UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

ADVISORY COMMITTEE ON REACTOR SAFETY SUBCOMMITTEE

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MEETING ON CLASS 9 ACCIDENTS

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Airport Park Hotel

600 Avenue of Champions

Inglewood, California

Wednesday, July 2, 1980

The meeting of the subcommittee was reconvened, pursuant to recess, at 8:30 a.m.

PRESENT:

W. KERR, Chairman

J. MARK, Member

C. SIESS, Member

D. OKRENT, Member

M. PLESSET, Member

P. G. SHEWMON, Member

H. ETHERINGTON, Member

G. QUITTSCHREIBER, Member

ACRS CONSULTANTS:

8007110435 T

J. LEE

R. SEALE

W. STRATTON

S. SIEGLE

FROM NRC:

D. BESSETTE

PROCEEDINGS

(8:30 a.m.)

MR. KERR: The meeting will come to order. This is a meeting of the Advisory Committee on Reactor Safeguards, Subcommittee on Class 9 accidents. My name is Kerr. I am subcommittee chairman. Other ACRS committee members present are Mark, Okrent, Plesset, Shewmon, Etherington, and Siess.

We have consultants, Lee, Seale, Stratton, and Siegle and ACRS, Bessette, also present.

This meeting will continue the subcommittee's examination of the role of Class 9 accidents in the licensing process. Specifically, we will also examine the question of possible design consideration and analysis of core mitigation features at the Zion and Indian Points Nuclear Plants.

The subcommittee will also continue its review of the FY 81 and FY 82 NRC research budgets, that part that is dedicated to severe accident phenomena and mitigation. In the process for at least some members of the subcommittee an effort is being made to define a Class 9 accident. We don't necessarily expect to be able to do that, but we shall perhaps continue to try.

Rules for participation in today's meeting have been announced as part of the notice of the meeting published in the Federal Register of June 17, 1980. A transcript of the meeting is being kept. It will be available as stated in the Federal Register notice. It is requested that each speaker identify himself and if possible make use of a microphone, otherwise speak, recognizing that your timeless words are being recorded. At least we are making an effort to do so, and you will be of considerable assistance to the reporter if you can get close to and remember that you are speaking at a microphone.

We have received more written comments or requests for time to make oral statements from members of the public. The designated federal employee for this meeting, Mr. Gary Quittschreiber, is on my left.

We will proceed with the meeting, and the first scheduled speaker is Mr. Thomas of NSAC.

Mr. Thomas.

DR. THOMAS: Thank you, Chairman Kerr and other members of the subcommittee, gentlemen and lady, I am Gary Thomas. I am from EA's Nuclear Safety Analysis Center, or NSAC for short.

This morning I am going to have a short delay in my talk.

(Pause.)

This morning I am going to summarize a report that was issued last March by NSAC. It is designated

NSAC-2. The title of it is Mitigation of Small Break LOCAs in Pressurized Water Reactor Systems.

I can proceed with some of the preliminaries. Hopefully, we will throw some more light on the subject later. I hope today to open your eyes a bit. I hope to widen your perspective on accident mitigation. The objective of the NSAC-2 report was specifically to provide a perspective on the ability to mitigate small break LOCAs in a pressurized water reactor system and as a result of that perspective provide assurance that the resulting threat to the containment, threat to failure of the containment can be virtually eliminated or can be eliminated through positive mitigating actions.

Now when I use the phrase "mitigation" it was used in the title of the report as slightly different than NRC's current use of the phrase "mitigation." I consider a mitigating feature or a mitigation of the accident any process throughout the portion or sequence of the accident which tends to reverse the direction or stop the progress of the accident.

I believe the NRC's current definition involves starting with a core melt situation and talking about mitigating features from that point on. In my talk I am talking about mitigation from the very start of the accident. Some people may phrase some of these actions as preventive actions.

(Pause.)

As I indicated, I hope to provide here perspective on the ability to mitigate a small break LOCA in a PWR system. It involves active mitigating responses. As a result I believe it is very realistic to assume a very, very high probability that containment can be protected using installed or potentially improvisable systems within a PWR plant.

The basic objectives in the report itself was to define primary observables that develop throughout a small break accident. Now observable is a physical major of the current state and trends of the system, the PWR system undergoing the accident. For example, temperatures, pressures, radiation monitor responses, neutron detector responses. These are all observables.

The report also reviews primary automatic and operator-initiated responses that are available for mitigating the small break LOCA. The report hopefully demonstrates the resiliency of a PWR system for mitigating the small break LOCA, and also demonstrates hopefully, potentially that the worth of the observables for realistically projecting emergency planning capability. By this I mean an integrated use of observables which provide you a tool to tell you where you are in the accident, where it is going and using extremely conservative projections when possibly can it threaten the public as a tool for realistically projecting emergency action.

The ultimate regulatory objective, I believe, should be focused on control of public risk due to environmental release of radioactive fission products. That is to say, ultimately the regulatory process should focus on protecting the public from containment failure.

As I said, my discussion today will try and show that there are many ways that the system can inherently be made to protect that containment.

DR. KERR: Say that again.

DR. THOMAS: Okay. I will get that right next time. The ultimate question of containment protection involves two primary major points: assurance that the small break condition can be identified and appropriately responded to. The observables definitely provide abundant evidence for this identification.

The second major point is assurance that some water and pumping source can be made available in the event that you lose all normal and installed backup systems.

Additionally, I have added in another item which should be added to the note: an assurance of eventual availability of a heat sink. And I will say eventual and emphasize that. It is not necessary to have a heat sink for the first several hours, several tens of hours potentially, in a PWR system, as long as eventually you can provide a heat sink for dumping the decayed energy out of the containment building.

I will try and display the ability to mitigate a small break LOCA. I have defined a operating space. The operating space involves four primary variables: the time available to react; the observables that define the system state and trends; the options that are available for countering accident progression, and options include both installed and improvised -- also should be added to your notes; I was making late additions last night -- and finally, the magnitude of the responses that are required of the available options.

A small break LOCA demands a very small response to completely contain or remove the decay heat from the core region. And when I say small I mean small with regards to normally installed coolant water injection systems, either engineering safety feature systems or normal systems such as makeup.

I tried to display this operating space in a graphical manner, representing it as a room, with the axis of time, system state observables, available and improvisable options for mitigating the accident. The two variables time, system state observables provide a historical development of the accident. Essentially they provide you an operating window that describe, through use of the time and the observables tell you where you are, again the system state and its trends.

The axis starts in this corner. As we move through this operating space on some directed path that is responding to automatic emergency safety feature systems, that is responding to operator-initiated actions, we will either progress towards an acceptable direction in this manner or under virtually non-- conditions we will proceed to a degree of the core condition and finally possibly to a core melt situation.

I have represented those boxes basically in the size that I feel really represent the situation. And that is something I want to emphasize very much.

For example, in Three Mile Island we spent over two hours moving around in this space before we hit this blue box. There was no core damage in Three Mile Island until about two hours approximately. We spent a lot of time wandering around in this space. Any time during then a proper continuous response emergency safety feature, such as HPI, or a proper, sufficient response of another cooling system would have taken us away from the core damage situation.

Additionally, if we enter this box, we can retreat from that box. We will not undo the damage but we can stop the progress of the damage and effectively prevent movement upwards towards that small corner.

I believe it is very realistic to state that also if we have reached the little red box, if we are in a fuel melting situation, under virtually any condition, we can pull out of that situation also. We can retreat from a melt situation.

Some more information on the use of the observables. The observables represent the deviations basically from normal or expected conditions during what would be normally a shutdown of the reactor or a scram.

There are abundant observable conditions indicating the state and the trend of the accident. The scope use of these observables in the time available provide a very rational basis for selecting of effective countermeasures, again both installed and improvised, as a very effective basis for conservatively projecting your potential public danger and emergency planning actions and in fact provide realistic set points to use for implementing emergency actions, deciding whether they are necessary to implement and implementing.

The next viewgraph schematically takes us through a small break LOCA. This is taken again from the NSAC-2 document. What we have are an increasing time scale in this direction, increasing damage in this direction, increasing probability of occurrence.

I have broken this into four categories, three basic times -- early, early meaning a few hours, possibly tens of hours; intermediate, being several hours to definitely tens of hours to a few days possibly; long-term, days and onward.

Four basic conditions: prevent core damage, terminate core damage, terminate core melting, maintain containment integrity, establish cooling a molten core as a debris bed.

If we move through this, we move through in a time sequence where initially in the early phase of a small break LOCA observables are showing immediately that we are in this situation, and integrated use of these observables, for example, through a safety panel, will tell us what the system state is, what the trends are, the system will respond with automatic responses, high pressure injection for example, engineering safety features, operator options are available based on the observables and we can move straight down to a cooled, mitigated condition.

For example, at Three Mile Island, the base PI had been left on. When it came on we would have moved directly to this condition with no core damage.

If we have no automatic responses or no operator responses or insufficient responses, we are going to move on to the next phase where now we have the chance to terminate core damage. We are still moving around in that operating ace and we are trying to direct it into a more reasonable position. The observables will tell us if we are successful. The trends of the observables tell us if we are successfully mitigating or if we are continuing into a worsening condition.

We go through the same process. We are developing more observables.

DR. PLESSET: May I ask you a question?

DR. THOMAS: Yes.

DR. PLESSET: -- -- you have in this first box, or the first distinguished set, if the operator has a small break LOCA? How would you know that it is a small break LOCA, not something else?

DR. THOMAS: That is --

DR. PLESSET: I mean if there is a way I would like to know.

DR. THOMAS: Okay, basically, if we use something, what we call -- we are trying to design what we call safety panels. A safety panel has five basic functions. It monitors criticality. It monitors core coolability or cooling. It monitors availability of heat sinks. It monitors, the fourth one, containment integrity, and monitors also release of fission products, particularly outside the containment.

Okay, an integrated use of a panel such as this provides you indications, for example, of a small break LOCA or something else that is disturbing the cooling of that core. The core very rapidly responds with observables, thermocouples for example, pressure, primary pressure lowering.

Maybe I should go on to the next viewgraph. That might help.

DR. PLESSET: Well, if you are going to explain this later that is fine. I just don't believe anything you have said so far tells the operator he really has a small break LOCA and not some other transient. It looks like a small break LOCA.

DR. THOMAS: Okay. My primary objective is initially to protect the core. Okay, if we are in a small break LOCA we are moving towards conditions which are telling us the core may become uncovered. That is represented by temperatures -- again core exit temperatures. It is represented by system pressure. It is represented by approach to saturation conditions in the primary system, again a temperature measure. It is measured by voiding in the system, which is seen by source range neutron detectors.

There are several incidents that are telling us we are approa g a condition that the core can be uncovered, whether it is a small break LOCA or something else. There are several indications that are preliminary to a core uncovering, preliminary to a core being damaged.

DR. KERR: Dr. Thomas, did you understand Dr. Plesset's question?

DR. THOMAS: Say again.

DR. KERR: Did you understand his question. From what you are saying I don't think you understood his questio.

DR. THOMAS: Okay. Excuse me, could you repeat it then?

DR. KERR: I think he is asking whether you have an unambiguous way of determining a small break LOCA is in progress.

DR. THOMAS: I cannot say whether it is unambiguous. I think there are unambiguous --

DR. KERR: I mean, we will accept an answer which is no, if that is an answer, that you don't have a way, that you hope to develop one. Maybe that is the answer.

DR. THOMAS: Okay, I think it can be developed.

DR. KERR: But you don't have one now?

DR. THOMAS: Well, specifically, no, I have not developed a procedure for it.

DR. KERR: Okay.

DR. PLESSET: Thank you. I have got the answer.

DR. THOMAS: I would like to supplement that though by saying I think it is very traceable by observables that we are entering a condition of threatened coolability of the core, and those observables develop very early in the incident whether it is a small break or some other incident. And those observables can be responded to.

This is again taken from NSAC-2, and this just presents some of the examples of primary observables that would be seen in an accident. For example, if we are in the initial phases of it, we will see the primary system pressure decreasing. We will see the pressurizer level changing in an abnormal manner. HPI actuates on a low pressure signal, 1600 PSI for example in Three Mile Island. That is definitely an observable that you are in trouble. It is also a very strong mitigating feature that should take you to a control full condition.

Your primary, your containment pressure, your containment temperature are increasing abnormally. These are all indications that you are in a small break LOCA.

I mentioned the source range neutron detector. That was a very, very sensitive instrument for telling you that you are approaching a core uncovering condition.

A direct measure of the system temperature versus pressure tells you the saturation temperature for the system and the current temperature of the system. Are we approaching saturation conditions?

DR. SHEWMON: Do you know if deviation and source range monitor signals is part of the operating procedures of reactors?

DR. THOMAS: I am sorry, say again, please.

DR. SHEWMON: Do you know whether the interpretation of deviations and source range neutron monitor signals is part of operating procedures?

DR. THOMAS: I don't believe it is now. I believe it can be effectively run into it.

DR. SHEWMON: Well, you are saying that a technically trained person the week after can look at these things and say something was going awry, but you don't think the reactor operators either have on their control panel or are trained to interpret this?

DR. THOMAS: I would say right now probably they are not trained to use the source neutron detector currently. A source neutron detector I believe will be an important signal involved in the safety panel development.

For example, at Three Mile Island, virtually the instant -- well.

DR. SHEWMON: I know, that is not my question though.

DR. THOMAS: Yes. Okay, it is not currently involved in the system. I believe it is a very important and a singularly accurate measure of system disruption.

DR. SIEGLE: At what power levels can a source range system be operative?

DR. THOMAS: The source range neutron detector at TMI was located outside the reactor. It responds basically due to power levels below, I would say about 30 percent. I am not sure. A normal shutdown of the source range detector is singularly projectable on simply a decay heat curve. You could project ahead of time. Once you scram the reactor you could project ahead of time what the curve for a source range neutron detector would look throughout all time. Once you receive a deviation from that, that is a strong signal that you may be in trouble.

DR. KERR: I don't think you understood the question. At least it doesn't seem to me you are responding to it.

DR. THOMAS: I am sorry, I thought I was.

DR. KERR: Maybe I misunderstood the question too, but --

DR. SIEGLE: I was concerned that source range instrumentation would essentially not be operable and would be swamped by other signals until the power level of the reactor was quite far down, and that may be much later than you want to have that information.

DR. THOMAS: No, at TMI, using the actual experience of TMI, the source range neutron detector provided a signal almost instantly from shutdown and it provided a continuous signal. It is still providing these unusual signals.

DR. KERR: What do you mean by shutdown? Do you mean effectively zero power?

DR. THOMAS: No. Well, the scram of the reactor.

DR. KERR: But this is Dr. Siegle's point, that until the reactor has been shut down -- I think, isn't it -- you won't see anything in the source --

DR. SIEGLE: That is one of my concerns, that you may have an operating condition where the source range instrumentation is simply blind.

DR. THOMAS: I see. That is a good point. I was assuming I was in the scram condition. I was assuming that we have scram based to upset conditions. Source range, I was using as one particular instrument because of its tremendous information content at Three Mile Island. I am assuming basically that since we are in a small break LOCA that some system parameter has scrammed the reactor.

DR. LEE: I am somewhat puzzled. The impression

I am gettin from your statement, that you an get somehow an ambiguous interpretation of source range detector signals.

DR. THOMAS: Did I say unambiguous?

DR. LEE: Uniquely or singularly accurate information or something like that. I thought you could interpret -- -- source range detector signals in a variety of ways.

DR. THOMAS: Okay, it can have a variety of sources. And if I would say actual final interpretation wanted, what this signal does represent is a long-term analysis effort. But the instant the signal starts to deviate from a very prescribed and projectable course we do know we are in an upset condition. It does not define what that upset is. The upset could be a failure of the source range monitor.

DR. LEE: It could be then something unrelated to a small break LOCA?

DR. THOMAS: Yes, it could be.

I am usin it as a -- okay, I am using it as an observable, and maybe I am focusing too much on the source range neutron detector. It does have a unique scram signal. A deviation from that signal is an observable that you are, that you have something of trouble that you should check out. There is some trouble in the system that you should check out. So from that point of view it is a very delicate source of telling you that there is an upset in the system. It doesn't tell you immediately what that upset is. It could be a recriticality, an increased signal.

It tells you I have to pay attention to that. From that point of view it is a useful observable.

DR. KERR: I think our concern, my concern stems from the fact that in order to mitigate a small break LOCA, unless it is done automatically, one needs to know that one has one.

I would have thought that you would have first told us how to unambiguously identify one. It seems to me that is fairly crucial in a mitigation process. Are we getting ahead of you? Are you going to tell us how to identify one rapidly and unambiguously?

DR. THOMAS: No. I am not going to get into a specific case of unambiguous identification. It is an integrated use of the observables. The observables that would show up in a small break LOCA again --

DR. KERR: Well, I would have an idea that if you were trying to tell a reactor operator what to do you might find what you are telling us confusing.

DR. THOMAS: I am not telling you procedures. Procedures do have to be developed. I am telling you that our first order of protection is core. We have to cool the core. There are very strong signals: primary system pressure, primary system temperature for example are two very prime ones -- that tell us that we could be approaching a condition of an uncooled core or a not fully cooled core. Those are unambiguous signals: primary system pressure and primary system temperature.

When you develop a superheat, for example, in a PWR system, a superheat of more than a couple degrees that could be involved from stored energy in the vessel components, you have an uncovered core. That is an unambiguous signal.

For example, if the core outlet thermocouple shows a superheat of 20 degrees Fahrenheit, you have a partially uncovered core. You must cool that core first of all, even if -- whether it is a small break LOCA or whatever the accident is. You must cool that core, which basically is bringing in more water.

DR. KERR: Mr. Thomas, I think what you have just said was well known to almost everybody before TMI, that if you have a temperature above saturation you would have a problem. And yet one had such signals available and they certainly were not unambiguously interpreted.

DR. THOMAS: That is true, and that is because I think primarily the engineering, or man-machine interface at Three Mile Island, and possibly as a general case for reactors prior to Three Mile Island. We do need to develop an integrated system that provides a physical, if I want to say it, a panel of maybe a two by three foot size that integrates these signals and tells them what the current state of the system is and the trends in that system.

DR. KERR: Your presentation takes up at the point at which one has identified a small break LOCA and goes on from there, but you are not going to be concerned with how one identifies it. Is that correct?

DR. THOMAS: No, but I am concerned that we do formulate a method of integrating the information so the operator can identify that. I believe it is an identifiable system or situation and that it can be identified if the operator has the proper information in front of him in an integrated way that permits that. I think it is most decidedly a situation that can be defined through proper man-machine interfaces.

DR. KERR: Continue.

DR. THOMAS: Okay, the next viewgraph tries to point out some of the differences between the NSAC-2 approach and the WASH-1400 methodology. I also managed to have three typographical errors in it on one page, and I attribute that to moving offices over the last three days over three miles. My secretary was almost in the moving van typing this in the process of moving.

The NSAC-2 report is not a contradiction of

WASH-1400 technology or methodology. I believe it is a preliminary, and I emphasize preliminary extension of the methodology. And I believe the extension falls into four basic areas: a reevaluation of some of the conservative assumptions that are involved in the WASH-1400 study.

The first major conservatism, I believe, in the WASH-1400 study is the assumption that reaching a high temperature in the core melt, or high temperature in the core, 2200 degrees Fahrenheit, represents a core melt situation.

In reality a core with 2200 degree Fahrenheit temperature can be recovered. You can retract from that condition through mitigating actions. In actuality, also temperatures, melt temperatures range from 3500 Fahrenheit up to over 5000 Fahrenheit for the components in the core. You have a lot of time and space, operating space, available to still return from a progressing damage to a control condition.

The core melt progress would tend to be a very noncoherent effect. Possibly it would be self-limiting, and this is an area that needs definitely more study.

I believe under almost any conditions it is reversable with cooling, and I think Three Mile Island provided a tremendous landmark and a gigantic experiment of this condition.

For Three Mile Island, if I go back to it, you see I got Three Mile Island basically was somewhere in this stage. It was a very late Condition 2 or a very early Condition 3. I believe there was some liquefication or melting in the core. And also we have I believe irrefutable, virtually irrefutable proof that once cooling was brought into the TMI-2 core it was almost monitonically decreasing temperature, an increasing coolability.

We started with a core that had at least some liquefication or melting. Water was brought in and it cooled, a very strong gigantic datapoint that I am extremely interested in investigating the details when we finally get into that reactor. It has tremendously important information for mitigation purposes. Try and trace down exactly how it did become coolable.

The second major conservatism in the WASH-1400 is that containment failure probability and environmental release are effectively a probability of one if core melt occurs.

Now the two conditions together provide a tremendous conservatism in the risk evaluation that is presented in the WASH-1400, and it provides a tremendous amount, again a use of the phrase, operating space, a tremendous amount of room to maneuver with available options or possibly improvisable options that will tend to move you into a cooled condition.

A third major conservatism is an accident mitigat on systems. If they fail on first call, are never again useable in the WASH-1400 type treatment. For example, if emergency diesels do not start the first time you try there is no option to try again. There is no option to fix a relay that may have failed that cause them not to start. If one of the coolant injection systems does not operate the first time because of valve misalignment there is no opportunity to correct that valve misalignment.

Basically, that falls in the, picks up the other conservatism. There is no consideration for positive use of time aspect in actual accident sequence. And the time aspect basically results in increasing time, accident time, provides opportunity for understanding the accident progress, for taking positive actions involving installed systems and for improvising new mitigating systems and actions if necessary.

