PRODUCTS FROM THE FUEL ARE SIMILAR TO TMI-II.(PREHAPS EVEN THE HYDROGEN). RELEASES TO THE ENVIRONMENT,HOWEVER,WERE ORDERS OF MAGANITUDE LESS AT TMI-II.

==========

source.terms/intro #14, from lleach, 658 chars, Tue Sep 30 23:33:16 1986
There is/are comment(s) on this message.
---

TITLE: Graphite combustion

A few points on graphite combustion may be of interest here. First, due to the low power prior to the excursion, the graphite was relatively cool at the start of the transient, ie 280 C. It was probably cooled further by the initial steam release. Thus

Second, it should be recognized that self sustained combustion of graphite is very difficult to maintain. Blocks of graphite will not ignite in air, even if heated to 3000 F. A channel configuration which limits heat loss, and provides the right air

It is our expectation that Zirconium burned first, driving the graphite reaction.

Comments would be appreciated.

==========

source.terms/intro #15, from lleach, 260 chars, Wed Oct 1 00:00:12 1986
This is a comment to message 10.
---

We need to be more open minded, if we are to really provide the level of safety necessary for Nuclear power to be acceptable. Before TMI, core melts were said to be inconcievable by many. In fact, it took GPU five years to get around to admitting they^.

==========

source.terms/intro #16, from bruegger, 439 chars, Wed Oct 1 05:38:14 1986
This is a comment to message 1.
There is/are comment(s) on this message.
---

There are additional comments to message 1.
---

Question for all participants :

On the basis of what we know today, can we definitely conclude that, in spite of Chernobyl fallout measurements showing high proportions of gaseous iodine, the pre-Chernobyl finding that iodine released from damaged fuel in a severe light-water reactor accident will mainly combine with cesium to form cesium iodide is still valid ?

J.ROYEN
OECD-NUCLEAR ENERGY AGENCY (NEA), PARIS

==========

source.terms/intro #17, from mgoldman, 124 chars, Wed Oct 1 08:28:10 1986
This is a comment to message 14.
---

I hope lleach re-enters his comment, since the last sentences of his first and second paragraphs are incomplete. mgoldman

==========

source.terms/intro #18, from alhomme, 542 chars, Wed Oct 1 10:35:14 1986
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TITLE: comment for the conference manger from A L`HOMME

I feel that this new type of conference is interesting but that some improvment has to be made in the practice. If you are interested I could give you later on my suggestion concerning improvment.

I don`t believe we can achieve before today night a sufficient degree of synthesis concerning the source-term topic. Could you extend tht discussion until Friday night without disturbing the discussion on other topics? I am available until the end of this week anyway.

==========

source.terms/intro #19, from asharon, 362 chars, Wed Oct 1 10:44:41 1986
This is a comment to message 16.
---

yes. Cesium Iodine is the expected form in LWR accidents. The gaseous iodine levels were the result of mechanical break up of the fuel The high fuel vapor pressure required to match the system pressure,for fuel dispersal during the prompt burst, causes the high gaseous iodine levels. These conditions will never occur in commerical western LWR power plants.

==========

source.terms/intro #20, from alhomme, 736 chars, Wed Oct 1 10:54:40 1986
---

TITLE: Comment on messages 4 and 9

Given the accidental sequence of Chernobyl, it is physically logical to have a rise of the first radioactive plum at high elevation (1000-2000m). It would be of course interestig to check if our models concerning the plum rise in function of the energy released

release had more than a day's warning. I believe the issues raised in item e should be deleted from this paper on the source term implications of Chernobyl.

Similarly, I take exception to the author's call for "points that have mainly to be looked at: a) probability of occurence of accidental sequences of the Chernobyl type - i.e., accidents leading to high radioactive release without a warning time". First, accident sequences "of the Chernobyl type" have no applicability to western power plants. Second, accidents involving postulated early releases have been the subject of both PRAs in the west and the enormous international source term effort. Quite contrary to this as a main point that has to be looked at, I believe the Chernobyl accident does not appear to add significantly to this subject, which has already been extensively studied for western power plants.

E.A. Warman

==========

source.terms/intro #11, from pkarahalios, 3584 chars, Tue Sep 30 15:17:04 1986
This is a comment to message 1.
There is/are comment(s) on this message.

---

From E.A. Warman of Stone & Webster

In addressing the radioactive fallout in the European territory of the USSR of 50 MCi or 3.5% of the radioactive core inventory on May 6, the authors points out that "these numbers do not include the inert gases (Kr, Xe) and the amount of radioactivity transported across the borders of the USSR" and go on to state "both contributions to the overall source term are assumed to be in the same order of magnitude as the 50 MCi mentioned above".

Herr Albrecht and Kuczera have touched on a very important aspect of the Chernobyl source term in the above quotations. My comments in this regard are as follows:

(1) The soviet report, and presentation in Vienna, mistakenly label the 50 MCi deposited in the European territory of the USSR as "Estimated releases of radionuclides from the accident". They are deposits in the USSR, not releases (i.e., not source terms).

(2) I agree with my collegues from KfK that the non noble gas radioactivity transported across the borders of the USSR are likely to be of the same order of magnitude as the 50 MCi deposited in the Europen territory of the USSR.

(3) With respect to I-131, the 20% reported to be deposited in the Soviet Union translates into 18 MCi of the 92 MCi core inventory as of the time of the accident 0123 on April 26, 1986. (This corresponds to 7.3 MCi on May 6.) By contrast, approximately 18 Ci of I-131 was released at Three Mile Island. In other words, one million times as much I-131 was deposited arround Chernobyl as was released at Three Mile Island.

(4) One would espect the larger and heavier fuel fines to fall out closer to the point of release and the very small particles, which behave aerodynamically like gases, to be transported over extremely large distances (e.g., to Scandinavia). Indeed in describing the above data, during the Vienna meeting Dr. V. F. Demin of the Soviet delegation stated that approximately 1.5 to 2% of the radioactive core inventory was deposited within a radius of 20 Km and approximately 1.5% beyond 20 Km.

However, the aggregate surface area associated with a larger number of very small (micron sized) particles would have presented condensation sites for volatile fission products which may have far outnumbered the larger particles which obviously were deposited close to the point of release. Thus, one would expect to observe a large fraction of the volatile fission products (particularly iodine) in Scandinavia and elsewhere in western Europe. It would not be surprising to find that the amount of I-131 which was transported beyond the boundaries of the Soviet Union substantially exceeds the 20% of the core inventory of I-131 which was deposited in the European territory of the USSR.

I ask the participants in this computer conference to comment on the above observations and specifically to report a summary of the results of any analyses of the fraction of the Chernobyl core inventory of specific radionuclides, particularly of the 92 MCi of I-131 and 7.7 MCi of Cs-137, which they have determined to be released based on measurements in their countries. These, of course, would be additive to those fractions reported by the Soviets, as they relate only to analyses of deposits on their territory.

E.A. Warman

==========

source.terms/intro #12, from aseltz, 824 chars, Tue Sep 30 15:27:46 1986

---

TITLE: Corrections to the Albrecht, Kuczera paper

The following corrections should be made to the paper "Source term
implications of Chernobyl" by H. Albrecht and B. Kuczera:

1. The authors are from the Karlsruhe Nuclear Research Center (KfK)
2. In the table, Isotopes, k4-83/85m should be kr-83/85/m
3. In the paragraph following, 50 MCL should be 50 MCi
4. Paragraph a, under number 3, Comments on the source term, should read as follows:

consequently to the rupture of all channel tubes. All that is of course an a posteriori analysis, using in particular a reconstitution by computer of the phenomena involved.

---

source.terms/intro #26, from alhomme, 610 chars, Wed Oct 1 14:11:07 1986

---

TITLE: comment to messages 10 and 15

I did not get entirely message 10 on my screen. But it seems to me that the author is misunderstanding my paper 3. Perhaps it is a bad effect of my poor and oversimplified english.

I don`t say that I worry very much about the possibiiity to have a Chernobyl type accident in western PWR`s: I feel this is physically inconceivable. But what I want to underline is that we have to give a lock, after Chernobyl, to the statements which led us in the past to say so.

In other words, as author of message 15 is writing, it is important, in safety business, to stay open-minded.

---

source.terms/intro #27, from alhomme, 866 chars, Wed Oct 1 14:31:18 1986

---

TITLE: comment to messages 16 and 19

Physical-chemical forms of iodine in source-term of severe accidents are depending:

- first of the fuel environment durring degradation (e.g. temperature, H2, control rod aerosols...),
- second of the residence time inside the containment: different chemical reactions develop inside the gaseous phase and the liquid phase (in particular it is not very easy to predict what happens in the liquid phase and what is the
equilibrium constant of different forms of iodine between the gaseous and liquid phases).

Anyway for western PWR accidental sequences, it seems that CsI is the major iodine form in the source-term. It is obvious that research has to continue in this area. For Chernobyl case, it seems to me that explanations given in papers 1 and 2 are sufficient.

It is 7.40 pm in France now: I am going back home. Good-bye.

---

source.terms/intro #28, from rhobbins, 2997 chars, Wed Oct 1 15:02:51 1986

There is/are comment(s) on this message.

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TITLE: Comments to All Source. Terms Conference Participants

An important aspect of the Chernobyl accident is how it fits into our understanding of severe accident technology. We have performed a series of reactivity initiated accident (RIA) tests at the Idaho National Engineering Laboratory which, along with more recent source term research , provides a basis for understanding the Chernobyl accident. We found that a reactivity insertion of about 350 cal/g produced a molten fuel coolant interaction which resulted in a steam explosion and fragmentation of molten fuel. The debris consisted of ruptured, rounded, hollow particles with many voids in the walls. I agree with Albrecht and Kuczera that the 3 - 4 % release fractions of low volatile and refractory fission products in the initial explosive release and in the subsequent five days of decreasing releases during the time the reactor core was being covered with various materials are consistent with the release of fuel particles. The initial release was likely due to the direct expulsion of fuel fragments and the release during the next five days may have been due to the resuspension of fuel particles by strong up flow of air through chimneys in the hot, burning core. The accumulation of materials dropped on top of the reactor core tended to cool the core by absorbing heat, including the heat of fusion, which caused the overburden to melt. In addition, the overburden in both solid and liquid forms probably provided some scrubbing of the aerosols exiting the core.

The releases after day six which showed increasing fractions of iodine and cesium relative to the low volitles are likely due to vaporization associated with increasing core temperatures and oxidation of core materials after melting the overburden.Finally , the elimination of air ingress at the bottom of the reactor by the introduction of nitrogen gas provided core
cooling and elimination of oxidation processes.

It would be informative to retrieve radioactive debris from the surroundings of the Chernobyl site and from more remote locations where fallout occurred and characterize the physical, chemical, metallurgical, and isotopic nature of the material. These results should then be compared with debris characteristics from previous RIA testing. The debris can be expected to supply the evidence requested by R. Copp in this conference (message #7) to determine whether or not a steam explosion occurred.

It is fortunate that reactivity insertions of the energy experienced in the Chernobyl reactor are not possible in Western nuclear power plants. I tend to agree with Torgerson that it is not likely the Chernobyl experience will point to areas of severe accident source term technology that have been overlooked. I do think it is important to demonstrate that we understand what happened at Chernobyl and that this understsnding falls within the context of our severe accident technology data base.

R.R. Hobbins

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source.terms/intro #29, from ani, 518 chars, Wed Oct 1 16:09:24 1986

This is a comment to message 3.

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TITLE: TWO QUESTIONS ON THE SOURCE TERM IMPLICATIONS OF CHERNOBYL

1. What were the radiation levels downwind of

are able to predict the Chernobyl plum rise with a good accurracy. But they are at least 2 difficulties:
- take a good account of the particular meteorological conditions during the first night of the accident,
- make a good guess of the energy release.
I agree with message 9 : it is not expected to have such an energy release for western PWR severe accidents, what is conservative for radiological consequences in the near environment.

==========

source.terms/intro #21, from zrevesz, 552 chars, Wed Oct 1 11:00:40 1986
This is a comment to message 1.
---
Questions to the authors of the initial paper from Dr. B. Pellaud, Electrowatt Eng. Serv. :
The table of your paper entitled 'Amount of activity released' was questioned by participants at the Vienna conference as to the consistency of the figures. In particular Mr. L'Homme stated there that some nuclides were some 100 times too high in comparison with French estimates (Cs,Zr).

Question #1: Are these discrepancies in the Russian information confirmed one month after Vienna?

Question 2: If so, what could be the cause of these discrepancies?

==========

source.terms/intro #22, from zrevesz, 718 chars, Wed Oct 1 11:11:04 1986
There is/are comment(s) on this message.
---
TITLE: Test Scenarion
A basic question from A. Jacobi, Jr., Electrowatt Eng. Serv.

At the beginning of the test which proceeded immediately the Chernobyl accident the stop valves to turbine 8 were shut closed. The reactor was operating at this time at a 200 MW(th) power level (but it was foreseen to run this test at 700 MW(th) reactor power level). Since turbogenerator No. 7 was already isolated beforehand and there was no incentive to scram the reactor, the power and hence steam generation in the primary circuit should have continued during the whole test period at the
same level/rate. Does anybody know where this steam would have been routed to in case the test would have been carried out successfully?

==========

source.terms/intro #23, from alhomme, 1074 chars, Wed Oct 1 11:18:13 1986
---
TITLE: comment on papers 1 and 2

I consider that these 2 papers give the good technical answer to my point 3.2b in paper 3. But:
- point 3.1f has to be studied further (release history during the days following the accident),
- point 3.2a also (I share the general point of view of the authors of papers 1 and 2 about the probability of occurrence of Chernobyl type accidents for western water reactors, but I feel that the demonstration has to be argumented more deeply).
- I make a special comment on one technical point developed in paper 2 (Hello Dave, see you next week in Paris: if you organize a dinner in Ile Saint Louis, I am ready to participate to prolong the discussion...): my distinguished colleague considers only the positive effect of water above corium -fission product filtration-; a negative effect -resuspension- has also to be considered, because this water, after some days is very contaminated, containing almost all cesium and iodine, especially if there is connection between the vessel cavity and the remaining bottom part of the containment.

==========

source.terms/intro #24, from asharon, 553 chars, Wed Oct 1 11:31:37 1986
This is a comment to message 7.
There is/are comment(s) on this message.
---
To my knowledge there is no clear evidence to the exact mechanism of tube failure. It could have been the result of overheating due to direct contact with hot fuel debris that was ejected through the ruptured cladding. There are however some experimental evidence from PBF tests and TREAT that fuel shattering in a power burst generates "fuel dust" with particle sizes of the order of few microns. If this hot fuel dust comes into contact with the coolant, violent boiling can produce a steam spike and may have further contributed to tube failure.

==========

source.terms/intro #25, from alhomme, 972 chars, Wed Oct 1 11:39:54 1986
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TITLE: comment on paper 1 and message 7
As an expert of INSAG, I participated in the Vienna conference at the end of August and in the writing of the INSAG report during the following week.
The soviet experts have in this frame said the following things and, as far as I know, no western expert objected:
- a criticality excursion happened and led to the dispersion of part of the fuel in the coolant, then to a steam explosion. The first failure of the channel tubes was mainly due, not to the pressure increase in the tubes, but to the loss of mechanical properties of Zr resulting from radiative heat exchange between the hot fuel fragments suspended in the coolant and the tube wall;
- this led to a pressure increase in the core compartment, thus to the lifting of the upper shielding plate,

of the test, as the Soviets report, bypassing steam flow to TG 7 would be a slow process of reaching synchronous speed and synchronizing to the grid. Also, it appears that the test involved quick closure of the TG 8 stop valves. It seems unlikely that TG7 could structurally handle the quick increase in steam pressure, even if TG 7 was at synchronous speed at 0% load.

==========

source.terms/intro #38, from halbrecht, 557 chars, Thu Oct 2 11:31:14 1986
This is a comment to message 21.

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We do not know these French estimates. If they were made for severe accidents in Western LWRs, the expected release fractions for Ru, Zr, Np, Pu, ... would - of course - be lower than 1 - 5 % by several orders of magnitude. For Chernobyl, the high release of these elements was only due to the dispersion and transport of very small fuel particles. Since release due to this effect does not occur according to the volatility of the fission products, we do not see any discrepancies in the release tables presented by the Soviets at the Vienna Conference.

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source.terms/intro #39, from wrossbach, 344 chars, Thu Oct 2 12:24:07 1986

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TITLE: A question to all participants from G. Lohnert, Interatom GmbH, Germany.

The fission product Xenon is a strong neutron absorber. In addition, for elevated fuel-element temperatures it is highly volatile. Is it conceivable that a rapid loss of Xenon enhanced the reactor, excursion which had been triggered by the positive void effect?

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source.terms/intro #40, from mkarasulu, 3812 chars, Thu Oct 2 15:34:00 1986

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TITLE: comments to messages 7,24,24 and 33
To : All source.terms participants
From : M.Karasulu,New York Power Authority
Subject : Comments on initiating-event scenario

---

Related messages 7,24,25 and 33 of this conference.

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First of all I thank Dr.Torgerson for his kind response to our questions and all participants for their valuable, enlightening comments and suggestions. We learned alot about source term in last two days. The following issue, however, appears to be still remains unresolved. It appears that we still have significant doubts about the initiating event scenario given by either Soviets or as conjectured by our phenomenological expectations.

Particularly, the sequence of events up to the first explosion is a blur. The amount of positive reactivity inserted due to steaming of under-cooled tubes, which can easily be above one dollar, can cause very sharp localized power spike and damage the fuel. But the amount of energy,i.e. the duration of the power spike, would not be high enough to cause fuel shattering and eventually ejection from raptured clad in the form of fuel dust as in the power burst tests like TREAT. This is very surprising especially for low enriched fuel like RBMK or LWRs. However, I certainly agree with Mr. A.Sharon that this is the case for very high enriched fuel,e.g. over 90% of U235. According to TREAT test participants they have never observed any fuel shattering or any fuel dusts with small particles in LWR type of fuel tests.

Therefore, my question to Mr.R.Hobbins (to his message # 28) is: If INEL RIA tests are valid for LWR fuels,then why are we still saying "WE FEEL THIS IS PHYSICALLY INCONCEIVABLE?" Even though,we know that some scenarios postulated for LWRs can introduce more than one-dollar worth of reactivity (prompt criticality).... Of course,other phenomena such as Doppler and coolant conditions have also be considered during the burst. But in terms of individual fuel tubes configuration of RBMK there is no big difference between CANDU pressure tubes or to a certain extend with in-channel conditions of a BWR bundle, except the void coefficent i.e., the direction of the feedback. Plus, we always count on our comprehensive protection systems to turn over the power in less than 2 seconds except in ATWS conditions. Therefore,

Additional questions on RIA are:

a) what was the energy deposition (cal/gr)

b) duration of burst in hot spot (sec.)

c) surrounding coolant condition (e.g. pressure,void fraction)?

Comments on related computer modeling of the event (related messages #25 and 33):

On the posteriori analysis of the event with computer models, as in the Soviet presentation. To IAEA or EG&G's RELAP5 and BNL's MINET predictions cannot give an answer (for the sequence of events up to the first explosion) to my first question above. However, with these models you can predict the amount of reactivity insertion and consequently the power excursion only on a core-wide basis. Therefore, LOCALIZED power excursion calculations and the duration of the power spike should be resolved first to hypothesize the fuel shattering and the following localized steam explosion.

According to MINET code predictions,BNL concludes that:

1- Broad agreement in analysis supports Soviet explanation.

2- Most mysteries now resolved,some remains e.g., worth of control rods.

3- Core-wide power peak marginally large enough for severe damage. May need RAMONA calcs to determine local peak.

==========

source.terms/intro #41, from psviatoslavskyx, 645 chars, Thu Oct 2 15:47:56 1986



40

Chernobyl as a function of time and distance during the first few days of the accident? Can this data be used in any way to confirm the models used in U. S. source term work?

2. Can the source terms reported by the Soviets at Vienna be verified by observations outside the Soviet Union? Can transport models be verified by observations outside the Soviet Union assuming the source terms are correct?

==========

source.terms/intro #30, from donf, 778 chars, Wed Oct 1 17:41:23 1986
This is a comment to message 22.
There is/are comment(s) on this message.
There are additional comments to message 22.
---
The steam flow boundary condition during the test and for the 4 minutes preceding the test remains a questionable area. The steam flow and separator pressure information shown in figure 4 of the USSR report appear to be inconsistent. If all steam outlet flow were terminated as this figure suggests then coolant system pressure would have risen. Since the pressure information in this figure represents actual plant data, while the steam flow represents a calculation, I would believe the pressure response is correct. This would imply there was steam outlet flow up to the time of the explosion. It is suspected this steam was bypassed to condenser, or perhaps to turbogenerator 7.
.....
comment to message number 22
from Don Fletcher, EG&G Idaho

==========

source.terms/intro #31, from cthomasjr., 179 chars, Wed Oct 1 17:53:16 1986
---
TITLE: TO: Conference Manager
I would like to support the suggestion of A.L'Homme to extend the discussion of source terms if at all possible.

==========

source.terms/intro #32, from lleach, 736 chars, Thu Oct 2 01:58:40 1986
---
TITLE: Graphite Combustion, Rev1
This is a revision to my slightly distorted message on graphite combustion. The points I wished to make were as follows.
1) Initial graphite Temperature should have been low due to low power operation, ie. 280 C.
2) It was probably further cooled by the steam release.
3) Thus, its contribution to the early FP release was probably to act as a heat sink and delay release.
4) Graphite is VERY difficult to burn.
5) Early burning in reactor (ie 10-24 hours) was probably driven by remaining Zirconium, which would drive graphite oxidation.
6) Graphite/water reaction was probably not significant, as graphite was cool when water was available, and no water was available later.

==========

source.terms/intro #33, from lleach, 325 chars, Thu Oct 2 02:04:55 1986
This is a comment to message 22.
---
The reactor should have scrammed at test initiation due to the two TG isolated scram.
It was overridden, and it was not stated what was intended.
We have had RELAP5 analyses performed of the transient.
We can't keep the pressure down without steam bleed, prior to the experiment.
There's something they neglected to tell us!

==========

source.terms/intro #34, from lleach, 259 chars, Thu Oct 2 02:08:34 1986
This is a comment to message 24.
---
The tubes will fail at 800 C or so at normal operating pressure.
Calculations we have done indicated tube rupture propogation is probable.
This would occur due to mechanical loading transmitted through the graphite.
Remember the control rods that went clunk?

==========

source.terms/intro #35, from lleach, 205 chars, Thu Oct 2 02:15:23 1986
This is a comment to message 28.
---
Hi, Dick. There was a fellow with the US delegation in Vienna who mentioned finding minute fuel fragments in Sweden. He said they showed evidence of fracture, not melting. Can anyone confirm?
Larry Leach

==========

source.terms/intro #36, from lleach, 93 chars, Thu Oct 2 02:18:11 1986
This is a comment to message 30.
---
Hi, Don. I assume this is based on the RELAP5 calculations you folks did for us? Larry Leach

==========

source.terms/intro #37, from psviatoslavskyx, 484 chars, Thu Oct 2 10:37:15 1986
This is a comment to message 30.
---
Bypassing steam flow to turbogenerator 7 (TG 7) would be a tricky thing to do. If TG 7 was not in use at the time

above about 300 cal/g. The threshold for severe rod damage and loss of geometry is about 240 cal/g, although rod failure has been observed at 140 cal/g. A best estimate assessment by Brookhaven National Laboratory indicates that the greatest peak axial radial average enthalpy possible in a U.S. LWR is less than 110 cal/g. Therefore RIAs do not pose a safety concern in U.S. LWRs.

References:

P.E.MacDonald, et al. "Assessment of Light Water Reactor Fuel Damage During A Reactivity Initiated Accident," Nuclear Safety 21 (1980) 582-602.

M.S.El Genk, R.R.Hobbins, and P.E.MacDonald "Fragmentation of Molten Debris During A Molten Fuel Coolant Interaction," J. of Nuclear Materials 113 (1983) 101-117.

M.S.El Genk, R.R.Hobbins, and P.E.MacDonald, "Molten Fuel Coolant Interaction During A Reactivity Initiated Accident Experiment," Nuclear Engineering and Design 66 (1981) 247-267.

==========

source.terms/intro #48, from jguy, 277 chars, Mon Oct 6 05:19:29 1986

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TITLE: SOURCE TERM DESCTRIPTIONS

APPENDIX 4 OF THE RUSSIAN DOCUMENT PRESENTED AT VIENNA CONTAINED VARIOUS TABLES OF MEASURED AND PREDICTED RADIOISOTAPE ACTIVITIES IS THERE ANY EXPLANATION FOR THE APPARENT DESCREPANCY IN 134 OS/137 CS ACTIVITY RATIOS/?

PETER JONES, CEGB , UK

==========

source.terms/intro #49, from jmatuszek, 190 chars, Wed Oct 8 09:49:01 1986

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TITLE: Question on a Comment by mhutcherson

In the comments on "containments", message #26 by mhutcherson reffered us to "source.terms" for the full text. I can't seem to find his comments.

==========

source.terms/intro #50, from mberman, 422 chars, Wed Oct 8 13:29:06 1986

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TITLE: COMMENTS ON STEAM EXPLOSIONS (Garbled)

==========

source.terms/intro #51, from mberman, 309 chars, Wed Oct 8 13:32:20 1986

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TITLE: COMMENTS ON STEAM EXPLOSIONS

Message 50 was somehow garbled. It says that a discussion on steam explosions has been added to the conference on Containments. For those of you interested in that subject, it is message no. 27.

Marshall Berman

==========

source.terms/intro #52, from halbrecht, 621 chars, Wed Oct 15 08:29:05 1986

This is a comment to message 21.

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To Zsolt Revesz

Please excuse me for answering so late: It is quite clear from the release mechanisms that all western estimates lead to much smaller release fractions for the low volatile fission products. These are normally based on release by vaporization during core melt accidents. Release by fuel dispersion to the environment are excluded, because accidents of the Chernobyl-type are ruled out for western LWR's.

Considering the unavoidable uncertainties in summing up all measured release data we presently do not see any discrepancies in the data published by the Soviets,cf. table of message #1.

H. Albrecht

==========

source.terms/intro #53, from mnh1, 9141 chars, Fri Oct 17 20:47:17 1986

There is/are comment(s) on this message.