I would like to try and emphasize -- I mentioned magnitude of response. I think it is a very salient point in a small break LOCA. You do not need a tremendous amount of response of systems to provide enough cooling to remove the decay heat from the core. And here I present a table. This is for a TMI type core, 180 megawatts electrical. In proportion these values according to the size of the plant.

A 2 percent full power, decay power, occurs at about 14 minutes or a quarter of an hour. At that time it will require about 340 gallons of subcooled water from the HPI coming in at 90 degrees Fahrenheit or 540 gallons a minute saturated water coming in, inlet to the core or inlet to the core region, to completely remove the decay heat.

By about two and a half hours you are down to 170 gallons a minute subcooled water coming in by the HPI. 170 gallons a minute would fill a deep bathtub to two foot deep in one minute. It is not a lot of water.

In less than a day you are down to less than 100 gallons required or slightly over 100 gallons for saturated water. In less than five days you are down to about 40 gallons of water required inlet to the core region to remove the decay heat.

These values are far less than many, many redundant emergency systems and even standard systems that can provide water to a PWR system, and they are also very small if you need to recycle with your -- recirculating the water using the secondary system. Again it is a very small capacity of that secondary system necessary to remove the decay heat.

I do want to emphasize also that a large break LOCA, if you have a large break LOCA and you initially respond to it, if the system automatically responds to a high pressure injection, low pressure injection if necessary, for the first ten to twenty minutes you remove the stored energy, you are now basically in a small break LOCA situation. That is, you are now in a condition where again this type of a response to the system would be sufficient to remove that decay heat.

The last viewgraph is some experimental data, and what we have plotted here are TMI core exit thermocouple time history, hours after the accident starting from three hours out to the end of one day, percent of thermocouples that were offscale. And as you recall at Three Mile Island there was a set point offscale at 700 degrees Fahrenheit or 370 degrees centigrade. No temperatures were measured above that. You look here at the fraction of thermocouples and percent that were above 700. At 174 minutes, just slightly before three hours, the 2B pump came on for less than 10 seconds. It dumped somewhere, 4000 to 8000 gallons of water into the pressure vessel. Virtually the instant that occurred we started on a cooling trend, a virtually continuously cooling trend. The core was in what I consider a coolable condition to maintain that coolability.a

About a half an hour later, at about three and a half hours the HPI came on and guaranteed that the core remain covered. That is a necessary requirement if you want to cool it, to make sure that it is covered.

But we continue cooling. This is the time when they were feed and bleed basically trying to burp the hydrogen out of the system.

Between four and five hours there were physical measurements of temperatures, basically around this area, physical measurements of temperatures as high as stainless steel melt, which is 2500 Fahrenheit. So there were definitely hot spots in the core, but it was in a progressively cooling condition. And I think that is a tremendously important experimental data point that has, I think, very wide implication on the ability of a very badly damaged core to be made coolable with injection of water.

That completes my presentation.

DR. KERR: Thank you, Mr. Thomas. Are there questions? Mr. Shewmon?

DR. SHEWMON: If you were advising the NRC on what research they should do to try to better determine how to mitigate or reduce the probability of a Class 9 accident, what would you suggest?

DR. THOMAS: I would suggest emphasis more on the first part of it. And the first part would be in the early phases ranging up to -- I would like to emphasize the ability to reverse a core, a damage process ranging in one, two, three; that is, where we are, early phases, virtually no damage. That is not too well unknown, you bring water into a system, that core is not damaged, you are not going to damage it.

In the area when we start damage, the damage I define as deformations, failure of the fuel rods, disruptions of normal coolant pads. Starting from that area up into this area, how can we assure that we can obtain a coolable geometry. I believe this is a very important area, because I believe, again if that operating space represents the room, we are moving around in this room --

DR. SLEWMON: Yes, I agree that you can cool if you haven't got trouble, but I don't, and I suspect everybody would agree that there is indeed conservatism in WASH-1400 in this particular part of the process where they had 2200 F. to a melted core. But the reason they do that, as I understand it, is because they have gone through sequences which have convinced them that they didn't have enough coolant to keep it from getting to 2200 F. and so they won't be able to do much thereafter.

And the only thing that you have talked about, I remember, in this vein so far is gee, if it didn't work once it won't ever work again, and we should look at that a little bit harder.

I guess what I am bothering you to do is to put it more in the context of the WASH-1400 scenario and how we got into this hole in the first place.

DR. THOMAS: WASH-1400, I think rightfully at the time, took very conservative assumptions on core damage and the ability to remove -- not remove, but prevent additional core damage, to mitigate the situation. I think at the time it was a useful document from that point of view. I think now, particularly in light of Three Mile Island, the lack of understanding before Three Mile Island and improved understanding since then show us that in fact we can move around a lot in that operating space beyond 2200 degrees Fahrenheit as a set point and still come back to a coolable condition.

DR. SHEWMON: That is a defensible credo but it still doesn't answer my question.

DR. THOMAS: I am missing everybody's question this morning. Sorry about that.

DR. SHEWMON: The question was what do we urge the staff to do for research so that they can feel better about the probability of mitigating events of this sort.

DR. THOMAS: Okay, my interpretation of that question is that I think we need to study the ability to cool a partially damaged core, and the partially damaged goes up to early melting as an area of focus. It is an area that has not been studied to any great detail, and the current plans of NRC I believe do not have that area covered very well.

DR. SHEWMON: Thank you.

DR. KERR: Mr. Plesset.

DR. PLESSET: Well, I just wanted to indicate a little concern in the kind of thinking that you have put into this, which seems to me to be pretty much limited along the line of the TMI-2 accident. It seems to me that that is not the way to get us into a more comfortable situation. You think entirely in terms of a small break LOCA and how it proceeded at TMI-2, and that bothers me a little bit, particularly since we can't tell the operator to recognize a small break LOCA when he has one, or maybe he doesn't have one and thinks he does.

DR. THOMAS: Okay, the small break LOCA is a relative high probability accident, and that is one reason for focusing on the small break LOCA. I will reiterate a point that the primary objective initially in an accident is to cool the core. And there are a progression of observables that tell you that you are heading towards an uncooled condition and they can be reversed with operator action or automatic engineering safety feature reaction.

Regardless of what the accident is, whether a small break or any other accident, if we are heading towards an uncooled condition it is a reversable condition, but you have to understand you are heading towards that uncooled condition and it does require an integrated use of the observables to define the current system state and its trends.

DR. SHEWMON: Well, I just might add in the past eight or nine months there have been four transients which look like a small break LOCA and there was no small break LOCA involved. Pressure fell, the pressurizer empty, and the operator proceeded as if he had a small break LOCA, which he didn't.

DR. THOMAS: No, but he certainly cooled the core.

DR. SHEWMON: Yes, that is true, and that is why I think that you shouldn't just think entirely in these terms.

DR. THOMAS: No, I am using a small break LOCA as a context. I do not want to limit it to a small break LOCA as far as the philosophy. But I was using it as a context in this presentation.

DR. KERR: Mr. Okrent?

DR. OKRENT: I am developing a strong interest in getting what I call quality assurance in probabilistic analysis, and in a sense this is a semiprobabilistic analysis, since on page 31 there is a statement that, quote, a preliminary evaluation by NSAC of the ESF and primary makeup, letdown systems and equipment using methods and data comparable to those used in WASH-1400 indicate that the installed hardware properly deployed by the operator is capable of reducing the probability of core melt and subsequent containment building failure resulting from a small break LOCA by a factor of 10 to a 1000.

DR. THOMAS: Correct.

DR. OKRENT: And later you say, therefore judged very improbable that the accident will proceed through the full progression without recognition of the increased established core cooling.

Okay, getting back to my first statement about quality assurance, it seems to me it is time for everybody involved -- that means the nuclear industry -- I am not sure whether that is the nuclear industry or not. When it was formed I originally hoped it was not, but I have decided it probably is. The NRC also. And in fact I will include intervenors or members of the public or so forth in the same comment. It seems to me there is a need for quality assurance, and to me, I will define that term in the followwing too abbreviated way, that after you have done a study you yourself critique it and examine where there may be things you haven't said that are relevant to, let's say the other side of the picture, that you have clearly stated the assumptions you have made and the things you hope will apply or whatever it is.

When I first read this document, I didn't have a feeling that it represented what I would call a document that had received that kind of quality assurance. In other words, so that someone picking it up, sophisticated or naive, could see what were the assumptions where there were things that hadn't been included in this that might be important and so forth. I suspect that while the statement is probably one that you could argue in a narrow sense can be justified, you could show, taking a narrow view, that if certain things applied you could include things by a factor of 10, I question the factor of a 1000 unless you are really assuming a very high probability initially.

But I equally well suspect that I could quickly make a short list of ten things that could defeat this improvement because they didn't fall into the pattern. And the sophisticated person might be able to read it and look at this and say, gee, well, you know he has omitted the possibility of a small leak affecting equipment that you really need to run by the environmental qualification or whatever, and not being right or a variety of things.

The less sophisticated person may take this at face value and be deluded, and if we are going to get to some kind of quantitative approach in this area I think it is really past due that each person trying to make a contribution in this area really do his own quality assurance on his study. I must say I didn't feel myself that this study in fact too would come anywhere close to meeting minimum standards in that regard.

I don't know whether you think it does or such a step is even appropriate but I think it is time.

DR. THOMAS: Okay, the NSAC-2 document is most decidedly a preliminary or a -- I almost feel it is somewhat a philosophical document because it is trying to raise questions. It is trying to raise new questions in people's minds about methods of mitigating accidents, about methods of statistically treating the risk assessment, and it is a preliminary document. It is not fully technically defendable.

It did receive quite extensive peer group review, both in the industry and particularly in NSAC. I would appreciate your detailed comments because I would like to improve it if possible. So if possible, I would like your specific comments on where you feel the assumptions were not made or were inadequately defined, because it would be very helpful.

DR. OKRENT: Well, it is conceivable I could provide comments, but I think you are missing in a sense the point of my comment. I think the author or authors of the document should begin feeling a deep responsibility, and in fact it should, in my opinion, become a necessity to clearly write out what the assumptions and clearly point out the things that might in fact go the other way and then possibly still conclude if they so dare after stating all of this that there is a trend in the direction that they hope.

But I think to only present part of the picture is no longer acceptable.

DR. THOMAS: Okay, point well taken. The probability analysis you did mention there, the 10 to a 1000, is very preliminary. We do go, I think, into basically a new field that is definitely an extension of WASH-1400, beyond WASH-1400, in trying to involve a time aspect, a positive --

DR. KERR: Well, I think you do get Dr. Okrent's message, don't you?

DR. THOMAS: Yes.

DR. KERR: Okay. Yes, sir?

MR. SUDMAN: I am Bill Sudman from NSAC. Dr. Okrent, I would just like to heartily agree with what you said. I had some hand in helping Gary investigate the probability aspect of this. We realized at the time we did not have a rigorous case for supporting a definitive probability argument, and in our desire to publish in a timely fashion we decided not to wait for a more thorough probabilistic analysis.

The reason why there are no more definitive claims than the ones that are in there is because we didn't have that case pinned down at the time, and I think you are absolutely right, that future work should try and pin it down very carefully and explain exactly the basis such that it becomes apparent to the reader what the case is being made.

DR. KERR: Mr. Shewmon?

DR. SHEWMON: I would like to make one other comment to the speaker and I guess to others, and that is that it seems to me that the staff in getting out WASH-1400 gave up a lot of lines of defense in taking the 2400 degree F. core equal to a core melt. And I hope that we can get some of them; that is, the staff and their research program, to develop some of these options and learn a little bit more about them so we don't give up those lines of defense, and I think in that regard this has been a useful exercise. Whether it has the adequate quality to be accepted by all concerned I think is not as essential as pointing out some additional lines of defense.

DR. KERR: Mr. Thomas, it seems to me we have seen in a number of reports recently comments indicating that in retrospect, at least as to Three Mile Island, the equipment performed very reliably, but that the operator performance, maybe because of training and other things, left something to be desired.

It is not clear to me whether in your

consideration of mitigation you are taking such comments into account, whether you agree or disagree with them. I would be interested in whether you think one should move more in a direction of less dependence on operators or more operator training or any comment you want to take on at least what I perceive to be a number of comments concerning the relative importance of operators and equipment.

DR. THOMAS: I think if I was able to manage the philosophy of the approach of an accident it would definitely be more operator training, a definite improvement in man-machine interface in allowing him to understand the current trends and also probably a greater or a response to the current state rather than, if I can use a quick story by Dr. Pigford from the -- Commission, he said the operators at Three Mile Island couldn't use a small break LOCA procedure because when they looked it up the first step in it said assume a loss of onsite and offsite power. And they didn't have that, so they didn't have a small break LOCA, and they had no procedure to follow.

This is a situation -- use of the observables can tell you where you are and tell you which direction you are going and tell you if mitigating responses are improving the situation. And I think a more real-time use of this information, involving operator training and integrated use of the safety systems would provide a much safer approach.

DR. KERR: Thank you.

DR. OKRENT: Just for the record, I doubt that it said a loss of onsite and offsite power.

DR. THOMAS: It may have been a loss of onsite power. I am paraphrasing what I remember his stating at Knoxville.

DR. OKRENT: All right.

DR. KERR: Mr. Lee.

DR. LEE: Could you comment on the observables --

DR. KERR: Excuse me, let me explain. The microphone really goes to the recorder. The other microphone is connected to the speakers.

DR. LEE: Could you comment on the observables available to the operator of a plant, especially in case they have not been able to arrest the accident in the initial case of the accident, and you might have some kind of degraded core, and especially in light of the type of problems we have experienced with the instrumentations at Crystal River?

DR. THOMAS: I do not think I would like to comment in detail on that, particularly at Crystal River. I have not been that close to the analysis of Crystal River. I would recommend reviewing -- I tried to present that in detail in the report -- a development of increasing, as you increase damage you increase existing observables, you bring in new observables, for example, production of hydrogen when oxidation occurs, that is a new observable, that tell you where you are, again defining state and trends

Rather than get in detail I would recommend, could we talk about this later, using NSAC-2 as a basis. I do have a few copies of NSAC-2 left, about four of these, and anyone interested in the document they could give me their business card.

I had better sign off.

DR. KERR: Thank you, Mr. Thomas.

I next have Mr. -- I am sorry, Mr. Siegle?

DR. SIEGLE: I would just like to make a couple of comments. It seems to me that Mr. Thomas has maintained that evidence is available that the small break LOCA exists. I think Dr. Plesset suggested that that evidence was at best circumstantial and that the interpretation of this circumstantial evidence indicates an additional input of plant design parameters that are plant specific and that we need a presentation then of the results of that evaluation in a coherent format that is useable by the operator and the STA if we have one.

And we also need to know whether or not that interpretation is unambiguous. It seems to me an appropriate recommendation at a risk of making one at 9:30 in the morning is that the research needs do include the addressing of the interpretive needs to assure the identification of whatever accident sequence is in progress and also indicate the needed interventions and also the extent to which a given intervention is relevant to various accident sequences, Crystal River and TMI, whatever.

And as the last comment I would make, it seems to me that the kind of thing we are looking for is the fact that even today people suggest that the availability of hydrogen as the result of oxidation was something that wasn't predictable. I would maintain that that was imminently predictable.

DR. KERR: Thank you, Mr. Siegle.

Mr. Cybulskis, we were going to get to you and I think we are there. And tell me if I am pronouncing your name correctly.

DR. CYBULSKIS: Cybulkskis.

DR. KERR: Cybulskis?

DR. CYBULSKIS: Yes, sir.

DR. KERR: I did leave off the "s," didn't I? Thank you.

DR. CYBULSKIS: That is a cardinal --

(Laughter.)

Good morning. I am Peter Cybulskis. I am with Battelle's Columbus Laboratories, and this morning I am going to talk to you very briefly.

Those of you that haven't heard me before, MARCH stands for Meltdown Accident Response Characteristics. In the first slide, very quickly and perhaps very basically, what I would like to review is some of the key phenomena that take place given a core meltdown accident, and we will go on to the MARCH code from there.

There is nothing particularly sacred about the nomenclature. It is a way we at Battelle like to think of a ---- accident. We have a meltdown for a thermal hydraulics box, which is just basically the core meltdown process -- -- associated with a meltdown thermal -- it is core melt, so overheat, and you have a fission product release factor which can have a feedback -- -- on the meltdown process. It can have, get to the terminal stages of a core meltdown, you have a potential for steam explosions. And of course if you are, particularly if you are in the degraded core cooling situation you are interested in what is happening in the containment in terms of pressure, temperature response, so that you can predict whether the containment will or will not fail or what are the relative probabilities.

Some of the features that have a containment response are things like hydrogen combustion; i.e., if the pressure gets too high the containment will fail. I alluded to the steam explosions earlier. That happens to be a mechanism thata then directly leads to containment failure, though nowadays it is believed to be a very low probability.

In the fission product release area of course you are interested in the fission product transport processes; namely, how are the fission products -- -- from the containment, how do they -- -- out, what happens to them during the course of the accident.

Of course the bottom line to all this exercise is the fission products released to the environment. If the fission products aren't released to the environment, presumably the accident is relatively benign. It may be economic chaos, from a public viewpoint, but there should be no great problem.

Let me just make a comment. The MARCH code does not treat sufficiently the product transport process. Gets to that later.

Now just thinking about those meltdown processes, let me go on to this slide, which is the amplification of the previous one, and in this we have tried to illustrate how the MARCH code addresses some of the aspects associated with core meltdown and which aspects it doesn't address.

In the meltdown thermal hydraulics area I have tried to indicate some of the key things that are included in the MARCH code or the parts of the problem that MARCH reached. We have a primary system trying to, basically it is a simple blowdown model for fission transients and small breaks.

We have the core meltdown analysis, which is a takeoff of the BOIL code that was developed for the reactor safety study. That is a meltthrough model predicting how the core chews through the bottom end of the vessel --

DR. SHEWMON: Mr. Cybulskis, pardon me for a minute.

DR. CYBULSKIS: Yes.

DR. SHEWMON: The last talk was only small break LOCAs. This talk is only large break LOCAs, is that right?

DR. CYBULSKIS: No, MARCH treats all accidents, so large breaks --

DR. SHEWMON: Okay, well we have talked core melt. Could you tell me a little bit about boil? Is this something that has some supplements to it, or is there any water around during the boil except from anything coming in or what?

DR. CYBULSKIS: Let me back up for a moment. The primary system transient, prime-P model, basically keeps track of the water inventory in the primary system for transients and small LOCAs. For large LOCAs it will typically take the blowdown results of other codes and use them as a starting point.

For small LOCAs and transients it will start out with a full system and the prime-P -- -- and keeps track of the water inventory. Leakage as well as makeup, if there is any.

When we get to the point where the core becomes uncovered, we transfer to the BOIL code, which basically then looks at the -- the assumption is made that as long as the core is covered it is well-cooled. Once you start uncovering the core, then the boil -- -- routines take over and parallel the uncovering of the core as well as the heatup of the fuel route.

Then if you reach melting point, a specified melting point in the BOIL code, then you get into the core meltdown problem.

DR. SHEWMON: And the melting point is 2200 F. or 3500 F.?

DR. CYBULSKIS: The melting point is an input option at the option of the user. In the typical calculations we use -- or shall I say typical, or our favorite number currently is 4,130 degrees.

DR. SHEWMON: Fine, thank you.

DR. CYBULSKIS: Something less than the melting point of the O-2. If you are using the code you can input whatever you like, however.

There are three meltdown models in the BOIL code, and that is a key aspect here. None of the meltdown models are mechanistic in the sense that we do not calculate through the flow of the molten fuel down the fuel rod or stress calculations in the fuel rod, and what have you. We do keep track of the metal-water reactions, the oxidation of the clad, the energy input, et cetera.

The slumping of the core is modeled in each of the three models in what we believe is a bonding type of approach. One of the meltdown models maximizes the downward progression of the core melting, but still assumes that the molten core is retained in the core region up to some trigger threshold when the core falls into the bottom HEAD.

The second of the meltdown models maximizes, if you will, the upper movement of the core, or if you want to look at it another way, the slumping of the upper portions of the core into the molten region, it still retains the tail or molten region of the core in its original area up to some trigger level when the core falls coherently in the bottom HEAD.

The third of the meltdowm models assumes that as soon as a mode is molten it falls into the bottom HEAD of the reactor vessel.

When the core gets down into the bottom HEAD of the reactor vessel, it boils out any residual water there, attacks the HEAD, falls into the reactor cavity. In the reactor cavity we have the HOTDROP option, which caused considerable controversy recently. It basically assumed that the debris fragments and interacts with the water in the cavity and can generate a high steam pressure.

After that it will go on to the core concrete interaction model, and we have adapted the Sandia developed INTER code for our purposes.

Throughout the processes we calculate fission product loss and redistribution of the core melt, and we have the meltdown calculations continuously coupled to the containment pressure and temperature.

The steam or gasses put out by the meltdown enter into the containment calculations, taking into account sprays, coolers, heat sinks, what have you. If we are interested in a hydrogen burning case, they can do that. That is a user-selected option. Again the code keeps track of the flammability of the mixture, and if you want the hydrogen to burn it will burn. It will not burn unless it is flammable though.

Going on to the containment failure mode, the code does not calculate when the containment fails. It requires an external calculation to specify failure pressure. If in any given calculation you reach that failure pressure, the code will open the containment up with a given hole size orifice coefficient and release the contents to the atmosphere.

A couple of words about steam explosions. There is no explicit modeling in MARCH of steam explosions except for the effects associated with steam explosions, again like steam production, vapor production, what have you, maintained as an energy balance.

DR. KERR: Mr. Cybulskis?

DR. CYBULSKIS: Yes, sir.

DR. KERR: You spoke of handling the combustion hydrogen if you decide you are going to call on that.

DR. CYBULSKIS: Yes, sir.

DR. KERR: That it won't burn unless it is flammable. That is right. Do you also discuss hydrogen generations in this code?

DR. CYBULSKIS: Yes, within certain limits. In the core meltdown area we take into account the reaction of the zirconium cladding with the water, and there is a fairly elaborate procedure. You have your choice of reaction rate laws. We calculate diffusion in the gaseous phase and in the solid state and pick the lowest of the two. So we do keep track of the reaction of the zirconium with the cladding.

During the initial portions of the core through the BOIL and the HEAD models we do not take into account any reaction on steel with water that may be available.

DR. KERR: Speaking to the zirconium, you have the reaction rates, and if the temperature gets high enough you can burn hydrogen at a great rate, which implies the presence of a considerable amount of water and in fact about ten times as much as is burned, because the efficiency of burning the steam is itself not 100 percent. You would have the cooling effect of that water in the coolant?

DR. CYBULSKIS: Yes, we do have the cooling effect of the steam. With regard to the reaction rate as well as the extent of reaction, one would typically see in fact that the extent of reaction or the rate of reaction is controlled by the availability of steam. It is not the reaction --

DR. KERR: Okay, it doesn't assume that the water appears magically?

DR. CYBULSKIS: No, it is all continuously coupled. The flow of steam through the core in fact is tracked undebatably, and that more often than not controls the extent of reaction.

DR. KERR: Well, I have seen some assumptions where the cooling of the water was not allowed for or its availability was magical. That would not have been a MARCH code calculation then?

DR. CYBULSKIS: That would not have been. If it was, it wasn't used correctly.

DR. SHEWMON: Carson, let me proceed that. The water you are talking about is steam from what is boiling inside the pressure vessel or are we talking about core spray at this period -- containment spray at this point?

DR. MARK: It is water which would have to be provided in order to give the substance of a hydrogen-metal reaction.

DR. SHEWMON: Okay, so that is inside the pressure vessel?

DR. MARK: Inside the pressure vessel, inside the core channels. And you have all seen this calculation of hydrogen generation in which the water was just provided out of thin air to burn as fast as it could burn.

DR. SHEWMON: Okay, but the hydrogen will burn in the containment?

DR. MARK: No, that is not hydrogen burning. It is hydrogen generation.

DR. SHEWMON: Okay, but he was talking -- all right, let me ask a different question then. Is there any option to discuss the hydrogen combustion in the containment with the core sprays on and off -- sorry, the containment sprays on and off?

DR. CYBULSKIS: Yes, certainly. Whether the containment sprays are on and off, that does not really affect that option, as a matter of fact, which is typically seen. If the containment sprays are on, the atmosphere will tend to be more flammable than with them off, if more flammable is the proper expression.

DR. SHEWMON: Sorry. You are saying it is easier to burn hydrogen in the presence of a steam droplet or water droplets than it is without?

DR. CYBULSKIS: Steam can act as a suppressant. Given enough steam in the containment atmosphere the steam acts as a suppressant to hydrogen burning. You move out of the flammability limit. With the containment sprays on, if they are in fact cooled, they condense the steam and you are basically down to an air-hydrogen mixture with a small partial pressure of steam.

DR. SHEWMON: No, you are down to an air-hydrogen mixture with a suspension of water droplets, which will tend to quench any flame.

DR. CYBULSKIS: In these particular calculations, if there is a quenching effect due to the water in terms of eliminating flammability, that is not taken into account. The heat transfer effect of the water droplets, once the burning takes place, is taken into account.

DR. SHEWMON: I am not sure what you just said.1

DR. CYBULSKIS: What I said is that given any --

DR. SHEWMON: If I have a suspension of water droplets and the burning front comes through that will tend to evaporate the droplets and slow down the burn, as I understand it, or cool the burn.

DR. CYBULSKIS: Well, it will cool the burn. I don't know whether it will slow it down. As a matter of fact, recently I heard a discussion of water droplets speeded up the propagation of a burn front. But the cooling effect is there.

DR. SHEWMON: Is in the code?

DR. CYBULSKIS: Yes.

Let me again allude to the fission product transport which we handle with the CORRAL code. MARCH does not calculate the fission product transport problem. MARCH provides the essential input that is needed for that problem, including determining whether there are released emissions in the containment, and defines the time dependent leakage rate out of the containment, which are all needed to define that problem.

Let me just go on a moment, indicate how we use MARCH and its companion code or what has become the companion code, CORRAL. We can start out with input accident deformation, physical description, what have you. We go through MARCH with all its various subroutines, and MARCH will calculate the thermohydraulics conditions, accident event times and give you the basis for calculating the containment failure mode.

We take these two branches, if you will. We feed them through a data processing code and feed them into the CORRAL code and then CORRAL takes the MARCH up and calculates the release to the environment.

There is one other step that I could have shown that I didn't. Of course you can take the release to the environment as well as some of the MARCH inputs and feed them into a consequence code such as CORRAL II, translate the results into property damage or fatalities or whatever measure of effects you want.

Before I get into some specific examples of MARCH calculations, let me just use an idealized picture of what we are talking about. A convenient way to represent results for meltdown calculations, a convenient framework, is a pressure and temperature history in the containment which is usually closely coupled to the events that happen.

In this particular idealized diagram you are boiling off the steam generators in this timeframe, uncovery of the core takes place here -- I am sorry, the lifting of the pressurizer, release all takes place here, and you start dumping steam into the containment. The pressure goes up, the core is uncovered at this point. As you start uncovering the core the rate of steam input to the containment goes down. The core is fully uncovered, slumps into the bottom HEAD of the reactor vessel at this point. There is an arrest in the pressure while the core melts through the bottom HEAD. At this point here the core is molten through the bottom HEAD to release primary system steam to the containment. You interact with acute core debris in the reactor. They accumulate the water. You get a high pressure spike. In this particular case, the containment fails to depressurize.

Somewhat of an idealized diagram as a lead-in to some of the specific calculations.

The calculations I am about to show are related to our recent exercises for the Indian Point/Zion study, and many of the people in the audience have seen these things quite a few times. This is a TMLB sequence based on Indian Point-3 reactor, loss of power conversion system, loss of all heat removal, loss of electric power. Leads to -- I won't read off the numbers in terms of the timing of the event. Again, start dumping steam to the containment. The pressure goes up. Core uncovers. A low arrest in pressure At this point here all the bad things happen. The head of the reactor vessel fails, and there are two key things that happen.

One, as the head fails, and in this calculation it is assumed to fail in a catastrophic way, meaning the very large opening in the bottom HEAD. Release the primary system steam -- let me back up for a moment. Throughout this sequence the primary system is at the release offsetting and the boiling off the water inventory, high pressure.

At this point you release the steam in the primary system. You get an increment of pressure up to about here. As the pressure in the primary system drops the accumulators discharge. The accumulators have not been able to discharge up to this point because the primary system pressure was high.

If you make the assumption that the accumulator discharge leads to some kind of a fragmentation interaction with the core debris, you can predict rapid steam generation, which takes the pressure way up --

DR. SHEWMON: How rapid is rapid?

DR. CYBULSKIS: In this particular calculation I forget the exact number, but the evaporation of the water takes place over a number of seconds, like 5 to 10 seconds, I assume.

DR. SHEWMON: So you are saying in seconds the core, the entire core no doubt, gets the water, immediately breaks up into small particles, and you then worry about transferring the heat from the small particles?

DR. CYBULSKIS. Have you ever done anything to try to make this more realistic? If people really got to where they thought they were going to put vented containment on things and believe this code?

DR. CYBULSKIS: As I made the comment to a question earlier, I would not treat MARCH as a design code. I wouldn't design a mitigating system strictly on the basis of MARCH results. There are too many approximations in there. I think it can be indicative of the types of problems you can run into.

Obviously, as you -- within the context of MARCH you do make the assumption that the entire core fragments. In this particular case the fragments are very small, though the size of the fragments is not quite as important as you might think.

DR. KERR: I have indeed heard you say you wouldn't use MARCH as a design code. Have you worked out a system to prevent other people using it that way?

(Laughter.)

DR. CYBULSKIS: That is not within our control obviously. The only thing they can do is stash it.

Let me just cross out the alternate case, if I can overlay this. Of course, if you don't get this fragmentation event, the situation is much more benign, and the pressure only goes up to about here, and then again it climbs back up to essentially the same level over a longer period of time, and the difference, which in this is the interaction of water with the core debris.

DR. STRATTON: Is this interaction an input that you can control if you have a specification -- --

DR. CYBULSKIS: Yes, you can either interact or not interact, and if you suppress the interaction, then the code assumes that the core debris will fall to the bottom cavity essentially in the slab, a monolithic form, and the heat transfers to any overlaying water as controlled by radiation and film boiling, and that is why you still get the relatively slow pressure rise.

DR. STRATTON: So the code can be used to at least identify certain problems at certain points in the sequence, and then if there is something alarming or suspicious, then you can examine the specific substance in MARCH --

DR. CYBULSKIS: In greater detail. Precisely, or specific points could be examined in greater detail in separate calculations. I think that is an excellent way of putting it.

I always seem to start out with bad calculations. In the last calculation the pressure spike was due to, I think, rapid steam production. And the atmosphere was not flammable due to the high partial pressure of steam. In this particular calculation the high pressure was due to a combination of hydrogen burning and rapid production of steam and again this happens to be a large LOCA.

Again, in the limiting case, you can get to very high pressures. If you eliminate the hydrogen burnoff from this particular sequence, you take out the pressure spike (inaudible)

DR. SHEWMON: All right, could you tell me what coolant is going on in the system during the scenario? Are the containment sprays operating?

DR. CYBULSKIS: In the 1 st sequence, as well as this sequence, there is no containment sprays operating. Loss of electrical power, so they are not available.

From my point of view these are limiting sequences. Sequence AB is a large LOCA with complete loss of electric power by definition.

DR. SHEWMON: So you are telling us that -- okay.

DR. CYBULSKIS: These --

DR. SHEWMON: -- -- system with water and hot metal we have got problems?

DR. CYBULSKIS: Basically, that is a very simplified way of putting it, and what MARCH does is go through the arithmetic and show how bad the problem is. But certainly that, you don't really need a fancy calculation if that is the right word, to tell you that you have a problem.

DR. LEE: Can I infer from the comparison of the two figures transference (inaudible) that somehow for the purpose of limiting all the pressurization in the containment you had rather not use accumulated water?

DR. CYBULSKIS: I did not make that statement, and I personally would not make that assumption. The calculation tries to follow the behavior of the system as realistically as it can, given the initial definition of the sequence.

In the previous case the sequence was a transient in which the, without any heat removal, and in fact, given that type of sequence you would boil off the primary system inventory through the relief valves. And the accumulators would not be able to discharge. And if you will follow that sequence logically to its conclusion, what happens is that when the primary system fails the pressure drops, the accumulators would discharge. And if you discharge the core out of the primary system at high pressure and follow it by accumulator water at high pressure, one has to recognize at least some possibility of interaction between the two.

If you take what might be considered a limiting case interaction, you get very high pressure.

DR. SIEGLE: Could I ask a question on a couple of your charts?

DR. CYBULSKIS: Certainly.

DR. SIEGLE: On your case one, without hotdrop, between starting up and core slump you have about 32 minutes. Then on the next one, the gamma base case between core melt, between starting up and core slump you have about 15 minutes. What specifically needs that factor of two difference?

DR. CYBULSKIS: In the first case, in B-prime sequence, the core is entirely covered with water, completely covered, and you have to boil off all the water effectively to get to complete core melting. In the AB-gamma sequence it is a large LOCA sequence where you have blowdown, excuse me, blown down the primary system inventory to the containment. You have also lost some of the accumulator water to the containment so the core is only partially covered at the start of the boil calculation.

So the major difference then is the amount of water that you have to evaporate before you melt the core.

DR. CYBULSKIS: Let me go on quickly to again run through some calculations. In this particular case I'm talking about as S2D sequence in the nomenclature of the safety study. S2D is an initiating event which is a small break. B is failure of the emergency core pooling system in the injection mode. The containment safeguards are operating in this particular case. And the specific calculation that was done and the containments considered were the containment coolers. The sprays were not included, and the reason why the sprays were not included was because in the initial phases of the accident, the containment pressure was extremely low, and the containment sprays were not be required. The coolers are more than adequate to take care of it until things get bad.

I don't have the numbers printed on here, but I believe the core starts to uncover somewhere in this phase. It's probably done (inaudible) at the bottom head at this point. Again, the head fails, and you get a high pressure peak. In this particular case again the high pressure peak is a combination of hydrogen burning as well as the discharge of the accumulator water.

DR. OKRENT: Excuse me. Do you have an indication somewhere of when the hydrogen gets into the containment for this event?

DR. CYBULKSKIS: We do have an indication. I do not have that in the transparencies. In the transients and the small breaks typically some of the hydrogen is released to the containment during the course of the meltdown. The majority of the hydrogen tends to be held up in the primary system until the failure of the vessel head when you discharge essentially out of the primary system inventory into the containment. That is a typical scenario for the small breaks and transients.

In a large break the hydrogen tends to be released to the containment a little sooner.

Again, if I overload these things -- the first one is the worst combination. You have both the burning as well as the steam reduction. In the second case if you eliminate the burning first, the pressure is somewhere close to the design pressure.

If you just go on and just look, this is the converse of that or another variation of it. Eliminate the steam production and look at the burning only. Again the pressure is up in the 80 psi level. Instead of 60 it's up to 80, but it's not anywhere near as high as the combination of the two effects.

Let me go on to the next case which is kind of interesting. Let's look at it singly. If you assume a small particle size for fragmentation, hydrogen burning and the pressure due to steam production tend to reinforce each other. If you assume a very large size for fragmentation, and if you have a large size for fragmentation and if you have containment safeguards operating, as in this particular case, what you see is a separation between the two effects.

This is a first peak due to the hydrogen burning which in itself isn't a factor, but due to the large particle size for fragmentation assumed in this calculation, it takes some time to develop to steam generation; so that by the time the peak due to the steam generation comes along, the hydrogen peak has already passed.

So it does make a difference on the types of assumptions that you make. And let me just go on to the last slide.

DR. OKRENT: Could I ask one more question on the hydrogen --

DR. CYBULSKIS: Yes.

DR. OKRENT: -- Release to the containment for the small break or the transient. Do you reach flammability limits in the containment prior to the point where you calculate that the molten core goes through the bottom head?

DR. CYBULSKIS: That's an excellent question. In the transients, the particular transient that I presented, specifically you in fact don't have enough hydrogen in the containment prior to the failure of the bottom head of the reactor vessel. When you release the steam as well as the hydrogen, then again it wasn't flammable because of the high partial pressure of steam.

In the small break locus you may or may not reach a flammable mixture before you go through the bottom head. It will depend on sequence and the specific things that are going on. We typically in the types of calculations that we do don't know when the hydrogen will burn. We have typically assumed that the failure of the bottom head and the falling of the hot debris on the concrete in the cavity is the most convenient or perhaps the most likely ignition point for the hydrogen. It tends to emphasize the burning or make the burning worse because at that time you have a lot of hydrogen in the containment.

If you make the assumption that the hydrogen burns as soon as it reaches a flammable composition, you have a tendencey to stretch out the burning and have much lower peak pressures as a result.

Does that address the question adequately?

DR. OKRENT: For now, yes. I'd be interested at some future time in seeing what the calculated curves of the hydrogen release are, but no rush, not today.

DR. CYBULSKIS: The last curve, just for illustration, it's all part of this S2D sequence. I pointed out that in this particular area here the coolers are operating, and the pressures are very low. For the purposes of the calculation we turn now to coolers at this point here to see what effect the coolers have on the peak pressure that one calculates.

And in terms of these real bad situations, we see that the coolers have essentially no or very little effect on the pressure spikes that you would reach in an event like this. Of course, they have (inaudible) factor in the tail.

DR. MARK: Could I ask in this case or one of these cases where you do have hydrogen burn after the failure of that baseplate, is the hydrogen generation then calculated by boil?

DR. CYBULSKIS: Yes.

DR. MARK: So that as the melting proceeding, there was hydrogen being formed.

DR. CYBULSKIS: Yes.

DR. MARK: And there was water being supplied in a sufficient rate for that much hydrogen to be formed. Does this have anything to do with a likely supply of water? I mean, if you boiled the core off, then you don't get any hydrogen unless you have some water coming in.

DR. CYBULSKIS: Well, the two effects are simultaneous to a great extent. As the water level in the core drops, it generates steam. The reason why it's dropping is because you're generating steam and not making it up.

The steam rises up through the core as portions of the core get hot. The steam reacts with the zirconium and goes off as hydrogen. So they're simultaneous effects, if you will.

Now, a point I might mention that is perhaps germane to this question or comment you raised, if you use our meltdown model C, which is the model where we assume that the molten nodes drop immediately to the bottom head of the reactor vessel as soon as they melt, no holdup on the rods, no holdup on structures or anything. What we would calculate is that as the nodes drop into the head, they generate more steam. The steam rushes up past the core, and you evaporate all the water in the bottom head before you get significant heatup of the core, before you get extensive amounts of metal-water reaction. In those cases you would in fact run out of available water, and you would predict less metal-water reactions.

That was the last of my prepared remarks. I would be happy to entertain any questions.

DR. KERR: Other questions?

Mr. Shewmon.

DR. SHEWMON: Tell me, this very sharp pressure spike that I see in the last three slides is due to steam generation?

DR. CYBULSKIS: In the S2D sequences?

DR. SHEWMON: Yes.

DR. CYBULSKIS: It's steam generation and hydrogen burning for the very high pressures, and then there are variations where I eliminated one or the other.

DR. SHEWMON: The original sharp drop then is due to what?

DR. CYBULSKIS: In the base case calculation, the sharp drop is due to the fact that you do have the coolers on in this sequence.

DR. SHEWMON: And what is the width of this spike, the first vertical line segment there?

DR. CYBULSKIS: I don't recall the exact numbers. It's of the order of seconds of fractions of minutes. For the purposes of containment loading it's a quasi-static pressure load.

DR. SHEWMON: I guess I'm trying to decide what it is that drops the pressure within seconds over a matter of 25 percent.

DR. CYBULSKIS: In the case -- well, again, in this particular case you have the coolers on and the capability of the coolers is more than adequate to quench this thing if it's stretched out in time. The problem comes in --

DR. SHEWMON: So there are fans which bring the air past coolers, and you're saying it will cool this, they are so vehement in their cooling that this will bring temperature down that far in seconds?

DR. CYBULSKIS: In seconds. It's probably not seconds; it's minutes. Of course, the temperature is very high in hydrogen burning cases, so you have an extremely large delta T to work with in the cooler.

DR. SHEWMON: Does your containment fail in the code under these calculations?

DR. CYBULSKIS: In these particular calculations there is no -- the containment did not fail whether we were doing this following the pressure response, assuming that the containment is intact.

DR. SHEWMON: So if indeed then the generation of this steam or the dropping of the core into the water below took to the order of minutes or whatever the relevant period is here, then the spike would be dropped back down by this 30 percent that I am looking at, is that right?

DR. CYBULSKIS: Yes.

DR. SHEWMON: Okay. Could you tell me what determines the fact that in a hundred minutes -- is this a hundred minutes from the rods in?

DR. CYBULSKIS: All these accidents start -- time zero would be the time of the initiating event in all these calculations.

DR. SHEWMON: And the initiating event is the SCRAM plus --

DR. CYBULSKIS: Is a SCRAM plus a break in the system.

DR. SHEWMON: Okay. And at that time then we lose any ability to put any water into the core, is that right?

DR. CYBULSKIS: By definition of the sequence you have lost the ability to put water in the core.

DR. SHEWMON: Okay.

DR. KERR: Other questions?

Mr. Lee.

MR. LEE: I have some comparisons of MARCH calculations with TMI-2 data, and I thought they were quite good considering, as you indicated, there are approximations inherent in the MARCH model; but I thought that calculation perhaps had to use some adjustment or the selection of certain parameters.

DR. CYBULSKIS: Yes. If you are referring to the report that we put out for the Rogovin inquiry, there was a substantial amount of adjustment, if you will, of parameters to get the agreement. We knew certain things that were -- to use Mr. Thomas' words, there were certain observables that we could benefit from in that particular case. There is no way that we could have predicted a priori or without any adjustable parameters what happened.

DR. LEE: Have any attempts been made to compare these components of the MARCH code or MARCH model against some experimental data?

DR. CYBULSKIS: Of course, that's one of the key aspects that is missing in the core meltdown area. Of course, there is no experimental data. In the development of the MARCH code we have compared components, say the large loca-type blowdown calculations, the peak pressures that we calculate in the containment, with other codes like CONTEMPT. We have calculated our heat sink subroutines against exact solutions where they are known.

We are in the process now of comparing parts of the MARCH code with corresponding codes that are being developed in Germany. They have a similar approach to what they're using, and that comparison is in progess and hopefully will continue.

DR. KERR: Mr. Stratton.