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TITLE: MNH STEAM EXPLOSION COMMENTARY

TO: All Interested Readers

FROM: Michael N. Hutcherson
Fauske and Associates
16W070 West 83rd Street
Burr Ridge, Illinois 60521
USA
312/323-8750

RE: COMMENTARY ON THE OCCURRENCE OF A STEAM EXPLOSION DURING THE CHERNOBYL ACCIDENT

DATE: 17 October 1986

I do apoligize for the slight delay since I originally put my note in the "Containments" section on 6 Oct 86 (Message 26), but here it is finally.

CLARIFICATION OF THE TERM "STEAM EXPLOSION"

I would like to respond to the claim that has been made that a steam explosion was participant in the Chernobyl accident. To my knowledge, no "direct evidence" exists that the fragmented hot fuel (and cladding) initiated a fuel/coolant interaction (FCI), however, at the same time, this is a very plausible explanation of the observed behavior. Caution must be taken in this case in ascribing the level of energetics to this potential FCI. Specifically, the term "steam explosion" has been floating around in some of the discussions that have followed Chernobyl, and it is on this point that caution should be exercised. The occurrence of a technically legitimate

This is a comment to message 1.

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To H. Albrecht:

In your introduction you mention that during the transient, reactor power met a peak value of 1 2 orders of magnitude above nominal conditions. In an EPRI Presentation (ref 1) , it is stated that there were two power surges, the first to 110 times full power, the second to 400-500 times full power. The presentation also states that the "Soviet model suggests" these power surges. Have you heard of these numbers used by EPRI?

(1) EPRI Presentation on Chernobyl:
The Soviet RBMK-1000 Design
The April 26, 1986 Accident
G. Vine, Revised September 10,1986.

P. Sviatoslavsky

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source.terms/intro #42, from mryan, 654 chars, Thu Oct 2 16:04:42 1986

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TITLE: Timing for Reproduction of Conference Proceedings

To all Chernobyl conference participants:

I have received several queries about when the proceeds of this conference will be available. It is our intent to edit out computer garble and reproduce the proceedings as soon as possible after the conference's conclusion. We are uncertain how long this will take, since we do not know how large (or how full of garble) the proceedings will be, but we hope to have the pages ready for reproduction within two weeks after the conference's end. Printing and mailing would then take a week or so more. Margaret Ryan, McGraw-Hill Nuclear Publications

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source.terms/intro #43, from halbrecht, 348 chars, Fri Oct 3 09:37:24 1986

This is a comment to message 41.

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The peak power mentioned in the introduction of our contribution (#1) should be considered
rather as qualitative than as quantitative figures. Since we did not perform detailed calculations on that question we would appreciate to get a copy of the cited EPRI report.

Thank you in advance

H. Albrecht

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source.terms/intro #44, from alhomme, 1049 chars, Fri Oct 3 10:39:57 1986

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TITLE: comment on messages 21 and 38

Sorry, I did not notice message 21 until today. Before to have the soviet report on the Chernobyl accident , we made in France an evaluation of the Chernobyl source term, starting only from the meteorological data and the radioactive measurements made in western Europe (concentrations in air, plateout on ground) and using current atmospheric dispersion codes. We found (this is in our report IPSN2\86 revision 2) that about 20% of the volatile fission products as Cs and I must have left the plant. Looking at relative ratios beetween Cs and low volatile fission products in the radioactive measurements made in western Europe, we found that these low volatile fission products were in excess by some orders of magnitude compared to NUREG 772 data. As NUREG 772 data are relevant to volatilization of fission products from melting fuel, this indicated that some phenomena, other than fuel melting, must have played a role. Obviously dispersion of finely divised fuel outside the plant is a good explanation.~

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source.terms/intro #45, from abrachet, 735 chars, Fri Oct 3 11:39:49 1986

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TITLE: general questions to all participants from

brachet.delalande electricite de france

1 a global and overall assessment of source term has been established but does anybody have indications on the time distribution of the released fission products I Te Pu ...

2 the doses to the population seem for the upper values linked to external irradiation or contamination by inhalation during the hours preceding the evacuation.

can this be confirmed?

what is the origin of the doses received by the population at this time period(nature of the P>F)?

3 what were the radiological protection measurement equipments: radiometers (gamma ambiant) or aerosols activity from samples?

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source.terms/intro #46, from jmatuszek, 338 chars, Fri Oct 3 15:37:45 1986

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TITLE: Comment to message 10

Quibbling over whether a first-day release of 25% of the iodine source represents a "high release" is symptomatic of the position taken by many in the U.S. power industry. Release of 5% of the core inventory on that first day is still a large release. The issue raised in Paper 3 appears to be a valid one.

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source.terms/intro #47, from rhobbins, 1141 chars, Fri Oct 3 18:03:26 1986

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TITLE: Comment on message #40

There is much literature on RIA testing from several series conducted at the INEL and numerous tests conducted by JAERI in Japan. Fuel fragmentation has generally been observed at axial peak radial average fuel enthalpies

matrix of graphite may have become the "slug" above the interaction/ blowdown zone that may have initially broken the upper boundary of the core compartment.

IN SUMMARY, based on what is technically known to be required to initiate and sustain a steam explosion, and our best knowledge of the conditions within the Chernobyl core immediately before the core disruptive event, it is difficult to positively conclude that the observed damage is the result of a technically valid steam explosion. However, at the same time, due to the rapid power burst and the most probable fine fragmentation of the fuel and cladding that resulted, whatever interaction occurred started at the fine fragmentation stage. Other mechanisms were available, and probably at play, that could have individually, and in unison, produced the witnessed damage. For purposes of genuinely understanding the Chernobyl event and taking what we can learn from it and try to improve the safety of our own reactors, it is not wise, nor does it serve any productive purpose, to simply allege that a steam explosion was at play in this event when there is no solid evidence to support this claim. This is also the difficulty encountered in supporting the claim of a steam explosion in most large scale, in-pile, integral tests, and the applicability of such information to genuinely understanding western style LWR core damage scenarios. Extreme care should be exercised in attempting to translate what apparently occurred in Chernobyl to western style LWRs particularly in view of the current consensus of the knowledgeable community that such explosive interactions may be dismissed in the latter reactor group based on well controlled confirmatory experiments.

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source.terms/intro #54, from mberman, 131 chars, Mon Oct 20 14:17:44 1986
This is a comment to message 53.

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Comment on Steam Explosions

A comment on M. N. Hutcherson's discussion is given in message 48 in the Containments Conference.

END OF SOURCE TERM CONFERENCE

steam explosion in this particular event is simply not known and is only speculation at best at this point because there is no solid proof to support the claim. This is not to say that an explosive type of event did not occur within the Chernobyl core due to the rapid reactivity insertion (perhaps prompt critical) thus creating steam at a very fast rate as we certainly do know occurred. However, this should not automatically be labeled a steam explosion. A distinction needs to be made here between the "free contacting" mode of steam explosion that has been hypothesized in western style LWRs which consists of five (5) major steps (course fragmentation, triggering, fine fragmentation, contained interaction zone, and expansion of the coolant vapor) and the event which occurred at Chernobyl. I agree in concept with Dr. Berman's comment that differences in definition of a steam explosion is not going to resolve what some people still consider as an issue in LWRs. But before an interaction is claimed to be a steam explosion, one must very clearly know what is scientifically meant by that! Concerning the necessity of a shock in Dr. Berman's definition of a steam explosion, such a shock is not nearly as important in this setting as in a chemical explosion because the amptitude of the steam explosion pulse is much less and its wavelength is much longer. In contrast, the major damage potential as a steam explosion is produced by the expansion work done by the steam rather than by the shock-ambient interaction.

REQUIREMENTS OF STEAM EXPLOSION

Due to the power burst in Chernobyl, if the foregoing explosive scenario did in fact occur, it started at the fine fragmentation stage and thus did not experience the previous two stages of course fragmentation and triggering that are required in the "free contacting" mode of this interaction. In addition, if a power burst was the fundamental cause of what ensued, this required the channels to be partially voided, and hence only a limited amount of water was available in the pressure tubes to participate in a hypothetical steam explosion. Also, if the Chernobyl event did start at the fine fragmentation stage, not all of the fragments were liquid, and the solid fragments could not participate in what is legitimately termed a steam explosion. The development of a potential, initially finely fragmented state is a fundamental and extremely important difference from the necessary conditions in western style LWRs which require the two initial steps of course fragmentation and triggering in the free contacting mode to produce such an interaction. It is precisely these two steps that have been shown to be extremely difficult to produce in western style LWRs, and thus, the steam explosion scenario in these reactors is considered to be highly incredible. Thus the choice of the words used to describe the Chernobyl scenario, if a steam explosive event is to be considered, is important and should be technically correct because their meaning and implication are very different in these two vastly different type of reactor designs.

TRIGGERING OF EXPLOSION. The comment has been made a couple places in this conferecne that a steam explosion is predicted (a verified to some extent) to not occur at high ambient pressure (greater than about 1 Mpa).This is ture in the absence of an external trigger. However, it has also been shown that steam explosions can be "made" to occur at higher ambient pressure when the external trigger is commensurately more energetic. This is certainly not to say that such an explosive event occurred in Chernobyl, however, at the same time, the potential triggering effect of the power burst (and ensuing interactions) were most probably quite energetic. When it all boils down, and an objective look is taken at the factual information, it is most difficult to make any convincing and defensive statement(s) about a steam explosion in this event.

OTHER POSSIBLE DISRUPTIVE MECHANISMS

There are at least two (2) other conceivable mechanisms that could have individually produced the damage witnessed at Chernobyl. Furthermore, in reality, the following two mechanisms may well have acted somewhat in unison to produce the observed damage.

VIOLENT FILM BOILING. This type of heat transfer probably occurred because the temperature difference between the overheated fuel, etc and the water coolant was ver. If such a large heat flux occurred over the extensive heat transfer surface area produced by the finely fragmented, yet partially solid fuel, this could no doubt have produced steam faster than it could have been relieved thus overpressurizing the pressure tube(s) and eventually the core compartment thus leading in large part to the observed damage. This illustrates a fundamental difference in safety philosophy and design between pressure tube reactors and western style LWRs. Namely, the latter can withstand a fuel rod(s) mechanically disassemblying due to such a power burst, as in the Chernobyl case, without breaching the primary system, whereas, the former can not. The ensueing local pressurization effect may well have been the mechanism that propagated the failure to surrounding pressure tubes that had been weakened during the power burst.

BLOWDOWN OF THE PRIMARY SYSTEM. Given that the primary system was breached in a few or more pressure tubes in effectively a simultaneous rupture by the rapid reactivity insertion, the blowdown of the primary system from the initial pressure of about 6.5 Mpa could supplied enough energy by itself to rupture the upper boundary of the core compartment. This is primarily because this compartment was designed to remain intact for the failure of only one (1) pressure tube. Of course, in reality, the observed damage may well have been the combined result of simultaneous violent boiling in the pressure tubes and system blowdown through the broken pressure tubes. At this point, it is not unconceivable to imagine that the originally rather tight

setting release mitigation targets.

Swedish experience so far indicates that substantial improvements in the capabilities of containments to cope with severe accidents can be achieved at moderate costs compared with total plant investment.

REFERENCES:
1. L. Hoegberg, The Swedish Program on Prevention and Mitigation of Releases in the Case of Severe Accidents. Proceedings of the ANS International Topical Meeting of Thermal Reactor Safety, San Diego, California, Feb. 2-6, 1986.
2. Nucleonics Week, Jan. 2, 1986, p. 9.

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containments/intro #2, from sysop. No entry.

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containments/intro #3, from sysop, 4698 chars, Tue Sep 30 15:45:46 1986

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TITLE: Paper 2

Implications for Containments
Prof. Enno F. Hicken
Gesellschaft fur Reaktorsicherheit
Federal Republic of Germany

Nuclear power plants must be designed and operated so that the public is protected as reasonable as achievable against the release of radioactive products without undue risk.

It is generally agreed that the prevention of an accident is of highest priority. However, there is an increasing demand to study and possibly mitigate the consequences of severe accidents.

The reactor safety systems will be operated with the objective that a release of radioactive products to the environment is limited to acceptable doses. It is postulated that an active or passive component of the reactor system can fail, e.g. pipes, valves, first shut-down system, etc. A failure is not postulated, if the possibility of a failure is extremely low, e.g. the failure of a reactor pressure vessel.

Nuclear power plants are usually equipped with a leak tight containment, designed to withstand the loads from failures as stated above.

Risk studies, the accident at TMI and results from safety research led to considerations how containments react to severe accidents with postulated scenario including core melt. At least the results, listed below, should be recognized:

-- The time period between initiation of a scenario leading to a major core degradation and the meltthrough of a reactor pressure vessel is usually more than 1 to 2 hours and thus allows for some counter measures (accident management actions).

-- Containments can withstand higher loads than they were designed for.

-- Buildings surrounding the containment have a good potential for fission product retention.

-- If an early failure of a containment can be avoided, several hours or even days are available before containment failure. This time period allows for appropriate accident management actions.

-- Design improvements decrease the potential of a release of radioactive material. There is growing consensus that some mitigation measures can reduce the risk substantially.

When analyzing the accident at Chernobyl and comparing the phenomena with those in postulated core melt accidents of western Light Water Reactors, the author would like to express his personal and first assessments:

1) A major difference exists for the energy input. Due to the super-prompt critical power excursion with a level up to several hundred times the design power, the energy input into the fuel and coolant happened within seconds. In core melt scenario without those power excursions the heat input (mainly decay heat) occurs in the range of hours. Therefore only preventive measures are possible for reactors of existing RBMK-type, e.g. avoidance of a large positive void coefficient, a fast and redundant shut down system, decisions related to safety should be shifted from the operators to an automatic protection system, etc.

2) The temperature of the graphite, resulting from a high initial temperature, several exothermal and endothermal reactions and decay heat from the fuel, causes a long-time fission product release. In light-water reactors without graphite the melt temperature decreases usually within hours resulting in major decrease in the release of fission products.

3) It has been stated by the experts from the USSR that the existing RBMK design with compartments designed to withstand the load from a failure of a single pipe or header was obviously not capable to withstand the loads from one or more power excursions. If those power excursions could not be brought to an acceptable low probability, it should be studied further, if a containment could be designed to cope with the loads from power excursions.

4) The analytical tools and experimental results developed to assess the severe accident phenomena and sequences can only partly be used for accidents as happened at Chernobyl.

# IMPLICATIONS FOR CONTAINMENTS

containments/intro #1, from sysop, 4704 chars, Fri Sep 26 14:43:46 1986

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TITLE: Paper 1

Implications for Containments
-- the Swedish Approach --
Lars Hoegberg
Swedish Nuclear Power Inspectorate

The Chernobyl catastrophe again underlines that prevention of accidents must remain a first priority in nuclear safety work.

Nevertheless, the discussion of the containment function will (and should be) intensified after Chernobyl. It is an understatement to say that the accidents at TMI and Chernobyl, taken together, clearly demonstrate the advantages of having a containment remaining intact after an accident involving severe core damage, compared to having to build or rebuild a containment afterwards, under great time-pressure and in the middle of a severely contaminated environment.

Furthermore, Chernobyl underlines the social, economic and political consequences of long-term land usage restrictions and food and water control measures due to ground contamination. To avoid ground contamination involving extensive land usage restrictions (say more than a few tens of a sq. km.) source terms for radionuclides such as iodine and cesium must typically be lower than about 0.1 percent of core inventory. By and large this is a stricter condition on the source term than that implied by avoiding risks of early fatalities and radiation illness outside the plant.

In Sweden, a new safety philosophy with respect to the containment function was formulated in the wake of the TMI accident. The key elements in this safety philosophy are the following:

(1) Even if preventive safety is high, the possibility of an accident involving severe core damage must be recognized.

(2) If an accident involving severe core damage occurs, releases to the environment of substances causing ground contamination (such as iodine and cesium) should be kept below 0.1 percent of the inventory of a 1800 MW (th) core for most events going beyond present design basis (Extremely improbable events such as fracture of the reactor pressure vessel need not be considered). Releases of noble gases may be accepted if necessary to avoid more severe releases.

The severe accident management strategies now being implemented in Sweden to reach the "0.1 percent mitigation targets" includes the following key elements:

(1) Prevention of early containment failure including
- protection of weak spots (e.g. penetrations) that may be damaged by direct attack of molten core
- hydrogen control
- means to handle single failure in the pressure suppression system in BWR's during a LOCA

(2) Control of pressure and temperature in the containment so as not to exceed design limits substantially in the first 10-24 hours. Evidently, use of containment sprays and containment flooding will be key factors in controlling pressure, temperature and radioactive aerosols.

(3) Attainment of a long term stable state for site recovery and clean-up, with the damaged core covered by water in a coolable geometry and in an intact depressurized containment.

The mitigation strategies summarized above originates in a Swedish government and parliament decision from 1981 and were reconfirmed in more detailed guidelines approved by the government in February, 1986. The necessary measures should be implemented by the end of 1988. They include installation of filtered venting systems and various other measures of a design-specific nature.

In summary, the large R&D efforts in recent years into severe accident phenomena have provided us with the tools to analyze and improve containment performance in severe accidents. The analyses performed so far indicate that western LWR containments in general have good basic capabilities to prevent large releases to the environment in case of a severe accident. However, as existing containments have not been originally designed to cope with severe accidents, they may have weaknesses that must be analyzed and addressed on a design-specific basis. From a regulatory point of view, the social and economic consequences of land usage restrictions due to radioactive ground contamination must be thoroughly discussed when

containments/intro #5, from ehicken, 1244 chars, Thu Oct 2 09:19:51 1986

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TITLE: A comment on paper 3
A coMMENT ON PAPER 3 PREPARED BY S.C.SHOLLY

I WOULD LIKE TO COMMENT ON THE RELEASED ENERGY. FOR THIS PURPOSE I WOULD LIKE TO PRESENT THE FOLLOWING HANDCALCULATION. THE ENERGY PROduced RESULTS AFTER ALL IN A STEAM PRODUCTION. THE MASS INVENTORY OF THE CORE IS ABOUT 30M**3 OR ABOUT 23 TONS. IF ALL THE WATER IS EVAPORATED, A TOTAL OF ABOUT 40 000 MJ IS NEEDED. TO MY KNOWLEDGE, ALL EXPERIMENTS WITH A FUEL/WATER INTERACTION HAS SHOWN A "EFFICIENCY" OF SEVERAL Percent OF THE TOTAL ENERGY RAPIDLY TRANSFERRED TO THE FLUID.

TAKEN THESE NUMBERS AN ENERGY OF ABOUT 1 000 TO SEVERAL THOUSAND MJ IS TRANSPORTED TO THE FLUID. HOWEVER, IT HAS BEEN STATED BY THE SOVJET EXPERTS THAT ONLY A PART OF THE CORE HAS PARTICIPATED IN THE POWER EXCURSION. THIS WOULD REDUCE THE ENERGY RELEASED TO THE FLUID FURTHER.

E.HICKEN

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containments/intro #6, from lleach, 527 chars, Thu Oct 2 16:24:39 1986

This is a comment to message 4.
There are additional comments to message 4.

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The 'steam explosion' is an hypothesis put forth by the Soviets. We should establish if this is generally agreed to. Our calculations indicate a tube rupture would propogate in their geometry. This would be exacerbated by high tube temperature or pressure, resulting from the power excursion. What evidence is there for steam explosion vs. silmultanous tube rupture? It is my understanding that calculations by others indicate three tubes is enough todisassemble the reactor biological shield. Can anyone confirm?

Larry Leach

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containments/intro #7, from rhobbins, 2010 chars, Thu Oct 2 16:31:28 1986

There is/are comment(s) on this message.

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TITLE: Comment to paper #3

Steam explosions can take place at high pressure as demonstrated by Reactivity Initiated Accident Scoping Test 4 (RIA -ST -4) which was carried out at 6.4 MPa initial system pressure with an energy insertion of about 350 call^H/g (much higher than can be achieved in a Western LWR) at the Idaho National Engineering Laboratory. In this test a PWR -type fuel rod melted and the melt interacted explosively with the surrounding water. The pressure rose to 34.6 MPa in 1.6 ms. The resulting debris was made up of small, hollow, ruptured, spherical particles containing many voids an the particle wall (shell).

On the other hand, water was introduced into the TMI core while large amounts of molten core materials were present without resulting in a steam explosion. A steam explosion occurred in RIA -ST -4 because finely fragmented molten fuel particles were created and were accelerated through water producing a large surface area for heat transfer to the water. There is strong evidence that vapor collapse and a chain -like fragmentation process occurred. However, in TMI a vapor barrier to rapid heat transfer likely formed of the melt crust when cooling water was introduced into the core during cycling of the B -loop coolant pump at 175 minutes. Similarly when molten core materials broke through the crust in the core flowed down into the lower plenum at about 227 minutes, relatively large streams were involved and the acceleration was that of gravity. As a consequence, vapor blanketing slowed the cooling of this material when it contacted water and relatively large particle sizes resulted. A 2 MPa pressure rise corresponding to rapid steam generation was measured at 227 minutes.

In my opinion, it is very important to add as much water as possible to a Western -type LWR as soon as possible. I do not believe conditions to produce a steam explosion during a severe accident in a Western LWR are at all likely to be encountered.

R.R.Hobbins

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containments/intro #8, from mryan, 2775 chars, Thu Oct 2 16:55:09 1986

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TITLE: Query on status of steam explosion energy calculations

Query to the authors of the Containment papers: Messrs. Hoegberg, Hicken and Sholly; and also to M. L'Homme of Source Terms:

Has anyone fine-tuned the calculations of thermal and mechanical energy release in the initial steam explosion(s) that destroyed the reactor? My understanding of the situation so far is as follows:

Over 50,000 megajoules (megawatt-seconds) of thermal energy were released in the first few seconds of the Chernobyl reactor accident, specialists at the IAEA have calculated. The upper boundary of thermal energy release could be something like 200,000 megajoules, the specialists believe, although the minimum is much easier to calculate.

The IAEA experts' conclusion, officials said, is based on the Soviet assumption--backed up by western and eastern experimental data--that the Chernobyl fuel would have disintegrated at energy loadings beginning with 300 calories per gram. The Soviets have told the IAEA that

With regard to the impact of the Chernobyl accident on containment design and reactor safety, the author would like to put the items listed below up for discussion.

1) Is it necessary to reassess power excursions for LWR's?

2) Is it necessary to improve the assistance to operators to cope with unplanned scenario in order to prevent accidents?

3) Should the evaluation be continued to study the possibility to mitigate the consequences of accidents with core melt by
   a) appropriate design provisions
   b) appropriate accident management actions?

4) Have new and relevant physical phenomena be identified which are of importance for the assessment of severe accidents in western LWR's?

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containments/intro #4, from sysop, 4633 chars, Wed Oct 1 10:03:47 1986

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TITLE: Paper 3

THE IMPLICATIONS OF THE CHERNOBYL-4 ACCIDENT FOR NUCLEAR POWER PLANT CONTAINMENTS

STEVEN C. SHOLLY
MHB TECHNICAL ASSOCIATES
1723 Hamilton Avenue, Suite K
San Jose, Cal. 95125
USA
(408) 266-2716

The Chernobyl-4 nuclear power plant accident raises anew many phenomenological issues associated with severe accidents and containment failure modes. It is by now widely understood that the key phenomenological event was a rather large steam explosion. The magnitude of the energy release has been estimated at from 200 MJ to 320,000 MJ, and it may be some time before sufficiently detailed information can be compiled to permit a more precise estimate. It should be observed, however, that the current range of estimates spans an energy release which would pose either a minimal threat to a containment or a release which could not be stopped by any existing containment.

The Chernobyl steam explosion happened under two circumstances of interest in other reactor designs. First, the explosion occurred under high pressure (i.e., greater than ambient). Steam explosions were previously thought by many analysts to be suppressed at high pressure. Second, if the Soviet analysis of the accident is correct, the steam explosion occurred without the fuel first becoming molten. Prior to the accident, both low pressure and the presence of a large quantity of molten fuel had been held to be prerequisites for a steam explosion large enough to threaten containment integrity.

The Chernobyl accident thus raises some interesting questions about the conditions under which steam explosions can occur. For example, in a PWR station blackout accident sequence, if emergency coolant injection is recovered while the core is "in extremis", should one inject coolant (and perhaps risk a steam explosion), or attempt depressurization, permit the core to melt, and deal with the core debris after vessel failure? In addition, the large power surge at Chernobyl (due to the positive void coefficient) has been suggested as the cause for the steam explosion which destroyed the plant. Even if this is so, however, this is not the only means of causing a steam explosion. Consider, for example, the concept of "accelerated meltdown" (suggested in the Sandia National Laboratory in NUREG/CR-3440) under which conditions sudden oxidation of cladding could lead to a ten gigawatt thermal power level due to reaction heat alone). The Chernobyl steam explosion suggests the desirability of a study of steam explosions in the context of a more sizeable experimental facility and over a broader range of simulated accident conditions.

The Chernobyl accident also raises a key question concerning LWRs in which spent fuel is stored in the reactor building. This is applicable, for instance, to BWRs with Mark I and Mark II containments in the U.S. Severe accidents -- in which large amounts of hydrogen and/or carbon monoxide (ex-vessel) are generated -- appear to have the potential for resulting, upon primary containment failure, in a large deflagration or explosion in the secondary containment which poses the issue of whether the spent fuel pool will survive (either due to loss of physical integrity or loss of cooling systems). It is possible, for example, to hypothesize a radioligically benign accident at a BWR Mark I plant which subsequently results in significant spent fuel pool accident release due to hydrogen burning in the secondary containment.

On the issue of containment at Chernobyl, it seems obvious that a containment did exist. The design basis for the Chernobyl containment (at least for the pump compartments) is similar to that for U.S. nuclear power plants. A pressure suppression capability also existed, although the specific capabilities of the suppression chamber at Chernobyl are not yet widely known. It also seems obvious, however, that there is an inherent weakness in the Chernobyl design involving pressure tube integrity under severe accident conditions that renders the Chernobyl containment less capable than a number of other designs. It should be recognized, however, that containments of the Mark I and Mark II BWR designs and the PWR ice condenser design may not be significantly better in the long run.

Any contrary positions on these issues would make for an interesting exchange, and I look forward to hearing from you.

My question to the last paragraph:
Do you refer to addressing on a design-specific basis, or to stregthening existing containments to cope with severe accidents?
Zsolt Revesz

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containments/intro #13, from abrachet, 647 chars, Fri Oct 3 12:02:38 1986

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TITLE: question and comment to paper 2 pr Hicken
you say in your paper that there is a growing concensus that some mitigation measures can reduce risk.Could you indicate which mitigation measures are under consideration in FRG ? Concerning your final items on safety our comments are the following
1. reassess power excursions for LWRs should be performed as a verification
2. improve assistance to operators is already a lesson learnt from TMI. It induced already for us development of safety panel, control room rearrangement, revised EOPs and U1 procedure, training for beyond-DBA accidents etc...

Brachet and Guimbail EDF

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containments/intro #14, from abrachet, 248 chars, Fri Oct 3 12:16:15 1986

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TITLE: question from a BRACHET and h GUIMBAIL to s SHOLLY
Your paper indicates the energy release as estimated to 200 MJ-320,000 MJ. Could you indicate on which basis these two numbers have been calculated?

A BRACHET and H GUIMBAIL

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containments/intro #15, from xjardi, 909 chars, Fri Oct 3 12:19:07 1986
This is a comment to message 1.

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COMMENT ON CONTAINMENT INTRODUCTION PAPER #1 BY LARS HOEGBERG.

A. WE WOULD LIKE TO KNOW SPECIFIC DESIGN MODIFICATIONS IMPLEMENTED IN THE SWEDISH CONTAINMENTS BASED ON THE POSTULATIONS OF SEVERE ACCIDENTS, IN PARTICULAR:
1) HAS THE FILTERED VENTING BEEN IMPLEMENTED IN ALL SWEDISH PLANTS?. DO YOU PLAN TO BACK FIT THIS REQUIREMENT?.
2) TYPE OF PROTECTION OF CONTAINMENT PENETRATION FROM MOLTEN CORE. HAS THEY BEEN IMPLEMENTED?.
3) NEW EQUIPMENT TO CONTROL HYDROGEN, IF ANY.
4) TYPE OF MODIFICATION TO HANDLE SINGLE FAILURE OF PRESSURE SUPPRESSION SYSTEM.
5) SOME INFORMATION ABOUT THE COST OF IMPLEMENTATION.
B. THE MEASURES DESCRIBED IN YOUR TALK WERE TAKEN PREVIOUSLY TO THE CHERNOBYL ACCIDENT. HAVE THEY BEEN CHANGED OR INCREASED AS A CONSEQUENCE OF THE ACCIDENT?. THANK YOU FOR YOUR ATTENTION.
J.TAPIA
EMPRESARIOS AGRUPADOS, MADRID. SPAIN.

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containments/intro #16, from xjardi, 328 chars, Fri Oct 3 12:27:38 1986
This is a comment to message 4.

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COMMENT ON PAPER #3 BY S.C.SHOLLY.

WOULD YOU PLEASE DEVELOP YOUR STATEMENT: "THE MARK I DESIGNS MAY NOT BE SIGNIFICANTLY BETTER IN THE LONG RUN". IN THE PREVIOUS SENTENCE YOU REFERRED TO THE INHERENT WEAKNESS OF THE CHERNOBYL CONTAINMENT DESIGN.
THANK YOU IN ADVANCE FOR YOUR ATTENTION.
J.TAPIA. EA. MADRID, SPAIN.

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containments/intro #17, from tmoore, 689 chars, Fri Oct 3 16:31:18 1986

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TITLE: QUESTION FOR PANELISTS AND PARTICIPANTS

Can anyone comment on the points made by the Union of Concerned Scientists about the number of electrical and pipe penetrations in typical U.S. LWRs? UCS is making the point in the press that these penetrations are major potential leak points in degraded core accidents, and that NRC records show numerous violations of rules pertaining to penetrations, open valves, doors, etc., that could become fission product release pathways. I bring up the points only for discussion.
Taylor Moore
EPRI Journal
Palo Alto, CA

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containments/intro #18, from tmoore, 154 chars, Fri Oct 3 17:46:16 1986

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TITLE: Addendum to last message.
My earlier question for conferees about UCS allegations refers, as does the UCS criticisms, to containment penetrations.

they believe at least 30% of the core (which contained a total of 30 tonnes U) reached ``more than" 300 cal/g.
The difficulty in calculating the energy released in Chernobyl based on past accidents (for example, the NRX reactor accident in Canada or the SL-1 accident in the U.S.) or experimental results, specialists say, is that earlier reactivity excursions were much more rapid than that at Chernobyl.

The IAEA calculation of over 50,000 megajoules of energy released is not necessarily in conflict with the figure of 200 megajoules announced at the end of the August 24-29 post-accident review meeting by Pierre Tanguy, the Frenchman leading one of the accident-review working groups. Tanguy was referring to the mechanical (not thermal) energy calculated to have been released in the Chernobyl power-excursion-plus-steam-explosion. The 200-megajoule figure, calculated by French experts admittedly in 10 minutes, was derived by ascertaining how much kinetic energy would have been needed to lift the 1,000-tonne reactor upper plate and displace it, which was one of the observable effects of the explosion. The French in fact said that the energy was at least 200 megajoules but could have been higher, and new calculations reportedly put it at around 500 megajoules. The IAEA team has not attempted to calcuate mechanical energy release from the accident; a specialist said that that calculation was quite complicated, requiring detailed consideration of factors such as mass and movement of material.

The mechanical energy figure is the significant one for determining whether the containment of a given reactor would have resisted the big bang of Chernobyl.

Does anyone have more up-to-date data? Best regards from Vienna, Ann MacLachlan

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containments/intro #9, from asharon, 874 chars, Thu Oct 2 18:58:42 1986
This is a comment to message 7.
There are additional comments to message 7.

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TITLE : STEAM EXPLOSIONS

When using the term "steam explosion" in context with western LWR's accidents the following stages must exist:
A. Molten fuel must be premixed with coolant seperated by vapor film.
B. The vapor film colapses due to some trigger (external or internal)
C. The local interaction escalates to a detonation wave.
D. The detonation wave propagates through the fuel/coolant mixture.

For such fuel/coolant interations molten fuel and pressure limitations are typically required. In the Chernobyl accident, however, fuel/coolant interactions took place via a totally different mechanism that is not applicable to LWR`s that can not undergo a power excursion. In Chernobyl fuel particles of few microns were mixed with the coolant allowing for rapid heat transfer and the generation of a steam spike (tube pressurization).

by: Ariel Sharon

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containments/intro #10, from lleach, 245 chars, Fri Oct 3 01:16:13 1986
This is a comment to message 7.

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Dick, is there any evidence on Hydrogen generation in those tests? I'm still not sold that the second explosion was Hydrogen. It's not clear there was enough time for it's formation. At least, with the event timing the Soviets gave.
Larry Leach

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containments/intro #11, from bruegger, 1567 chars, Fri Oct 3 06:11:38 1986
This is a comment to message 3.

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Paper 3 states that "it seems obvious that a containment did exist at Chernobyl. The implied contradiction between the words "seems" and "obvious" makes me uneasy, but I shall not dwell on that. I am more concerned about the proper use of the term 'containment'. For reactors in OECD Member countries, the containment can be defined to include both the primary and secondary enclosures as well as the systems, components, physical phenomena, processes which ensure that the risk posed to the environment is acceptably small should radioactive materials escape from the cooling system of the reactor; the containment must also assure the proper operation of systems important to safety as long as postulated accident conditions require. This definition hardly fits the Chernobyl 'containment'. I do not think the Soviets have published detailed plans of RBMK containment systems but what we have seen so far is probably better represented by the words 'partial containment' or, perhaps, by a mix of containment and confinement (considering the large volume of the reactor hall). I therefore submit it would be better to stop using the term 'containment' in the context of RBMK reactors without proper qualification, in order to aboid confusion and misunderstanding and in order to avoid creating artificial problems for Western-type containments. For RBMKs I suggest using the phrase 'partial containment' (although, very likely, the term 'pseudocontainment' would be more accurate). Any views on this ?

J.ROYEN
OECD-NUCLEAR ENERGY AGENCY (NEA)
,PARIS

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containments/intro #12, from zrevesz, 176 chars, Fri Oct 3 11:04:39 1986
This is a comment to message 1.
There are additional comments to message 1.

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reactors by the existing safety systems.
Does anyone know more details?
Best regards
Enno Hicken

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containments/intro #25, from mryan, 1102 chars, Mon Oct 6 11:02:21 1986

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TITLE: Comment on Questions from J. Tapia, message 15
From Lars Hoegberg, Swedish Nuclear Power Inspectorate, Stockholm
Replies to questions by J. Tapia, Madrid, Spain

A1: Systems for filter venting will be backfitted at all Swedish reactors by the end of 1988.

A2: The principle protection of penetrations will be provided by flooding the lower dry-well floor in some BWRs. Systems for automatic or manual triggering of such flooding in case of core overheat have been installed. In addition penetration will be fitted with special thermal shields, the design of which has not been finalised.

A3: Our BWRs are inerted; our PWRs have large dry containments. Residual hydrogen risks will be analyzed.

A4: A free-blowing safety valve (rupture disk) with high reliability isolation valves, reclosing after the pressure spike.

A5: Present, very tentative post estimates are in the range $3- to $5- million per reactor. No additional outage is envisaged.

B: The Chernobyl accident has resulted in no major technical change in the program. The implementation has been accelerated, where possible.

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containments/intro #26, from mhutcherson, 474 chars, Mon Oct 6 13:30:27 1986
This is a comment to message 4.

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TO: Distribution
FROM: Michael N. Hutcherson
Fauske and Associates
Burr Ridge, Illinois 60521
USA
RE: Comments to Steven C. Sholly's Paper (No. 3)
DATE: 6 October 1986

I would like to respond to this paper, and since I am responding to this issue (steam explosions) in also the 'source terms' section of this conference, I will register my responce in the latter region.

Thank you.
The end.

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containments/intro #27, from mberman, 10465 chars, Wed Oct 8 13:11:14 1986

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A DISCUSSION OF STEAM EXPLOSIONS AND COMMENTS ON RELATED PAPERS

TITLE: A DISCUSSION OF STEAM EXPLOSIONS AND COMMENTS ON RELATED PAPERS
4,6,7,8,9,10,21,26, etc.

Marshall Berman
Sandia National Laboratories

My knowledge of the Chernobyl accident comes primarily from articles in the lay and technical press, and from References 1-3. There are many aspects of the accident that seem uncertain and speculative, especially the chronology of the last few seconds before the "explosion." The prevailing descriptions of these seconds are quite inconsistent [e.g. see 1, p. 21; 2; 3]. Several assumptions pervade the discussions at this conference: The fuel was not molten at the time of the steam explosion; the solid fuel was prefragmented into "small" pieces before it contacted the "coolant;" the "coolant" was predominantly steam. It is not clear to me which, if any, of these assumptions are true. The following discussion provides my speculations concerning possible scenarios at Chernobyl, based primarily on the computer-generated parameter histories prepared by the Soviets and my own extensive experience in this field.

High Ambient Pressure

The modelling histories show that the ambient pressure was at least 90 bars, and could have been much higher. Steve Sholly (message 4) is correct in stating that if there was a steam explosion at Chernobyl, it would contradict what some experts have claimed - that such events are incredible at such high pressures. The RIA-ST-4 accident discussed by R. Hobbins (message 7) lends even more credence to the possibility of steam explosions at high ambient pressure. He states that an energy insertion of 350 cal/g "melted" a fuel rod "and the melt interacted explosively with the water." His description of the post-explosion debris is very similar to debris we have observed in some of our experiments [4]. This experiment also leads me to question the Soviet statement that the fuel did not melt, even though their calculated energy insertions of 300 cal/g (mean value? +/- how much?) could easily have been sufficient to melt much of the fuel and the cladding, considering the power asymmetries.

Coolant Quality

It is difficult to read the P (volumetric steam quality, 0-1.2%) and O (mass steam quality, 0-6%) curves in the plots of the computer simulation. However, the upper

containments/intro #19, from rhobbins, 404 chars, Fri Oct 3 18:25:19 1986

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TITLE: References for RIA-ST-4 (discussed in message #7)

M.S.El Genk, R.R.Hobbins, and P.E.MacDonald, " Molten Fuel Coolant Interaction During A Reactivity Initiated Accident Experiment," Nuclear Engineering and Design 66 (1981) 247-267.

M.S.El Genk, R.R.Hobbins, and P.E.MacDonald, " Fragmentation of Molten Debris During A Molten Fuel Coolant Interaction," J. of Nuclear Materials 113 (1983) 101-117.

containments/intro #20, from mgoldman, 1130 chars, Sat Oct 4 11:22:19 1986

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TITLE: comment on papers 1 & 2

It seems more and more apparent that the limiting factors for containment performance design need to focus on the prevention of the escape of fission products which can contaminate land and facilities and deny their use. TMI clearly indicated that core damage prevention systems are more strongly justified in terms of protecting the owner's investment (an economic perspective) than by the need to provide public health and safety protection. So also it would seem to me that, given the relatively modest (for a "disaster") and likely imperceptible costs to public health, the Chernobyl experience confirms the purely economic justification for the modification of containment systems to prevent the release of those fission products such as cesium and iodine with capabilities for significant residual contamination.

In this regard, the Swedish movement toward filtered, vented containments (particularly if the cost is as modest as suggested in the first paper) is in the right direction, and deserves attention by the rest of the western nuclear community.

M Goldman, NUS

containments/intro #21, from rhobbins, 866 chars, Sat Oct 4 19:06:43 1986

There is/are comment(s) on this message.

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TITLE: Comment to message 10

Larry, in test RIA-ST-4 there was some oxidation of the fuel rod debris and the zircaloy shroud which was heated by the impingement of molten debris.

I did a very rough calculation for the Chernobyl reactor which indicates that at 2200 K a flammable quantity of hydrogen could be produced in about 1 s and an explosive quantity in about 3 s.

I have one difficulty with the hypothesis of a steam explosion at Chernobyl. From what I understand, the presence of liquid water is required to produce a steam explosion of the type we observed in RIA-ST-4. However, the production of voids (to quite high fractions?) was a necessary element in the power excursion at Chernobyl. Thus, it is not clear to me that conditions to generate a steam explosion by molten fuel coolant interaction were present at Chernobyl.

Dick Hobbins

containments/intro #22, from lleach, 369 chars, Sun Oct 5 23:29:27 1986

This is a comment to message 21.

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Dick

Check Don Fletchers calculations. Although he's not getting the same time to "lift off", he is getting a large power surge and shutting the inlet check valves like they did. I think this happens at less than 50% core average void fraction. That is, there's still plenty of water. I'd be interested in knowing if that's true....I haven't seen details yet. Larry

containments/intro #23, from lleach, 245 chars, Sun Oct 5 23:33:26 1986

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TITLE: Containment Capability

I keep getting asked if US (or all Western) containments could have withstood the explosion which occured at Chernobyl. Can we get answers for each containment type, ie. dry, ice condenser, suppression, and CANDU?

containments/intro #24, from ehicken, 1979 chars, Mon Oct 6 10:16:39 1986

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TITLE: Responce to cont. intro 11

Response to intr # 11

Dear jacques ,I agree with your concern about the proper use of the term 'containment' The Senior Group of Experts on Severe Acidents has defined: The "containment" is astructural envelop which completely surrounds the reactor system and is designed to hold the releases from design basis accidents with little or no release to the invironment. The term is used in its broader sense to include associated leakage paths and buildings which contain the releases of severe accidents. Because the DBA is a failure of 'one' pipe,the RBMK reactors are capable to avoid a release to the environment ,because a rupture of a pressure tube between the upper core schield and the inlet of the separators can be managed by the ventilation system. We were told that all compartements with pipes containing primary circuit fluid are under a slight underpressure. I feel that the main question is the definition of a 'DBA'. It is my feeling that e.g. most sequences with an ATWS cannot be managed in RBMK

No. 6083, 4- 10 September 1986,

4. L. S. Nelson and P. M. Duda, "Steam Explosion Experiments with Single Drops of Iron Oxide Melted with a CO2 Laser," SAND81- 1346, NUREG/CR-2295, September 1981.

5. M. Epstein, H. K. Fauske, "Steam Film Instability and the Mixing of Core-Melt Jets and Water," ANS Proceedings of 1985 National Heat Transfer Conference, Aug. 4-7, 1985, Denver, Co.

6. R. P. Feynman, Contribution to the President's Commission Investigating the Challenger Accident, 1986.

7. M. Berman, Section E-1 in "Steam Explosion Review Group Report: A Review of the Current Understanding of the Potential for Containment Failure Arising from In-Vessel Steam Explosions," NUREG-1116, February 1985.

8. M. F. Young, M. Berman, L. T. Pong, "Hydrogen Generation During Fuel-Coolant Interactions," presented at the American Chemical Society Meeting, Anaheim, California, Sep. 12, 1986.

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containments/intro #28, from mryan, 1058 chars, Wed Oct 8 13:45:57 1986

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TITLE: Containment issues from Source Terms

To all participants: In the source terms section of this conference, there were several issues raised relevant to containments. Dr. Alain L'Homme noted there is agreement that a sequence of events precisely like Chernobyl's could not occur in an LWR. But he asked whether we are equally certain there is no other sequence that would give LWRs an accident of similar characteristics: fast and catastrophic, with essentially no warning time. So:

1-Is there certainty about the fastest, most powerful accident LWR containments could confront? What is it? Is it part of containments' design bases?

2-There was some uncertainty expressed in the source term section about the exact sequence at Chernobyl and the role played by a steam explosion. There has been much discussion in U.S. source term research over the role steam explosions might (or might not) play. Is this uncertainty accounted for in setting containments design bases?

I'd appreciate information from anyone on these questions. Margaret Ryan

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containments/intro #29, from mryan, 551 chars, Wed Oct 8 13:46:48 1986

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TITLE: Containment design bases

To all participants: For the Super-Phenix breeder reactor, calculations were made of the maximum explosive force to which the an accident might subject the containment--it was 800 megajoules. This was then made the design basis for the containment.

Does anyone know whether such calculations have been made for other containment designs? Can anyone identify the maximum force an LWR is expected to generate--and/or the force the containment is expected to withstand?

Thanks for any assistance. Ann MacLachlan

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containments/intro #30, from lleach, 940 chars, Thu Oct 9 13:20:19 1986

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TITLE: Accident Sequence

It would have been nice to have a conference on the accident sequence. This looks like the best place of what's available.

As I reported wearlier, we have been unable to duplicate the very rapid power increase reported in the Russian simulation. It is interesting to note it occurs right after the SCRAM was pushed.

We now may know why.

I have been told some people at PNL have observed that the graphite control rod followers are not full length. That is, at the full withdrawn position, there will be water in the lowest part of the channel. Thus, the initial effect of a SCRAM from full withdrawn position, particularly with a bottom peaked flux profile, would be to cause a reactivity insertion. This could be the "trigger" we have been looking for. We will follow this up.

Does anyone have any informationon this?

It will be ironic if the act of pushing the SCRAM is what really triggered the event.

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containments/intro #31, from bmann, 369 chars, Thu Oct 9 13:21:19 1986

There is/are comment(s) on this message.

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TITLE: Tube Failure Propagation

On either this conference or the Source Term conference, reference was made to studies which indicated that the failure of one tube in the RBMK design could lead to other tube failures due to the mechanical transfer of energy through the graphite. I would like to get more information on that if possible.

Brian Mann, Virginia Power

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containments/intro #32, from halbrecht, 1151 chars, Fri Oct 10 06:02:21 1986

This is a comment to message 4.

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In the Western LWR safety research, large violent steam explosions are discussed in the frame of scenarios where masses of molten fuel may drop coherently into a water



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limits of the ranges of these quantities imply that a great deal of liquid water could have been present in some fuel channels at the time of the "steam explosion." If one takes into account asymmetries in the fuel channels and the potential errors in the computer simulation, I don't see how a large amount of liquid water can be precluded. The computer histories also show that the main circulation water flow began to increase during the last two seconds before the explosion. Therefore, it is possible (or likely) that some channels were beginning to refill with water. Molten fuel and clad could have fallen into these "puddles" of water. An explosion in one channel would have set off a chain reaction of explosions in adjacent channels, resulting in a coherent explosion.

In summation, I believe that melting of the fuel and clad, subsequent contact of these materials with liquid water, and a resulting steam explosion is a credible sequence of events.

R. Hobbins (message 7) says that large amounts of molten core materials contacted water at TMI without an explosion. He notes, however, that a 2 MPa pressure spike was observed (far away from the vessel lower plenum region) above a background pressure of 10 MPa. I submit that such a spike could easily have been created by one or more steam explosions, or by a highly energetic fuel- coolant interaction. Furthermore, the supposition that "relatively large streams were involved" resulting in "relatively large particle sizes" is, in fact, just supposition. Current experiments at Sandia indicate that streams of diameters comparable to the diameters of the holes in the structural plates could indeed break up into small particles. Ref. 5 presented a calculational model of jet breakup. The authors stated that "a 10-cm diameter jet of core-melt material ... is predicted to disintegrate after falling only 27 cm through water." Hole diameters for the various TMI support and grid plates ranged from about 8 to 16 cm.

We have a great deal of experience with steam explosions involving melt masses ranging from 50 mg to 50 kg. We have observed explosions for fuel falling under gravity into coolant and for coolant pouring onto quiescent molten pools. At large scale, occurrence of explosions sometimes appears random. This variability has been observed extensively both for controlled experiments and industrial accidents. We believe that this is primarily due to a randomness in the availability of a trigger. We have also observed fuel-coolant interactions with a wide range of intensities, from benign boiling, to mild explosions, to extremely violent explosions. To conclude that Western LWRs are not vulnerable to steam explosions based on the "presumed" absence of an explosion at TMI is to make a grave error. Richard Feynman said [6]: "When playing Russian roulette the fact that the first shot got off safely is little comfort for the next."

Ariel Sharon (message 9) has provided his definition of steam explosions, which contains a plethora of modifiers. I prefer my own definition given in Ref. 7: "A steam explosion can be simply defined as a boiling process rapid enough to generate shock waves." Many people have profferred complex definitions of steam explosions. I think that such definitions tend to cloud the safety issues, and introduce artificial barriers to knowledge. Our primary concern is to prevent damage to the reactor. Artificially restrictive definitions should not be used to create logical and semantic barriers to understanding the Chernobyl explosion and assessing its relevance to Western reactors.

L. Leach (message 10) questions whether there was sufficient time to generate hydrogen for the second explosion. We recently presented a paper [8] that included models for predicting the rates and amounts of hydrogen that could be generated during steam explosions. Two different models were used; one model assumed that molten metal reacted with a steam environment, and the other with an environment composed primarily of liquid water. In both cases, large quantities of hydrogen were predicted to be generated on the time scale of the steam explosion, 0.5 to 5 ms.

R. Hobbins (message 7) feels that one should add as much water as possible to a Western LWR as soon as possible. I will address this comment in my paper in the Accident Management Conference.

In summary, I believe the following sequence of events at Chernobyl is a plausible scenario: The reactivity transient leads to melting of some of the zirconium and fuel. The molten material is expelled into the fuel channel where it contacts water, or it falls into water residing at the bottom of the channel. One of the channels spontaneously triggers a steam explosion, and the generated shock triggers a large number of other channels to explode. The steam explosions produce large quantities of steam and hydrogen which lift off the reactor roof. The steam and hydrogen mix with the ambient air initially in a mixture too rich to explode. As the hydrogen-steam-air-hot fuel/metal spreads into the building, it generates more hydrogen from metal-steam reactions, and mixes with more air until it finally reaches a detonable concentration. A detonation is then initiated by the hot fuel and hot oxidizing cladding. Witnesses would hear these events as one explosion after another, just as the Soviets have reported.

REFERENCES

1. USSR State Committee on the Utilization of Atomic Energy, "The Accident at the Chernobyl Nuclear Power Plant and Its Consequences," Draft, August 1986.

2. "Chernobyl: The Soviet Report," Nuclear News Special Report, Sept. 11, 1986.

3. "Chronology of a Catastrophe," Nature, Vol. 323,

combination with rapid vapour generation is, as far as I know, a new and previously unobserved phenomenon. The explanation of the event as a steam explosion appears capable of accounting for the pressure tube rupture and subsequent explosion, and there does not seem to be a need to postulate a solid fuel fragmentation/ vapour generation mechnism to explain events. Having said this, solid fuel fragmentation effects do desereve further investigation.

Steven Hall
Safety and Reliability Directorate, UKAEA.

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containments/intro #37, from lleach, 63 chars, Tue Oct 14 17:05:52 1986
This is a comment to message 34.

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Would a deflagration be reported by witnesses as an explosion?

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containments/intro #38,

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Withdrawn

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containments/intro #39, from ksoda, 2241 chars, Tue Oct 14 21:03:28 1986
This is a comment to message 38.

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COMMENTS TO PAPER #3 BY DR. HICKEN.

I would like to see more discussions on the items Dr. Hicken raised in his paper in terms of items 1) through 4). Item 1) referred to a consideration of reassessment of power excursion of a light water reactor. It is our concern that multiple rod ejection could be excluded from safety consideration based on some justification.

A single rod ejection is currently considered in the safety analysis for the licensing purposes in Japan. However, a multiple rod ejection is not the licensing purposes in Japan. However RIA intiated from multiple rod ejection is not considered. I believe that we need to justify this by either probabilistic approach or design to prevent such occurrence. In that respect, It is necessary to reassess power excursions for LWR's as well.

Item 2) asked necessity to improve the assistance to operators. It is not easy to prepare for unplanned scenario at this time by some of hardware or computer. If operators are given enough time to cope with an accident they should be able to manage the accident by reasonable manner. Only requirements they should be able to manage the accident by reasonable manner. Only requirement may be well-trained and well-qualified operators to cope with unplanned scenario. Perhaps we need a standard qualification for operators in the world.

For Item 3), it is the Japanese regulatory position that no additional safety measures are necessary. A severe accident must be prevented by proper maintenance and qualification of components and operators. However I believe that design change and mitigation measures should be investigated in order to at least we know how it works and to what extent it is effective and why other countries are going to implement. I support this activity.

Item 4), I believe that most of physical phenomena can be explainable from our knowledge on severe accident. Only concern I have now is chemical forms of fission products released to the environment.

K. Soda/ JAERI/ Tokai

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containments/intro #40, from shall, 68 chars, Wed Oct 15 11:19:23 1986
This is a comment to message 37.