DR. STRATTON: Mr. Lee covered certainly part of my question. I note that the MARCH code is a collection of -- in part a collection of subroutines, some of which look to be pretty significant. If we should wish to examine one of these or several of these for our own satisfaction or because Mr. Kerr might ask us to judge them, could you furnish Mr. Quittschreiber with a letter that would just list the references for each of these is described and where each has been tested against some physical experiment?

DR. CYBULSKIS: The MARCH code is in the process of being documented presently. Our commitments to the Nuclear Regulatory Commission call for the MARCH documentation to be completed prior to the end of the fiscal year. That document would, of course, be the prime starting point for anyone who would wish to examine the code.

DR. STRATTON: Very good. Thank you.

DR. KERR: Mr. Thomas.

DR. THOMAS: I have a couple of questions.

The BOIL code originally was a quasi-static calculation; that is, temperatures in the steam region, temperatures in the cladding are dependent strictly on level, not how you reach that level. Has that been improved in the MARCH code?

DR. CYBULSKIS: I'm not sure to which version of the BOIL code you are referring. The temperatures in the steam are calculated as a function of flow rate through the rod which in itself is calculated, and as a function of what's happening. The steam can cool the rods or vice-versa. The rods can cool the steam depending on the relative --

DR. THOMAS: That is not my point. The MARCH or the BOIL code when I have worked with it is a quasi-static solution; that is, the temperatures above the water level are dependent only upon the water level, and the resulting steam produced at that water level. They are not dependent on how you reach that level. They are not dependent on prior history.

DR. KERR: Mr. Thomas, if you want to go into details, we are behind schedule, and I would appreciate it if you could maybe get together with Mr. Cybulskis.

DR. THOMAS: Okay. I would like to question your statement or realism. You said you use realistic methods of trying to calculate the events like in TMLB'.

DR. CYBULSKIS: As realistic as we can. There are obviously limitations. As realistic and as self-consistent as we can.

DR. THOMAS: Okay.

DR. CYBULSKIS: Given the definition of a C-flex(?) -- this is somewhat --

DR. KERR: We all recognize the limitations on "realistic," I think.

Are there other questions?

Mr. Shewmon.

DR. SHEWMON: As the keeper of this code you're probably more familiar than many of us with what sorts of things lead to the problems of concern to us here in class 9 accidents. If you were looking for ways to mitigate the occurrence of a containment rupture, what avenues do you think would be most fruitful to pursue?

DR. CYBULSKIS: I'm not sure I fully understood your question.

DR. SHEWMON: The problem of concern that this code addresses itself to is when there is substantial fission product release. Our problem is to try to generate regulations or research to support those regulations which would reduce the risk to the public from this kind of an accident.

My question is if you were involved in trying to formulate that or look for ways to reduce that risk, what do you think are the most crucial avenues?

DR. CYBULSKIS: I keep hearing the words "reduce the risk," particularly in the case of say the Indian Point/Zion study in which I've been involved. I have not heard any words as to how much one desires that risk to be reduced, and I think that's kind of an integral question.

DR. SHEWMON: Let me start with an order of magnitude. I'm certainly not interested in anything less than that.

DR. CYBULSKIS: I think the types of things you would do in a design basis would depend on the particular design you are considering. Some designs may be more amenable than others in terms of reduction of risk.

But obviously, the primary thing that controls risk to the public is early containment failure, early in relation to the coremelt process. And if you re trying in fact to reduce the risk to the public, you would want to do something that in any given accident -- I'm sorry -- in any given design would reduce the probability of that early containment failure given the coremelt accident. In some cases that may be vented containment. I'm not sure though. There may be limitations to what you can do with vented containment. I don't think it's a fruitful idea.

In other designs there may be very little that you can do given, again, the starting point that you have an accident that is going involved.

DR. SHEWMON: Thank you.

DR. KERR: We now come to -- thank you very much, Mr. Cybulskis -- Mr. Paddleford from Westinghouse.

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DR. PADDLEFORD: Thank you. I'm Don Paddleford from the Westinghouse Nuclear Safety Department. This morning I want to take about 15 minutes to describe some calculations that we did at the request of Dr. Stratton last August and September when he was directing Kemeny Commission technical staff.

Next slide.

Basically he sent us a letter asking us to address -- if we could assist in addressing some questions on inadequate core cooling area, and this slide shows the four particular areas that he asked us to address.

The first was to look at the core, coolability of the core in the vessel with no emergency cooling other than the throttled makeup flow that was being provided at the time, and asked us to take a look at what would happen in the transient if the block valve for the relief valve line did not be closed when it was.

Secondly, he asked us to look at the coolability of the core in vessel as either a particle bed or as a molten pool, assuming that the vessel was flooded externally all the way up to the nozzles with water, and in the latter case suggested that we go about it first looking at it as if the vessel was a bare hemisphere cylinder, and then go back and take a look at what conclusions we would draw with the actual insulation and structural considerations.

Finally, he asked us to take a look at the cooling on a flooded reactor cavity floor as a debris bed.

And he gaves us about four weeks to work on these, so we did these in parallel, and I guess there were areas where we would have like to have put in more work.

First, for the case of coolability as a core, we didn't do a full system model. We made use of an NSAC report that had come out about a month or two previous to his request to us which showed -- provided a lot of information on the water level history was expected to be.

Basically we assumed the core was covered until approximately 100 minutes. The block valve was open and never closed. The RCS pumps were with no safety injection. The makeup flow we used we obtained from the NSAC report in discussions with Dr. Stratton. It was like 41 GPM of cold water which is like 10 percent or even less of what normal ECCS flow is, which is up in the 500 GPM area.

For the 41 GPM, this was cold water; we treated it as 60 GPM saturated water up to the core inlet, based on the capability of it quenching some steam that would either go through the loops or through the barrel check valves.

Okay. Oh, originally we thought that the water in this skate and bleed (?) process might be able to cool the core several times between being injected and escaping through the relief valves, but we couldn't justify this. We didn't have enough information on the secondary systems, and there were questions about the possibility of the loop being blocked by hydrogen and things, so we didn't really look at that.

Finally, we did a little looking at the pressures and what we would think might happen over the next half an hour or so, and we didn't think we could justify accumulator discharge, so we didn't account for that.

Next slide shows some additional assumptions. In the water double transient we used the Gay-Boyd fraction for coming up with mixture level, and we accounted for, at least during the depressurization and the core uncovering phase, we accounted for some steam flow from heat losses and boiling of the lower plenum.

This slide shows the mixture level that we obtained from the NSAC report. The actual transient is somewhere right along in here. The block valve was closed out here. The reactor coolant pump was started, and out here safety injection was turned back on. In our transient we just allowed the water level to continue boiling off and asytotically(?) approach the level where the core was covered like two to three feet. And the decay heat in the submerged part of the core was just matching the boiloff energy of this makeup flow, and the steam generation rate which was cooling the upper parts of the core decreased and dropped down to about less than 30,000 pounds per hour.

The core model created a best estimate zirc-water reaction model. We used the Cathcart model from Oak Ridge, and made one modification based on the observations of the PBS test that it looked like a substantial portion of cladding beyond CHF test was being oxidized by the uranium on the inside rather than the water on the outside. The rates appeared comparable.

We assumed that 40 percent of the clad would be oxidized by the fuel on the inside, and this reaction is essentially energy-free compared to the zirc-water reactor.

We used melting points of 4900 degrees for both the UO_2 and zirc oxide. The model included radiation and convection of steam. We used a best estimate TMI decay heat curve that Los Alamos had provided the Kemeny Commission staff We included a model for volatile escape as the fuel heated up based on one of the appendices of WASH-1400. It was like a linear model between 2500 degrees Farenheit and the melting point of uranium dioxide.

We used a single channel axial model, and the fuel rod we analyzed was the average rod in the hot assembly. The hot assembly had a peaking factor of about 1.49.

The results showed that the (inaudible) of the clad would completely react and that we had an area near the top of the core about eight-tenths of a foot in length where the temperatures -- actually the clad temperatures went above 4900 degrees, so the zirc oxide would have melted. The fuel temperature lagged the clad temperature by approximately 200 degrees because of a gap. At lower levels the oxidation was occurring much slower, and we felt that the blockage of the assemblies due to that .8 feet of clad length, that we did conclude would melt, would not have any significant blockage effects. It was distributed over some lower, cooler elevations.

This slide gives an indication of kind of the time period where the zirc oxidation was taking place. It didn't really take very many minutes before we had the bulk of it, and after approximately 20 or 25 minutes the reaction wasn't progressing hardly at all.

DR. SHEWMON: Could you reorient me on the amount of core uncovery on that draft?

DR. PADDLEFORD: This is the mixture level showing --

DR. SHEWMON: Where's the top of the core?

DR. PADDLEFORD: Twelve foot.

DR. SHEWMON: Okay. And the fueled section is how long?

DR. PADDLEFORD: About twelve foot.

DR. SHEWMON: Okay. So you're saying that the zirc oxide doesn't start to form until you get down to that three foot level, and then it forms very fast.

DR. PADDLEFORD: Right.

DR. SHEWMON: Thank you.

DR. PADDLEFORD: Okay.

DR. LEE: Wa che effect of radiated heat transfer to steam important in this analysis?

DR. PADDLEFORD: We included it. I'm not sure it was very important.

DR. LEE: Thank you.

DR. PADDLEFORD: The convection at these high temperatures should have been very adequate. The coolant temperatures or the steam temperatures at the top of the core were very close to the clad temperatures.

Okay. The second area we looked at was the water-cooled particle bed in the bottom of the reactor vessel. Now, based on what we went just through, if we had the 41 GPM or some kind of an ECCS flow even throttled, wouldn't have expected the core to have gotten to that point. And if there was no flow, you couldn't could a particle bed because it would be dry.

So we just took a look at two cases. One was arbitrarily assumed that we did have a particle bed at a time like five hours in the lower vessel and that the 41 GPM makeup water was available. We plugged into the Harding-Neilson particle bed correlation to look at dryout, and we concluded based on our non-volatile decay heat and particle diameters of one millimeter and above, even if we had a bed void fraction of as low as .3, we wouldn't have dryout. But that assumes you have to have sufficient water to remove all the heat by saturated boiling as a minimum. And with 41 GPM, you only have enough flow to cool 40 percent of the core. So our conclusion was that you couldn't cool the bed at this time with the amount of water that was given to us as a boundary condition until the bed had cooled and decayed for something like 38 hours.

The third area we looked at was a molten pool in the vessel, and again we used the five-hour decay heat for the non-volatiles, and that was like 65 percent of the total of five hours. We assumed the external vessel surface was flooded up to the nozzles and started off ignoring any structural impediments.

In this case what we were after was trying to come up with the heat flux from this melt to the vessel, through the vessel, so that we could check and see whether we exceeded CHF on the outside of the vessel, also to check if part of the vessel wall would melt, what would be the remaining thickness and was there significant strength.

We postulated several melt configurations, the main significance of these being that if you pick a layered melt and assume that your fission products are really tied up in the UO_2 layer, you increase your heat flux to the vessel, and this was the limiting configuration of those that we looked at.

This shows the fact that then as you get into, you'd have to just reach in with the volumetric heat source and freeze the crust. It's fairly thin, and the heat transfer model that we used was based on some calculations by Mayinger from Germany who indicated that if you have roughly equal wall temperatures, hemispherical geometry, that you have the average heat flux into the hemisphere is like one and a half times the heat flux to the upper surface. We used that.

Secondly, we included in the vessel wall the contribution from the frozen layer. And thirdly, Mayinger and some others' calculations and tests indicated that at the top corners of the melt you may have twice the -- because of the convection patterns you may have double the average heat flux into the hemisphere, so we counted that.

What we came out with were heat fluxes like 135,000 average over the hemisphere, and maybe 280 or 290,000 up at those top corners. Based on that, with the average heat flux there would be essentially no vessel surface melting. We'd be left with like 4.8 inches. And this is wrong. This is really the layer case, the homegenized case. The remaining thicknesses were 6.8 and 3.4 inches.

The minimum thickness was 2.4 inches. And our conclusions based on some simple hoop stress analysis, looking at the temperatur gradient through the wall, was that not only could we carry the weight of the melt, but the 700 psi that was in the vessel at the time when the analysis that we were looking at started.

DR. PLESSET: Did Mayinger consider any motion in these layers? Did he assume they were static?

DR. PADDLEFORD: No. They were convection patterns established.

DR. PLESSET: Oh, he did take that into account, and did he think that there was overall stability in the pattern?

DR. PADDLEFORD: I believe so. Yes, they were calculated as stable patterns.

All right. Then back to part B of this was what happens if you don't have a bare vessel? Well, the first thing we did was contact Metropolitan Edison and Babcock and Wilcox and got some information from them about what the arrangements for the insulation and vessel support were.

The vessel is supported by a cylindrical ring that has in it 12 like 9-inch holes. Insulation sits off the bottom of the vessel about two or three inches, and even though there are some clearances around the penetrations and so forth, some discussion with the people at the site indicated that they made a good effort to try to block those; so our conclusion was that with the insulation in place, there would be very little chance for leakage to cool the vessel.

Secondly, we postulated what would happen if the vessel wasn't there, if it would blow away from steam generation or something like that. We were left with the problem up here, this zone up here would steam blanket, and we couldn't include the vertical conduction through the vessel's field would be adequate to prevent failure if we had a melt that went above the top of these holes.

We did feel that without the insulation present that you could contain melt in the vessel up to about the top of those holes or maybe a little above, which would correspond to maybe 10 to 15 percent of the core.

The last area we looked at was the debris bed in the containment floor. Here we assumed the whole reactor, the whole core particulated and was in the cavity. The cavity at TMI has a cross-sectional area of 200 square feet.

Next slide.

These calculations indicated that with one millimeter particles the core, the full core would be coolable even with a void fraction of .35, and with higher void fractions it could even be coolable with smaller particles at the 200 square foot cross-sectional area, five-hour decay heat and a void fraction between .4 and .5, you would conclude that you could cool for particles down in the 500 and 600 micron diameter range.

DR. SHEWMON: Do particles usually pack that closely in such cooled beds, or have experiments of this sort, measurements been done?

DR. PADDLEFORD: I asked Bob Henry what he had seen regarding information where a particle bed had formed from a quench, okay, and he thought that they were up in the 400 to 500 -- 40 to 50 percent range.

DR. SHEWMON: That's void or solid?

DR. PADDLEFORD: Void.

In conclusion, we felt that the reactor as designed, even with much lower than the normal ECCS flow required to meet Appendix K, could provide coolability even though you would have substantial clad oxidation; felt that major core damage had occurred by the time the block valve was opened. We didn't think that leaving the block valve open a little bit longer would have led to that much additional damage based on the calculations that we made; and we didn't calculate any fuel melting. For that transient we did have something like one foot of clad melt.

That's all I have.

DR. KERR: Thank you, Mr. Paddleford.

Are there questions? There being none, I declare a 15-minute break.

end tp 3

(Brief recess.)

DR. KERR: We will now hear from Mr. Peoples concerning a number of things.

MR. PEOPLES: Members of the ACRS Subcommittee and ladies and gentlemen, my name is Lou Peoples. I'm with Commonwealth Edison Company, and I'm here today representing Commonwealth Edison, Consolidated Edison, and the Power Authority of the State of New York as owner-operators of Zion and Indian Point nuclear stations.

We were thrust into consideration of Class 9 accidents on December 7, 1979, and it was on this day that the NRC staff called a meeting in Washington, D.C. to discuss their concerns related to the safety of operations of nuclear power plants near large population centers.

As a result of that meeting on December 5th, the three utilities embarked on a 60-day study program aimed at assessing the comparative risk posed by Zion and Indian Point stations and evaluating concepts for the mitigation of severely degraded core accidents. And that 60-day study has been extended at this point and is still continuing, and we will explain that.

The results of the utilities' 60-day study effort were presented in a report to the NRC on February 20, 1980. This was followed by presentations to an ACRS Subcommittee and to the full ACRS in early March of 1980. Since that time the utilities have pursued a longer term probabilistic risk assessment study, have continued to research basic phenomenology related to severely degraded core accidents, and have continued to evaluate mitigation concepts.

At this point I would like to stress that I believe that the utilities and the NRC have common objectives. These objectives are, one, the safe operation of both Zion and Indian Point facilities in both the short term and the long term; and two, any changes in plant procedures or equipment must be carefully considered and result in meaningful improvements in safety.

The purpose of today's presentation can be broken down into six segments. We wish to convey both the seriousness and depth of the utility work related to Class 9 accident considerations. Each of the utilities has from the very beginning date in December 1979 approached these studies with the view that a well-organized, thorough and best effort approach was to be our mode of operation.

Today we will review the short-term, mini WASH-1400 probabilistic risk assessment, which included plant specific features which were different from WASH-1400, and therefore changed the risk assessment.

We will also review the longer term probabilistic risk assessment being conducted under the direction of Pickard, Lowe and Garrick. Our presentation will review the state of the art and describe the utility program related to the phenomenology of degraded core behavior, hydrogen burn, steam generation, core coolability and containment structural response.

We will also indicate the direction and scope of the utility and industry ongoing work related to both basic phenomenology and conceptual design work on mitigating features. And in closing we will indicate how the Zion and Indian Point studies fit to the degraded core rulemaking.

Today we are seeking ACRS Subcommittee support with regard to three lessons we have learned from our work related to

Class 9 accidents. First, we would ask the ACRS to encourage the development of a safety goal so the designers may work toward a fixed objective. Such a goal is required if objective decisions are to be made.

Second, the probabilistic risk assessments of Zion and Indian Point stations have taught us the value of a disciplined, consistent approach to the evaluation of reactor safety. We ask that the ACRS promote the use of probabilistic risk assessment as it relates to a broad view of safety. This broad view of safety should include not only design but also siting and evacuation.

With respect to design considerations, one could segregate existing and new systems into two categories -- prevention and mitigation. And for our discussion, prevention is defined as systems to prevent the severe degradation of the core, and mitigation is defined as systems which reduce the radiological impact on workers or the public after the core has become severely degraded.

And the third request we have to the ACRS Subcommittee is that analysis of Class 9 accidents, which are highly unlikely events, should be done on a realistic basis using logical mechanistic or physical models.

DR. OKRENT: Can I ask a question about your third point?

MR. PEOPLES: Yes, sir.

DR. OKRENT: Could you put that back on, please?

MR. PEOPLES: Sure.

DR. OKRENT: We had a meeting just yesterday, as you may know, which was on the subject of quantitative risk criteria and so forth.

MR. PEOPLES: Yes.

DR. OKRENT: And one of the more important points made at that subcommittee meeting, and which has been made at previous subcommittee meetings on the subject, was the question of how well we knew what the risks were, whether it was the risk of coremelt or the risk of certain health effects, and how to deal with uncertainties, defined uncertainties and perhaps more difficult, those that you may suspect but don't know how to deal with and so forth.

MR. PEOPLES: Yes.

DR. OKRENT: I think there's a considerable school of thought, say among third parties who are not, let's say, in the pro or anti-nuclear camps, that this is an important area, and that one has to address it somehow.

When one tries to compare risks from nuclear with risks of electrocution or lightning or fires in general and so forth, on the one hand, namely the categories I've just been mentioning, there are lots of statistics, and you have fairly well-defined values. In the case of the nuclear, I think with reason we have a rather broad range of values to think about.

unrealistic things that just aren't going to happen.

With regard to risk assessment itself -- and you will see later on in our presentation we have identified explicitly a treatment of uncertainties in our longer term study where we're able to have the time to do that effectively and propeily -- that does try to address both the mean, if you will, best estimate and a range of uncertainty that we carry through in our analysis to give us an uncertainty band about our final results.

And so I think we have recognized that in our study and will be addressing it quite thoroughly for you. We understand that.

DR. SIEGLE: I have another comment, if I may make it now, on that same third point, which perhaps only paraphrases what Dave Okrent says.

I find a certain logical contradiction between using the phrase "best estimate" and the associated recognition that the Class 9 accidents are very unlikely. I think you somehow have to carry along in parallel both the course you propose of best estimate in the face of a great deal of uncertainty because of the low level of probability, together with some kind of a bounding approach, which you say is not a best estimate, we don't believe this is a best estimate, but it is a bound which we lay out explicitly. And then the decision as to which of the two courses is ultimately adopted by this third party is not one that maybe you or we can make. We have to lay them out in

It's not clear to me that if we use realistic best estimates we're addressing this question. Now, I'm not arguing that we should somehow use unphysical conservatisms in some kind of analysis. That's not what I'm arguing about or for. But that question of best estimates ends up being presented in terms of numbers. I see numbers from your own group in fact presenting best estimates, as you know.

MR. PEOPLES: Yes.

DR. OKRENT: And it's not really clear to me that to meet the concern about what are the uncertainties that using best estimates is in any way adequate for the purpose. And it seems to me that it behooves all of us, including your group, to come up with an approach that is not only acceptable in what I'll call the technical community that's trying to review this but to an uncommitted third party.

MR. PEOPLES: Yes.

DR. OKRENT: And so I suggest you rethink your item three and --

MR. PEOPLES: Okay. I would like to say that I think we have thought through in very much the same line of thinking that you are offering, and if I did not communicate it effectively, I'd like to do so.

The thought is here that the realistic best estimate has to be toward physical models in the sense that you do not assume instantaneous heat transfer, for example, or other parallel.

MR. PEOPLES: I think it is important, though, to distinguish evaluation of Class 9 accidents, to distinguish that from a design basis analysis where you put in every conservatism almost imaginable as you go through the analysis, and that what we want to do is treat those conservatisms in a Class 9 accident to understand where we have taken them out and understand what we've done with them.