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To a layman, anything that goes 'bang' is an explosion.
Steven Hall

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containments/intro #41, from psviatoslavskyx, 647 chars, Wed Oct 15 11:41:51 1986
This is a comment to message 35.

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I believe there is a major difference between the Chernobyl containment and western LWR containments that has been overlooked in this conference. Not only must western LWR containments be capable of withstanding LOCA loads, they are capable of withstanding outside loads (i.e. missles caused by tornadoes, aircraft crashes). From the diagrams of the Chernobyl reactor, it appears as if the reactor hall is not designed to withstand outside loads. Is it so unreasonable to postulate a senerio in which a tornado could cause the refueling machine to collapse on top of the reactor?

Pete Sviatoslavsky
WPSC

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containments/intro #42, from mberman, 3145 chars, Thu Oct 16 11:23:47 1986

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TITLE: Comments on Hydrogen Explosion
TITLE: Comments on the Nature of the Chernobyl Second Explosion

Marshall Berman

I agree with most of Steve Hall's comments in messages 34 and 36. My description of the accident sequence (message 27) assumes that a steam explosion led to the destruction of the reactor, but that a hydrogen explosion occurred in the room above and lifted off the roof of the building. However, I believe that the second explosion was a detonation (or highly accelerated flame), and not an ordinary deflagration.

I do not believe that the geometry above the reactor



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would make a detonation "virtually impossible to achieve." In fact, I think that a detonation could be very likely under the circumstances for the following reasons: The hydrogen-air mixture could have been locally very sensitive; a great deal of turbulence would have accompanied the expulsion of steam and hydrogen from the reactor, and the mixing with air would be fast and turbulent; the very hot core materials would have provided multiple strong ignition sources for a "nearly" direct initiation of detonation; the turbulence generated in the vicinity of the fuel loading machine and the adjacent walls could have caused a rapid transition to detonation [1].

I believe that it is a mistake to assume that strong shock waves are a necessary prerequisite for initiation of detonation. If the burning rate of the gases is sufficiently intense, a detonation can be induced. Various methods for initiating detonations are discussed in Reference 1.

Deflagrations usually do not sound like explosions. The burn at TMI was described by people in the control room as "a thump, a bump, a whoomp..." [2]. Experimenters at Sandia have described the sound of a deflagration as a whoosh or whoomph, sometimes drawing colorful, but R-rated, comparisons to the bodily functions of large mammals.

A constant-volume deflagration involves a spatially uniform increase in pressure. If the "explosion" broke the fuel-loading machine, it would mean that shock waves had been developed. Furthermore, the thermal loads induced by detonations would be considerably smaller than deflagration loads. If there was little or no scorching of the side walls, it would be further evidence of a detonation rather than a deflagration.

REFERENCES

1. M. Berman, "A Critical Review of Recent Large-Scale Experiments on Hydrogen-Air Detonations, Nuc. Sci. and Eng. 93, pp. 321-347, 1986.

2. M. Rogovin, Director, Vol. 1, Three Mile Island, A Report to the Commissioners and to the Public, Nuclear Regulatory Commission Special Inquiry Group, 1980.

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containments/intro #43, from mberman, 3035 chars, Thu Oct 16 23:34:56 1986

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Repeat of #42

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containments/intro #44, from ssholly, 14892 chars, Fri Oct 17 11:43:14 1986

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TITLE:

NOTE FROM STEVEN C. SHOLLY, MHB TECHNICAL ASSOCIATES

I regret that illness has prevented my timely participation in the conference. I am now able to respond to the numerous comments and papers in the conference, and would appreciate hearing from anyone still interested either via the conference or by correspondence/telephone.

RESPONSE TO PAPER 1 BY LARS HOEGBERG
STEVEN SHOLLY, MHB TECHNICAL ASSOCIATES

Dr. Hoegberg is quite correct that only limited source terms can be accepted if extensive ground contamination is to be avoided. I would observe, however, that iodine releases would impose such restrictions only for the short term. Of greater concern are cesium and, if released in even single percent quantities, the lanthanide species.

RESPONSE TO PAPER 2 BY ENNO F. HICKEN
STEVEN C. SHOLLY, MHB TECHNICAL ASSOCIATES

Dr. Hicken states that the time period between accident initiation and vessel failure is "usually more than 1 to 2 hours". I have observed that it is frequently the case in severe accident calculations undertaken with the MARCH code that the time period between accident initiation and vessel failure is usually of the order of 1 to 2 hours. It should also be observed that for containment bypass scenarios such as Event V and steam generator tube rupture (in PWRs), this time period is largely meaningless. Rather, the time period of interest for counter measures is the time from accident initiation and the start of core degradation, a rather short period of time. These are also the accident scenarios which are likely to contribute substantially to public risk.

Dr. Hicken also states that if early containment failure can be avoided, several hours or even days are available before containment failure, and that this provides time for accident management actions. I would add that such actions should be well considered before hand, otherwise there is a non-trivial risk of taking inappropriate actions that could lead to containment failure at an earlier time. In addition, for PWRs, there is a tendency to lose steam inerting over a period of hours, thus presenting the possibility for a rather large hydrogen burn. Of course, the source term at such late times may be limited, but containment failure is nonetheless quite possible.

COMMENT ON R. HOBBINS COMMENT #7
STEVEN SHOLLY, MHB TECHNICAL ASSOCIATES

I agree that it has been shown experimentally that steam explosions can occur under high pressure. My point was that many severe accident analysts appear rather thoroughly convinced that the likelihood of this occurring under severe accident conditions is vanishingly small. Of course, I disagree with this conclusion, and believe that experiments at Sandia National Laboratories and elsewhere speak eloquently to the matter.

TITLE: Containment from E. Hicken

I like to respond to the question if the second explosion was a power excursion or a hydrogen detonation.

1) The upper core plate is located now in a vertical position. During its movement from the horizontal to the vertical position the jet coming out of the reactor space must have been redirected. This must have cause a lot of damage to the building.

2) During the week following the Vienna Conference I asked Dr. Kalugin about the position of the graphite blocks. He told me that the blocks were totally dislocated. We came to the assumption that something must have happened in the lower part of the core after the upper core plate lifted. I believe that a hydrogen detonation can not dislocate the graphite blocks in the lower part of the core. Therefore, I believe that the second explosion was mainly a power excursion with possibly an additional hydrogen burn.

I like to respond to remark Nr. 13 from Mr. Abrachet. I stated that there is growing consensus that some mitigation measures can reduce risk. This statement is based on studies and decisions in several countries, e.g. Sweden, France, FRG, etc. In the FRG we have installed devices in two power plants to limit or decrease the pressure in a PWR containment. This measure would allow to avoid an overpressure failure. In one case the decision was based on the assessment that a leak-before-break could not be guaranteed. In the FRG and - to my knowledge - also in other countries efforts are underway to further study mitigation measures

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containments/intro #48, from mberman, 15980 chars, Mon Oct 20 14:12:31 1986

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TITLE: Chernobyl "Steam Explosion"

TITLE: Chernobyl "Steam Explosion" - Comments on Message 53 by M. N. Hutcherson

Marshall Berman
Sandia National Laboratories

DEFINITION OF "STEAM EXPLOSION"

Although the occurrences of steam explosions have been noted for centuries [1], a universally accepted definition does not exist. Phenomena essentially the same as steam explosions have been called "physical vapor explosions" [2], "rapid phase transitions" (RPT), "explosive boiling," or "fuel-coolant interactions" [3]. However, the scientific definition of a phenomenon should not be so comprehensive that all the properties of that phenomenon are embedded in its definition. This is especially true of poorly understood phenomena for which all properties have not been determined and characterized.

The "free contacting" mode of steam explosions might be defined as consisting of the five steps proposed by Mr. Hutcherson. However, in my discussion in Containment Conference message 27, I described a scenario in which all five steps do occur. Fuel melt is ejected and falls into water in the bottom of the channel ("contained interaction zone"), coarsely fragments before, during, or after its contact with the water, undergoes a spontaneous "triggering," followed by "fine fragmentation" and "expansion of the coolant vapor."

In addition to the free contact mode, other "modes" of steam explosions have been experimentally observed, and are believed to have been involved in some major industrial accidents [e.g., see 4]. Steam explosions have been observed for fuel coolant configurations which are initially separated or stratified [5,6]; i.e., the coarse fragmentation does not arise from the active injection of one liquid into another; instead, the two liquids begin to spontaneously mix themselves. This explosive self-mixing escalates until an energetic steam explosion is spontaneously triggered. Another example of explosive self-mixing involves the occurrence of a small steam explosion which produces an efficient coarse mixture in which a larger explosion subsequently occurs [7]. This bootstrapping effect is commonly observed in gas phase combustion and leads to flame acceleration and sometimes to transition to detonation.

On the other hand, many experiments have also shown that if the fuel is too fragmented or dispersed prior to or during its entry into the water, then explosions usually do not occur [8-11]. Distinguishing between legitimate and illegitimate steam explosions can be of value only if one can show that illegitimate steam explosions are of no concern in reactor safety. This has not been shown.

The published literature also contains definitions of physical vapor explosions which encompass both the violent mixing of two liquids at different temperatures and the rapid mixing of a finely divided hot solid material with a much cooler liquid [2]. The definition that I provided dealt with the propagation of a steam explosion, not the damage mechanism. If the collapse of the vapor film about a single fuel particle does not produce vapor sufficiently fast to generate shock waves, then the film collapse will be an isolated event, unable to influence the neighboring fuel particles and their surrounding vapor blankets - there will be no steam explosion. However, if pressure waves are generated by the fragmentation of a single particle which are strong enough to cause the fine fragmentation of more than one additional fuel particle, then a steam explosion can be propagated through the mixture. This process is analogous to the chain reaction required for fission. The PARTITION of the energy released from a steam explosion is also analogous to an underwater HE (e.g.TNT) explosion. Part of the energy is



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RESPONSE TO ACCIDENT MANAGEMENT PAPER BY M. BERMAN
STEVEN C. SHOLLY, MHB TECHNICAL ASSOCIATES

Regarding your comments on evacuation, much work has been done on when and why to initiate evacuation. In my opinion, far too much attention has been paid to evacuation planning, per se, and emergency response -- the latter including evacuation as well as sheltering, respiratory protection. I am most concerned that public officials could be persuaded by this unfortunate focus on evacuation -- to the exclusion of other means of protection -- that in the event of another severe accident in the U.S. there will be almost a knee-jerk reaction to recommend an early evacuation. Should an early containment failure or bypass occur, this may be the worst thing to do. It may make considerably more sense to shelter and relocate following plume passage.

Is anyone aware of any recent work on comparative benefits of various emergency response schemes? It seems to me that some thoroughly credible work in this area is needed both to inform emergency response officials of the situation and to give the public some reason to listen to emergency broadcasts during reactor accidents. In the absence of such a program, I am concerned that the response of the public will be to evacuate at the first hint of real problems.

Regarding your comments on "Control of Plant Operations", it is my view that insider sabotage causing core melt is almost a trivially simplistic proposition for the saboteur. Mostly what is missing is motivation. The bunkered safety system approach in Western European countries may provide a workable countermeasure to this threat, and, at the same time, lead to reductions in risk from non-sabotage accident scenarios as well.

Does anyone have information concerning what safety system capabilities are provided by the existing bunkered safety system concepts? It would seem that by providing both high-rate, low-pressure and low-rate, high-pressure pumps and the capability to use these pumps for vessel injection, containment sprays, pump seal injection, cavity flooding, and other purposes, one could design a bunkered safety system concept that would be extremely beneficial in terms of fending off insider sabotage, external sabotage, and a host of accidents initiated by other causes. Any comments?

RESPONSE TO COMMENT #10 IN ACCIDENT MANAGEMENT
STEVEN C. SHOLLY, MHB TECHNICAL ASSOCIATES

Mr. Holmstrom appears to forget the "martial law" option available to civil authorities in Western democracies.

In addition, I agree with the various responders to this comment that the key is considering one's options and capabilities before the accident happens.

There is a very instructive report available from the U.S. Federal Emergency Management Agency concerning how emergency response developed during the TMI accident. I would commend report to the attention of interested parties: "Evacuation Planning in the TMI Accident," prepared for FEMA by Human Sciences Research, Inc., William W. Chenault, Principal Investigator.

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containments/intro #45, from jmatuszek, 1838 chars, Fri Oct 17 13:15:46 1986

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TITLE: Comment on message #44 from ssholly

Your comment about evacuation being the knee-jerk reaction to a NPP accident is absolutely correct. In the US, the responsible authority for initiating public action is the governor of the state or his designee. While either will be receiving advice from a host of specialists (or other "experts"), experience shows that there is great reluctance on the part of such elected or appointed officials to heed such advice if it runs contrary to their preconceived notions, or gut reactions, or even political exigencies. Sheltering and respiratory protection are viewed as "doing nothing", while evacuation is viewed as an action which shows leadership. In New York, the head of the Disaster Preparedness Commision happens at this time to be the Commissioner of Health--a physician who also happens to be knowledgeable in radiation risks--so his response during the Ginna accident or during emergency exercises has been very much a sane one. However, lower-level functionaries serving in his stead during exercises have consistently shown a prediliction to start premature mock evacuations. After Chernobyl, these functionaries are even more likely to want to evacuate--so I am concerned as to what might happen if an accident occurs at a time when the current Health Commissioner is away, or has left his position, or has had his responsibilities usurped by the Governor.

The same issues arise on the use of KI for thyroid blockade (see discussion in the "health" section of this Conference).

I hope that there are others out there who will add their comments towards affirming sheltering as the first method of protection, to be followed by whatever action is necessary as indicated by developing events, or as more data become available to the emergency response staff.

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containments/intro #46,
No message

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containments/intro #47, from ehicken, 2681 chars, Mon Oct 20 09:28:00 1986

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consensus of the knowledgeable community that such explosive interactions may be dismissed in the latter group (Western LWRs) based on well controlled confirmatory experiments." If Mr. Hutcherson is referring to Ref. 15, he is very much mistaken. To my knowledge, no "well controlled confirmatory experiments" exist anywhere in the world. I would recommend that interested conference attendees read Ref. 15 to note the strong caveats concerning the subjectivity of the conclusions, and the nearly unanimous recommendation by the group that further research be conducted in order to confirm the assumptions. It would also be reasonable to contact the chairmen of that study (T. Ginsberg, BNL, M. L. Corradini, Un. of Wisconsin, and C. Allen, USNRC) and determine the existence of these "well controlled confirmatory experiments." In addition, References 24 and 25 evaluated all of the important current assumptions and hypotheses used to support some experts' low subjective probabilities for large steam explosions. Experimental data and technical arguments were provided which showed that all the hypotheses were either inconclusive or disagreed with experimental data.

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END OF CONTAINMENT CONFERENCE

carried off in an external shock wave which immediately begins to decay when it leaves the explosive region. Part of the energy is used to evaporate water and create a steam bubble. In the case of HE, the energy is roughly equally partitioned between the bubble and the emitted shock wave. Close to the steam explosion, pressures are generally much lower than for solid explosives because, as Mr. Hutcherson points out, the explosion propagation speed and power density are lower than for HE. However, at a distance of the order of 10 to 20 radii, the underwater shock wave pressures from TNT and steam explosions can be quite comparable.

I believe that we can all agree on the following. There is circumstantial evidence that the in-vessel Chernobyl "explosion" could have been a "steam explosion," sharing many of the characteristics usually ascribed to such explosions. The key question, for which agreement probably does not exist, is: Does the occurrence of a steam explosion or pseudo-steam explosion at Chernobyl affect the consideration of such events in LWRs? I believe that it does. I also believe that an "energetic" FCI, or possibly a number of smaller steam explosions may have occurred during the TMI accident, and may have been responsible for the 2 MPa pressure spike that occurred at about 3.8 hours.

## REQUIREMENTS OF STEAM EXPLOSIONS

No one knows how much of the fuel was liquified, nor what fraction was fragmented, nor what the fragment sizes were. The void fraction of the surrounding coolant is also not known. What is likely is that, in various parts of the core, there were solid and liquid fuel fragments ranging from "fine" up to fuel pellet size (11.5 mm), and steam and liquid water. It doesn't matter if "only a limited amount of water was available," or if "not all the fragments were liquid." An upper limit to the thermal energy that could be stored in molten core debris for an LWR is about 186,000 MJ, corresponding to 134,000 kg of melt at 2750 K [12]. IDCOR [13] has estimated that a PWR vessel may fail if 830 MJ were deposited in a steam explosion. According to their calculations, the complete thermal reaction of 2260 kg of fuel would be sufficient to fail the vessel. Gittus et al. [14] stated that "Prompt containment failure cannot be ruled out at explosion energies exceeding about 1500 MJ." Clearly, only a small fraction of the available energy is needed for vessel or containment failure.

I am not aware of any data which show that coarse fragmentation and triggering are "extremely difficult to produce in western style LWRs." Quite the contrary, there are many experiments which indicate that the opposite is true: that coarse mixtures can be readily produced and triggered for LWR simulant materials [e.g., see 6,7,10,11].

The statement that "the steam explosion scenario in these reactors is considered to be highly incredible" is not correct. There are certainly many experts who believe that alpha-mode failure is unlikely. However, I believe that the majority, if not all, of these experts recognize that this is a highly subjective judgement, and cannot be scientifically demonstrated today. A review of the experts' comments in Ref. 15 will confirm my statement. [Many of the pertinent comments have been selected and published in Ref. 16]. I know that many other experts would not accept the words "highly incredible." References 12,14 and 17-20 is a sampling of published results that indicate far less certainty in the probability of a steam explosion than the words "highly incredible" imply.

## TRIGGERING OF AN EXPLOSION

I do not see any important safety distinction between a "spontaneous" and an "external" trigger. Triggering translates simply into an event which causes film collapse and fuel fragmentation. Such a trigger could be caused by natural oscillations of the vapor film blanket, or by a pressure pulse. I believe the distinction is irrelevant in a reactor, where triggers can be created by falling objects, pumps turning on, etc. Many steam explosions have been accidentally triggered in aluminum, copper and steel foundries, in the paper industry and in nuclear reactors. It appears that such triggers may occur for accidents at low or high pressures.

## OTHER POSSIBLE DISRUPTIVE MECHANISMS

Further analysis of the Chernobyl accident may indeed lead to alternative explanations of the two explosions that occurred. However, I believe that the scenario involving a steam explosion followed by a hydrogen explosion currently fits the evidence as well as, or better than, most other hypotheses. The recovery of what appears to have been molten ruthenium [21], melting point of 2770 K, provides evidence for fuel melting. Ref. 22 also implies rather strongly that some of the fuel was molten. The recovery of particulate matter "some tens of microns in size around the site" [26] is quite consistent with the debris sizes to be expected in a strong steam explosion [7,10,11]. The rapid generation of hydrogen that would accompany a steam explosion also explains the origin of the second explosion - a hydrogen detonation (or accelerated flame). Furthermore, this scenario seems to agree quite well with the Soviet model of the course of events preceding the explosion [23].

## SUMMARY

I don't believe that any definitive conclusion can be drawn concerning the degree of fragmentation of the fuel, or that such fragmentation was uniform throughout the core.

It is very unfortunate that no references were provided for the concluding sentence referring to "the current

can be estimated from the mean consumption rates of different foodstuffs and their activity concentrations. When we consider the activity concentration limit for a foodstuff, we should estimate the radiation doses for both adults and children. Groups with special diets, such as vegetarians, should also be considered. They often eat a great deal of natural products, such as mushrooms and berries, frequently containing much more activity than cultivated products. For such population groups additional limits for consumption of natural products might be needed.

4. What is the suitable limit for radiation doses causing no unacceptable health consequences?

Except for in the immediate surroundings of Chernobyl, no acute health consequences are expected. Therefore only dose limits for stochastic effects of radiation are considered. In its publication 40, ICRP gives two such dose limits for the population or the critical group. The lower limit for the first year dose after an accident is 5 mSv, below which countermeasures are not warranted. The corresponding upper dose limit is 50 mSv above which countermeasures are almost certainly necessary.

The experiences in Finland from 1965-69 showed that the half-life for the Cs-137 body content in humans was about 2.5 a, when no correction for continuous annual fallout was taken into account. If we conservatively assume that the effective half-life for both external and internal exposure rates is 5 years and the radiation dose during the first year is 5 mSv, then the committed dose equivalent during the next 50 years is about 36 mSv. This gives an annual mean exposure rate of 0.7 mSv/a, which is below the value 1 mSv/a given in ICRP publication 43 as the limit for people's life-time doses for humans. The dose rate 5 mSv/a decreases to 1 mSv/a in 12 years. However, the lifetime dose limit 1 mSv/a is not meant to be
used in the case of accident.

If the first year dose for humans living in the area of highest fallout is of the order of 1 mSv, as estimated in Finland from the Chernobyl fallout, the risk to an individual is of the order of ten to minus fifth and thus negligible.

In addition to the radiation doses for members of the critical group, the committed collective dose equivalent for the total population must also be assessed. This means that if a large population is irradiated to 1 mSv/a, there will be health consequences, but the additional risk is small compared to other risks in everyday life. However, if there are countermeasures, which can be implemented easily, with only minor inconveniences to people, these should be considered.

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health/intro #2, from sysop. No entry.

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health/intro #3, from sysop, 12770 chars, Mon Oct 6 10:12:59 1986

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TITLE: Paper 2, Health

# HEALTH AND SAFETY IMPLICATIONS OF THE CHERNOBYL ACCIDENT -- A SUMMARY PERSPECTIVE --

Morton I. Goldman
NUS Corporation
September 22, 1986

## I. INTRODUCTION

The immediate effects of the Chernobyl accident on the health of the plant staff and fire and rescue workers are unfortunately too clear. More than thirty have already died and about two hundred remain in hospital. It will be several decades before the impact of the accident on public health is as completely understood as possible, given the nature and apparently small magnitude of these consequences. During the recent IAEA working meeting in Vienna, a substantial body of data was presented by Soviet representatives with respect to the accident, the releases and environmental measurements in the affected regions, and estimates of individual and collective exposures in the affected populations.

The purpose of this summary is to review those estimates of the doses (and projected health effects) from the Chernobyl accident as they might have been (or be) affected by alternative
emergency responses and actions, for example, from the implementation of emergency plans and action levels similar to those for nuclear power plants in the U.S. A secondary objective is to comment on the basis for current emergency plans and, incidentally, the usefulness to public health agencies of such concepts as protective action guides (PAGs)in the light of the public and political reactions to radiation exposures of any magnitude above normal background levels.

## II. DOSE ESTIMATES

Based on the information presented in the USSR Working Group report to the IAEA, about 50 MCi of noble gases and the same quantity of other radionuclides, prominently radiodine and radiocesium, were released from the Chernobyl plant over the period extending from 26 April to 9 May, with maximum release rates observed at the beginning and ending of that period.
Evacuation of Pripyat, a workers' city within 3 km (two miles) of the plant, was begun at 2 PM on 27 April, about 36 hours after the accident at 1:23 AM on 26 April. Evacuation of the remainder of the zone within 30 km (19 miles) was carried out at unspecified times through 5 May. Collective doses presented in the USSR report,

# HUMAN HEALTH IMPLICATIONS

health/intro #1, from sysop, 6624 chars, Fri Oct 3 11:41:02 1986

TITLE: Paper 1

PROTECTION FOR PEOPLE FROM ACCIDENTAL RADIATION DOSES

Matti Suomela
Finnish Centre for Radiation & Nuclear Safety
P.O. Box 268
SF 00101 HELSINKI
FINLAND

## 1. Introduction

In this paper I will discuss factors which should be taken into account, when making plans for protecting people against accidents such as that which occurred at Chernobyl. I assume that the people in question live at some distance from the accident reactor so that there is no need for evacuation. I will consider separately internal and external exposures, although the sum of these is essential for predicting health consequences.

## 2. External exposure

After the agreement ensuring the early notification between IAEA member states in 1986 we can hope that information on accidents causing radioactive releases is obtained in good time. If the warning is too late, the network of continuously working exposure rate meters should give alarm in response to rising exposure rates. The Chernobyl accident showed that these meters should measure reliably exposure rates from 0.010 mR/h upwards. The integrated values of the meter readings should be automatically fed into the computer system in continuously manned "headquarters". The computer system should be capable of producing maps, which show isodose curves and integrated doses.

A computer program to calculate air trajectories of the radioactive cloud or plume is needed and the radionuclide composition of the fallout must be known for predicting the behaviour of exposure rates.

The next problem will be at what point the public should be informed. From the point of view of radiation protection an exposure rate of 1 mR/h, caused mainly by shortlived radionuclides, might be a reasonable value for informing the public. However, the Chernobyl accident showed that this limit might be too high. People should perhaps be informed after a doubling of the normal exposure rate. We must be careful to exclude incidents which do not last more than a few minutes (probably caused by a meter error) or which last hours but are caused by radon daughters. In the latter case the rise of the exposure rate is typically less than of 0.005 mR/h.

## 3. Internal exposure

The internal exposure starts via inhaled radionuclides. The most important nuclides are the isotopes of iodine and especially iodine 131. For predicting thyroid doses, the activity concentration of iodine in the breathing air must be measured. This requires gammaspectrometric measurements of aerosol filters. In order to be able to measure the iodine in organic form, the filter system must include an activated carbon filter. In the Chernobyl fallout about 85 per cent of the iodine was in an organic form and passed through the particle filter. To prevent thyroid contamination, stable potassium iodide can be administered to block the thyroid. But at what point should this countermeasure be taken, if indeed it should it be taken at all? Is the expected dose of 50 mSv to a one-year-old child the correct measure?

The whole-body counter measurements made in Finland of people evacuated from Kiev, about 100 km south of Chernobyl, showed that those who returned on April 30 were almost uncontaminated, but those returning on May 1 were contaminated with the whole composition of Chernobyl fallout nuclides. The contamination had probably occurred via inhalation. The measurements showed that, in addition to iodine, other radionuclides must also be taken into account as a source of internal contamination already in the early phase.

The most problematic question regarding internal contamination is the hot particles which, when fixed in the lung, may cause very high local doses. Although there were abundant hot particles in the Chernobyl fallout, we were not able to find such particles in whole-body counting measurements.

The radiation doses caused by radioactivity in foodstuffs

only 10-12%, although if it had applied to all milk supplies the calculated thyroid mortality would have been reduced to a total of about 75, or an average individual lifetime risk of 1 in one million. There are, however, some very practical constraints on the ability of any nation to mobilize the resources necessary to screen both urban and rural milk supplies for 75 million people in a timely manner.

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health/intro #5, from xjardi, 525 chars, Wed Oct 8 05:08:03 1986

This is a comment to message 1.

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COMMENT TO PAPER 1.

GREAT ATTENTION IS BEING PAID TO THE COMPUTER SYSTEM CAPABLE OF CALCULATING EXTERNAL DOSES AFTER AN ACCIDENT BASED ON REAL EXPOSURE RATES AND METEOROLOGICAL CONDITIONS.
WE WOULD APPRECIATE YOUR COMMENT ON THE COMPLEXITY OF THE DISPERSION MODEL TO BE USED: LINEAR OR VARIABLE TRAJECTORY, 2 OR 3 DIMENSIONS, ETC. AND THE PARAMETERS TO BE CONSIDERED TO SELECT A SPECIFIC MODEL. WHICH IS THE FINNISH EXPERIENCE ON THESE MODELS ?.

THANK YOU . J.TAPIA.
EMPRESARIOS AGRUPADOS. MADRID. SPAIN.

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health/intro #6, from jmatuszek, 144 chars, Wed Oct 8 09:40:19 1986

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TITLE: Correction to "health/intro #4"
In line 51, the sentence beginning "If very doses of radio-" should read "If very high doses of radio-".

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health/intro #7, from jmatuszek, 2170 chars, Wed Oct 8 12:50:28 1986

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TITLE: Comments to Paper 1
1. The Lawrence Livermore National Laboratory in the US appears to have done a reasonably good job of estimating concentrations and doses with their ARAC suite of meteorological codes.
2. If you propose informing the public at a threshold of 1 mR/hr, what do you expect to tell them? Short-lived radionuclides at that dose rate are unlikely to exceed the ICRP limit of 500 mrem, and may only produce 10 to 20 mrem of total exposure. Analert level which is too low will only serve to heighten the public's apprehension - especially if you have no other substantitive information to transmit.
Alerting at only double the normal gamma dose rate seems even worse. The levels you propose produce a risk which is at least tenfold less than that from radon in ordinary OUTDOOR air, and 100- to 1,000- fold less than those from ordinary indoor air. High-radon houses can scale up to risks which are 1,000- to 10,000- fold greater than your proposed action levels. Such conservatism in the case of NPP accidents doesn't seem to be warranted. 3. A 5-rem action level for thyroid prophylaxis with KI seems too low. In the US, the Food and Drug Administration has recommended an emergency action level (PAG) equivalent to 15 rem --- but , the recommended action is merely to divert the contaminated food from commerce. The FDA also set a preventative PAG equivalent to 1.5 rem to the infant thyroid for which the recommended action is to reduce the concentration of the contaminant in the food (milk). There is NO recommendation to invade people's bodies with a drug of unknown safety (see my message #4).
The American Thyroid Associationhas stated that clinically significant thyroid disease would be unlikely below 100 rad, but that blockade may be considered above 50 rad for children and pregnant women. However, the ATA does NOT recommend distribution of KI to the public.
4. If, as you say, the doses in Finland were indeed "negligible", why do you propose to take so many actions? This seems to be another case wherein the radiation expert is willing to substitute risks about which he knows little for those which he knows to be small.

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health/intro #8, from jmatuszek, 631 chars, Wed Oct 8 13:05:27 1986

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TITLE: Comment on Paper 2
My compliments, Mort, on a well-reasoned paper, especially for pointing out the trap of trying to apply the linearity hypothesis.
One correction, however. In the US the emergency PAG of 0.15 microcuries I-131 / liter of milk would have applied to those zones similar to Byelorussia and the Ukraine. The preventative PAG which you use in your paper only calls for a reduction in radionuclide concentration, not diversion. Since this accident happened in early spring, stored feed was probably not available. It appears that the Russian authorities acted well within the bounds of US regulations.

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health/intro #9, from oparis, 408 chars, Wed Oct 8 15:03:26 1986

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Withdrawn

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health/intro #10, from jmatuszek, 1618 chars, Wed Oct 8 15:24:21 1986

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TITLE: Effect of Chernobyl Contamination Zone on Source Reduction Proposals.

supplemented by my own estimates based on data in that document, are summarized in the table below.

## III. COMMENTS

Given these estimates of doses and projected public health effects (and with the perfect clarity that hindsight and distance provide) several comments are appropriate:

### Effects of Alternative Protective Actions

A. No explanation is provided in the Soviet document for the delay in removing the 24,000 residents from the 3 - 15 km zone following the completion of the Pripyat evacuation on the second day. With all due respect to the linear dose-effect hypothesis, real health effects are most likely to appear in the residents of this zone, whose external exposures alone averaged about 45 rem/capita. This value is well beyond the bounds of U.S. PAGs for the public of 1 - 5 rem (although it falls within the corresponding Soviet values of 25 - 75 rem). Had more timely evacuations maintained the per capita external dose of this population below 5 rem (not a difficult challenge, at least based on the external dose rates presented in the report), the total external collective dose for the 30 km population group would have been less than 40 % of the present dose estimates and the health consequences certainly less than that fraction of the present estimate.

B. To the extent that controls on milk distributed to urban residents were successful in limiting radioiodine concentrations to the USSR standard of 0.1 microcurie/liter (uCi/l), the collective thyroid exposure for European-USSR would be half the value shown in the table, which is based on the geometric mean of the reported concentration ranges and neglects any dose-suppressive effects of KI administration in those regions. The health effects estimate (using the ICRP-26 mortality risk of 5 x 10*-6/rem) would be reduced to about 1000 deaths. A more restrictive standard (such as the US Food and Drug Administration preventive PAG of 0.015 uCi/l) applied to urban milk supplies would have further reduced the estimated collective dose by

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## USSR DOSE AND CONSEQUENCE ESTIMATES

| DOSE/ CONSEQUENCE | REGION (Population) | | |
|---|---|---|---|
| | Pripyat (45,000) | 3-30 km (90,000) | Euro-USSR (74.5 million) |
| External gamma (whole body) | | | |
| Collective Dose, person-rem | 0.15 x 10*6 | 1.4 x 10*6 | 8.6 x 10*6 (1986)<br>29 x 10*6 (50 y) |
| Cancer Deaths | <------- < 280 -------> | | < 4750 |
| Per Capita Dose (Zone Range), rem | 3.3 (4.6-54) | 16 | 0.11 (1986)<br>(.003-.98)<br>.008/y (50 y) |
| Radioiodine (thyroid) | | | |
| Collective Dose, person-rem | [3 x 10*5]* | [3 x 10*6] | [4 x 10*8] |
| Cancer Deaths | [4] | [40] | [2000] |
| Per Capita Dose, rem | [6.1] | [30] | [5] |
| Cs-137 in food (whole body) | | | |
| Collective Dose, person-rem | ----- | [3,600] (10 d) | 1.2 x 10*8 (1st y)<br>2.1 x 10*8 (70 y) |
| Cancer Deaths | | | <38,000 (70 y) |
| Per Capita Dose, rem | | [.04] | 1.6 (1st y)<br>.04/y (70 y) |

* Bracketed estimates by author based on USSR-reported concentration/dose and population values and ICRP-26 thyroid mortality risk

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health/intro #13, from mgoldman, 855 chars, Wed Oct 8 19:44:12 1986

This is a comment to message 8.

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I would agree that the PAG is intended to be applied in all regions, whether urban or rural. My application to the urban population alone was based upon the Soviet report's statement that the control of milk consumption in rural areas was not practical -- the problem of regulating the family cow -- and hence the assurance that milk above the Soviet norms was not used by the rural populations could not be made. With respect to the preventive PAGs, my understanding of their intended application is under those circumstances in which the actions cause minimal or no adverse effects (economic or nutritional) on the affected populations. Diversion for cheese or milk solids production, if feasible, would have served as well to reduce both dose and economic impact; however, I suspect neither was practical to implement under the circumstances.

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health/intro #14, from mgoldman, 1388 chars, Wed Oct 8 20:15:12 1986

This is a comment to message 10.

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At the outset, I caution all against placing too high a degree of confidence on the projections of dose from cesium intake via the food chain; as stated by Marvin Goldman of UC-Davis, a participant at the Vienna meeting, these health effects are calculated from exposures not yet received from crops not yet sown on fields and in soils not yet characterized as to radionuclide availability and based on risk factors at doses and dose rates which have no demonstrated health consequences. Having said that, I would repeat a comment made in the "Containment" session. It would seem that the limiting factors for containment (and other engineered safety systems) performance are not the health effects, but rather the need to prevent the escape of fission products which can contaminate land and facilities and prevent their subsequent use. TMI clearly demonstrated that core damage prevention systems are more strongly justified by the need to protect the investment in the plant (an economic justification) than by the need to provide public health and safety protection. It seems to me that Chernobyl has confirmed the economic justification for the modification of containment systems to prevent the release of such fission products as cesium and iodine with capabilities for significant residual contamination, or economic impact on exporters, either international or domestic.

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health/intro #15, from mgoldman, 1929 chars, Wed Oct 8 22:01:51 1986

This is a comment to message 12.

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The concern with the tendency to provide estimates of cancer induction and mortality is understandable in the context of the attention paid to them by the media and their misuse and misunderstanding by certain elements of the public. Nevertheless, intelligent application to health protection of whatever lessons may be learned from this disaster are more likely to result from the consideration of risks to health than from consideration of "source terms" or "doses" in isolation. I think it is recognized by most (and certainly by me) that risk estimators are open to question when used in this fashion; in my case, the choice of risk estimators to use was largely driven by the Soviet report. However, no matter which are chosen, whether linear or linear-quadratic from BEIR, or linear from ICRP, the uncertainties in risk factors are probably not more important than the uncertainties in dose estimates under these kinds of circumstances, the former being no more than about a factor of 2.5.

For example, the 30 km region, with an estimated collective external dose of 1.6 million person-rem, has a cancer mortality expression estimated by the Soviet report as about 280 (2 % of 14,000); the ICRP linear risk factor of 1 x 10**-4 per person-rem would yield 160; and, the BEIR III linear-quadratic risk factor (on an absolute risk basis) yields 120. I would venture a guess that the total collective effective dose equivalent to that population is not known within that factor. Nevertheless, if we are to be able to plan effectively for the future, it is important to know that there may be more risk to health from exposure to externally deposited cesium than to inhalation of iodine, or from ingestion of cesium than from ingestion of iodine. And it is the health effects expressions or risks that must be compared, since doses alone (especially organ doses) will not provide that information.

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health/intro #16, from mgoldman, 2649 chars, Thu Oct 9 07:31:11 1986

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TITLE: Comments and Questions for Paper 1 - Matti Suomela

These are some comments and questions primarily for Matti Suomela:

1. The problem raised about the radiation dose rates at which the public should be INFORMED should not be a difficult one, IF such information is conveyed together with recommendations as to the actions (if any) that are appropriate to be taken. Whether the value is 1 mR/h or some arbitrary fraction of background is almost unimportant, as long as the report is accompanied by information that will guide the public actions and information they can use for perspective to evaluate their risks; e.g., that the 'current' exposure rate is double the normal background in that area, but is still less than the normal background radiation level in Denver, Colorado, or Chamonix, or that the risk from the increased exposure, if continued for X years, would be equivalent the risk from

Comparing the number of cancer deaths in Paper 2 to the release estimates for Chernobyl,it appears that Cs-134 and Cs-137 may be more limiting than I-131 for a severe accident. Estimates of deaths due to external gamma radiation and from food consumption over decades exceed those from radioiodine uptake by an order of magnitude for the near-in populationsand by four-fold for the Euro-USSR population. Assuming half the WB gamma dose is from radiocesium, of which only 1 to 10 MCi were released, it appears that about 10 to 100 cancer deaths per MCi released might develop. For I-131 and Te-132, the release of 30 to 50 MCi suggests that about 0.1 to 0.5 deaths per MCi might develop in the near-in population. Thus radiocesium seems to have about a hundred-fold greater risk per curie than does radioiodine.

The proportion for the larger offsite population appears to be of a similar magnitude.

Per capita doses of the magnitude listed by Goldman indicate that large populations will be relocated from large land areas if EPA's proposed relocation PAGs are adopted. This would be analogous to what the Russians have been forced to do within the 30-km zone.

It now appears that CsI, instead of being the bellcow to lead the US nuclear industry from its emergency response problems, will prove to be an albatross.

Does anyone care to comment on the effect that Goldman's estimates may have on the future of NPPs which can impact on very large populations (eg, Indian Point, Zion, Pilgrim, Shoreham to mention a few)?

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health/intro #11, from oparis, 497 chars, Wed Oct 8 15:33:58 1986

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TITLE: Decontamination off Vegetables:
A research team at the University of Konstanz reported in ENVIRONMENT, Vol. 28, No. 5, pp. 6-43, that fallout from Chernobyl could not be emoved from locally grown vegetables by washing them in cold water. By personal communication they reported that washing removed only 20% of the radioactivity on vegetables. Do any conference participants have additional information on this subject?

Oscar Paris
U.S. Nuclear Regulatory Commission

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health/intro #12, from dbunch, 3325 chars, Wed Oct 8 16:01:50 1986

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I have two principal comments to make, that apply to nearly all participants so far.

1. The matter of thyroid exposure.

It appears to me that the USSR data are particularly scant with respect to the magnitude and extent of the thyroid exposures. Their estimate of a one-percent increase in thyroid cancers is hard to believe without extensive substantiation of the derivation of that number. As Mort Goldman has indicated, the collective thyroid doses may be substantial; more importantly, however, the average doses, e.g., in the population living within 30 km, may be of the order of 100 rad or so, considering the contribution of short-lived radioiodines, inhalation exposures, and external gamma exposures. If (say) 100,000 people would have received doses of this order of magnitude, one might expect (using risk factors from NCRP Report 80 (1985)) between 100 and 1000 radiogenic thyroid cancers, and maybe three times that number of thyroid nodules. Given a normal life time incidence of 250 thyroid cancers in a population of that size, even the lower estimate should be easily detectable .Matuszek's estimate of a 20 to 70 fold lower effectiveness of radioiodine exposure vs. external x-rays appears out of thin air, and many of his other statements of a medical nature are controversial, to say the least. Nevertheless, his statement that the thyroid issue is one of the important unresolved issues is correct, and I hope that we will see considerably more information forthcoming from the USSR.

2. Risk estimates.

There appears to be a strong tendency to predict cancer cases or deaths in the exposed populations, notwithstanding the extremely imprecise dose estimates, and the fact that the 10,000-manrem-per-cancer-death number is not the correct number to use (as repeatedly stated by NCRP and ICRP). This number implies assumptions that are adequate for radiation-protection purposes (i.e., for planning of radiation protection measures), but are not meant to be used for the calculation of actual health effects. In the first place, the number assumes complete expression of the risk. In a population of different age groups, this assumption is not tenable, and one would have to use a life-table approach to calculate the corrected risk numbers. I estimate, very roughly, that this alone reduces the "cancer deaths" by a factor of two. Secondly, the number is derived from high-dose, high-dose-rate exposure data, especially the Hiroshima/Nagasaki data. Assuming a dose rate efficiency reduction factor of 2 or 3 (NCRP, BEIR III), this would further reduce the published numbers with a like factor. The major problem I have with analyses of this kind is that they tend to use "conservative" numbers, rather than "best estimates". You have seen what confusion the use of a conservative number for the population exposure from cesium intake has brought about, especially when a reduction with (at least) a factor of ten (!) was ascribed by opponents of nuclear power as an effort by pro-nukes to cover up the "real" danger of radiation. In short, let's be a little more scientific in our efforts to estimate the health and environmental consequences of Chernobyl, and somewhat less inclined to be "conservative"!
(10/8/86;jwthiessen)

which is transferred to our computer through telephone network. FMI uses a mixed Gaussian-gradient transfer model, i.e. the model uses a gradient transfer description of the vertical distribution of activity, while the horizontal distribution is treated as a simple Gaussian. The trajectory is calculated to the length where either the distance from the power plant exceeds 100 km or the time exceeds six hours. It must be mentioned, however, that the first version of our system, which is not operable yet, will use source term data rather than measured exposure rates as its basis; measured exposure rates will be used only for correcting isodose curves calculated first on the basis of source term data.

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health/intro #19, from jmatuszek, 2204 chars, Thu Oct 9 12:48:38 1986

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TITLE: Comment to health/intro #12 from dbunch

The estimate of a 20- to 70-fold lower effectiveness of radioiodine exposure vs. external x-rays was neither mine nor "out thin air". The estimates of so much lesser risk were first made by Maxon and Saenger and were verified independently by Holm and coworkers. Dr. Eugene Saenger appears to be well qualified for such studies, and his work has been published in appropriately reviewed journals. While not as well controlled as the Saenger or Holm studies, the survey by Rallison lends support to Saenger's estimate of lowered risk. Rallison was unable to find any excess of thyroid nodularity despite exposures estimated at 10 to 600 rad. References for these studies are listed below.

Dr. Rosalyn Yallow has testified before Congress that her review of a few-hundred-thousand cases of diagnostic I-131 administration also failed to show any statistical difference in the incedence of thyroid tumors despite exposures in the range 50 to 150 rad. Her review is not been published, except in committee records, as far as I know.

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3. Holm,L.E., et al: Incidence of malignant thyroid tumors in humans after exposure to diagnostic doses of iodine-131. J. Nat. Cancer Inst. 64: 1055-59, 1980.
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5. Holm, L.E., et al: Thyroid treatment and its possible influence on occurence of malignant tumors after diagnostic I-131. Acta Radiol. Oncol. 19: 455-59, 1980.
6. Rallison,M.L., et al: Thyroid diseases in children -- a survey of subjects potentially exposed to fallout radiation. Am. J. Med. 56: 457-63, 1974.
7. Rallison, M.L., et al: Thyroid nodularity in children. JAMA 233: 1069-72, 1975.


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health/intro #20, from jmaclellan, 345 chars, Thu Oct 9 13:00:07 1986

This is a comment to message 12.
There is/are comment(s) on this message.

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It seems to me that if we are trying to instill public confidence in our estimates of health effects we should be reporting what we believe the most likely effects will be based on the most probable dose and risk values. Uncertainties in the estimates should be covered by applicable error estimates rather than reporting worst case scenarios.

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health/intro #21, from lleach, 193 chars, Thu Oct 9 13:37:51 1986

This is a comment to message 14.
There is/are comment(s) on this message.

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This implies that core melt mitigation systems should be provided on an economic basis. The studies I have seen indicate that few, if any additional measures would be justified on this basis.

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health/intro #22, from mgoldman, 849 chars, Thu Oct 9 14:40:26 1986

This is a comment to message 20.

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I agree totally with the suggestion that, when dealing with a situation about which one has first-hand, direct knowledge, best estimates of both dose and health effects should be used. However, in the current instance I have no basis for second-guessing the dose estimates as presented in the Soviet report except as noted in my paper for the thyroid dose and as modified in Vienna with respect to the cesium ingestion dose. Given the uncertainty in the dose estimates by the Soviets, and the current flux in effects as influenced by the Japanese data, the differences between the Soviet effects estimates and those obtainable by using BEIR III linear-quadratic relationships are, as noted in my comment #15, not materially different insofar as they may influence post-accident planning either in the USSR or elsewhere.

Mort Goldman, NUS Corp

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health/intro #23, from mgoldman, 234 chars, Thu Oct 9 14:46:55 1986

This is a comment to message 21.

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I would not argue that more systems are needed; my point, somewhat poorly phrased, was that providing



12

smoking Y cigarettes per year.

2. There would seem to be slight reason to change from the ICRP recommendations for countermeasures, in my view. The risks from such accidents are low both in terms of probability of the same population being affected by more than one such event in a lifetime and, as has been unfortunately demonstrated by the Chernobyl accident, by the very low per capita doses at greater distances from the site. The risks to the public, however, need to be considered in the context of the ease with which they can be avoided; if the risks are high to a particular group (infants from fresh milk or Lapps from reindeer meat, for example) it may be simple to avoid a smaller dose in one case (by providing alternate milk supplies to infants, for example) and very difficult to avoid a larger dose in another without substituting a greater risk (starvation or social dislocation). These are not decisions or judgments that can be made by ICRP, but must be made in a particular national perspective.

3. In the same vein, you have estimated the first year dose in the area of highest fallout to be on the order of 1 mSv (100 mrem); from your effective half life value, that would indicate a dose commitment of about 7 mSv (700 mrem). Jack Valentin of the Swedish National Institute of Radiation Protection, at an AIF meeting this week, indicated a corresponding value in the area of maximum deposition of about 5 mSv (500 mrem) over 50 years, and an average for the entire population over that period of about 1 - 2 mSv (100 - 200 mrem). Have you made an estimate of the collective effective dose commitment for the population of Finland from this event?

Mort Goldman, NUS Corp

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health/intro #17, from ppirila, 1607 chars, Thu Oct 9 09:13:08 1986
This is a comment to message 5.
There are additional comments to message 5.
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Calculation of the transport of radioactivity from Chernobyl to Finland
From: J. Rossi and P. Pirila, Technical Research Centre of Finland

The calculations of the long range transport of radioactivity originating from Chernobyl have been performed in Finland using the model TRADOS developed jointly by the Meteorological Institute and the Technical Research Centre /1/. The model TRADOS utilizes air trajectories, based on observational data, which are calculated at three hour intervals in a two-dimensional grid with a mesh width of 150 km (at 60 degrees North). The calculations are performed using numerically analyzed winds at the 850 mbar pressure level. This corresponds quite closely to the altitude at which the dominant transport of radioactivity to the Finnish air-space is believed to have occured.

The simulation of the cloud is based on dispersion conditions (stability, mixing height, rain occurenced). These parameters are determined along the trajectory on the basis of temperature, humidity, horizontal winds on standard constant pressure levels, vertical winds of the layer 500-1000 mbar, net radiation, surface wind speed, character of terrain etc.

The model is capable to consider changing dispersion conditions, such as rain, mixing height and wind speed during the transport. On the other hand it is difficult to take into account possible transport of radioactivity at very high altitudes.

In general, the agreement between the model calculations and observed radiation levels has been quite good.

/1/ G. Nordlund et al, Health Physics Vol 49, No. 6, pp. 1239-1249, 1985.

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health/intro #18, from msuomela, 2213 chars, Thu Oct 9 10:14:33 1986
This is a comment to message 5.
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9.10.86
The choice of the dispersion model depends, of cource, on the nature and purpose of the system in which the model will be included as a part. If the system is intended to operate as a real-time system that is used by emergency control staff in early phases of the accident, it should be an easy-to-operate system and it should perform calculations as quickly as possible; so the complex and computer-time-consuming three dimensional dispersion models are almost certainly out of question. On the other hand, if time is not considered to be a crucial element, the choice between two and three dimensional models depends on other factors such as computer capacity, desired accuracy of results, and so on.

As for choice between linear and variable trajectories, it is known that under certain meteorogical conditions simple linear trajectory models give quite erraneous results. Therefore the system should be capable of treating variable trajectories properly - at least to some extent. Also, the dispersion model should include a possibility to use different deposition velocities for different nuclides.

We in STUK have for some time been developing a computer-based real-time system for off-site dose calculations during nuclear power plant accidents. In our system trajectories and dispersion patterns are calculated by the Finnish Meteorological Institute (FMI). During accident conditions FMI will calculate a new trajectory once or twice an hour. The data is then stored in a file

radiation protection practice, conservatism in evaluating the uncertainties would necessarily drive the Task Group and especially the Committee 57 members and other reviewers to adopt a value which would present the least potential harm. After all, an excessively high risk factor in radiation protection practice may only cause an inconvenience to the researcher, physician or waste disposer. The resulting dose saving from being overly conservative would seem to be easily justified for the little cost of the inconvenience.

HOWEVER, in the case of a severe accident the cost of being overly conservative on the iodine risk factor is to force the public to ingest a potentially harmful drug; a drug for which the risk is not well established. As I have stated in my other messages, we need to use the best-available risk factors for both the radioiodines and for KI, not the most conservative. Presumably, when we have such factors the uncertainty in each will be comparable, so that the full range of risks can be assessed for each side of the risk/benefit equation.

In answer to the other part of your comment concerning the shorter-lived radioiodines, I have focused my discussion mostly on the issue of I-131. But that is because the issues which relate to the short-lived radioiodines have even greater uncertainties than those for I-131, Sheltering is an especially effective, low-risk option to mitigate the effects of the other radioiodines; as is the use of simple breathing protection. Unless KI were distributed in advance to each household, a call to the public to come to KI distribution centers in the midst of an accident would only worsen the exposure to short-lived radioiodines. Furthermore, I question the effectiveness of late use of KI -- an excess of stable iodine circulating in the bloodstream may have the effect of increasing the dose to varios organs, rather than decreasing it. This is a very complex question for the emergency planner.

There remains much to be done before I would be prepared to risk the use of KI in New York.

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health/intro #28, from sysop, 1403 chars, Wed Oct 15 09:57:02 1986

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TITLE:

J. LAFUMA
Institut de Protection et
de Surete Nucleaire
CEA, FRANCE

At CHERNOBYL when considering the actions undertaken by soviet specialists faced with 200 people overexposed some questions arise :

- Are the 4 categories of severity (AIEA ) for selection of the patients used in other countries?

- The selection of the patients were carried out based on

- the situation at the moment of irradiation
- clinical signs such as naushea, vomiting, paleness, shock
- hematological depletion, especially lymphocytes in the first 3 days, then on cytogenetic evaluation of doses

- Are these criteria envisaged in other country ?

In the soviet report presented in Vienna, Dr GOUSKOVAA is affirmative on the fact that the irradiations were homogeneous. She based this affirmation on the results of cytogenetic studies, saying that distribution of cells with dicentrics were not differents of the theoritical one (indicated by Poisson distribution).

The question may be asked if this criteria is sufficient for the estimation of the homogeneity of the irradiation, which is crucial for the treatment.

On the whole, is it possible to have the detailed organisation set up in different countries represented at this conference? No more unread messages in this topic.

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health/intro #29, from msuomela, 822 chars, Fri Oct 17 05:38:30 1986

This is a comment to message 7.

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TITLE: Response to comment 7

1. I agree that in the point of view of radiation risks 1 mR/h is low and there should not be any need to inform the public. (The limit to warn people in Finland is 20 mR/h). But if you don't inform the public in the very early phase, which the exposure rates are still increasing, you will be accused that you have hidden essential information the public has right to know. What else you can tell depends on your emergency preparedness system. I think similar problems were encountered in the case at TMI-2 accident.