And we will show you in some of our presentations today what we have done in that regard. And the containment structural analysis is, I think, our best example today that will illustrate that. And what we have done, I think, is very much what you are suggesting, if we have defined, even after backing out the conservatisms, what we would consider a very high confidence level bound with regard to containment structure capability. It's not suggesting that we know exactly where it will fail. We say that we have a high confidence that it will survive until this pressure limit; and we have consultants here today who will go into that in some detail.

But that if you were to apply all of the normal design conservatisms in a Class 9 accident, I honestly believe you could not build a nuclear power plant and operate it today. And so that, you know, you want to design to one set of criteria and yet be able to look in what I have called a more realistic sense at what might happen in some really quite severe case.

DR. STRATTON: Do you have the definition of a Class 9 accident?

DR. KERR: Excuse me. Mr. Stratton, would you use the mike?

DR. STRATTON: Do you have a definition of a Class 9 accident that you could share with us? What is your interpretation?

MR. PEOPLES: The interpretation is the one that has become, I think, common in the usage, if not defined explicitly in any one place, which is what your problem is, and I haven't got it written down here in front of me either. But it's certainly beyond the design basis and results in greater than design basis radiological effects to the populace. And we have been using that as a synonym, I think, for a severely degraded core, coremelt, and ultimate release to the public. And we're looking for a way to prevent that release to the public in some fashion, and that that is, you know, what we're trying to prevent.

DR. KERR: Is that clear, Mr. Stratton?

MR. PEOPLES: Probably not.

DR. STRATTON: It helps. Thank you.

MR. PEOPLES: Before we start the detailed technical presentations, I'd like to review the current status of Zion and Indian Point. Zion 1 and 2 and Indian Point Units 2 and 3 continue to operate Confirmatory orders have been issued by the NRC to each of the three utilities, and these confirmatory orders specify certain hardware, procedural and operational actions which have been or are being taken.

The utilities are responding to the NRC's acceleration of current generic and plant specific licensing act:.ons as enumerated in the NRC action plan for Indian Point-Zion. These licensing actions include 35 pages of listing for various actions that relate to the four units under consideration.

Six meetings to exchange tecnnology between the utilities and the NRC and their consultants have been held. These meetings included in vessel sequences and phenomena, ex vessel sequences ard phenomena, hydrogen behavior and control, filter vented containment systems and core retention devices, containment structure response in short-term probabilistic risk assessment and sequence selection.

The Nuclear Regulatory Commission research efforts are also continuing, and these efforts relate to phenomenological studies, to mitigation concept design work, and to probabilistic risk assessment.

The NRC will be reviewing the longer term probabilistic risk assessment being performed by Pickard, Lowe and Garrick in some detail. In addition, IREP studies have been initiated in four other plants which will expand the data base beyond the Crystal River pilot project. Also, the NRC has asked Limerick(?) station for a 120-day probabilistic risk assessment.

The NRC has proposed a degraded core rulemaking with advance notice of rulemaking likely to be issued this summer.

The actions described before fit within an overall program which has been developed by the NRC for both Zion and Indian Point plants. First, the assessment of risk of operating Zion and Indian Point stations, given their current physical siting, is being identified in detail.

This assessment of risk to nearby populations is being conducted on a plant specific and site specific basis.

Second, interim actions have been or are being taken which help to ensure safe operation.

Third, generic and plant specific licensing actions for Zion and Indian Point are being accelerated so that outstanding issues are resolved for these plants as soon as reasonably possible.

Fourth, severely degraded core accidents are being studied to assess the likely impact on both plant operation and the surrounding population. As part of this study, mitigation features such as filtered vented containment systems, core retention devices, and hydrogen control measures are being reviewed.

The conclusion of this program requires a definition of a safety goal with appropriate methods of measurement of achievement. Design and operational decisions that explicit functional criteria be clearly stated.

Now, to give you a more detailed description of some of the technical work which we have conducted, we have six speakers today, each addressing topics within their own expertise.

I'll name them now, and each in turn will introduce himself and the topic of his presentation. They will be Dr. D. Walker with Offshore Power Systems; Dr. John Garrick with Pickard, Lowe and Garrick; Mr. Nick Liparulo with Westinghouse; Dr. Robert Henry with Fouske and Associates; Mr. Adolph Walser with Sargent and Lundee; and Dr. Richard Toland, United Engineers and Constructors.

At this time I'd like to turn the floor over to Mr. D. Walker, and he will be addressing the mini WASH-1400 studies.

DR. SHEWMON: May I ask one question?

DR. KERR: Mr. Shewmon.

DR. SHEWMON: I'm not quite sure what a safety goal is. Is that what we were talking about yesterday afternoon where you would say things of 10^{-5} and 10^{-3} for something or other?

MR. PEOPLES: I believe Mr. Ed O'Donnell made a presentation yesterday that represents some of the atomic industrial forum thinking, and that gives us an insight into it.

DR. KERR: Thank you, Mr. Peoples.

Mr. Walker.

MR. WALKER: I'm reporting today on the short-term risk assessment study conducted on the Indian Point and Zion plants. This study was conducted in a period of about six weeks, relying heavily on the methodology, approach and information developed in WASH-1400 and similar follow-on studies. We did not develop new approaches, for example, in the common mode failure area, but rather largely utilized the system failure and operator error estimates from WASH-1400.

One of our objectives was to preserve a frame of reference to WASH-1400 for comparative purposes. We at OPS led the study and reported the results. There was extensive input on systems operation and specific plant reliability data from the utilities.

The objective of this short-term study was to establish within a limited period of time a reasonable estimate for the risk associated with coremelt accidents for the Indian Point nuclear station units 2 and 3 and Zion units 1 and 2, and that compared with that calculated for the reference PWR in WASH-1400. And also to determine the dominant accident sequences contributing to coremelt risk for these plants.

To calculate the risk from coremelt accidents, one first identifies accident sequences beyond the regulatory design basis and establishes their probabilities. Knowing the characteristics of the accident, one proceeds to determine accident consequences. Finally, probability and consequence are combined to arrive at risk, and this process is indicated on the first vugraph, which is also in the handout.

As we've indicated, we start with identified dominant accident sequences, develop system unreliability estimates, follow a similar approach in the containment, combine the containment failure modes with the dominant accident sequences, and assign these to fission products release categories, utilize CRAC for consequence estimates, and plot the risk curves.

We indicate at the bottom of this vugraph some of the sources of information we utilized in this short-term study.

Before I describe the results of the short-term study, I want to summarize the areas in which we utilize WASH-1400. This summary then emphasizes areas where we were similar to WASH-1400, and they are these.

Generally we utilized the methodology of WASH-1400. We started with the WASH-1400 list of dominant sequences. In calculating the accident sequence probabilities we used the safe pipe break probabilities for the initiating events. We redid the transient and V-sequence probabilities, as I'll talk about later.

We generally utilized the WASH-1400 component of failure data base. With respect to containment failure mode probabilities we utilized the same five containment failure modes as were in WASH-1400, and for the isolation and melt-through failure modes, we utilized the same probability values.

I'll talk about the other three as I go through the discussion where we made some modifications.

Next vugraph.

In the fission product area we utilized the same core inventories basically, with some slight adjustment for power level. We utilized the same spray washout functions as WASH-1044 had.

We utilized the same containment release assumptions and the same seven fission product release categories. And for the consequence calculations we utilized the CRAC code developed for WASH-1400 and also utilized the WASH-1400 evacuation model.

With this introduction I want to talk some about the headings on these vugraphs and the basis that we utilized for some of the more important results; and I'll start with the accident sequence heading.

Okay. Regarding accident sequences, the starting point of our study was the table of dominant accident sequences from WASH-1400, and this is Table 314 in Appendix 5 of that report. Our initial assumption was that accident sequences which dominated risk in WASH-1400 would likewise dominate risk for Zion and Indian Point.

From the WASH-1400 list we chose to consider those sequences which had probabilities calculated in WASH-1400 10^{-6} or greater. Two of the sequences omitted because of this cutoff had probabilities of 10^{-7} in WASH-1400, and the others had probabilities of 10^{-8} or less.

Now, during the examination of the Zion and Indian Point plant systems, cause was found to both add to and delete from this initial set of accident sequences. First, with regard to sequences that needed to be added because -- there were sequences that needed to be added because of shared equipment, it was found for these plants that failure of the containment spray recirculation could result directly from loss of the emergency coolant recirculation capability.

Sequences involving coincident loss of containment spray recirculation and core recirculation were not dominant in WASH-1400 because of the independence of these systems, and hence their failures. However, for these plants such sequences were added, as indicated on the vugraph. And those are the AH, S1-HF, and S2-HF sequences -- the first letter, of course, indicating the type of pipe break and the last two indicating concurrent loss of recirculation and the spray capability.

In WASH-1400 a major contributor to risk was the S2C sequence which proceeds from loss of containment spray injection following a small pipe break. In the sequence, containment failure occurs from steam overpressure before sufficient water collects in the containment sump to support containment spray recirculation.

The capability to recirculate cooled sump water to the core is lost and coremelt results. Both Zion and Indian Point plants have fan coolers whose failure is independent of the spray injection system and which provide redundant containment cooling capability. Thus, the result obtained in WASH-1400 requires an additional independent failure, and so these sequences were deleted because of their lower probability in these plants.

In the aftermath of Three Mile Island it is widely believed that for at least some of the sequences, coremelt requires failures beyond those which were assumed sufficient in WASH-1400.

One example is loss of auxiliary feedwater or heat sink following shutdown. More recent studies indicate that emergency coolant injection systems can provide the cooling necessary to avoid coremelt. For this reason, sequences were deleted which involve loss of secondary heat sink, and an exception is the special case where there coexists complete loss of AC power; that is, the TML sequence was deleted, but the TMLB' sequence was retained in our considerations.

Finally, two accident sequences involving transient followed by failure of the reactor trip system were deleted. These sequences, which are the TKQ and the TKQM, have been analyzed by Westinghouse, the NSSS vendor, and found not to result in coremelt.

And so the next vugraph then indicates the sequences, the 12 sequences which we retained and evaluated in this study. And you'll see there the pipe break, the large pipe break sequence, the A sequences, the S1 intermediate pipe break sequences, the S2 small pipe break sequences, the interfacing check valve failure, sequence V, and the two transient sequences. We split the TMLB' into two cases, one being longterm sustained loss of all power, and then in the B'' sequence we assumed that some power was recovered in time to operate sprays before containment failed and so we split that sequence in that way.

Estimates for the probabilities for occurrence for each of these sequences for the plants as designed were developed utilizing the approaches of WASH-1400. Before showing you th results, I want to talk about the assumptions employed in our study which were different from those of WASH-1400, and these differences are summarized in the next vugraph.

First of all, the probability of containment failure due to steam explosion, producing in vessel steam explosion which produces a missile which can fail containment was reduced from 10^{-2} value used in WASH-1400 to 10^{-3} for the large break sequences, and 10^{-4} for all other sequences.

The rationale for these reductions has been discussed at length in technology exchange meetings with the NRC, and Dr. Henry will talk about it again briefly today later.

The operator error contributions to LOCA sequence is identified in WASH-1400, and their associated probabilities were modified in the following ways. First of all, in WASH-1400 the failure to shift from cold leg to hot leg recirculation at 24 hours into the accident was considered to result in coremelt for the LOCA sequences. Since the cold leg to hot leg shift is not essential for safe termination of the accident, this operator error contribution was deleted from consideration. We believe its deletion is probably appropriate for most of the PWRs. However the probability of operator error during the shift from ECCS injection to recirculation mode was retained. However, it was reduced by a factor of ten in the intermediate and small break sequences.

This reduction takes credit for the presence of the shift technical advisor in the control room and also recognizes that for many potential operator errors, ample time and indication exists to justify credit for this corrective action. We did not take the credit for the large pipe break sequence where events happen much more rapidly.

With regard to the probability of interfacing check valve failure of the V-sequence, our study applied the identical methodology of WASH-1400 to plants substantially different from WASH-1400 PWR. Plant specific features related to the interfacing check valve will be summarize in a moment in my presentation.

Regarding offsite power, a probability of 0.04 was utilized for such a transient at the Zion station, and that is based on data for such transients collected by Commonwealth Edison. The probability of 0.2 utilized in WASH-1400 was applied to the Indian Point plants.

Sequences other than TMLB' were assigned to combine containment overpressure failure probability of 0.1. This probability estimate was thought to be conservative based on containment pressure calculations for a broad spectrum of hydrogen production and combustion scenarios.

We chose to take this approach because the probability of overpressure failure is only listed in WASH-1400 for TMLB' and for those large break sequences with spray system failure. Overpressure failure probability for the large breaks without spray capability was generally in the range of 0.1 to 0.2 in WASH-1400.

In light of TMI, we thought it necessary to re-examine the probability of overpressure failure to acknowledge the potential for failure resulting from hydrogen burning. We therefore assigned a combined overpressure failure probability of 0.1 to all sequences. When I say combined, I mean we combined the gamma and the delta failure modes from WASH-1400.

We believe this approach is more conservative than that of WASH-1400.

For TMLB', containment failure probabilities for the gamma and delta failure modes were taken the same as WASH-1400.

The last entry is the diesel generator common mode failure. We discussed our basis for the numbers in Appendix A of the report we've issued.

To summarize, a common mode probability of 10^{-2} was assessed in WASH-1400 on the basis that all the diesel engines might trip because of normal starting currents. We frankly disagree that this is a realistic common mode. Diesel generator inability to withstand normal starting duty is a design fault that will be detected during preoperational tests.

Other potential mechanisms are suggested in WASH-1400. However, the probability that one such event would disable all generators is judged to be of no higher probability than 10^{-4} per demand. That's the probability we utilized for common mode diesel failure in this study.

Okay. The next vugraph is the one specific example I wanted to talk about, and this is a summary of the plant specific features that affect the V-sequence or interfacing check valve or failure mode. Those features are indicated both for the three types of design we considered for the WASH-1400 PWR.

You note these three plants have check valve test connections which the WASH-1400 PWR did not have, and allow one to reduce the probability of this sequence through testing of the check valves.

I've indicated on the vugraph on the next line the periodic test interval that was practiced in these plants up until December. At the time that this exercise was initiated, you note that periodic testing was not done at Zion and was done done at Indian Point and Zion.

As a result of the interim order from the NRR director, testing is performed at each reactor coolant system repressurization for these plants currently.

Another difference, particularly for Indian Point plants, are the low pressure piping inside of containment. The high pressure piping terminates inside containment. Part of the low pressure piping is inside containment. So in the Indian Point plant the likelihood is high that if you have check valve failure that the low pressure piping will rupture inside containment, and you'll simply have a LOCA inside a containment rather than a blowdown outside of containment.

DR. LEE: Question.

MR. WALKER: Yes.

DR. LEE: In your characterization for the check valve unreliability I notice that the unreliability is directly proportional to the square of the inspection intervals in units of years. Could you comment on what it really implies?

What I'm a little bit curious about is if you go to, let's say, weekly check intervals or something like that, you could reduce your unreliability by two orders of magnitude or something like that easier.

MR. WALKER: All I can do is -- I agree with you. But we simply utilized the model out of WASH-1400 to maintain parallelism in that respect.

DR. LEE: But I thought you indicated WASH-1400 didn't include that test interval and the credit for that.

MR. WALKER: Well, there was a model in there for indicating the interval, and I think the --

Do you want to put that up?

If you want me to, I can go through this model; but this is generally the model that was developed in WASH-1400 for the failure of one check valve error, and the indicated failure rate and the T^2 is the time between testing.

DR. LEE: In your evaluation of the methodology that was used in WASH-1400, you believe this is a realistic model when the test intervals go down below the annual inspection?

MR. WALKER: Well, I guess we really didn't go into that kind of depth, you know. We do have some problems with this particular model, and it would take, I think, a subject for separate long discussion if you want to get into that.

DR. OKRENT: Could you put the previous vugraph up, please?

MR. WALKER: Sure.

DR. OKRENT: I have a question for Mr. Peoples probably. It says that on Zion the testing is presently performed but that it was not done. When was testing begun?

MR. WOSSLAND: You say that's on the test interval?

DR. OKRENT: No. The question is when did testing of the check valve --

MR. WOSSLAND: That started with the interim order in terms of the periodic testing of repressurization.

DR. OKRENT: So it was not done prior to that time.

MR. WOSSLAND: It had been done to verify original installation, and I don't know the frequency of testing before then. Certainly not as frequently as we do now. It hadn't been done at some irregular frequency, but for the purposes of this to try to establish any type of numerical data or something on it was almost impossible, so we just stated it was not done for the purposes of the study.

DR. OKRENT: What does "irregular" mean?

MR. WOSSLAND: It was done more after -- well, during the initial startup time there were a lot of discussions originally in the various ISI works that went on, and some change in departments, and discussions on when things could be tested and when they should not be tested; and so it became, as I classed it, more irregular.

There were also a few times when some maintenance areas went on, and there was some testing that went on. And in the initial startup of the station there was some early development work looking at some of the capabilities of the various systems and how you may go about testing them.

So as I said, there were several different reasons why things were tested, but as I said, it was done more on an infrequent basis. There was really no established periodic tests to systematically check those out.

DR. OKRENT: That's what I'm curious about. If I can indicate my area of interest in case it's not clear, WASH-1400 draft indicated that the check valve failure sequence was an important contributor to risk according to that analysis, and the final report in 1975 didn't change that.

I'm sort of interested in the reaction of the operators of Zion to this information. Did they review it, decide it was wrong? Did they review it and decide it was right but not use it? Did they not review it? I'm trying to understand.

I mean, we've heard a minute ago that we should deal, if we can, with safety in a quantitative way and use probabilistic methods. Well, here was perhaps an interesting lesson, assuming WASH-1400 was correct, that this failure probability was significant.

I'm trying to understand what the logic was at the Zion plant, what they did and so on.

MR. WOSSLAND: No. I can't comment --

DR. KERR: Do you understand the question?

MR. WOSSLAND: Yes, I understand the question. He wanted to know whether we used basically WASH-1400 results and then applied them to the plant. In this particular case I do not know of any conscious effort to do that, but I might make the comment that in this particular case -- these are the interfacing check valves -- that (inaudible) from RHR systems where the operability of them to open is actually verified upon use, and if they would fail in several types of modes that it would be reflected back into the system itself.

We also have some test connections which do allow, as I said, some indication; so there is some -- in the normal operation there is some quasi-verification in the normal course of the state, even though it is not in the (inaudible) check the failure of a non-controlling valve.

But to the first question, no, I don't know of any particular study that had taken this particular sequence.

MR. PEOPLES: I'd like to respond on a broader scale.

I believe that one of the lessons --

DR. KERR: Excuse me, Mr. Peoples.

Did you get the name of the previous speaker?

Thank you. Please proceed.

MR. PEOPLES: Peoples here.

That on a broader scale both the nuclear utilities and the industry through NSAC and MPOL(?) are reviewing information that is available to them and have (inaudible) in a much more systematic manner in the last six months and is ongoing further and will continue.

We have organized an (inaudible) based on a specific responsibility for that so that we will try to identify strong, broad scope reports or specific reports at other places that might be helpful to us.

DR. KERR: Thank you, Mr. Peoples.

Further comments, questions?

DR. LEE: Just one question. I might be jumping a little bit ahead. If I am, please stop me. But in the transparencies you just showed us briefly, in calculating the reliability of the check valves, subsequent to that, I think, depending upon whether it's unit 2 or unit 3 of Indian Point, you applied another unreliability factor and so on. But nowhere -- and I believe this particular sequence really contributes quite a bit toward the overall risk to the public in your analysis --

MR. WALKER: Not these plants as analyzed now.

DR. LEE: Not the way it is analyzed, but because of the reduction in unreli bility, it doesn't contribute very much; but if you go back to WASH-1400, it would have, am I correct?

MR. WALKER: That's right.

DR. LEE: In doing so this calculation of unreliability in my opinion has some bearing to your overall risk assessment, and in doing so what I'm a little bit puzzled is where y 1 have tried to account for, for example, operator errors or any other common mode failures if you do not want to include operator errors as a part of common mode failures.

Could you comment on that?

MR. WALKER: Okay. Just a brief answer to that question, we didn't carry the analysis beyond a simple WASH-1400 model in this short-term mini-study. That level of detail is being picked up in the detailed study which Pickard, Lowe and Garrick is doing, so it's really the next generation, the common mode failure and the integration of other factors into the model.

DR. LEE: WASH-1400 did consider some amount of common mode failures and operator errors.

MR. WALKER: But I think not in the V-sequence.

DR. LEE: But, in general, the report did.

MR. WALKER: Right.

DR. LEE: But when you're going to reduce the unreliability in this particular case by I don't know how many orders of magnitude, a couple orders magnitude maybe, wasn't it somehow fair, in your opinion, to consider the operator errors or common mode failures?

MR. WALKER: Well, I don't know what you mean by unfair, you know. We did what we could do in a short-term, six-week study, and we intend to pick this up in the longer term followup study; so I guess I lose the aspect of unfairness.

DR. LEE: It's something along the line that Dr. Okrent was proposing earlier in the day, but when you present numbers that show that, okay, you can reduce unreliability by a factor of 10, but I just lose the perspective unless you dig into some of the numbers and begin to realize maybe there is a problem.

MR. WALKER: Yes. We're not saying there isn't a problem. What we're saying is we did a quick study to try to get some overview of what the totality of sequences looked like for these plants, and then the intent was to look at the details that you mention in the longer term study. They require a longer time than we had available to us in this study.