4. There might be a misunderstanding. The actions or countermeasures were discussed on general level. The risk to in individual is of the order of ten to minus fifth and thus negligible to individual, but the discussed actions can be used to lower the collective dose.

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health/intro #30, from msuomela, 569 chars, Fri Oct 17 05:41:32 1986

This is a comment to message 16.

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TITLE: Response to comment 16

1. I agree with your proposal to relate radiation risk to other more well understood risks. However, when we compared the exposure rates to normal background rates this was also critisized. People experienced e.g. five times normal background as very dangerous in spite of opposite

systems which produce acceptable public health protection does not necessarily produce adequate investment protection.

Mort Goldman

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health/intro #24, from dbunch, 2207 chars, Thu Oct 9 16:03:28 1986

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TITLE: Comment on health/intr #19
Comment on health/intro #19

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The 20 - 70 fold lower effectiveness of "radioiodine" refers, I presume, to Iodine-131, as compared to x-rays. The references given nearly all apply to Iodine-131; the Rallison studies are now in the process of being updated. Their lack of findings may be related to the relatively short time between exposure and effect (and, of course, to uncertainties in dose). NCRP Report No. 80, "Induction of Thyroid Cancer by Ionizing Radiation", 1985, contains all of Matuszek's references, and quite a few more. The Committee, looking at all the data, however, only recommended that "I-131 be considered to be no more than one-third as effective as external radiation in the induction of thyroid cancer in the general population" (NCRP, loc.cit., p.54). Although this recommendation is explicitly meant to apply to radiation protection practices, it clearly is based on uncertainties with respect to the protective effects of dose rate and dose distribution usually referred to when discussing the lower effectiveness of I-131. Finally, Yalow's numbers have not been published in the scientific literature, and until that has been done, no conclusions can be made based on her statements.

But above all, the situation in Chernobyl, especially at short distances, is not characterized by exposure to I-131 only, but to a range of shorter-lived radioiodines (I refer here to the risk from inhalation only, of course), combined with external exposures of tens of rads received within a period of two days. The I-131 exposure through the milk pathway may well have been minor, as the evacuees were provided with milk containing "allowable" concentrations within two days after the event. As far as I know, there is no good information of the impact of all exposure pathways on thyroid doses, and I do not feel that the assumed lesser effectiveness of I-131 is a major argument in rejecting the possibility of an increased incidence of thyroid cancer and nodules in the exposed population close-in to the plant.

(jwthiessen; 10/9/86)

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health/intro #25, from jmatuszek, 2384 chars, Fri Oct 10 15:10:15 1986

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TITLE: Further comments on health/intro #12 from dbunch.

In addition to questioning the lower risk factor for I-131, which I hope my message # 19 answered, you say that "other statements of a medical nature are controversial". Since you don't say what the controversy may be, I will take the liberty of amplifying on those parts of my message #4.

The question I raise concerning radioiodine contribution to early deaths and injuries stems from SANDIA calculations summarized in NUREG-0772, wherein 50% of early deaths are attributed to radiodines; something like 30% of early injuries, also. How else can that be except from radioiodine irradiation of blood-forming organs?

As for the detrimental effects from using KI, there is a multitude of references in the medical literature that verify their existance. What is missing from the medical literature is a definitive value for the risk associated with the use of iodine. Typically they are quoted as being "low", a nonquantitative and completely unsatisfactory description for the needs established for severe accidents. After 15 years with the Health Department, my experience suggests that most physicians and pharmacologists consider risks between 1% and 10% as "low". Risks between 0.1% and 1% are often described as "very low". Usually risks below 0.1% are not worth considering. So, when I read that the use of KI presents a "low" or "negligible" risk, I tend to consider that statement of risk as large against 5 x 10*-6 per rad, even when the thyroid dose may be in the range 10 to 100 rad. If I-131 is indeed less effective for inducing cancer death, the risk from I-131 may approach 10*-7; in which case, there is almost no thyroid dose high enough to warrant administration of KI within the limits of our current understanding of its risks.

If anyone knows of definitive studies which establish quantitative s13
risk factors for KI administration, I would appreciate the references.

We must require the same level of certainty ( or uncertainty) on both sides of the risk equation when we attempt ot advise the public as to the protective steps it should take. It serves everyone ill, if we radiation specialists substitute for a "known", small risk some unknown, but much larger risk. As the old proverb stated, "Don't pray too hard for the death of the king, for the new king may be worse."

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health/intro #26, from jmatuszek, 84 chars, Fri Oct 10 15:20:21 1986

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TITLE: Correction to #25

Ignore the line reading "s13". I missed it on the listing.

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health/intro #27, from jmatuszek, 2276 chars, Mon Oct 13 18:19:07 1986

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TITLE: Comment on Message #24 from dbunch

You answer in your own message why NCRP 80 does not apply in a case like this one. Because the NCRP recommendation is explicity stated to apply only to

methods, screening techniques, etc. are necessary and are somewhat different than in the operating case.

--Data Acquisition:
As a prerequisite to an effective plant decontamination effort, it is necessary to determine the source and characteristics of the radioactive material in question. These data may be particularly difficult to obtain in areas of high background dose rate and multiple radioactive sources, and innovative methods may be required. Examples of information needed are radionuclide mix and distribution, adherence to surfaces, depth of penetration, etc. Without an effective data acquisition program, decon efforts can be ineffective, both from ALARA and a cost and resources standpoint.

--Ex-Reactor Criticality Prevention:
Potential presence of fuel outside the reactor in unknown quantities and configurations demand a very conservative and cautious approach to preclude inadvertant criticality. As an example, in flushing piping systems and cleaning cubicles, positive steps must be taken to ensure that accumulations of fuel in critical configurations are prevented in all cases. Such positive measures may include use of neutron poison and critically safe geometries in flushing systems and waste receptacles.

--Decontamination Techniques:
There is much to be learned regarding the effectiveness of different techniques under different circumstances. In particularly severe situations, the use of robotically applied methods deserves special attention. Consideration must also be given not only to the removal effectiveness of various decontamination agents but also the manageability of by-product waste forms.

--Recontamination Prevention:
Proper steps must be taken to prevent recontamination of decontaminated areas. These steps include isolation or sealing of cubicles, modification of heating and ventilating systems, and the like.

--Liquid Waste Processing Methods:
Cleanup of the contaminated facility is likely to involve decontamination of large quantities of contaminated liquids. Again, this situation is likely to be very different from operational water processing ones because of higher concentrations of radioactive materials, potential presence of fuel, unusual chemistry conditions (such as from decontamination agents) and the like. Consideration must be given to use of simple and flexible systems. For example, at TMI-2, a very satisfactory experience was gained in the use of inorganic ion exchange media which permitted very high and stable fission product loadings.

--Storage and Waste Disposal:
Solid waste material extracted from decon fluids and water processing activities must be safely handled, stored and then transported to ultimate disposal facilities. Again, special attention is required because of unusual radionuclide combinations, presence of fuel, high overall rad levels and the like. Also, positive steps may be necessary (such as installation of catalytic converters) to prevent buildup of explosive concentrations from radiolytic decomposition within waste storage containers.

--Determining Suitable Post-Recovery Conditions:
Certainly at Chernobyl the long-term post-cleanup condition is not one previously envisioned, either by regulatory agencies or the plant designers. Care must be taken to insure that the conditions selected are acceptable with respect to potential for further accidents and/or environmental hazards. Sensible and conservative analyses must be employed to establish suitability of the conditions over an extended period.

The third category of interest is in off-site decontamination. We understand here that the Chernobyl problem is severe and largely unprecedented (although the Windscale experience in England in 1957 may offer at least a starting point in this area.) The lessons being learned at Chernobyl now in off-site cleanup have potentially very high value and should be followed closely, particularly in the following areas:

--Contamination levels and distribution correlated with accident meteorology
--Decontamination techniques and their effectiveness
--Contamination mobility, including recurrence of contamination in decontaminated areas, apparent migration, or other changes in contamination levels as a function of time, again correlated with meteorology, and the like.

In general, the importance of good instrumentation and thorough, methodical recordkeeping is extremely high. As unfortunate and consequential as the accident at Chernobyl has to be, the simple irreversible fact remains that it happened. It is our responsibility now to learn as much as we possibly can from that experience, to use that knowledge in a positive way to effect an even safer nuclear generation on a world-wide basis.

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management/intro #3, from sysop, 5304 chars, Thu Oct 9 17:00:31 1986
There is/are comment(s) on this message.

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TITLE: Paper 2

THE CONTROL OF OFF-SITE CONTAMINATION

SF Hall and JH Gittus
Safety & Reliability Directorate, UKAEA

INTRODUCTION. During and after the accident at Chernobyl the Soviet Union has faced enormous

reassurance effects.

3. The effective half-life value used in example was quite conservative. An estimate of the average dose commitment for the entire population is approximately 1-2 mSv and the corresponding collective dose commitment 5000-10000 mSv.

END OF HUMAN HEALTH EFFECTS CONFERENCE

# THE IMPLICATIONS OF THE CHERNOBYL-4 ACCIDENT FOR ACCIDENT MANAGEMENT

Marshall Berman
Sandia National Laboratories
Albuquerque, New Mexico, USA

Advanced knowledge concerning severe accidents plays an important role in risk assessment, reactor regulation, accident management, and the public acceptance of nuclear power. This paper addresses the question of accident management and what can be learned from Chernobyl.

The key issues as I see them are:

1. Evacuation management.
2. Fire control.
3. Minimization of the threat to the atmosphere and the water table.
4. Long-term decontamination.
5. In-vessel and in-containment accident termination.
6. Ensuring control of plant operations.

## Evacuation

Although there was some delay, the Russians were able to evacuate 49,000 residents in 2.5 hours with a fleet of 1000 buses. An accident in an American plant might involve the evacuation of many more people. The U.S. Nuclear Regulatory Commission requires evacuation plans for all plant sites. The various parties (utilities, government, regulators) should review these plans in light of Chernobyl. All parties should agree as to what accident indicators would require an evacuation to begin, how it would be conducted, and where the people would be evacuated to.

## Fire Control

Hydrogen combustion occurred at TMI and Chernobyl. Research has been conducted to ensure that key safety equipment within containment would survive hydrogen burns and other types of fires. However, containment failure due to energetic phenomena (steam explosions, hydrogen detonations, high-pressure melt ejection) could involve the dispersal of molten fuel and cladding outside of containment. Fires ignited and maintained by these hot materials could be very difficult to extinguish. Furthermore, fire fighters would place themselves in great jeopardy. Additional research might be devoted to fighting fires outside of containments, possibly using automated equipment, robots, and aircraft. Other extinguishing materials besides water need to be available.

## Threats to Atmosphere and Water Table

If containment is breached, attempts should be made to minimize the spread of fission products. It is important to understand that pouring water on molten fuel might cause a steam explosion, quench the fuel, or do nothing. Similarly, the rate of production of aerosols from core-concrete interactions needs to be understood, and mitigation schemes developed if necessary (e.g., what materials should be poured on the melt?). Research has produced computer codes capable of analyzing some of these questions. Calculations should be performed to determine the threat from aerosol production and the potential for basemat penetration. It might be wise to develop some advance strategies and mitigation systems that would decrease the vulnerability of the air and water table in the unlikely event of an accident.

## Long-Term Decontamination

We should be able to learn much from the Chernobyl experience, both with respect to short-term responses (days) and over many years. The Russian plans for long-term decontamination and isolation appear to be well thought out. We need to carefully monitor their progress.

## Accident Termination

Given that an accident has been initiated, the most desirable result would be to arrest the accident before the failure of the vessel or containment. Some people have suggested a "blind" approach "to add as much water as possible to a Western-type LWR as soon as possible." This is probably an excellent strategy prior to core melt. However, experiments at Brookhaven and Sandia National Laboratories have shown that pouring water onto molten core materials can sometimes lead to a steam explosion. With this knowledge, there may indeed be situations where the blind pouring of water would not be advisable. More research is needed on this subject. It may be possible to show that explosions in this contact mode (water onto fuel) are less likely at high ambient pressure, or with saturated water. If this can't be shown, then a reasonable accident strategy might be to introduce quantities of water that are insufficient to breech the vessel, and attempt to trigger a sequence of small steam explosions, perhaps by cycling the pumps on and off. Introducing water in pulses in this fashion might permit cooling the core without endangering the vessel's integrity. TMI may also have shown that the core can continue to melt within a "protective" crust, even after water flow has been restored. Strategies for safely terminating the accident under these conditions may again require that water be introduced in pulses and triggered to cause multiple small steam explosions, rather than a large one.

The Russians drained their equivalent of a "suppression pool." This may have been done to avoid the possibility of another steam explosion if molten core materials poured into the water. There may be some Western reactors that are vulnerable to ex-vessel steam explosions, possibly the BWR Mark II design. These plants should be analyzed to assess this possibility.

# IMPLICATIONS FOR ACCIDENT MANAGEMENT

management/intro #1, from sysop, 10149 chars, Fri Sep 26 14:45:51 1986

TITLE: Paper 1

## ACCIDENT MANAGEMENT AT CHERNOBYL

## WHAT WE CAN LEARN

By J. C. Devine Jr.
Program Manager
Advanced LWR Program
Electric Power Research Institute

The accident at Chernobyl offers extraordinary opportunities to learn and to improve upon nuclear safety in all respects. It provides a basis in reality with which to supplement and adjust our analytically based assumptions about accident management. It is important that we take full advantage of this opportunity.

Within the topic of accident management, there is a wide spectrum of potential discussion areas. It is useful to think of these as comprising three broad categories: first, the accident response; secondly, the on-site recovery (or clean-up efforts); and thirdly, off-site decontamination.

The accident response category covers those near-term activities taken at the site to regain control of the situation, to protect people and the environment, and to restore stability such that the longer term corrective aations can proceed safely. The accident response phase at Chernobyl is basically complete. It is advisable at this point to gather as much information as possible and to analyze and extract from it conclusions which may be useful in preparing for possible accidents at other plants.

It is important to note that, in many respects, the accident response activities at Chernobyl were unique to the RBMK reactor and to the specific accident scenario at that plant. For that reason, applicability of the Chernobyl experience to other reactors around the world may be somewhat limited. Nevertheless, there are certain common denominators which deserve careful attention. Two of these are:

--Readiness:

The effectiveness of any organization's--or nation's--efforts in coping with a major accident is a function of its readiness. Readiness includes thorough accident planning, physical preparation (such as ensuring availability of resources including people, material and equipment) and training.

--Communication:

Experience in management of major accidents of all kinds continues to reinforce the vital importance of effective communications. This includes external communications among accident managers, the government, the media and the public, as well as internal communications within the plant and within the technical community. One cannot underestimate the significance of this one facet of accident response.

The second major category of interest is on-site recovery and cleanup work. In this area, despite the evident differences in reactor design, accident specifics, and cleanup approach, there is generic applicability of the Chernobyl experience to reactors worldwide, and undoubtedly some commonality among Chernobyl experience, TMI-2 and other reactor accidents and incidents in our history. Important topics within this category include the following:

--Radiological Protection:

Cleaning up in the aftermath of a reactor accident presents a unique and very challenging set of radiological control problems. The most obvious of these is that it is necessary to manage safely personnel working in very contaminated, high radiation areas. Under such conditions, seemingly routine matters such as work planning, protective clothing, communications, radiation monitoring instruments and procedures, air breathing equipment, communications, real time dose. tracking, and industrial safety all become magnified in importance and essential to the safe completion of every job. It is important to note that learning in this area also has applicability and high value to non-accident conditions in nuclear facilities.

Compounding this problem is the fact that the radionuclide mix generated by a nuclear accident (and contributing to the high radiation fields and contamination levels) may be quite different from that produced in normal reactor operations. This requires special vigilance as well as some important changes in procedures, methods and equipment used in protecting personnel.

Similarly, dealing with fuel material (including alpha emitters) rather than the fission product and activation materials normally encountered in operating reactors presents unique challenges. Use of gamma spectroscopy

One of the conclusions from the IAEA expert meeting in Vienna was that it is necessary to have a central authority with great powers to cope with an accident of this scale. Different Soviet bodies seem to have been well codinated and took to drastic measures when the public was evacuated. There has also been reports about a large number of civilians who has been sent into contaminated areas for decontaminationwork.

Do you consider it possible to act as swiftly and determined as the Soviets, if an accident of the same magnitude had occured in a Western Democracy?

Mikael Holmstrom, Science Writer,
Magazine: Ny Teknik, Stockholm, Sweden

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management/intro #11, from shall, 1028 chars, Tue Oct 14 05:06:30 1986
This is a comment to message 10.
There is/are comment(s) on this message.
There are additional comments to message 10.

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TITLE: EXAMPLE OF ACCIDENT MANAGEMENT IN WESTERN DEMOCRACY.

Although not a nuclear accident, the freight train fire and explosion in Mississauga, Ontario in November 1980 is a good example of what can be achieved without a strong central authority. During the course of one day (11 November 1980) 218,000 people were evacuated over an area of 120 square km. The evacuation was very efficient and smooth by all accounts, and was possible only because the various agencies (fire, police, etc.) had plans, prepared in advance, which they were able to put into effect on a very short timescale.

These plans included the means by which the various agencies should communicate and work together, and communicate with the public. All the indications are that the Soviets also were well prepared and it was this, rather than the fact that they have a strong central authority that enabled a quick response and evacuation. As we said in our paper above, preparedness is the key!

Steven Hall
Safety and Reliability Directorate, UKAEA

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management/intro #12, from mberman, 4494 chars, Tue Oct 14 11:29:53 1986
This is a comment to message 9.
There is/are comment(s) on this message.

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TITLE: Accident Management - Response to Message 9 by K. Soda

Marshall Berman

Although advances have been made in fire-fighting technology, I do not believe that that this area has received sufficient research attention. Considering the extent of the threat posed by fire to civilians, industrial structures, transportation, military personnel and structures, and to the environment, I think that fire-fighting technology is still surprisingly primitive in many respects. Extinguishing fires caused by severe reactor accidents would pose many special problems.

If containment is breeched by an energetic process such as steam or hydrogen explosions or high-pressure melt ejection, then core materials at very high temperatures (2500 to 3000 K) could be scattered over a large area. The high temperatures could start fires in almost all combustible materials, and the dispersion could lead to very large scale fires. High levels of radiation could accompany these fires. Although firemen are often brave, they may be very reluctant to approach a fire if doing so could lead to death from irradiation.

Much research has already been conducted on combustion hazards. However, I believe that additional work on extinguishing fires could be very cost-beneficial. This research could address the following two topics: 1) The physics and chemistry of combustible materials including ignition and flame characteristics, emissions of toxic gases, the best materials for quenching flames, possible hazards in using some quenching agents for some combustibles, and possible new chemical, physical and mechanical concepts for extinguishing flames; and 2) automated methods and high-technology equipment for fighting fires. Since so many aspects of modern society could benefit from this research, I believe that the Chernobyl accident could provide the incentive for cooperative national and international efforts in this area. Potential supporters and users of this research include governments, various industries, insurance companies, regulatory agencies, and even the military.

I don't believe that it is necessary to establish international standards for operator training and qualification, although such standards might be useful. The point I raised dealt with the hostile takeover of a control room - the exact opposite of beneficial accident management! I am suggesting that utility managers study their plants in terms of the capability to overide control room direction, if necessary, to bring the plant to a safe shutdown condition. Some plants may be more susceptible to hostile takeover than others. If necessary, one- way engineering changes could be considered to prevent deliberately harmful procedures from being initiated from the control room. Limited access and increased plant security measures might also be instituted.

You also discussed accident prevention and accident management. Clearly, accident prevention will pay the highest dividends in terms of public acceptance of nuclear power, increased economic benefits from plant operation, and decreased liability. The TMI and Chernobyl accidents should convince plant operators of the importance of very high safety standards, high operator skill and training, good management, and a solid understanding of reactor physics and engineering under normal and abnormal

challenges in the area of accident management and control. This paper looks at the challenges of limiting the spread of radiation and of cleaning up off-site contamination in order to limit long-tem effects. The speed with which the Soviet authorities have organised large scale decontamination and civil engineering works is most impressive, and points to a large degree of preparedness and co-operation between agencies.

The control of off-site contamination depends on three principles: Immobilisation, Decontamination and Monitoring. This paper examines these principles in the light of Chernobyl experience.

In a 30 km radius around the plant the measures taken are appropriate to an agricultural region in spring/summer. Another type of land use in another season might necessitate different measures.

IMMOBILISATION. The immobilisation of radionuclides is vital for control of contamination where decontamination is impossible or incomplete. First, further release from the source must be prevented. Since removal of the core is impracticable (in contrast to TMI) this is being achieved at Chernobyl by entombment of the damaged rector, and site decontamination. Then, redistribution must be controlled :

i) FROM SOIL. The main method used around Chernobyl has been to treat soil with dust suppression compounds to prevent resuspension. Ploughing in contamination using deep-digging ploughs has been tried experimentally. This method reduces ground-shine by a factor of 5-15 but may be only moderately successful in preventing resuspension (10% contamination is left on the surface), and has the disadvantage of encouraging entry of contaminants into underground water.

ii) IN SURFACE AND UNDERGROUND WATER. In order to prevent contamination entering the Pripyat' river, large scale civil engineering work has been required to redirect and treat surface water, and to lower the water table.

iii) ON PERSONNEL/VEHICLES. This can only be controlled by zoning with strict control of movements. At Chernobyl, personnel are trans-shipped and vehicles decontaminated at zone boundaries.

iv) ON PLANTS AND SEEDS. This necessitates harvesting and destruction of crops and grass. Treatment of soil with lime inhibits root uptake of Cs and Sr in future crops, which must be chosen to have low root uptake. In the Ukraine, trees are a particular problem because of their part in the ecosystem (moisture conservation). Fire is the most likely cause of redistribution of radionuclides fixed in trees and forest litter and so fire prevention is the easiest remedy.

v) ON ANIMALS. Prophylactics such as bentonite limit Cs uptake from feed and, by fixing the activity in dung, contribute to immobilisation of contamination. The movements of wild animals could hinder contamination control, particularly if habitats are destroyed by decontamination work.

DECONTAMINATION. Decontamination, where possible, is obviously the best method of contamination control. Working "from dirty to clean", the need for personnel access to clean areas and prevention of re-contamination are principles that need to be taken into account in formulating a decontamination strategy.

a) BUILDINGS, ROADS AND VEHICLES. The use of decontamination solutions for washing by jets or by hand, and the use of removable polymer coatings (as at TMI) are well-known and well-tried technologies, albeit on a small scale.

b) SOIL. Decontamination on-site has been achieved at Chernobyl largely by soil removal and storage with replacement either with fresh soil or concrete. Methods have been proposed to remove radionuclides from soil by chemical scrubbing but this is only practicable for small areas. Removal of radionuclides by irrigating the soil with complexing agents is possible for larger areas but has the effect of moving the contamination into ground water. Plant varieties with large root uptakes of Cs and Sr could be considered in this respect, since harvesting and destruction would have the effect of soil decontamination. Unstable ground covers such as sand or snow pose a particularly serious problem.

MONITORING. Accurate and widespread monitoring is necessary to provide information for the formulation of decontamination strategies, both in quantifying the levels of contamination and in assessing its movement. Continual monitoring over many months, and perhaps years, is the only way of ensuring that decontamination and immobilisation measures are effective, and that stability at a low dose level has been achieved. This information is required by those who have to make decisions about repopulation, replanting and other returns to normal life.

CONCLUSIONS. Accident management on this scale demands rapid action, cooperation between a large number of national and international agencies, and the immediate availability of diverse commodities (concrete, reagents, measuring equipment). Preparedness demands planning. Effective measures, usable on a large scale in various conditions, must be the aim of research.

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management/intro #4, from mberman, 7097 chars, Fri Oct 10 10:04:01 1986

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TITLE: IMPLICATIONS OF CHERNOBYL FOR ACCIDENT MANAGEMENT

taken under circumstances which fell well under established guides in this area. Yet these actions, whether witholding crops/milk from use or evacuating all or selected segments of a population, carry their own costs. These costs range from economic and social dislocations to psychological trauma to actual risk of injury or death during evacuations or harm from such prophylatic treatments as KI.

It seems to me that the guidance for levels at which protective actions are launched needs some fresh perspectives in terms of the risks of the actions themselves, perhaps in terms of the alternatives available in particular circumstances. In any event, we ought to be focusing our post-Chernobyl accident management learning on the real needs, rather than posting low-probability "boogie-men" as valuable objectives for further study.

M I Goldman, NUS Corp

This note applies equally to comment #12.

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management/intro #16, from lleach, 160 chars, Tue Oct 14 16:46:26 1986
This is a comment to message 10.

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It may also be necessary to have a central authority with great powers and limited public involvement to have an accident of this magnitude in the first place!

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management/intro #17, from lleach, 734 chars, Tue Oct 14 16:53:48 1986
This is a comment to message 11.

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The soviets admitted in Vienna that their emergency plans were inadequate for the situation that occured. They had not imagined an accident of this magnitude. While their response does indeed appear to have been very good given the circumstances, it's difficult to say how good and if a similar response would have occured elsewhere.

Certainly in countries with more free press and communication, and with more individual automobiles, the problem would have been more difficult. It is difficult to imagine effectively implementing a sheltering decision, for example, in an event of this magnitude.

Thus, I support the conclusion that preparedness is the key, in spite of some different interpretations of the Soviet response.

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management/intro #18, from lleach, 757 chars, Tue Oct 14 17:01:19 1986
This is a comment to message 12.

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It appears as though the protection of the firemen was not given high priority. In particular, little mention is made of use of respirators (SCBA), and dosimiters, particularly PADI's (Personal Alarm Dosimiters), which we would routinely use before sending personnel into such an environment. I also do not recall mention being made of surveys prior to sending in the fire fighters. The implications are that extinguishing the fires was considered of absolute priority. Some mention was made of concern for other combustibles. Let me propose some discussion questions:

1) Under what circumstances does one risk the lives of firefighters, eg. not take time to dress and dosimiter properly?

2) What are the implications for muliple unit sites?

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management/intro #19, from shall, 1400 chars, Wed Oct 15 10:47:33 1986
This is a comment to message 18.

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FIRE FIGHTING AND RISK TO FIRE FIGHTERS

SF Hall
Safety & Reliability Directorate, UKAEA

From the account of the accident given in Vienna it appears that the response to the fire by firemen from the towns of Pripyat and Chernobyl was quite rapid and it may have been that the speed of action precluded concern for personnel safety. Also in this case it is unlikely that respirators would have provided much protection since the ulk of the dose came from direct radiation from core materials rather than airborne contamination.

How much protection and real-time monitoring could be afforded fire-fighters on a short timescale in a nuclear accident is a question that ought to be addressed for all nuclear installations. It is certainly a question which has caused a great deal of worry to the fire brigade trade union in the UK since Chernobyl, and led that union to take a strongly antinuclear stance at the recent Labour party convention. In an emergency, as we saw at Chernobyl, people are capable of showing great heroism; however, our emergency planning should not rely on it.

Obviously, as has been stated by several contributers, robotic fire-fighting equipment is the answer but in the interim those involved in emergency planning should make this sort of question a priority.

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management/intro #20, from shall, 427 chars, Wed Oct 15 10:57:15 1986

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EVACUATION - HOW DO YOU DECIDE?

SF Hall
Safety & Reliability Directorate, UKAEA

Do any participants have views on the following

Control of Plant Operations

As a consequence of the accidents at TMI and Chernobyl and the contributions due to operator error, we can hope that plant operators will be much more safety conscious in the future. However, a different aspect of accident management needs to be considered if plant operations are seized by hostile groups (terrorists, disgruntled or suicidal employees, etc.). Although measures should clearly be taken to prevent hostile takeovers, it might be prudent for plant managers to have available to them measures to manage and terminate accidents under such conditions.

In conclusion, the approach to reactor safety primarily involves accident prevention and containment. Accident management, however, involves the minimization of consequences. There is much that could be learned from Chernobyl regarding accident management. It would be wise to consider how we would respond to an accident of the same magnitude. If there are important questions that require some answers, it would be prudent to begin to procure the necessary information now.

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management/intro #5, from psviatoslavskyx, 376 chars, Fri Oct 10 10:13:04 1986
This is a comment to message 3.
There is/are comment(s) on this message.
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Comment on paper #2.

It was my understanding that, depending on the type fo soil, particulate contamination is readily filtered by a few of soil and that runoff into streams and rivers is the major concern. Any comments on this by conference participants would be appreciated.

Pete Sviatoslavsky
WPSC Green Bay WI

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management/intro #6, from psviatoslavskyx, 100 chars, Fri Oct 10 10:28:26 1986
This is a comment to message 5.
There are additional comments to message 5.
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My Comment #5 is refering to the contamination of ground water.
Pete Sviatoslavsky

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management/intro #7, from jmaclellan, 29 chars, Fri Oct 10 13:00:34 1986
This is a comment to message 5.
There is/are comment(s) on this message.
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Filtered by a few ? of soil?

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management/intro #8, from psviatoslavskyx, 121 chars, Fri Oct 10 14:37:44 1986
This is a comment to message 7.
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Sorry for the errors. The statement should read, "filtered by a few feet of soil."
Pete Sviatoslavsky

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management/intro #9, from ksoda, 1605 chars, Sun Oct 12 06:37:46 1986
This is a comment to message 4.
There is/are comment(s) on this message.
---
Whenever we discuss "accident management" we should discuss "accident prevention" with an equal weight. As one of countries which depend on nuclear energy to produce electricity, Japan continues to construct nuclear power plants although with slower pace.

However the Chernobyl-4 accident had an impact on Japanese public perception of reactor safety, perhaps the impact was less severe than some of European countries. Neverthless, the accident gave us a good opportunity to review safety issues in our country as well. There are two different concerns among publics. One is accident management and the other is accident prevention. Residents near a nuclear power plant which was already built are more concerned about accident management, while residents near a power plant which are in planning stage are concerened about accident prevention. If we emphasize too much on our readiness to cope with a severe accident and a well defined accident management, people tend to believe that a severe accident is a reality. Therefore our first priority to convince public of safety of a nuclear reactor is to prevent a severe accident and to let public believe the safety of a nuclear reactor. Then as a backup system, accident management should be prepared.

Questions to Dr. Berman. Do you see needs to develop new tools or new considerations on fire fighting techniques after the Chernobyl-4 accident? Relating to your comments on the hostile group etc., perhaps we need to establish a sort of an international operator training or qualification throughout the world.

K. Soda, JAERI, Japan.
Read:

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management/intro #10, from mholmstrom, 864 chars, Mon Oct 13 11:00:23 1986
There is/are comment(s) on this message.
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TITLE: DOES THE MANAGEMENT OF A NUCLEAR ACCIDENT REQUIRE A STRONG CENTRAL STATE?
TO ALL PARTICIPANTS
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COMMENT ON J. ROYEN COMMENT #11
STEVEN C. SHOLLY, MHB TECHNICAL ASSOCIATES

Monsieur Royen may feel free to call the Chernobyl containment whatever he wishes. The fact remains that the Chernobyl containment was designed around a design basis accident that is nearly identical to that assumed for Western LWRs. Of course, a quite different approach was taken, one which resulted in a containment which is poorly suited, if at all, to preventing a substantial radiological release in the event of an accident beyond the design basis. Concerning whether this makes the Chernobyl "containment" a "partial containment" or a "pseudocontainment", there is a saying in my native Pennsylvania which I will paraphrase -- if it walks like a duck and talks like a duck, there is a very high conditional probability that it is a duck.

In addition, I think that most observers can readily draw the distinction between the Chernobyl containment approach and that adopted for Western reactors. If, however, the Chernobyl containment is a "pseudocontainment", then I submit that the same could be said -- with considerable justification -- for Mark I and Mark II BWR containments and the ice condenser PWR containment. None of the latter perform very well for a range of accident scenarios, although each exhibits some containment capabilities for certain severe accident scenarios.

RESPONSE TO COMMENT #14 BY A. BRACHET AND H. GUMBAIL
STEVEN C. SHOLLY, MHB TECHNICAL ASSOCIATES

The 200 MJ figure I understand to have been calculated by French participants in the IAEA meeting; I am unsure whether this figure was for the thermal release or the kinetic energy imparted to the structures above the core. In regards to the 320,000 MJ figure, this was a thermal energy release calculated by NRC sources; I would prefer that these gentlemen step forward with their calculational method and results. Indeed, I would be most interested in any refinements that have taken place since I spoke to them, since their calculation was preliminary and ongoing.

RESPONSE TO COMMENT #16 BY J. TAPIA
STEVEN C. SHOLLY, MHB TECHNICAL ASSOCIATES

Mark I containments are notoriously ill-equipped to respond to the containment loadings likely to be imparted by a variety of severe accidents. Owing to their small free volumes, Mark I containments are particularly susceptible to early failure under any impulsive loadings (such as hydrogen burns when the containment is de-inerted or such as high pressure melt ejection resulting in direct heating). Mr. Tapia should be aware of the exchanges underway in the U.S. between the NRC (Robert Bernero, et al.) and the BWR Owners Groups concerning possible design and operational changes for Mark I plants to increase their capability to resist early failure in severe accidents. I suggest Mr. Tapia contact NRC regarding the details. I would be pleased to discuss Mark I containment vulnerabilities in more detail via correspondence should Mr. Tapid (or others) desire.

QUESTION FOR L. HOEGBERG FROM S. SHOLLY

Are the Swedish BWRs permitted to de-inert their containments while at power? U.S. reactors are permitted by NRC regulations to de-inert 24 hours prior to shutdown for refueling or any other major outage, and may remain de- inerted at power during the first 24 hours of operation following such an outage. The Limerick PRA (and the subsequent review by Brookhaven National Laboratory) assessed that there was a 1% chance of early containment failure due to hydrogen burn/detonation resulting from this practice. I would add that this practice is being re- examined in the discussions in the U.S. concerning possible safety improvements for Mark I containments.

QUERY FOR MICHAEL HUTCHERSON, FAI
STEVEN C. SHOLLY, MHB TECHNICAL ASSOCIATES

I may have missed your response to my containment paper concerning steam explosions. Could you possibly re-transmit or send a copy by mail? I am most interested in FAI's views on steam explosions post-Chernobyl.

RESPONSE TO COMMENT #34 BY S. HALL, SRD, UKAEA
STEVEN C. SHOLLY, MHB TECHNICAL ASSOCIATES

Mr. Hall observes that a very small hydrogen explosion would be sufficient to destroy the comparatively weak structure above the operating floor at Chernobyl. I would agree, and further observe that the same is true of BWR Mark I and Mark II containments. Has anyone done any calculations as to what structural damage could be expected from hydrogen burn/detonation in the reactor buildings of Mark I and Mark II facilities?

RESPONSE TO COMMENT #41 BY P. SVIATOSLAVSKY
STEVEN C. SHOLLY, MHB TECHNICAL ASSOCIATES

You raise an interesting aspect of the RBMK design philosophy. Does anyone have any information concerning the design bases for external events for RBMK reactors? I would be particularly interested in seismic, aircraft crash, wind, and snow loadings.

pool. As a consequence, high energetic pressure peaks are expected for those cases where the coherent molten fuel mass isof the order of 10 metric tons. To my understanding, in the Chernobyl accident the power excursion has rised rapidly fuel temperatures associated with transient fission product release and partly fuel vaporization which together have caused fuel pin overpressurization and clad rupture. Almost simultaneously finely fragmented overheated fuel was ejected into the pressurized coolant channel where the fragments interacted with water and steam. These spacially distributed events will have extended the load period of the core and increased the damage energy.

In order to avoid misunderstanding, it seems to me that in Chernobyl a fuel-coolant (or water) -interaction (in the Fast Breeder Accident Analysis well known as FCI) has occured rather than a steam explosion as discussed in LWR safety investigations.

B. Kuczera

==========

containments/intro #33, from lleach, 798 chars, Fri Oct 10 15:26:10 1986
This is a comment to message 31.
---

Brian, that was my message on tube rupture propogation. We have a model for the N Reactor, which is also a pressure tube, graphite moderated reactor. It uses the RELAP5 code to generate loads on the graphite resulting from tube rupture, and calculates strain on adjacent tubes. It's validated based on rupture tests performed at Hanford. At N Reactor, void space was designed into the graphite moderator to vent the steam and reduce the load transmission. (Also results in negative void coefficent of reactivity...not sure which came first but now we're sure glad about the later!) Anyhow, we have had Impell modify this model for the RBMK, and the calculated loading exceeds our criteria for rupture propogation. If you need more info please contact me by mail.
Larry Leach (aka "lleach")

==========

containments/intro #34, from shall, 901 chars, Tue Oct 14 12:07:56 1986
This is a comment to message 10.
There is/are comment(s) on this message.
---
TITLE: HYDROGEN EFFECTS

As Marshall Berman points out, large quantities of hydrogen can be generated on a very short timescale if the metal being oxidised is molten (or dispersed in some other way). However, it is unlikely that a hydrogen explosion could have contributed to the destruction of the core since the hydrogen generation under these circumstances would preclude oxygen. If a hydrogen explosion occurred, it must have been in the building above the operating face of the reactor after the floor had failed due to the earlier explosion. If this is so, then the geometry would make a detonation virtually impossible to achieve. However, it is my opinion that a deflagration of the order of that that occured at TMI would be sufficient to destroy the comparitively week structure above the operating floor if it were still intact after the initial explosion.

==========

containments/intro #35, from shall, 1153 chars, Tue Oct 14 12:13:11 1986
This is a comment to message 24.
---
TITLE: DEFINITION OF CONTAINMENT

It is clear that the Chernobyl 'containment' is designed on the same basis as any western LWR, i.e it is designed to withstand the pressures generated by a 200% guillotine break in the largest primary pipe. (The possible exception to this is that it is not clear whether the refuelling hall floor/reactor roof at Chernobyl is designed for this pressure load - does anyone have any infor- mation about this?) As pointed out by EnnoHicken, the main question concerns the definition (choice?) of DBA. Margaret Ryan (#28) asks what is the fastest, most powerful accident LWR containments could confront and whether this accident forms part of the design basis. This is a question which deserves attention but it must be put in the context of frequency of occurence. What Chernobyl might teach us is that the choice of DBA should be made after an analysis of the sort of loadings that migh occur over a wide range of fault conditions, rather than assuming that what is a reasonable DBA for one reactor is necessarily appropriate for another.

Steven Hall
Safety and Reliability Directorate, UKAEA.

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containments/intro #36, from shall, 1210 chars, Tue Oct 14 12:17:47 1986
This is a comment to message 32.
---
TITLE: STEAM EXPLOSIONS

Firstly, I would like to say that I agree with Marshall Berman's comments (#27).

My feeling is that it is perhaps premature to try and draw a distinction between the rapid steam generation event at Chernobyl and steam explosions as normally considered in LWR safety assessments. It is true that if the pressure pulse at Chernobyl were a result of solid fuel fragmentation and dispersal into water, then the event would differ from a steam explosion as usually understood because the fuel would not be molten, as has been said above. However, although there is experimental evidence of solid fuel fragmentation, the occurence of this in

management actions taken by government could increase rather than decrease the risk to the public.

I know nothing about the comparative efficacy of evacuation vs sheltering/respiratory protection. What I would suggest, however, is that the controversy embodied in the above concerns needs to be addressed and resolved BEFORE the next accident occurs. My paper and those of many others supported the accident management concept of being prepared, and knowing the appropriate actions to take. Disagreements as fundamental as evacuation vs sheltering should not be argued DURING an accident.

Mr. Sholly believes that insider sabotage causing core melt would be a trivially simplistic proposition for a sufficiently motivated saboteur. M. Goldman on the other hand (message 15) believes that the seizing of plant operations by hostile groups is a "fringe effect[s] of vanishingly low probability." How were such low probabilities calculated? Given that "success" is trivially simplistic, the occurrence would depend only on opportunity and motivation. I think that it would be quite difficult to quantify the probability of these two factors coming together. I would be very skeptical about any "human factors" analysis of this probability. I believe that it is in the utilities' best interest to assess the vulnerability of their plants, and to take appropriate measures, especially if such measures could significantly reduce accident consequences, regardless of "probabilities."

---

END OF ACCIDENT MANAGEMENT CONFERENCE

conditions. Accident management, however, is more like insurance. Having taken all the appropriate steps to prevent an accident, we must still must be prepared for its occurrence. The public will accept nuclear power when they realize that plants are operated with the safest possible procedures, and that accidents, if they occur, will pose hazards no greater, and generally much less than from other modern technologies.

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management/intro #13, from mberman, 3297 chars, Tue Oct 14 11:35:07 1986
This is a comment to message 10.
There are additional comments to message 10.
---
TITLE: Accident Management - Comment on Message 10 by M. Holmstrom
Marshall Berman

Mr. Holmstrom asked the question whether management of nuclear accidents required a strong central state. My answer is that nothing in society requires a strong central state. The rapid response of an authoritarian state sometimes generates awe among those who live in free societies. My advice is to reread the history books of this century and quickly abandon such foolish notions.

Dictatorships act quickly because they do not need to consult with or consider the wishes of the people or their representatives. However, the correctness of their actions is ultimately determined by the results. If the Soviets had (or have) taken actions which were wrong in retrospect, then the centralization of power was a negative factor. The evacuation of the people from Pripyat was delayed, and an even longer delay was involved in evacuating others in the 30 km zone. This may or may not have been the aappropriate action.

Holmstrom seems to be implying that civilians were ordered into contaminated areas by the Soviets at great personal risk. I'm not sure that this is so, but surely he can't be recommending this for Western countries. Many measures are available for decontamination that do not involve high risks: using robots, limiting exposure time, special equipment, etc.

Western democracies are quite capable of taking fast action when needed. This has often been demonstrated in natural disasters such as earthquakes or hurricanes. The key is being prepared. American response to hurricanes and floods is frequently fast and effective because all parties are prepared, and the public believes and acts on information it receives. The TMI accident was a case in which some of the worst aspects of accident management dominated. The public was confused by the chaos and contradictions which emanated from GPU, the NRC, the press and TV, and the government. The relationships among these groups has sometimes been adversarial in the past. Perhaps the Chernobyl accident can instill more of a spirit of cooperation.

Good accident management requires: 1) an accurate knowledge of the phenomena which may occur and their consequences; 2) an agreement by utility, government and regulatory agency to take specified actions based on defined accident indicators; and 3) a belief by the press and public that all three organizations are telling the truth, and attempting to protect the public. TMI was an example of the wrong way to manage an accident. If we work very hard to achieve the three above requirements, accident consequences will be minimized or eliminated. No strong central authority will ever be needed.

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management/intro #14, from abrachet, 492 chars, Tue Oct 14 11:39:06 1986
---
TITLE: General question for all participants and specially for electricity producers ones ,from aA BRACHET AND B NOC

Following chernobyl accident,have you initiated studies and what kind of studies, dealing with protected means and/or roboted means in view to work near the accidented reactor and for operator's protection of the same site reactors, specialy whit the objective to maintain the reactors in state of safe shut down?

A Brachet and B Noc
Electricite de France

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management/intro #15, from mgoldman, 2032 chars, Tue Oct 14 11:46:25 1986
This is a comment to message 12.
There are additional comments to message 12.
---
This is a comment on Paper #4

One of our major concerns in learning from the Chernobyl accident must be to maintain a sense of perspective and to resist the temptation to translate that accident to potential accidents in U.S. reactors without considering the results of extensive evaluations which have already been performed. In this context, I find it somewhat disturbing to note concerns about "dispersal of molten fuel and cladding outside containment" and "plant operations..seized by hostile groups", concerns which are focussed on fringe effects of vanishingly low probability. Certainly, we have more significant areas in which to focus our attention insofar as accident management is concerned, a number of which are identified in Jack Devine's paper.

One such area that strikes me as important, and unresolved, is that of effective protection action guides for these circumstances. As TMI demonstrated here, and much of the behavior in Europe following Chernobyl, so-called "protective actions" were implemented more on political than on radiological grounds, i.e., actions were

question: In an accident situation, in order to decide whether evacuation is necessary and when, how much reliance should be placed on computer modelling and how much on direct measurement of radiation levels in the field?

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management/intro #21, from mgoldman, 1562 chars, Wed Oct 15 21:22:20 1986
This is a comment to message 20.

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The decision as to the necessity for evacuation under accident circumstances is always going to depend upon the particular situation. It seems obvious that the best data would be that obtained from measuremeents in the field, although such measurements necessarily reflect only what has proceeded up to the point in time at which the measurements are made -- they cannot include future projections which are inherent in modelling, whether by computer or slide rule (if any still remember those antiques!). Computer modelling on the other hand provides a basis for projecting the future course of accident consequences, given the validity of the input data with respect to source terms and meteorological information, and a mathematically reasonable representation of the transport and dose relationships.

The degree of reliance to be placed on one or another of these tools would, it seems to me, be determined at the time by the nature of the release (i.e., monitored or not, puff or steady), the quality and quantity of monitoring data from the field, the nature of the meteorological condition at the time with respect to the model capabilities (i.e., calm winds with variable direction indicated, precipitation scattered in time and space, direction and speed shear with height, etc.), and the magnitude of measured or projected doses relative to promulgated action levels, among other factors.

Probably among the more certain of the conditions at the time of an accident is our inability to predict them in advance!

Mort Goldman, NUS Corp

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management/intro #22, from ani, 1444 chars, Thu Oct 16 13:10:09 1986
This is a comment to message 1.

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TITLE: IMPLICATIONS OF THE CHERNOBYL ACCIDENT AT MULTI-UNIT SITES

The accident at Chernobyl can provide a worst-case assessment of the RISK involved in the operation of multi-unit sites when one of the units suffers a very serious reactor accident with significant releases to the environment. ANI would like to initiate a discussion of that RISK by posing the following questions:

1. Did the accident at Chernobyl 4, because of radioactive releases, interfere with the operation of other reactors on the site?

2. What kind of difficulty and costs were encountered to decontaminate other reactors on-site, in order to provide safe unprotected access for plant personnel?

3. Was it costly to decontaminate on-site units which are under construction?

4. Was it necessary to decontaminate the interiors/ exteriors or both of on-site structures?

5. What provisions have been made to preclude the further on-site spread of radiation from Chernobyl 4 to other on-site reactors and structures?

6. Has on-site decontamination produced liquid and solid waste? How has this been disposed of?

7. What techniques were used to remove contamination from various structures?

8. Was a priority list for decontamination established? (For example control room first, equipment cubicals second, etc.)

9. Were there shared systems between Chernobyl 4 and other units on-site which led to damage or contamination to adjacent structures?

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management/intro #23, from mberman, 2515 chars, Mon Oct 20 13:30:39 1986

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TITLE: Comments on Accident Management

TITLE: COMMENTS ON EVACUATION AND CONTROL-ROOM TAKEOVER

Marshall Berman
Sandia National Laboratories

Steven Sholly (Containments Conference, message 44) says that much work has been done on when and why to initiate evacuation. However, he raises the concern that evacuations may be initiated prematurely, or inappropriately. John Matuszek of the New York Department of Health (Containments Conference, message 45) concurs that evacuation might be ordered more for political than health reasons. Both of these messages imply that sheltering and respiratory protection could be more effective responses.

In spite of the "work" already done on evacuation, the concerns of these two gentlemen appear to be that accident

standards. Those efforts need to be supplemented by an improvement of the national and international regimes of compensation for nuclear damage. Victims of nuclear accidents should be entitled to claim compensation directly against the operator of the nuclear installation wherever it is situated. Minimum standards of liability and equal treatment of all victims have to be guaranteed. The existing international instruments on nuclear liability, namely the Paris and the Vienna Conventions, provide for an adequate liability system, provided the maximum amounts of liability will be raised. An appeal to all States to accede either the Paris or the Vienna Convention and to negotiate about improving these instruments would be a desirable outcome of the IAEA Conference.

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finance/intro #2, from sysop, 9549 chars, Fri Sep 26 14:48:15 1986

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TITLE: Paper 2
Some Financial Implications of the
Chernobyl Accident for
U.S. Electric Utility Companies
By Paul C. Parshley, Vice President
Donaldson, Lufkin & Jenrette

While the financial implications of the Chernobyl accident for U.S. electric utility companies defy precise measurement, incremental costs attributable to that accident are likely to be substantial. This paper identifies several ways in which the Chernobyl accident has, or could increase the cost of commercial nuclear power in the U.S. The list below is intended to be illustrative rather than exhaustive. Also, I am not suggesting that Chernobyl is the sole cause of these higher costs, but it may have tipped the balance in the direction of tougher regulation. Finally, my projections of specific cost impacts are intended to depict orders of magnitude and not precise amounts.

Institutional Investor Reactions to Chernobyl

In early July, 1986, we polled our key institutional clients to get a sense of the extent to which Chernobyl had changed their perceptions of the riskiness of investments in electric companies which own nuclear plants. 35% of the respondees said nuclear utilities now seem "significantly more risky" than non-nuclear utilities; and, another 45% now regard nuclear utilities as "somewhat more risky." These investor perceptions suggest nuclear utilities now might have to pay a "Chernobyl risk premium" in order to access capital markets. If the Chernobyl risk premium were 10 basis points (which I suspect is a reasonable assumption), a nuclear utility coming to market with a new offering of $100 million in senior debt now would pay $1 million more than before the accident in order to attract institutional lenders. About $3 billion in nuclear utility debt has come to market since mid-June, 1986, which suggests an aggregate increase in cost of capital to date of about $30 million.

Emergency Planning Concerns

State and local opposition to participation in emergency planning has caused the owners' target in-service dates for several nuclear units to slip. The estimated cost increases for each additional month these units are not commercial are as follows:

| REACTOR | LICENSEE | MONTHLY COST |
|---|---|---|
| Shoreham | Long Island Lighting Co. | $30mm |
| Seabrook | Public Service of NH | $50mm |
| Perry 1 | Cleveland Electric Illum. | $60mm |

If commercial operation of each of these units is delayed by 6 months, the aggregate incremental costs due at least in part to Chernobyl would be about $850 million. Keep in mind that delays at these plants could be significantly longer than 6 months, and also that other reactors could be effected.

Reactor Containment Issues

For several years, operating reactors with General Electric Mark-1 containments have been the focus of NRC research because of their apparently high probability of containment failure during a large nuclear accident. Quite conceivably the Chernobyl accident will tend to push NRC staff to require licensees to take certain steps to upgrade the Mark 1's ability to contain some smaller accidents. The average retrofit cost per reactor likely will be on the order of $3 million for upgrades in the following areas: dry well spray installation; back-up water sources for spraying; containment venting, core debris control; and, training and procedures. The combined costs, tehrefore, for the 24 Mark-1 reactors would be about $75 million.

NCR Staff Regulation of Operating Reactors will be Tougher and More Costly

Several commercial reactors have been removed from service for undetermined periods of time. Davis-Besse, Rancho Seco, and Fort St. Vrain, were shutdown prior to Chernobyl. NRC's decision to allow these reactors to return to service could be delayed in part due to Chernobyl. While, once again, the impact of Chernobyl is intangible, each month of outage has the following approximate costs:

| REACTOR | LICENSEE | MONTHLY COST |
|---|---|---|
| Davis-Beese | Toledo Edison | $8mm |
| Rancho Seco | Sacramento Muni Util Dist | $8mm |
| Ft.St Vrain | Public Service of Colo. | $8mm |

# FINANCIAL IMPLICATIONS

finance/intro #1, from sysop, 5245 chars, Fri Sep 26 14:47:03 1986

TITLE: Paper 1

Financial and Liability Aspects
of the Chernobyl Accident
By Dr. Norbert Pelzer
University of Gottingen

1. The Chernobyl accident of April 26, 1986, proved the assessment to be true that nuclear incidents may cause widespread and long-range radioactive pollution, including transboundary effects. Damage to man and property was caused in the USSR. Various European countries suffered damage to the environment from the radioactive emissions. Agricultural products were contaminated, importation and exportation of fruits and vegetables were hampered. Seasonal farm workers lost their jobs. Tourism industry specialized on East European countries lost its clientele. In the Federal Republic of Germany, e.g., the damage amounted to approximately half a billion Doutsche Mark.

This situation raises the question whether and to what extent the USSR and the operator of the Chernobyl nuclear power plant are liable, under public interntional law and under civil liability law respectively, for damage caused by the accident.

2. The U.S.-Canadian Trail-Smolter Arbitral Tribunal (1941) stated that "no State has the right to use or to permit the use of its territory in such a manner as to create injury...in or to the territory of another or the properties or persons therein, when the case is of serious consequence and the injury is established by clear and convincing evidence." Today, this statement is generally accepted as a rule of international custom. Its breach entails State responsibility under public international law.