DR. LEE: Let me try to pursue this in a little bit different perspective, again depending upon whether it's unit 2 or 3, you end up with different unreliability for sequence V, and that is something to do with whether the (unintelligible) involved is normally supposed to be closed or opened.

In your opinion that difference is meaningful to change the risk to the public by, let's say, 10 percent and so on, apart from perhaps the quantitative assessment you came up with. I follow the mathematics.

MR. WALKER: Well, let me answer it in terms of I'm not even sure that a change in 10 percent of the risk to the public calculated by these means is meaningful at all to begin with. You know, I think that's a much sharper differentiation than we ever claimed was available in a study like this. I wouldn't even classify a 10 percent difference or a 10 percent change out of a study like this as being meaningful at all to begin with.

DR. LEE: Okay. Thank you.

DR. KERR: Please continue.

MR. WALKER: We talked some then about the low pressure system piping inside containment. The check valve isolation by a normally closed valve is practiced at Indian Point 2, and this was just mentioned in the comment; and this factor was included in the analysis we did.

Then we've indicated here the number of low pressure piping -- I'm sorry, the number isolated by check valves, which were four in these plants, three in the Surry plant. And the number of check valves in each path are indicated here, they being three at Zion and two at Indian Point similar to the Surry plant.

Okay. The next vugraph contains a tabulation of the probabilities we calculated for the dominant sequences for these plants and for the WASH-1400 plant. Let me show you some of the more significant things in here.

One thing is the HF sequences, the S1, S2, and the AHS sequences. Our probabilities for these plants are substantially higher than they were for the WASH-1400 PWR. As we indicated, the S2C sequence we put here is not applicable because the probability was very low, and in fact we didn't calculate it.

As we've just discussed, the probabilities for the V-sequences are lower for two plants by a couple of decades and substantially lower for Indian Point 2. And you'll note the TMLB' sequences have significantly different probability, due mainly to more reliable power supply for these plants.

And so these are a summary of the calculated probabilities for each of the plants we analyzed.

The next step in the process of evaluating coremelt probability is to combine the accident sequence and containment failure modes, calculate the probabilities for the pertinent combinations, and place the combinations in appropriate release categories.

The containment mode probability values are summarized on the next vugraph. As I've indicated earlier, we utilize different values than WASH-1400 for the steam explosion. For the containment isolation failures we utilize the same value as WASH-1400.

I've discussed with you the basis for the overpressure values that we selected. We utilized 0.1 for all the pipe break sequences where WASH-1400 simply utilized a value in the range 0.1 to 0.2 for the large break sequences without sprays.

We utilized 0.8 for TMLB', the same as WASH-1400, and for the meltthrough failure mode, we calculated two cases. In WASH-1400 the probability of containment failure by basemat meltthrough was assigned a residual probability after the other failure modes were subtracted from one. Thus, it was assumed that the containment body would always be violated in the case of a coremelt accident only by basemat meltthrough.

As has been discussed in the NRC technology exchange meetings, water on the basemat at the time of meltthrough is likely and will probably result in the cooling of the core debris on the containment basemat. Because of this possible mechanism, results have been compiled with two different assumptions regarding the probability of the epsilon failure mode.

One employes the WASH-1400 approach, while case two we utilized a value of 10^{-2}.

Now, for each combination of accident sequence and containment failure mode there resulted a particular quantity of fission products released to the environment, and the possible spectrum of fission product releases was divided into seven discrete categories in WASH-1400 for the purpose of evaluating consequences via air pathways.

Each combination of accident sequence and containment failure mode was placed in the most appropriate release category along with its estimated probability of occurrence. Then the probabilities of all entries in each release category were then summed to arrive at the probability of occurrence, the overall probability of occurrence of that release category. We utilized the WASH-1400 approach here also.

The next vugraph summarizes how we categorized the sequences into release categories, and if you just look across here you can determine what these release categories refer to.

Category 1 and category 3 are steam explosions with spray, category 1 being without spray and 3 being with sprays. And you can see the sequences we placed in here, the HF sequences being those without sprays. We split them into low pressure and high pressure at the time of containment failure.

You note the TMLB' sequence at Indian Point was placed in category 1, in category 3 for Zion. This reflects the presence of a diesel-driven, independent diesel-driven spray pump system in Zion.

In category 2, overpressure failures without sprays, category 5, those with sprays. Again, we split the placement of the TMLB' between the two plants. And then 6 and 7 are the melt-through categories with and without sprays.

Sequences in which containment failure results from meltthrough and in which the containment -- I'm sorry. Let me go on to the next vugraph.

Okay. Now, for each plant this process produces a summary table which indicates the probabilities for each of the sequence-containment mode combinations by release category. This is one example for the Indian Point 3 plant. And looking then, just as an example of the AHF sequence, you'll see we placed the steam explosion failure mode in category 1. If the spray drop radiant(?) goes in category 3 as it does in AH, the gamma failure mode without spray was placed in category 2, with the sprays it goes over in category 5 on the AH sequence. And then, of course, 6 and 7 are similar, with and without sprays.

The end point of this process is the total category probability obtained simply by summing down the columns, and these numbers are listed at the bottom for each of the release categories.

The next vugraph then summarizes the results for these three plants, as well as for the WASH-1400 PWR by release category.

Here there are some significant differences. You will note for the category 1 and category 3, the steam explosion sequences our numbers are substantially smaller as a result of the assumption that the probability of steam explosion is less.

In category 2 the numbers for these plants again are smaller, reflecting the reduced probability and the elimination of S2C. The bulk of the probability in 2 is due to the HF sequences.

In category 5 we have higher probability, reflecting the -- we have higher probability than the WASH-1400 plant, reflecting our assumption with respect to overpressure failure. And the numbers in category 6 and 7 are about the same.

Now, some comments regarding the significance of these numbers, in particular with respect to the risk calculations which I'm going to show you in a minute. For the short-term consequences, which include early fatalities, categories 1, 2, and 3 are the only contributors to short-term fatalities, and their contribution decreases with increasing category number. Risk, however, short-term risk, is dominated by category 2 releases with its higher probability of occurrence than categories 1 or 3.

For longer term effects the principal contributors to risk are the category 2 and category 5 releases, so with respect to risk numbers, these two categories of releases are the dominant contributors.

Before presenting the risk results I want to again emphasize the reasons for the differences between these plants and the Surry plant, and the next vugraph indicates for the significant sequences the principal design differences in the plants which influence the probability.

First of all, Zion has a diesel-driven spray pump. There are containment fan coolers at all three of these plants. The Indian Point plants have parallel low pressure recirculation systems. All of these plants have three diesels as opposed to the two that were present in Surry. The Indian Point plants have gas turbines. As we've discussed, the three plants each have check valve test connections. And the last entry, the WASH-1400 PWR had containment spray recirculation separate from the ECCS recirculation, and this, of course, affects the probability of occurrence of the HF sequence which was low in Surry, higher in these plants.

DR. SHEWMON: Sir, will you stop for a minute there? I'm somewhat perplexed, if I understand your column 7, that's containment failure or mat failure with spray.

MR. WALKER: Right.

DR. SHEWMON: I guess partly I don't quite understand where the water goes or why the spray does you no good. But in a different vein, you're saying there's a difference here between the two plants because the spray recirc was in WASH-1400, and it's not, but yet it doesn't seem to make any difference to 6 or 7.

DR. KERR: Is the question clear?

MR. WALKER: No.

DR. SHEWMON: Okay.

MR. WALKER: I'm lost.

DR. SHEWMON: Well, let's start with a simple one. The slide that was up there, just to confuse your slide expert here, said that the containment spray research was separated from ECCS research. What sequence does that impact?

DR. KERR: It's a summary of the differences.

DR. SHEWMON: A summary of the differences.

MR. WALKER: Let's come back to the summary of differences slide. All right.

DR. SHEWMON: What does that last -- what does that impact?

MR. WALKER: This affects the HF sequences, and it also is an indication --

DR. SHEWMON: Is that columns 5, 6, 7 or 8?

MR. WALKER: No. The HF sequences are placed in category 2 in all of the -- if you look back on your slide, they're all placed in category 2.

DR. SHEWMON: Then the other question has to do with why going through the map is so probable if the containment sprays are indeed in operation. Does the water from those run off some place else into the lake or what?

MR. WALKER: No. We believe that it's not probable if you get water down on the basemat. However, in the scoping study we took the approach -- as we indicated, we calculated two cases for category 6 and 7. In case one we said it was highly probable, and in case two we said it was not very probable. Okay?

DR. SHEWMON: Yes.

MR. WALKER: Bob Henry will discuss subsequently that we believe in practically all these sequences you'll have water down in the basemat area.

DR. SHEWMON: And so what you're doing here is for the dry basemat.

MR. WALKER: Yes. The --

DR. KERR: Well, you are, in effect, following the assumptions of WASH-1400, aren't you?

MR. WALKER: Yes. In one case we're simply following the assumptions of WASH-1400. If you have the core drop on to the basemat, it will melt on through. That was the assumption in WASH-1400.

DR. SHEWMON: With the sprays on.

MR. WALKER: With the sprays on.

DR. SHEWMON: Thank you.

DR. OKRENT: Could you put the vugraph on with the probability numbers for each category?

I didn't bring my copy of your document which gives the details on this. I did look at it before. I can't recall whether in the document after you gave these numbers there was any statement suggesting, for example, that there are other accident initiators than the ones you considered, using the approach you've defined, which could lead or might plausibly be expected to lead to a category 1, 2, or 3 type release with a frequency 1, 2, 3, or 4 times greater magnitude.

I don't think there was such a statement.

MR. WALKER: No, there was not.

DR. OKRENT: Do you think that it's plausible that there might be such initiators?

MR. WALKER: Well, we don't think it's likely. However, it is not impossible.

DR. OKRENT: So do you find it unlikely that there could be an initiator that would lead to a category 3 release one, two, or three orders of magnitude compared to 10^{-9}? Is that what you're telling me?

MR. WALKER: Yes. We believe that's -- you're talking about going from 10^{-9} to 10^{-6}, is that correct?

DR. OKRENT: Yes.

MR. WALKER: We think it's not likely.

DR. OKRENT: And I suppose that's true also for your categories 1 and 2.

MR. WALKER: That's right.

DR. OKRENT: I see. Well, I must say I flatly disagree, and I think that represents one of the problems in the use of the probabilistic methodology. You did define what you were doing.

MR. WALKER: Correct.

DR. OKRENT: I think that's proper. But I think you did not identify paths that, let's say, were not included in what you were doing, even though by implication if WASH-1400 didn't do it, you didn't do it, and therefore, you know, you could say well, I really covered it by this blanket kind of declaration. But nevertheless, there is a group of numbers being presented here which are very low.

MR. WALKER: Correct.

DR. OKRENT: And as I think I've indicated to you before, I don't know what the 10^{-9} or even 10^{-7} earthquake is, as just one example, and we have sabotage; and these plants, at least some of them were not all that well designed for fires, and there are a range of things you can quickly think of that in fact are not, I think, covered in either your analysis or the WASH-1400.

So it seems to me that without spelling out at least that here are some areas that might change the numbers significantly, you can say we don't believe they will, but at least saying for the benefit of those who don't follow this business in detail, look, here are some things that we have not included, and there probably are some others, though we can't try to list them all. I would say you have not adequately informed the unsophisticated or the partly sophisticated reader. And I have to include Commissioners for the most part in that group.

I'm not trying to be unfair to them, but they don't have time; there are very few people that have time to -- not only have time but do go into this stuff in detail.

MR. WALKER: Okay. I acknowledge your comment. I think it's worthwhile. I guess we were too close to it in the sense we were picking it up in the Pickard, Lowe, Garrick studies.

DR. KERR: Without trying to defend what they did -- I don't think it needs defense necessarily -- but my understanding was that the original statement which led to much of this activity was one that was based on taking WASH-1400 plants and putting them on the Zion/Indian Point site. There is, therefore, some logic in taking the same approach in analyzing the plants that are actually there. It does not demonstrate necessarily that those plants are safe, but it does give some numbers to compare with the result one gets by taking Surry and putting it on that site.

DR. OKRENT: But there were changes made selectively. We saw changes on steam explosion probability, and we saw --

DR. KERR: I said the same -- presumably these were justified on the basis of the differences in the plants. Whether they actually were or not --

DR. OKRENT: No. Not steam explosion probability, as one example.

DR. KERR: That was a technology --

DR. OKRENT: Look. I can understand doing the analysis on the basis that they did, and in the short time I agree you can't do much more. But I do have a problem with the results being presented and not properly qualified; and I tried to indicate in the way how I think --

I really think you're hurting yourselves, you're hurting the whole business by not adequately qualifying things, because people are going to pick this up and in fact turn it against you, is the only way I can say it.

DR. SIESS: Dave, I can understand your point in terms of the absolute risks, but it's not clear to me whether you are suggesting that these initiators that you say have not been included would be unique to Indian Point and Zion or would also apply to the WASH-1400, and thereby not change the relative values.

DR. OKRENT: They may in fact be dominant. If in fact they believe that the numbers are anything like this for the chains they've looked at, then there are other things that are going to be dominant there, and in fact, they may end up making the probability of these different categories rather similar for the two reactors. I don't know. But it's not inconceivable to me that the things that were not included end up being rather similar.

The safe shutdown earthquake at either Indian Point or Zion is probably of the order of a one in a thousand year thing, roughly, less than one in 10,000 I'd be willing to speculate by the analyses that have been done lately, like one in a thousand. And, you know, as I said, I don't know what it means then to talk about even a one in a 100,000 year kind of thing and what happens in that case, you could be in a category 2 or 3 kind of situation.

DR. SIESS: The things that are neglected you believe then could dominate the risk so much that these changes would be lost.

DR. OKRENT: Yes.

DR. SIESS: Let me ask a question. On the steam explosion assumption, was that included in the WASH-1400 line that I'm looking at?

MR. WALKER: No, it was not. Let me just make one comment on the steam explosion. We basically did the risk calculations, the short-term risk calculations with the steam explosion assumption, and with the steam explosion assumption it didn't make any difference to the risk curve. The reason is that the short-term risk is so dominated by category 2.

DR. OKRENT: I agree, but nevertheless I'm saying you did make changes selectively of that type.

MR. WALKER: Right. Okay. Let me just comment, too, that it will be interesting to see the Pickard, Lowe, Garrick study when those results appear, because they do pick up that earthquake stuff.

DR. LEE: One study that could have helped me gain much greater perspective on the numbers you present as compared to WASH-1400 numbers and so on could have been to redo the Surry analysis with the new numbers, the new technology that you have applied in your mini WASH-1400 study.

Has any attempt or is any attempt being made to approach it in that way?

MR. WALKER: Well, you know, in this kind of a short-term study I guess the only thing I see in here that we've done sufficiently different that would require a change in Surry is the steam explosion values, and that wouldn't make any difference in the curves anyway.

DR. LEE: Well, the check valve, for example, that could also change it.

MR. WALKER: Well, I think with the check valve calculation parallel to the Surry approach, I see no reason to change it in the context of this study. I don't think you'd change Surry.

DR. LEE: What about the deletion of some of the anticipated transient without SCRAM sequences and the (unintelligible 10 reduction in reliability in some sequences because of the presence of shift technical advisers. There are many generic items in nature, so if you redid the calculations for a typical reactor like Surry, you would probably see two orders of magnitude difference here, too.

MR. WALKER: I doubt you would. You may some difference. Dave. Dave Goejer from Westinghouse.

MR. GOEJER: Dave Goejer from Westinghouse.

I think there are two important things with respect to this short circ category with short-term risk assessment, one of which was mentioned by Professor Kerr. The second one, this work was used because in the course of that study we were also doing an evaluation of mitigating features in an attempt to put together the kinds of transients and, if you will, functional requirements for mitigating features that might be evaluated.

But part of the purpose of the study in going through these aspects was to establish which of those sequences were likely to be the ones that were potential candidates as the functional requirements for mitigating features that would be evaluated, so that that effort could be focused and moved forward.

And the recognition made by the utilities very early on that a subsequent study would be done to look more thoroughly while we used this as a basis for the kinds of things that were being done in analysis work and evaluation of mitigating features during the course of the 60-day study and on through the technology interchanges.

DR. KERR: I think we have elaborated this sufficiently.

MR. WALKER: Okay. Let me go on to the risk results. I'm going to do these both in terms of short-term and long-term risk. And first of all the short-term. For the short-term, the risk characteristics, the complementary (inaudible) distribution factors for the reactors at Zion and Indian Point sites were preliminarily calculated, and the probabilities of each release category by using the CRAC code developed for the Reactor Safety Study.

The core inventories of fission products used in the Reactor Safety Study were adjusted proportionally for the respective power outputs of the Zion and Indian Point reactors. Actual site demographic and meteorological data were employed in these calculations.

An annual wind erosion was used. WASH-1400 evacuation models were employed. A more sophisticated representation of the meteorology and evacuation was not feasible within the time available but will be utilized in future work.

And you've seen this curve before. WASH-1400 at the composite site. The two reactors -- I'm sorry -- the Surry reactor is placed at the two sites, and a band here for the Indian Point-Zion reactors, indicating -- well, the curve for the three plants fall within these bounds. I've forgotten which are the upper and lower, but you can pick that off the tabulation in the report quite quickly. That's just a band between the three reactor calculations rather than an uncertainty band.

Next are the results for the long-term risk estimates. Again we used site specific meteorology and demography. They were produced in a different format for the long-term risk. In this instance, long-term consequences were calculated for each release category, assuming that releases of that magnitude occurred. To obtain risk values, the consequence values were multiplied by the probabilities for each release category, and the risk values were summed for all release categories.

This approach simply offers a quick numerical technique for comparative evaluations of the plants at their respective sites, as well as the effect of the proposed design features on long-term risk. And the values tabulated are in terms of Man-Rem.

And you'll see then that going through this exercise the total Man-Rem numbers per year are tabulated out in the far column. The two Indian Point plants and Zion are essentially the same kind of numbers, about a factor of 2 1/2 less than Surry at the Surry site.

I don't have the numbers for Surry at the composite site, but my recollection is that those numbers are a factor of two or three higher than Surry at Surry for comparative purposes.

Okay. The major contributors to risk are summarized in a qualitative way on the next vugraph as calculated in this study. The major contributors to risk are the overpressure failures where fission product removal capability is absent. AHF gamma and the S1-HF gamma are examples.

It is important to recognize that for Indian Point and Zion there are installed fan coolers. The threat is from rapid pressure spikes and not from longterm overpressurization as it might have been at Surry. Values like that for Surry were employed in our analysis, that is, 0.1. This is an important fundamental conservatism in this analysis.

For these sequences at Indian Point and Zion the containment threat is assumed to be from rapid steam pressure spike generated by debris-water interaction and vessel meltthrough or a pressure spike from rapid hydrogen burn. For the sequences of concern, that is, of high probability, spray injection has functioned initially to reduce pressure, and the fan coolers have functioned to keep the ambient pressure at a relatively low value. Thus, the pressure spike is imposed on an initially lower ambient pressure than is true for a case like TMLB' where the ambient pressure at the time of the spike is going to be higher.

The results of our mid-June technology meeting with NRC on containment pressure capability are encouraging, for they tend to show increased containment pressure capability. Also encouraging is the likelihood that hydrogen burn would involve significantly less than 100 percent zirc-water reaction, and that the steam pressure spike will occur over many minutes rather than one minute or less.

All of these factors indicate that more detailed assessments can be expected to show that the magnitude of the pressure spike is less than those generated by conservative analysis and assumptions. The results of our detailed analysis are likely to demonstrate that the probability of containment failure for these sequences is small. Not only could such a demonstration remove these sequences as major risk contributors, but also markedly reduce risk for these plants.

At the intermediate level of risks are sequences like AH gamma and AD gamma, and the corresponding small break sequences. Again, the overpressure threat results from rapid pressure spikes which evaluations are starting to show have little probability of failing containment.

Remembering that it is the pressure spike sequences for which filtered vented containment systems are least effective, so that it's the higher risk sequences which we feel that these systems are least effective. If these sequences were removed, of course the calculated risk values would be quite small. So these sequences have been listed with a question mark.

The effect of the interim order was to reduce the sequence risk contributions such that it is now a small risk contributor, and it should therefore properly be listed as a low risk contributor.

In the intermediate grouping with a question mark is also the TMLB' sequence. It is listed in both intermediate and low risk categories because its risk contribution is different at different plants. At Zion with its diesel-driven spray pump, TMLB' is placed in fission product release category 5, while at Indian Point it's placed in release category 2.

The important point is that there are still system options available to reduce risks from TMLB' if judgment is reached that such additional risk reduction is required.

Filtered vented containment is certainly not the only option for reducing risk from the TMLB' sequence.

I would also like to mention a couple of bounding sequences utilized by NRC and their contractor Sandia for the purpose of evaluating the filtered vented containment system concepts. And while we feel some of these sequences are not important as overall design basis conditions, in particular there is the AB burn sequence. This sequence assumes large pipe break and loss of all AC power. It results in an early and high ambient pressure at the time of the pressure spike. It's a low probability sequence which imposes an unnecessarily severe ambient design condition, and therefore does not seem appropriate to us as a design condition.

Similarly, the S_2C sequence which I've described earlier does not occur in these plants with high probability. It involves early loss of spray injection and failure of the containment before there is sufficient water in the sumps to initiate spray recirculation flow.

Now, the probability of this sequence is very small for these plants, and we think it, too, is not appropriate as a design sequence.