There is another international customary obligation relevant to Chernobyl: In emergencies involving the risk of transfrontier damage the State of origin is obliged to inform other States in time in order to enable them to take preventive measures. The USSR did not fulfill this international obligation to timely information and notification of the accident, thus hindering States from minimizing the extent of damage to their territories.

Result: There seems to be strong evidence that the USSR acted wrongfully by violating both international obligations, and therefore is liable under public international law to compensate damage in other States caused by the Chernobyl accident. Since the USSR has not accepted the obligatory jurisdiction of the International Court of Justice, negotiations on obtaining compensation will be necessary.

3. In Western European countries the Paris Convention on Third Party Liability in the Field of Nuclear Energy and the Brussels Supplementary Convention to the Paris Convention provide for a widely harmonized basis for compensation of nuclear damage. The Paris Convention establishes strict liability for the operator of a nuclear installation and grants to the victim of a nuclear incident direct claims under civil law against the operator, irrespective of which member State the installation is situated in. The Brussels Supplementary Convention provides for additional State compensation in case the damage exceeds the maximum liability amount of the operator pursuant to the Paris Convention.

While the Paris Convention covers the Western European States, including Greece and Turkey, the Vienna Convention on Civil Liability for Nuclear Damage provides for a liability regime on a worldwide range. This Convention, however, has only been accepted by 10 States, most of them developing countries.

The USSR -- like the USA, Canada, and Japan -- is neither a member of the Paris nor of the Vienna Convention. That means that a foreign victim has to sue under national law, which causes great practical and legal difficulty. Generally, the victim has the choice to raise a claim against the Soviet operator either at a court of his country or at the Soviet court competent for Chernobyl. According to the rules of Private International Law the judge will decide which national law is applicable: that of the victim or that of the operator. This procedure bears considerable uncertainty for the claimant. Moreover, it is not at all sure whether the Soviet court will admit him as a claimant, and, in case he sues at a court of his own country, whether the judgment will be enforced in the USSR.

4. At IAEA's Special General Conference from September 24 to 26, 1986, member States will discuss the possibilities of improving nuclear safety, e.g. by establishing internationally binding nuclear safety

# PROPOSED BWR
# SEVERE ACCIDENT
# CONTAINMENT REQUIREMENTS

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R. M. BERNERO

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DECEMBER 9, 1986

# NRC SEVERE ACCIDENT POLICY

- AUGUST 8, 1985
- PRESENT REACTORS ARE SAFE ENOUGH, BUT...
- SEARCH FOR OUTLIERS
- CONSIDER BALANCE OF PREVENTION AND MITIGATION
  - SPECIAL CONSIDERATION OF CONTAINMENT PERFORMANCE

# THE SEARCH FOR OUTLIERS

- SEARCH FOR SIGNIFICANT VULNERABILITY
  - FIND OUTLIERS NOT NECESSARILY QUANTIFY INLIERS

- INDIVIDUAL PLANT EXAMINATION
  - UNLESS ALREADY DONE
  - IDENTIFY OUTLIERS
  - BACKFIT AS APPROPRIATE

- WHERE TECHNICAL ISSUE GOES BEYOND CURRENT REGULATORY REQUIREMENTS
  - GENERIC RULEMAKING PREFERRED
  - ALSO USE BULLETINS, ORDERS OR GENERIC LETTERS

GDC 16:

CRITERION 16 - CONTAINMENT DESIGN. "--AN ESSENTIALLY LEAK-TIGHT BARRIER AGAINST THE UNCONTROLLED RELEASE OF RADIOACTIVITY TO THE ENVIRONMENT AND TO ASSURE THAT THE CONTAINMENT DESIGN CONDITIONS IMPORTANT TO SAFETY ARE NOT EXCEEDED FOR AS LONG AS POSTULATED ACCIDENT CONDITIONS REQUIRE."

GDC 50:

CRITERION 50 - CONTAINMENT DESIGN BASIS. "--AS REQUIRED BY SECTION 50.44, ENERGY FROM METAL-WATER AND OTHER CHEMICAL REACTIONS THAT MAY RESULT FROM DEGRADATION BUT NOT TOTAL FAILURE OF EMERGENCY CORE COOLING FUNCTIONING, (2) THE LIMITED EXPERIENCE AND EXPERIMENTAL DATA AVAILABLE FOR DEFINING ACCIDENT PHENOMENA AND CONTAINMENT RESPONSES, AND (3) THE CONSERVATISM OF THE CALCULATIONAL MODEL AND INPUT PARAMETERS."

DRAFT

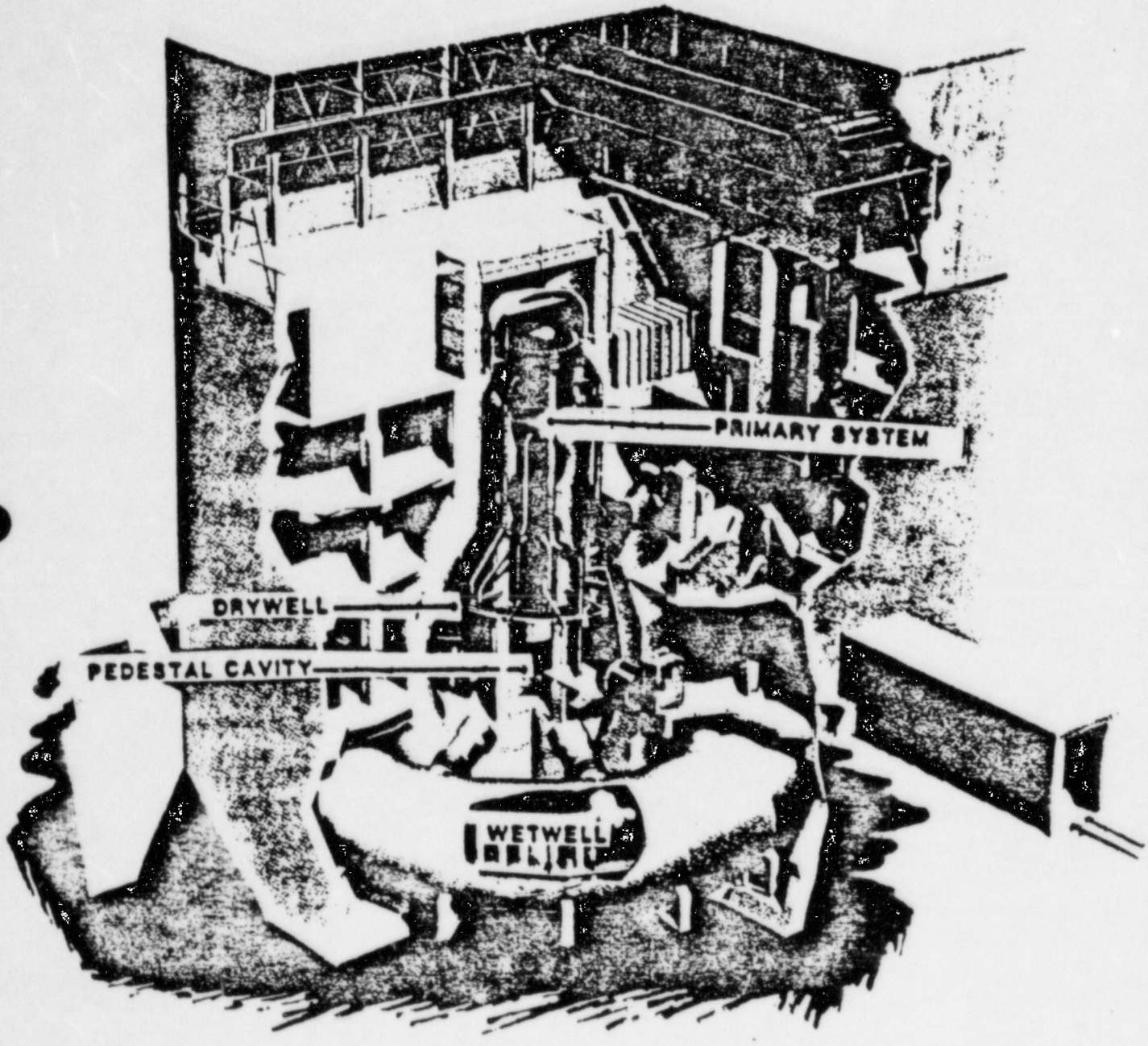
TABLE 1 - U.S. BWR PLANT-SPECIFIC PRA STUDIES

| PLANT | PROGRAM NAME | REPORT | REPORT YEAR | CORE/ CONTAINMENT | REACTOR POWER (MWT) | CORE-DAMAGE FREQUENCY PRA ESTIMATE | EVENTS CONSIDERED | MEDIAN, MEAN OR POINT ESTIMATE | CONTAINMENT CONDITIONAL FAILURE PROBABILITY |
|---|---|---|---|---|---|---|---|---|---|
| Peach Bottom | RSS | WASH-1400 | 1975 | BWR-4/MK I | 3293 | $3 \times 10^{-5}$ | Internal/ External | Median | Not evaluated |
| Peach Bottom | IDCOR | Tech Summary Task 21 | 1984 | BWR-4/MK I | 3293 | $4 \times 10^{-5}$ | Internal | Mean | 0.2 |
| Peach Bottom | IPE | IPE | 1986 | BWR-4/MK I | 3293 | $2 \times 10^{-5}$ | Internal | Mean | Not evaluated |
| Millstone | IREP | NUREG/CR 3085 | 1983 | BWR-3/MK I | 1727 | $3 \times 10^{-4}$ | Internal | Median | Not evaluated |
| Millstone | NUSCO | Millstone 1 PSS | 1986 | BWR-3/MK I | 1727 | $5 \times 10^{-4}$ | Internal | Mean | Not evaluated |
| Brown, Ferry | IREP | NUREG/CR 2801 | 1982 | BWR-4/MK I | 3293 | $2 \times 10^{-4}$ | Internal | Point Estimate | Not evaluated |
| Vermont Yankee | VYCSS | VYCSS | 1986 | BWR-4/MK I | 1593 | $3 \times 10^{-5}$ | Internal | Mean | 0.07 |
| Big Rock Point | Consumers | Big Rock Point PRA | 1981 | BWR-1/Dry | 158 | $1 \times 10^{-3}$ | Internal/ External | Mean | 0.25 |
| Big Rock Point | EG&G/BNL | EG&G-EA-5533 Rev. 1 | 1982 | BWR-1/Dry | 158 | $1 \times 10^{-3}$ | Internal/ External | Mean | 0.25 |
| Limerick | PEPCO | Limerick PRA | 1981 | BWR-4/MK II | 3293 | $7 \times 10^{-5}$ | Internal/ External | Mean | 1.0 |
| Limerick | BNL | NUREG/CR-3028 | 1983 | BWR-4/MK II | 3293 | $1 \times 10^{-4}$ | Internal/ External | Mean | 1.0 |
| Shoreham | LILCO | Shoreham PRA | 1983 | BWR-4/MK II | 2436 | $5 \times 10^{-5}$ | Internal | Point Estimate | Not evaluated |
| Shoreham | BNL | NUREG/CR-4050 | 1985 | BWR-4/MK II | 2436 | $1 \times 10^{-4}$ | Internal | Point Estimate | Not evaluated |
| Shoreham | IPE | Shoreham IPE | 1986 | BWR-4/MK II | 2436 | $8 \times 10^{-5}$ | Internal | Mean | Not evaluated |
| Susquehanna | IPE | IPE | 1986 | BWR-4/MK II | 3293 | $2 \times 10^{-7}$ | Internal | Mean | Not evaluated |
| Grand Gulf | RSSMAP | NUREG/CR-1659 | 1981 | BWR-6/MK III | 3833 | $4 \times 10^{-5}$ | Internal | Median | Not evaluated |
| Grand Gulf | IDCOR | Tech Summary Task 21 | 1984 | BWR-6/MK III | 3833 | $8 \times 10^{-6}$ | Internal | Mean | Not evaluated |
| GESSAR | GE | GESSAR II PRA | | BWR-6/MK III | 3579 | $4 \times 10^{-6}$ | Internal/ External | Mean | Not evaluated |

## KEY RESULTS FOR BWR CONTAINMENTS

- REACTOR SAFETY STUDY - PEACH BOTTOM 90% EARLY RELEASE
- IDCOR - PEACH BOTTOM 20% EARLY RELEASE
- VERMONT YANKEE - 7% EARLY RELEASE
- NUREG-1150

DRAFT




**TYPICAL MARK I CONTAINMENT DESIGN**

FIGURE 1

## BWR CONTAINMENT ISSUES - MARK I

- SMALL VOLUME
  - MORE RAPID OVERPRESSURE
  - ESPECIALLY VULNERABLE TO HYDROGEN BURN

- SMALL DRYWELL FLOOR
  - LOWER HEAD AREA CLOSE TO DRYWELL WALL
  - POTENTIAL FOR DIRECT DEBRIS ATTACK
  - DIRECT RADIATION AND CONVECTION HEATING

- LIMITED PASSIVE CAPABILITY BUT OPTIONS FOR ACTIVE RESPONSE

- 5-ELEMENT APPROACH
  - HYDROGEN CONTROL
  - SPRAY IN DRYWELL
  - PRESSURE RELIEF
  - DEBRIS CONTROL
  - PROCEDURES AND TRAINING

## CONTAINMENT IMPROVEMENT STRATEGY

- PREVENT HYDROGEN COMBUSTION BY INERTING

- REDUCE DRYWELL SPRAY FLOW RATE
  - PERMITS ALTERNATE SUPPLIES TO PRODUCE SPRAY
  - EXTENDS WATER SUPPLIES

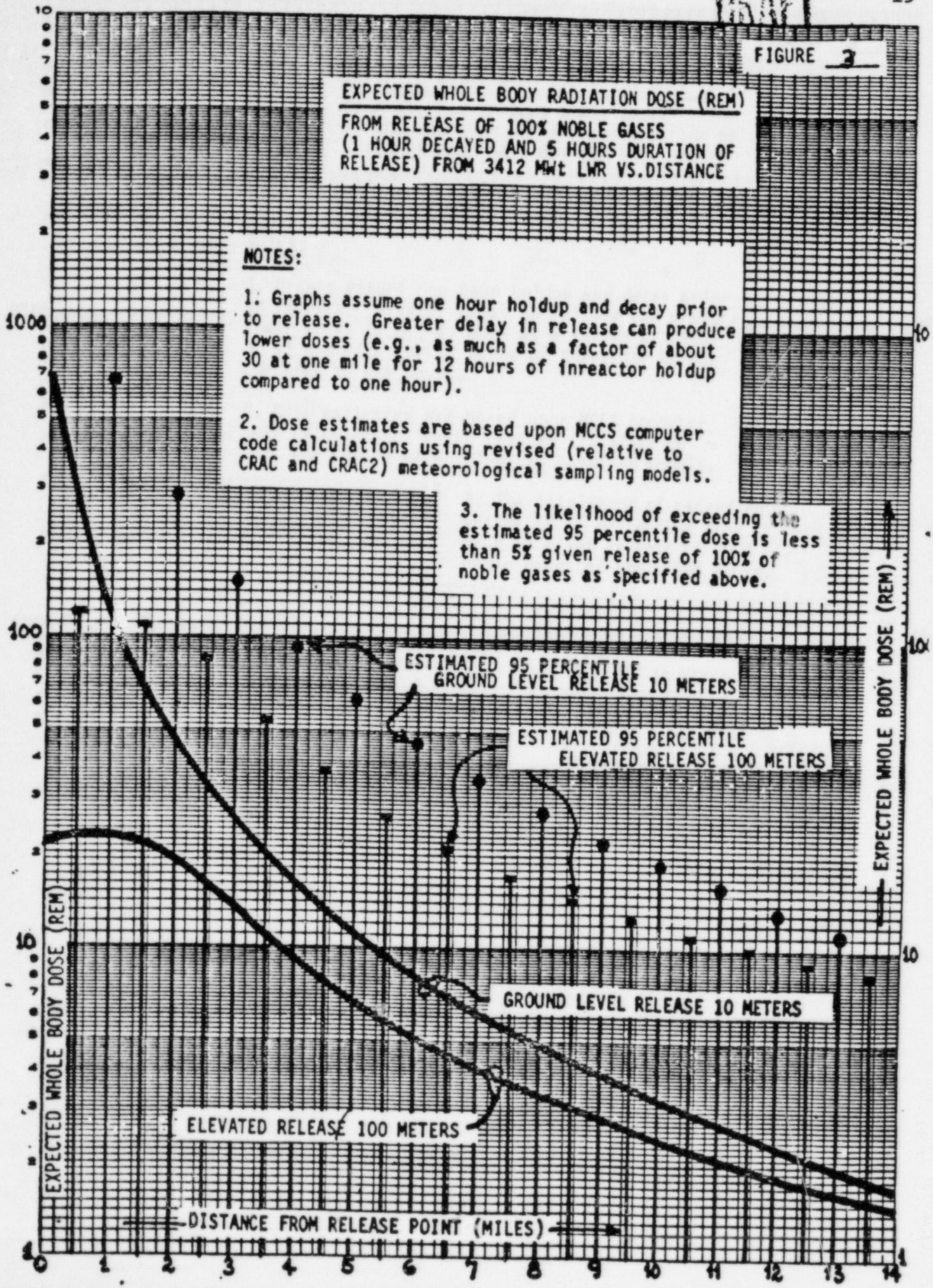
- PROVIDE RELIABLE BACKUP SUPPLIES FOR DRYWELL SPRAY
  - PROVIDES SHALLOW POOL OF WATER ON DRYWELL FLOOR
  - DIRECT SPRAY COOLING OF ANY CORE DEBRIS LEAVING LOWER HEAD AREA
  - SPRAY SCRUBBING OF DRYWELL VOLUME
  - DIRECT COOLING OF WALLS

- WETWELL PRESSURE RELIEF TO STACK
  - POOL SCRUBBING
  - ELEVATED RELEASE

- DEBRIS CONFINEMENT

- TRAINED OPERATORS

FIGURE 3




DIETZGEN CORPORATION
NO. 340R-L410 DIETZGEN GRAPH PAPER
SEMI-LOGARITHMIC
4 CYCLES X 10 DIVISIONS PER INCH

# CONDITIONS

## QUALITY AND DESIGN STANDARDS

SINCE THESE REQUIREMENTS ARE INTENDED TO BE AN OPTIMIZED USE OF EXISTING EQUIPMENT IT IS EXPECTED THAT ADDED EQUIPMENT, OF ITSELF, NEED NOT MEET THE QUALITY OR DESIGN STANDARDS OF SAFETY RELATED EQUIPMENT. NEVERTHELESS, MODIFICATIONS TO OR NEAR EQUIPMENT OR SYSTEMS WHICH ARE ALREADY SAFETY RELATED SHALL NOT COMPROMISE THE QUALITY OF SUCH EQUIPMENT OR SYSTEMS.

## IMPLEMENTATION

THE EQUIPMENT CHANGES REQUIRED HEREIN SHALL BE INSTALLED DURING THE FIRST REFUELING OUTAGE WHICH BEGINS NINE (9) MONTHS AFTER THE EFFECTIVE DATE OF THIS LETTER. THE PROCEDURES AND TRAINING REQUIRED SHALL BE IMPLEMENTED ON A SCHEDULE REVIEWED AND APPROVED BY THE NRC. GIVEN THE IMPLEMENTATION OF THE GENERIC IMPROVEMENTS OF MARK I CONTAINMENTS THERE IS NO NEED FOR AN INDIVIDUAL PLANT EVALUATION (IPE) FOR CONTAINMENT PERFORMANCE. THIS DOES NOT REMOVE THE NEED FOR AN IPE WHICH COVERS THE SYSTEM RELIABILITY OR CORE MELT FREQUENCY PORTION OF THE SEVERE ACCIDENT QUESTION.

## SEVERE ACCIDENT POLICY STATEMENT

- OPERATING NUCLEAR POWER PLANTS REQUIRE NO FURTHER REQULATORY ACTION TO DEAL WITH SEVERE ACCIDENT ISSUES UNLESS SIGNIFICANT NEW SAFETY INFORMATION ARISES TO QUESTION WHETHER THERE IS ADEQUATE ASSURANCE OF NO UNDUE RISK TO PUBLIC HEALTH AND SAFETY.

- IN THE LATTER EVENT, A CAREFUL ASSESSMENT SHALL BE MADE OF THE SEVERE ACCIDENT VULNERABILITY POSED BY THE ISSUE AND WHETHER THIS VULNERABILITY IS PLANT OR SITE SPECIFIC OR OF GENERIC IMPORTANCE.

- THE MOST COST-EFFECTIVE OPTIONS FOR REDUCING THIS VULNERABILITY SHALL BE IDENTIFIED AND A DECISION SHALL BE REACHED CONSISTENT WITH THE COST-EFFECTIVENESS CRITERIA OF THE COMMISSION'S BACKFIT POLICY AS TO WHICH OPTION OR SET OF OPTIONS (IF ANY) ARE JUSTIFIABLE AND REQUIRED TO BE IMPLEMENTED.

- IN THOSE INSTANCES WHERE THE TECHNICAL ISSUE GOES BEYOND CURRENT REGULATORY REQUIREMENTS, GENERIC RULEMAKING WILL BE THE PREFERRED SOLUTION. IN OTHER CASES, THE ISSUE SHOULD BE DISPOSED OF THROUGH THE CONVENTIONAL PRACTICE OF ISSUING BULLETINS AND ORDERS OR GENERIC LETTERS WHERE MODIFICATIONS ARE JUSTIFIED THROUGH BACKFIT POLICY, OR THROUGH PLANT-SPECIFIC DECISION-MAKING ALONG THE LINES OF THE INTEGRATED SAFETY ASSESSMENT PROGRAM (ISAP) CONCEPTION.

COMMISSION RESPONSE TO A HEARING QUESTION

JULY 16, 1986

QUESTION

IS A 90 PERCENT CHANCE OF FAILURE IN THE EVENT OF A CORE MELTDOWN AN ACCEPTABLE FAILURE RATE?

ANSWER

THE NRC HOLDS THE POSITION THAT THE LIKELIHOOD OF CORE MELT ACCIDENTS IN ANY PLANT SHOULD BE VERY LOW AND, IN ADDITION, THAT THERE SHOULD BE SUBSTANTIAL ASSURANCE THAT THE CONTAINMENT WILL MITIGATE THE CONSEQUENCES OF A CORE MELT SHOULD ONE OCCUR IN ORDER TO ENSURE LOW RISK TO THE PUBLIC. IT IS NOT MERELY A QUESTION OF HAVING LOW RISK BUT OF HAVING AS WELL THE DEFENSE-IN-DEPTH ASSURANCE OF COMBINED PROTECTION BY PREVENTION AND MITIGATION...

TABLE 3 COST-BENEFIT ANALYSIS

COST: $0.7-2.2M

| BENEFIT:(1) | FCM | CCFP BEFORE | CCFP AFTER | AVERTED LOSS/YR | AVERTED LOSS PRES. VALUE |
|---|---|---|---|---|---|
| BASE CALCULATION | $1\times10^{-4}$/yr | 0.5 | 0.05 | $\$4\times10^{5}$/yr | $3M/$12M |
| LOWER FCM | $1\times10^{-5}$/yr | 0.5 | 0.05 | $\$4\times10^{4}$/yr | $0.3M/$1.2M |
| LESS CHANGE IN CONTAINMENT | $1\times10^{-4}$/yr | 0.5 | 0.1 | $\$4\times10^{5}$/yr | $3M/$12M |
| BETTER CONTAINMENT TO START | $1\times10^{-4}$ | 0.2 | 0.05 | $\$2\times10^{5}$/yr | $2M/$6M |
| "OPTIMISTIC" CALCULATION | $1\times10^{-5}$ | 0.2 | 0.05 | $\$2\times10^{4}$/yr | $0.2M/$0.6M |
| "PESSIMISTIC" CALCULATION | $3\times10^{-4}$ | 0.9 | 0.1 | $\$2\times10^{5}$/yr | $16M/$60M |

(1) FCM = Frequency of Core Melt
CCFP = Conditional Containment Failure Probability
AVERTED LOSS PRESENT VALUE expressed as A/B where A is the averted loss per year times 8 (roughly equivalent to discount at 12%/yr rate) and B is the averted loss per year times 30 (no discount).

## PROPOSED ACTION

- DEC. 9 & 12, 1986 ACRS REVIEW
- DEC. 19, 1986 CRGR REVIEW
- JANUARY 1987 REVIEW OF ACRS AND CRGR REACTION WITH COMMISSION
- FEB. 1, 1987, PUBLISH PROPOSED GENERIC LETTER FOR COMMENT
- MAY 1987 ISSUE FINAL GENERIC LETTER

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SIMILAR LETTERS ON MARK II AND MARK III TO FOLLOW

BWR OWNERS' GROUP

## SEVERE ACCIDENT CONTAINMENT ISSUES

## NUMARC APPROVED SEVERE ACCIDENT CONTAINMENT ISSUES APPROACH

1. OBJECTIVE
   EVALUATE CONTAINMENT INTEGRITY. IF APPROPRIATE, ASSESS POTENTIAL IMPROVEMENTS TO MINIMIZE OFFSITE RELEASES FOR SEVERE ACCIDENT CONDITIONS (BEYOND DBA) WITHIN AN APPROPRIATE COST/BENEFIT GOAL.

2. IDENTIFY CHALLENGES TO CONTAINMENT *
   1.) H2 GENERATION
   2.) OVERPRESSURE
   3.) TEMPERATURE
   4.) CORE DEBRIS ATTACK
   5.) FISSION PRODUCT CONTROL
   6.) HUMAN ACTIONS
   7.) DIRECT CONTAINMENT HEATING

3. IDENTIFY INITIATORS TO EACH CHALLENGE FROM EXISTING ANALYSES
   PICK KEY EVENTS/INITIATORS (MOST SEVERE - LESS SEVERE) SEQUENCE THAT PRODUCES MOST SEVERE CHALLENGE

4. ASSESS PLANTS' ABILITIES TO MEET CHALLENGES

5. ASSESS PLANT VULNERABILITIES

6. PROPOSE ALTERNATIVES TO ADDRESS VULNERABILITIES

7. EVALUATE ALTERNATIVES

8. REACH DECISIONS

* OTHER ISSUES TO BE ADDED