My last vugraph summarizes what we believe are appropriate design sequences for mitigation features. You will note that they have the large, intermediate and small break sequences with loss of either injection or loss of recirculation and sprays. Both the large break and the intermediate and small break sequences need to be considered.

With small and intermediate breaks the sequence which is most limiting would be selected as the basis for design studies and evaluations. We believe it is important to concentrate on sequences which are dominant or important with respect to risks for design evaluations.

NRC employed a similar approach when they selected the S2D sequence for such evaluation. We both selected TMLB' as an important sequence to be examined before our overall risk evaluation was available and based on its importance in the WASH-1400 study and the challenging condition it imposes on containment.

The results of our short-term risk study indicate, however, that TMLB' may not be a major contributor to risk, and for this reason we believe that at a later time it may not be appropriate to utilize it as a design basis for containment protection features.

The WASH-1400 type study reported here is a short-term scoping study useful in providing insight with respect to the dominant sequences. The longer term Pickard, Lowe and Garrick study will provide a much firmer basis for selection of dominant sequences. We expect the sequences we have selected as a result of this study to remain as a sequence appropriate as a basis for design studies. However, until these studies are completed, we believe it is appropriate to continue to look at the TMLB' sequence in our design studies.

Generally, then, our approach to the selection of sequences to serve as a basis for design studies appears to have much in common with that of NRC except for the inclusion of the bounding sequence AB Burn. We do not believe AB Burn is appropriate. There is no unique phenomenology associated with this sequence when it is compared to the other sequences we've indicated on the vugraph. We therefore believe it's appropriate to drop AB Burn as a sequence for design basis studies.

The discussions presented here have indicated the factors which affect the worth in relative risk of the various sequences. This measure is useful in providing guidance in the selection of functional requirements for mitigating features. As suggested by Dr. Kerr at the recent hydrogen technology interchange meeting with NRC, the most desirable objective would be agreement between NRC and the applicants on the basis from which functional requirements for mitigating features can be evaluated and identified. We have derived a set of such sequences from the short-term probabilistic risk assessment to serve as a basis until the more detailed studies are completed this fall.

And that completes my presentation.

DR. KERR: Questions.

Mr. Stratton.

DR. STRATTON: The one experimental result of the TMI-2 accident was the release of xenon and noble gases was pretty large, several million curies; the escape of iodine was very small, about 13 curies, and this is a ratio of between 10^{-5} and 10^{-6}. I think it's quite significant.

Has there been any effort to understand this ratio quantitatively and feed this into your consequence model for these accidents? Certainly the TMI-2 accident would be different from any other in your list, but some of the physical effects that would lead to this interesting ratio might be applicable.

MR. WALKER: The answer is mixed. That was not included in this short-term study. We do intend to examine those results and include them in the fission product source terms we utilize in the longer term study.

DR. STRATTON: Is that the one coming off this other one?

MR. WALKER: Yes, right.

DR. STRATTON: So someone is trying to understand, to examine this experimental result and --

MR. WALKER: Yes.

DR. STRATTON: -- As to why it went this way.

MR. WALKER: We're just initiating that part of the program, but yes, it's our intent to do that.

DR. CYBULSKIS: May I make a comment in that regard, please

DR. KERR: Just a minute.

DR. STRATTON: Who are "we?"

MR. WALKER: Well, the utility group. I guess I have the lead in that particular area.

DR. STRATTON: Thank you.

DR. KERR: Mr. Cybulskis.

DR. CYBULSKIS: With regard to your question, Dr. Stratton, the small releases to the atmosphere of iodine in the case of TMI actually we believe are quite well understood, and the reason why the release of iodine to the atmosphere is low is because all the releases in the case of the TMI incident took place through cold water, all the releases took place through the pressurized relief valve. And to the best of our understanding there was cold water, relatively cold water in the pressurizer when the significant releases were taking place. The iodine was removed by the water and in fact appeared in the water in the containment eventually. Of course, the water has no effect on --

DR. STRATTON: I'm sorry. The escape of iodine to the environment did not go through the pressurizer and the relief valve. It went through the letdown line, to my understanding, to the auxiliary building.

DR. CYBULSKIS: The iodine that eventually wound up outside in the environment may have gone that way. The majority of the iodine inventory was released from the primary system through the containment, through the pressurizing relief valve and had to pass through water to get there. As I understand it, the iodine is in fact in the -- was measured in the water in the containment building, and it did not appear in the containment atmosphere.

DR. STRATTON: Well, I think we're not together yet. May I have just one more crack at it, Mr. Chairman?

The xenon that escaped I assumed followed letdown lines in the auxiliary building, or it was in the tanks and outgassed and leaked out through a bad header and up the stack.

I assume that iodine, being in the water, would have followed the same path, or it would have tried to follow the same path; and so there must have been a proportionate amount of iodine that tried to get to the auxiliary building. Why didn't it escape?

DR. CYBULSKIS: Because the water acts as a -- there's a certain partition coefficient between the water and the atmosphere in the liquid phase, and in fact the iodine in whatever form, presumably molecular iodine, would have been retained by the water, and unless you boiled the water off completely, that iodine would be retained by the water.

DR. STRATTON: Do you have this described quantitatively? I'd most appreciate the document that you have in front of you.

DR. CYBULSKIS: I don't recall how well that is documented Much of this was looked at as part of our effort with the Rogovin inquiry, and I'd be happy to check and see what documentation is available.

DR. STRATTON: Thank you.

DR. KERR: Other questions or comments?

Thank you, Mr. Walker.

Sorry.

DR. SHEWMON: I admit to getting no more than a D, I'm afraid, on having all this stuff committed to memory. An AB Burn, from looking around, would be a large pipe break followed by what?

MR. WALKER: Total loss of power. A coincident large pipe break and loss of power capability.

DR. SHEWMON: Thank you.

DR. KERR: Other questions or comments?

DR. OKRENT: Well, a comment. I think after you have your longer range study, you'll certainly have further insight into what you think is the risk situation, let's say, at Zion. It's not completely clear to me that the question will be closed. There was a lot of work done on WASH-1400. It still had some things that people find warrant revision. I doubt that however capable the group is that's going to do this longer range study that they'll not only be able to think of everything but be able to deal with the things they do think of. Some of these problems are really, you might say, not subject to quantification in a ready fashion, but nevertheless they're real.

It would seem to me that in looking at possible features to mitigate accidents that lead to a large release of radioactive material from the fuel, one might better take a, oh, you can call it a humble attitude to how much he knows or a broad approach, or whatever words you prefer; but to say that these seem to be the dominant features and this is the way we should orient any design looks we're going to take leaves me less than satisfied as an adviser to the NRC. It would leave me less than satisfied if I were the executive vice president of Commonwealth Edison.

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and tried to look at the picture in some kind of a long term, if I look backwards at how many things are occurring each year that are "surprises."

So while I'm not arguing for any specific sequence -- in fact, what I've been arguing, in fact, is that one not, that one not narrow himself to specific sequences -- it seems to me the approach to mitigating features might be done in terms of what are the different engineering approaches that are practical, what happens with each of these, what can they do or what will they not do, and not to tie it down strictly to some alphabet soup.

DR. WALKER: Well, let me just make one comment. And maybe Mr. Peoples wishes to make one, in addition. But I believe in the sequences we have selected we've pretty well covered the ball park of the possible phenomenology we ought to look at.

There's the large break sequences, the small break sequences; and we've gone ahead and carried the transient sequence in the -- we have chosen to do that in the design studies, this one being particularly challenging as a result of the total loss of power.

So I feel like from the standpoint of system sequences we have, at least, selected a bunch that are -- we selected three that are representative of something that's meaningful in probability space and pretty well covered the waterfront on what might be reasonable types of sequences.

Our objection to AB burn (?) is, that's we believe kind of an unnecessarily severe condition to impose on system design features.

DR. SHEWMON: Will we get a chance -- I don't understand what he is saying at all.

DR. OKRENT: Well, there'll be a discussion later.

DR. KERR: What he's saying is that one should be very certain that one hasn't missed anything, that they've taken a general approach to this. He even suggested that they look at specific sequences. He said they have been looking at specific sequences. I think he's saying don't neglect the general work either.

DR. SHEWMON: It sounded like we could believe specific sequences to get us into trouble but we can't believe them to get us out of trouble. So I'm glad yours -- I'd rather have your interpretation, I guess.

DR. OKRENT: I'm not sure what your question is. But --

DR. SHEWMON: My question is to the Chairman as to whether we'd have to discuss this amongst ourselves, because I --

DR. KERR: We have an hour scheduled this afternoon to do that. (WORDS UNINTELLIGIBLE).

Do you have comments or questions?

DR. OKRENT: Nor I.

DR. KERR: Mr. Walker, I think my revised schedule shows Mr. Garrick as the next presenter. Is that correct?

(Pause)

It's getting clear that we are not on schedule. And there are people who would like to eat lunch. I'm going to suggest that after this presentation we take a break for lunch.

DR. GARRICK: My name is John Garrick. I'm with Pickard, Lowe and Garrick and am managing the long-term study.

I say "long-term" cautiously, because I've had a hard time convincing myself that six to seven months is long-term. And I think it's important to put the study in perspective. As far as the man years are concerned, we're talking about an effort somewhere of the order of 10 percent of the productive part of the effort of WASH-1400, not counting the peer review or any of the follow-on activities.

I will give a kind of an overview of the study. Unfortunately, we're not far along, enough along to be able to present results, as Dee (?) was able to do in the short-term study. But I think what we can do is give you a pretty good idea of what we're up to.

Conceptually I am confident that we are addressing the questions that Dave Okrent has raised, particularly with respect to quality assurance and with respect to uncertainty. And I guess the other word that people have been using to mean quality assurance has been the word "scrutability," so we have --

(Laughter)

-- we have tried desperately to found our analysis with those things, those objectives in mind.

Now, I will be supported in answering questions by other members of the team. I want to stress something that came up today, and that is that while we do have responsibility for the long-term risk analysis, this effort has indeed been a team effort between the three utilities involved, other consultants they have, and Westinghouse.

The people that are here to support me are Stan Kaplan and George Apostolakis, and our scenario expert had to leave because of an urgent matter that developed and he just left, and that was Dennis Black. In those areas we'll lean on people from other parts of the team, like Bob Henry and Ward Lokslan (?).

What I'd like to do is give you some idea of what we see as the purpose of the long-term risk analysis. Fundamental in this area of trying to address questions of uncertainty and questions of quality assurance is the concept of risk that's been adopted, and of course the bigger picture of that -- the methodology and the general approach. And most of the remarks that I will make will deal with the general approach or the methodology. And at the closing of my presentation I'll give a little thumbnail sketch of just where the project is.

As far as what our objective is is concerned, or what our objective is, naturally, what we want to do with this study is quantify the risk. And by that we mean the health and safety risk, and we are having a considerable amount of dialogue at the present time as to if for part of this we also want to quantify property damage.

Two outputs o f the study that we view as extremely important are the next two, namely, the quantitative basis for evaluating the impact on risk of plant modifications -- it's highly desirable when you are asked to do something, be it procedural or be it to install a new pie of equipment, to be able to look into the analysis and see what the impact of that change is on the risk curves -- we're also trying to be quite responsive to the current interest in emergency planning and how you can use quantitative risk analysis to assist you in that area, particularly with emphasis on the site-specific part of the problem.

And finally, maybe as generic a problem as we have is the problem of training, and we have high expectations that the output of the study will aid in that aspect considerably. And there are two perspectives there. There's the perspective that has to do with transferring this kind of thought process, this kind of technology into the utilities such that they can perform their own risk analyses. And then there is the perspective that provides deliverables such as the systems material, the systems analyses, the scenarios themselves, that turn out to be extremely valuable aids in training.

So that's what we're attempting to do.

It's important to ask the question of what do we mean

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by a risk analysis. And without getting complicated, what we're trying to do is answer some very simple questions. We want to know what can happen, what kind of things can go wrong. And we'd like to know what the likelihood of that is, and as a part of that we would like to know what our uncertainty is. And finally we would like to know what are the consequences, what is the damage.

And the format of the answer, the format of the answer is kind of summarized here -- in three different forms, as a matter of fact.

The way we like to look at a risk analysis is that it's essentially a list: it's a list of scenarios, it's a list of things that can go wrong. And the whole effort in risk analysis is trying to complete that list, trying to make that list as meaningful as one can.

And of course in something where there are millions of such scenarios, one ends up categorizing the scenarios.

But attendant with each scenario, and what really defines the scenario, is its likelihood, or its frequency of occurrence. And so we must address the question of how frequent and then how -- and finally we must ask ourselves what's the impact. And so the scenario is really defined by the impact and its frequency.

And so that in itself could be the output of a risk analysis -- simply a table. Another way of characterizing this table is to characterize it as a set of triplets; that is to say, a risk analysis is a set of L triplets, or the set of triplets such that we have essentially completed the space of scenarios that can contribute to risk.

And more familiar, a more familiar form of the risk analysis is what has become pretty much adopted as the risk curve. And the risk curve is nothing more than this table arranged and put in this form. And in particular, if you arrange this in order of increasing damage and accumulate it from the bottom up and plot that information, it becomes the risk curve.

Dave Okrent set -- set us up for this. What we have to do in our risk analysis, of course, is, express our state of knowledge about what we know and what we don't know. We know there is uncertainty about what these -- the frequency of occurrence of these scenarios. And the way in which we express that uncertainty is to express it as a probability of that frequency. Similarly, there is uncertainty with respect to damage, and again the way we express that uncertainty is with the probability.

And so our risk curve becomes a family of curves, each curve being a probability. We may choose to have it the 5-percent curve, the 25-percent curve, the median curve, the 75-, the 95-. So this gives us a full picture of the risk in terms of what the frequency of damage is and what is -- what our uncertainty is. So conceptually, at least, the whole effort is to try to construct that curve.

Now, there's one other little point we want to make here. We all know that when we attempt to do a risk analysis there is always the question of what did you leave out, what have you forgotten. Well, the truth is, you do your best to not leave anything out, but you know that you can't always think of everything. But one thing you can do is make some allowance for having not been able to think of everything. And so, in a sense, you can treat that as a scenario category just as you treat any other scenario. And so the problem now is not so much what you have left out but, rather, what frequency shall we assign to it and what kind of damage do we anticipate from it.

So at least this is -- this is the fuller framework that we're trying to -- to implement.

When we talk about risk analysis, therefore, then we're talking primarily about a search for scenarios. And so let's talk a little bit about how we're structuring the scenarios.

A risk analysis tends to fall in[illegible] major segments, and that's the segment having to do with e[illegible]ing the source condition and the segment having to do with, given a source condition, what is the frequency of different levels of damage. And so it turns out to be a convenient way to conduct a risk analysis, and these activities can go concurrently.

So you might say this is the risk curve where the damage is release category. Damage could be any -- anything. I just did one where the level of damage was not release, or not release category, or not dose, but rather temperature.

So in the first part of the analysis, that is to say, the scenario part of the analysis that takes you through the fuel damage sequences, through the containment analysis, to a point of release, we develop a risk curve.

Similarly, given that release condition, or a release category, or release states, we can proceed immediately with determining what the consequences are -- and that's the same kind of exercise. It's a matter of given a -- it's a matter of developing scenarios, weather scenarios, evacuation scenarios.

And then the problem becomes one of combining these two risk curves into our final risk curve. So we structure the scenarios to allow us to do that.

One of the early tasks in the whole process, of course, is trying to develop a list of initiators, because the initiator is kind of the cornerstone, or the beginning point, of the scenario.

A way to do this that has worked pretty well for us is to start with something we call the master logic diagram. The perspective on this diagram is to try to start with the top event and at each level be complete in how things can happen with respect to the upper level.

So, in a sense, this might be viewed as a logic diagram, or a master logic diagram, for Zion and Indian Point. And we take this to a level to where we begin to start asking questions about what are the initiators. And again, we have to remember that these kinds of things are categories of initiators and can be subdivided. But the point we try to achieve is that we are complete here. And this later becomes valuable because when we start making the calculations and start working up this then we can put the contribution of each of these boxes to the top event. So it becomes a reasonably effective way to communicate where things are happening.

Now, the structuring of the scenarios involves a lot of models. And there's a -- there's no particular right way to develop the scenario. It's whatever way seems to work the best. But the -- there does seem to be some categories of models that fit pretty nicely. For example, there are the system models, by which we mean those models that map from failure modes to component failure modes to system failure modes; and the most common tool for doing that has been the fault tree. There's the plant model, and that is the model that takes it from the initiating event, such as was shown -- at least, categories of such -- in the previous slide, taking the initiating event to the system and interaction -- to system and human interaction to fuel damage; often this is done in the form of an event tree. There's the containment model, which tends to pick it up from the position of fuel damage and reactor containment systems to containment release. And then finally there is the atmospheric dispersion and health effects model.

Now, another way to kind of characterize this process a little bit and to extend it just a little beyond what the prior slide did -- at the plant level we have the model that maps the systems to the plant, and of course we have a series of initiators that feed into our scenario. The scenario format is most often an event tree.

Now, these branch points tend to be systems, but they can also be functions. But whatever they are, we must establish what their probability of occurrence or not occurring really is. And this is where we often go to the fault tree concept.

So -- and then finally we go to the cause level. And here is a kind of a fundamental point, that we think enhances the quality assurance aspect of the problem. And that is that we limit the fault tree to the task of mapping hardware. We do not ask the fault tree to map causes.

We treat the causes separately. And we treat them in the same way that we structure a list. We delineate the causes: there are hardware causes; there are coincident random failure causes; there are environmental effects; there are human errors; there's combination of those; and there are causes that are common to different systems in our sequence. So the problem is to construct a list of such causes.

There's just one minor point we want to make on the event tree. And that is that as far as the plant model is

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concerned, one is not limited to two-state systems; there can be as many states or as necessary to represent or model the problem.

DR. KERR: Mr. Garrick, excuse me. I am a little bit puzzled as to what I should be learning from your presentation. I could get the impression that you're giving us an introduction to risk analysis, and I don't think that that's what you have in mind. So tell me what -- what it is that you are telling us.

DR. GARRICK: Okay. What, really what I'm trying to do is to address the question of how we're developing the various steps the total of which constitute a risk analysis, to -- as it relates to Zion and Indian Point, how we are handling some of the important facets of it.

I guess another way to try to characterize what we're trying to do is, there have been three or four areas where there's been quite a bit of work done since WASH-1400, and those areas are things like how do you handle data and the extension of some of the ideas that were used in WASH-1400 for handling data, the question of how do you address the matter of site-specific consequences. So I was -- I wanted to try to at least highlight that, because this is --

DR. KERR: I think if what you are doing is to show us how your analysis is going to depart from the procedures used in WASH-1400, if you highlight those things, I think, that'd be helpful.

DR. GARRICK: Okay. We'll do that.

DR. KERR: Okay.

DR. GARRICK: I think maybe what I'll do is jump to the form of the system analysis.

DR. KERR: Okay.

DR. GARRICK: Okay. Of course, one of the things that have proven to be extremely valuable in risk analysis is the systems analysis, because it puts in one single place the analysis of the system, the data that we used, the mission of the system, the sensitivity of the system, the sensitivity of the overall risk to the system. And here what, I guess, I want to emphasize is that we are drawing a separation between the hardware part of the problem and the cause part of the problem, so that for each system we will develop a cause list, and that, that cause such that we ask the system, well, you see, you have a variety of causes that you will see: you will see random failures and how do you respond to that; you will see human error and how do you respond to that. So that the thing that we're -- we have tried to do, in order to keep the problem as tractable as possible, is to separate that and pull it out of the logic of the hardware mapping.

The next slide is kind of a format that we've adopted, where we have these causes or cause sets and we have the frequency of occurrence of the causes, what responses are possible -- in other words, some of them may be the kind of causes where human interaction or some other impact can have an effect --

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we may, we want to know, well, what's the probability that there will be that kind of response, and of course what's the impact of these causes on the components in the particular model that we're looking at, and in the system, and are there systems, and, for that matter, on the initiating event. So -- and this, indeed, is kind of an infinite list.

And what you attempt to do is to go down this systematically, and in most instances you do find that you get to a point beyond which the results that you're getting are not contributing to system performance.

The other thing that is a little different from WASH-1400 is the question of the ex-plant consequences, that is to say, the health effects and how do you get to the health effects and how do you get to the consequences given a source condition.

What we have done there is, we have extended the CRAFT code. This is basically a diagram of the CRAFT code. And the places we have made changes are the places that have anything to do with specific sites.

And of course there are two or three areas that are most -- seem to be most important in order for the consequence analysis to have credibility. One of the areas is that when you are at a specific site you are no longer in as good a position to make simplifying assumptions about straight-line trajectories of plumes and straight-line evacuations as you are in a composite site. So that was one area that had to be expanded and redeveloped.

The other area that had to have quite a bit of development work in was to account for very special effects, such as at the Indian Point site there is a considerable number of questions that have been raised about the effect of terrain and the site-specific meteorology, and in the case of Zion the effect that's of greatest interest is the lake effect. And I think that I have a Vu-graph here that tends to highlight some of the main differences, and I'll only -- I only show it to show, mention a couple.

The main differences between the consequence model that we're using and the WASH-1400 is that we have built in it the ability to move the plume around, dependent upon wind direction. Doing that has enabled us, therefore, to model evacuation, the evacuation options that are available to us, rather than perhaps ideal lines.

The other thing is, if you do have the ability to move the plume around and if it turns out that there are some -- some contribution to risk from distances quite far away from the plant, then, of course, you would like to be able to take advantage of local meteorology as much as possible.

So those, those three things plus these other items, some of which are not too heavy of impact, are the main differences.

And I put these slides in this morning, and these two or three slides are not in your handout, but I can make them available if they are of interest.

Just to give a kind of a pictorial demonstration of what we're talking about here, if this represents the plume path such that you can move the plume depending upon the weather, this can represent an evacuation path, and the gray areas are the regions where dose calculations were made. So one can put in whatever weather scenarios he wishes and whatever evacuation routes and, at least, get some ide^ as to whether or not having this kind of extra capability makes much difference.

Okay, one of the other things that we want to talk about briefly, and that there has been quite a bit of work since WASH-1400, is this question of data handling. And we, in the data handling we, want to focus on specific equipment, specific failure modes, or maybe specific initiating events. What we really desire from this information is a failure rate or an occurrence frequency. And what we want to be able to do is take full advantage of all the information at our disposal in establishing what those numbers ought to be.

And of course the information you like very much is historic information. We'd like to have a time histogram on each item.

But you want to be sure that your approach takes advantage of any other information that you have -- design information, history of similar equipment.

And then finally, in the handling of the data, you want

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to be sure that you systematically utilize, combine your information such that it does, indeed, represent your state of knowledge.

So that's what's motivated us.

And here is some models that we use for implementation.

There is a typo error here. This is the prior distribution rather than the posterior. I think it is correct in your handout.

But we've sort of separated the data handling question into these kinds of groupings. Model one is the case where you put forth a number, a frequency of occurrence or a failure rate, and this comes from wherever you can get it, but because you want to tell the truth about that number, you express your uncertainty about that number as a probability distribution. And then you perhaps get some information that you want to take account for. You might come upon some data of operating that machine. And the way in which we take account of that new evidence on this distribution is through Bayes' theorem, and that gives us the posterior that if this is the model we adopt then that's the input we'd adopt to our analysis.

Model two, and a little better model, is what we call the variability population; in other words, we have a population of such machines, and we can express that population this time as truly a variable, a fluctuating random variable, based on measurement. So now we have a population distribution of the frequency that we're trying to utilize in our analysis.

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Now, what we have to do, though, is, we have to put forth our state of knowledge about the item of interest, about the machine that we're buying. And in the absence of anything else, we probably would put forth that to be essentially the same as our population variability curve. If we have some other information, then this curve would reflect that.

In the meantime we get some information -- decay failures and M trials. And so we want to -- such as you might get, this might represent a generic distribution that we get from all plants, all PWRs, but now we have some Indian Point and Zion data and we want to specialize this to Zion and Indian Point. And again the way we do that is through Bayes' theorem.

Now, while we haven't carried it this far, the principle becomes the same, is that if aging turns out to be something of particular importance, we want to incorporate that into our model as well.

DR. LEE: Could you perhaps say or go through about the difference in uniform population modeling, variable population modeling in curves?

DR. GARRICK: Well, all we've got here is, this is our -- this is the number we put out as the number that we think is representative of the failure rate of that item. Now, what I'm -- so that's a fixed number. That's a number. That's not a random variable; it's not anything. But we're uncertain about that number. So all we're saying with model one is that we express our uncertainty in this fashion.

Now, here we have -- we have lots of machines that we have operated, each of which has its own failure rate, and we plot those failure rates and actually develop a population variability.

DR. LEE: So whenever possible you would use your variable population model?

DR. GARRICK: Yes.

DR. LEE: To determine the prior distribution?

DR. GARRICK: Absolutely.

DR. LEE: Okay, thank you.

DR. GARRICK: The approach is that we use all the information we can get. And that's why we keep calling it the state-of-knowledge approach.

For example, on model two-A we may get a number from WASH-1400, and we may use that as the basis for our prior. Or we may choose to go behind that and ask what was the detailed data that led to that distribution or any other. So we may back up as far -- further. But the process again is the same.

DR. KERR: Mr. Garrick.

DR. GARRICK: Yes?

DR. KERR: About how much additional time do you expect to take to finish this?

DR. GARRICK: I'm all through -- I'm about through. I think about two minutes.

So what I tried to do here is to highlight two or three areas where the emphasis and the perspective is a little different than WASH-1400. One of those is that in the way in which the scenarios are constructed, we try to give a little more emphasis to the event tree as far as using the event tree to account for system interaction and human interaction; also, as a part of the scenario, we have tried to establish some guidelines on what constitutes an event -- or a fault tree, what it should do, and some guidelines on how to develop the causes and the form of those causes, such that we have, essentially, a book on each system. Thirdly, we've talked about the data and trying to propagate uncertainty through the model; that is to say, all of the models will have input to them distributions, the distributions will take the form of whatever our state of knowledge will dictate, and we'll propagate that through the event -- fault trees, the event trees, and then in the combination process. And finally, the consequence model has been modified to accommodate site-specific characteristics in general, A), and B), in particular with respect to the peculiarities of Zion and Indian Point.

And as to where we are, this is a kind of a quick summary as to where the project is. As far as the scenario construction is concerned, that is very well along, and we're primarily exchanging scenarios now and trying to validate them and finalize them. The quantification process, which is about 10

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percent of the total effort, is about 20 percent complete. The inplant consequences, by which we mean the development of things like the containment event tree and the -- picking it up from the fuel damage, frequency of fuel damage to the release, that's about 20 percent done. And the ex-plant consequences, which is a substantial part of the effort, is about -- is quite well along. And so forth.

As you can see, if you take the consequences and the systems analysis, which is kind of this, together with the report preparation, we really are talking about about 80 percent of the effort.

DR. KERR: And what is the probability that they will be completed on schedule?

(Laughter)

DR. GARRICK: I have my own histogram on that. We're giving a -- we have a high confidence that it will be completed on schedule.

DR. KERR: Thank you.

Are there questions?

I will declare a one-hour recess. We will reconvene at ten after two.

END TAPE 6

(Whereupon, at 1:10 p.m., the meeting recessed, to reconvene at 2:10 p.m. this same day.)

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

MR. KERR: On the record. Mr. Garrick, will you continue, please?

MR. GARRICK: Yes. In view of the guidance you gave me earlier before lunch, trying to address the specific differences between what we're doing and WASH-1400, let me just summarize those again. They are primarily, first of all, in the data handling area, and the principal difference there is that we're representing the data first in some sort of generic form and then we're specializing it to the specific plant based on plant-specific information. And the output will be in the form of a probability curve.

The data handling is also involving when we have to use something like WASH-1400 data, using it with our state of confidenc or state of knowledge. That means where they might have data that is a 90% confidence interval, we have found that in order to give us the added confidence that we need, often we treat that as a 60% confidence interval.

We're trying to take full advantage in the data handling area of the Human Error Handbook. We've done an LER analysis, particularly to find some things that you saw on the cause table like analysis error, and see what the contribution is there. The other somewhat distinguishing feature of the data handling activity is the cause table. Trying to construct the cause table that displays, on a system-by-system basis, just exactly what the input is and where it came from and how it's used.

T he second area that is quite different from WASH-1400 is in what I recently described as the scenario area. We are spending a lot more time with respect to the containment scenarios and also, of course, in the conduct of that, addressing the greater than 2200 degree scenario, rather than progressing to melt.

Consequences -- I think I said enough about that. The primary thing there is to make the consequence model site-specific and that primarily has to do with accounting for the peculiarities of the site, accounting for direction or dependence of the plume, accounting for evacuation models options available to the site.

Earthquake is another area that's very different from WASH-1400, or for that matter, from any risk effort. There, what we're doing for each site is developing an earthquake frequency curve in the same form that we showed you at the outset as to what a risk curve looks like. Namely, the probability of frequency of occurrence of certain kinds of earthquakes. We're representing earthquake in terms of equivalent peak ground acceleration, and from there we go to fragility curves, by which we mean failure curves against peak acceleration or against earthquake type. And we're also handling uncertainty and dependencies in the earthquakes.

That model we have had quite a bit of experience with so this isn't really the first time, except we're refining it and fine tuning it a little bit.

So those are --

MR. KERR: Will you be looking at earthquakes of magnitude larger than the historical earthquake of the region?

MR. GARRICK: We were trying to do that. We're going to the expert seismologists and we're asking them to develop a probability of frequency curve of earthquakes for that site. So in a sense, this information should tell us what the frequency of occurrence of any earthquake is at that site.

MR. SIESS: Where has your experience with this? You said you've had quite a bit of experience with earthquakes.

MR. GARRICK: As far as analyzing the earthquake in a risk model is concerned, our principal experience is the two-year study we performed for Jersey Central Power and Light on Oyster Creek.

MR. KERR: Thank you. Are there other questions? I'm told that the next presentation will be made by Dr. Henry.

MR. HENRY: Chairman Kerr, members of the Committee, my name is Bob Henry, I'm from Fauske Associates, and I'd like to discuss with you this afternoon very briefly the efforts that we had during the 60-day study relating to steam explosions and core coolability. In the interest of time, I'll be somewhat brief with summary statements. If you'd like to have some more details, please feel free to ask me at any point. I'd also, then, like to discuss with you our continued thinking along these matters that relate to core coolability that have been developed since the end of the 60-day study.

I'd first like to address steam explosions, which was discussed this morning, and as was discussed by Dr. Walker, there were two probabilities set and this was more or less a result of the fact that we saw this particular phenomenon being addressed in two different fashions that differed with scenarios. One was related to the in-vessel steam explosions; as you recall, in WASH-1400 it was the in-vessel steam explosion which caused a rupture -- it was calculated to cause a rupture of the pressure vessel, make make it missile out of part of the pressure vessel and it was that missile which then ruptured the containment.

We broke it down into the in-vessel steam explosions -- to be a little bit more definitive, in terms of those which were occurring at elevated system pressures, it would be expected to occur at elevated system pressures which might be something like the S2D sequence or TMLB prime, and those which would be of lower system pressures such as the large break sequence.

Given the analyses and experimental data which were available for pressures greater than 10 atmosphere is 150 psia, we do not feel that steam explosions would occur.

On lower system pressures --

MR. KERR: Excuse me. What is meant by the statement that we do not feel steam explosions would occur?

MR. HENRY: The experimental data shows that at pressures above 150 psi, explosions have not been able to be produced. The analyses also predict that in that area, since it is a boiling phenomenon, boiling would become more benign at elevated pressures.

I think the best justification for this that one has right now is the available experimental data with similar fluids and with real reactor materials, but on different size scales. And if I had to assign a probability of a steam explosion at high pressures, it would be zero, in my own estimation.

MR. OKRENT: Excuse me. I vaguely recall hearing a paper by somebody from Sandia in which he suggested that you could have circumstances, if I remember correctly, a strong enough trigger, that you could induce the explosion above 150 psi. Is that a difference of technical opinion or what?

MR. HENRY: I think that's well-characterized as a difference of technical opinion. I don't mean to misrepresent anybody else's ideas. In that paper, their feeling, if I could characterize it, they're suggesting that the explosion is, indeed, trigger-dependent. And certainly, experiments have been done which demonstrate that the trigger has something to do with the point at which pressure cuts off. But the experiments that have been done appear to show that the pressure overwhelms the trigger very early, if you will, in the event.

Let me state it a little differently. The experiments which were done with similar fluids show that at one atmosphere you can have explosions; from one to two atmospheres you would not have any explosions but a strong trigger would induce them at two atmospheres; but at three atmospheres even a strong trigger wouldn't induce the explosion. So there is a very slight dependence on trigger which appears to be overwhelmed at pressures of something like this.

On the other hand, if one deals with a depressurized system, lower system pressures, we feel the probability of a steam explosion would have to be assumed to be unity. However, given any reasonable levels of fragmentation which would lead to an explosion, we cannot identify any which one would be able to generate a continuous overlaying liquid layer. It was that continuous overlaying liquid layer in WASH-1400, for instance, which resulted in rupture of the pressure vessel and then consequently made a missile out of the vessel head.

In this environment, steam explosion would most likely resemble a shallow underwater explosion. Consequently, we can't identify any way in which the continuous overlaying liquid layer could be produced since the probability would be one, we'd also have to say that the probability of a vessel failure by steam explosion is insignificant. Because without this, you don't have a vessel failure. The pressure from the explosion itself is less than the steady state operating pressure that the system has been designed for.

MR. SHEWMON: By continuous overlaying liquid layer you mean a thick enough layer to act as a piston or have a fair amount of momentum to -- it is not accelerated.

MR. HENRY: Exactly. If I remember the numbers, for instance, one of the transients which was shown in WASH-1400, the pressure which was driving the slug did go super-critical with the pressure that the slug generated when it was accelerated over several tons of feet and brought to rest very rapidly, it far exceeded the ultimate stress which was like 10,000 psi. Excuse me, it developed pressures which were in excess of 10,000 psi. That was a failure mechanism.

MR. OKRENT: This same Sandia talk or an accompanying one seems to me -- and maybe a paper from Sandia -- have raised a question about whether a steam explosion under what you have as Category 2 could bother the steam generator integrity. Do you have any comments in that area?

MR. HENRY: That's certainly one thing that we're looking into, and I think, if I can give you the benefit of a most current thought, I believe if I can again characterize their thought processes correctly, it's not necessarily just a steam explosion; they're just looking at rapid steam production. In other words, there wasn't a shock wave that was required; it was merely just very high pressure steam production in the primary system over a time scale which was short compared to the heat sinks.

We have, indeed, begun looking into that. We are currently calculating pressures which do not appear to test the integrity of the steam generators. That wasn't included in the

60-day study and I was not going to address that today, but I would certainly be happy to at any future time.

The calculation is basically one of just coming to equilibrium in a very short time inside the primary system. It was quenching the core very rapidly when you already started off with a pressure of about 2000 psi.

MR. OKRENT: I'll wait to hear about this at some future time, then.

MR. HENRY: In addition to the in-vessel considerations, we also then looked at ex-vessel. Of course, as you recall in WASH-1400 the assessment was made that this had no logical way of failing the containment via the steam explosion itself. I think that one would also have to assume a probability of unity. It's difficult to assign anything other than that, since it's already low pressure, shallow in the water. An allogenic(?) is particularly relevant here because one's dealing with fairly short depths, small depths of liquid. There's also a very short length of any slug acceleration. In these particular designs, the in-core instrument shaft is a vent for the explosion which means that there is no real long-term acceleration and consequent missile potential. And the shock waves themselves, from a very energetic explosion inthe reactor cavity, by the time it expands to the wall would be not anything close to the design pressures. In other words, they would not fail the containment wall themselves. Again, this is in agreement with what was done in WASH-1400.

In summary, for the steam explosions, in-vessel elevated system pressure sequences which -- for instance, TMI with the minimum pressure is about 400 psi -- we feel steam explosions would not occur based on the applicable experimental data.

At lower system pressures, steam explosions can occur but will not fail the reactor vessel because there's no way of identifying any way of achieving any continuous overlaying liquid layer which is necessary for failure.

And the ex-vessel steam explosions can occur with the shock waves generated at much less than the containment design pressure.

I think, to try and highlight the --

MR. PLESSET: I just wanted to make sure that Dr. Okrent's point is not missed too easily. I think by ruling out steam explosions at higher pressure, you do rule out the possibility of steam generator damage, for example. And I think it's terribly important to be sure of that. And I think we ought to get more information on it.

MR. HENRY: Excuse me, Professor Plesset, I didn't mean to rule it out because all I'm saying is that the steam -- you do not have to have a steam explosion in order to calculate the kinds of pressures that were referenced by Dr. Okrent. It's merely a very rapid steam production; it's not a shock wave pressure that we're talking about. We're just calculating the system coming to equilibrium which gave them pressures of about

4000 psi.

MR. PLESSET: I was going back to the question of a steam explosion at higher pressure. If it does occur, then you could get steam generator damage, for example; if you're already up at pressure, then you'd have --

MR. HENRY: You would surely get rapid steam production, yes.

MR. PLESSET: So I think it's an important point and I just want to be sure that we don't forget it.

MR. HENRY: It is a very important point and I just want to make sure that what I said is interpreted correctly. You don't have to have a steam explosion to get rapid steam production at high pressures, also. Because we say ruling out steam explosions at high pressures, we're not saying that you couldn't have significant steam production.

MR. OKRENT: It is sort of a path out of containment. Now, you might or might not be able to isolate.

MR. HENRY: I think it's also something where we have to be a bit more definitive on the way we treat the heat sinks within the primary system before we can come up with a number which is, indeed, worthy of discussion on a technical basis because just to say it comes to equilibrium is not sufficient. Because in the primary system you obviously have a lot of heat sinks which act much faster than they do outside of the containment.

MR. SHEWMON: Would you briefly tell me the difference between an explosion and rapid steam generation in your mind? Talk about time constants or something.

MR. HENRY: Time constants is certainly one way that somebody might choose to characterize it. I would choose to characterize it in the following fashion. Steam explosion is one wherein the vapor is generated at a sufficient rate so that the system cannot acoustically relieve itself. In other words, it can produce a shock wave, and the shock wave itself would then have to be analyzed in terms of its ability to do damage.

On the other hand, we could have steam production over a time scale several orders of magnitude longer, like a few seconds but still faster than the heat sinks within the system could be active. So you could essentially come up with the same final pressure, but the rate or path at which you get there could be far different. And the individual concerns that you would have over the two events could be considerably different.

MR. SHEWMON: With these few seconds it could also be much faster than the pressure relief valve which, for example, could open and relieve it.

MR. HENRY: It could be, yes. I don't mean to minimize the problem. I think we feel that that's one area which there certainly must be more definitive treatment so that we can indeed identify whether or not the steam generators are the bypass mode.

Given all the organizations which have looked at this particular problem, as I say here, the method of calculation, I should say also the mechanisms which have been treated, are somewhat different. The various organizations all appear to conclude that the failure of the containment structure as a result of either in-vessel or ex-vessel steam explosion is highly unlikely. And here I include the utility group, the group from Los Alamos, and their reason for this, or at least one of the reasons, was that they felt that the bottom of this system, the bottom of the reactor vessel, would fail before you could even accelerate the slug to the top of the vessel. And also, Sandia felt that the failure, if anything, must be a fairly small part of the primary system such as the control rod drive mechanism, which they felt then would not be failing the containment.

MR. OKRENT: By the way, where do Bourne and Ball sit these days with regard to the question of whether you can get steam explosions let's say at high pressure or have they offered any opinions on whether they think they can be severe enough to disagree with either of the two conclusions you've got there which, in fact, Sandia does seem to agree with?

MR. HENRY: Let me take the second part of your question first. In discussions with them, I have not had any discussions with them for the past year, but they offered no real opinion as to whether the containment structure could be failed.

On the other hand, if I could characterize Dr. Bourne's comments at the last meeting in Bournemouth, he certainly -- it's his opinion that this type of thing must be trigger-dependent. Certainly, in that aspect his technical view is not that different from the paper you talked about at Sandia; that one could induce contact and once you induce contact you must be able to force it from there on.

MR. ETHERINGTON: In all these cases you're considering water layer on top of the melt?

MR. HENRY: In these cases we're considering an intimate mixture of the melt and water.

MR. ETHERINGTON: Whether the (?) is introduced from above or below makes no difference?

MR. HENRY: That's correct. We did not look at it as making any specific difference in this formulation. I think the following speaker will identify some of the problems that the lower structure of the reactor vessel provides in getting any kind of intimate mixture. In that case, that's also -- in dealing with that particular sequence then, one would have to treat those specific characteristics of the vessel design, for instance.

I'd like to address this high pressure part when we get into core coolability because I think it plays a logical role there.

We also then evaluated coolability of the core, and we addressed it from both an in-vessel and an ex-vessel standpoint.

I'd like to make a couple of points. These reactors have I think give several options, let's say, to being able to remove the decay power within the core region, assuming a badly damaged core.

For the sake of argument, let's assume that the damage has progressed to the point where the inlet is totally blocked, so the only mechanism by which one could cool is from above. The particular plants such as Zion and Indian Point, have the potential for injecting into the hot legs when the pressure is below something like 1500 psi. They also have vertical steam generators and they are elevated so that one has the possibility of setting up a heat removal path between the damaged core and the steam generators which only relies on the outlet leg. In other words, steam going up this way, water returning back down like so.

MR. ETHERINGTON: Do you visualize that the core debris could stay in there like that?

MR. HENRY: Yes. I visualize that this could, indeed, remain here like this. In a long-term cooling mode, as long as the water is available and as long as you're extracting the heat down from the steam generator.

MR. ETHERINGTON: You mean all of the heat from the bottom that's coming up by conduction through the --

MR. HENRY: No. This is being cooled as a debris bed. Water is draining down through the debris and vapor is coming up. It's a countercurrent flow of liquid and vapor.

MR. ETHERINGTON: Is there evidence that the water can drain to the bottom without being held up by the escaping steam?

MR. HENRY: Yes, there are models which describe that. In fact, Don Paddleford was talking about that exact type of formulation this morning when they provided an evaluation for TMI.

I'd like to show you the calculations we have to describe that and also the sensitivities that it has to some parameters in the model.

Since this is a vertical U-tube steam generator, then one has -- for limited damage to the core of, say, 50% and the core oxidized, for instance, the hydrogen cannot totally block the steam generators and one could just pressurize the hydrogen to the top of the steam generator and still have the heat sink available. So there's another very important difference between that, for instance, and TMI.

MR. LEE: So you expect that you can have a flooding situation there with a water returning?

MR. HENRY: Yes.

MR. LEE: What guarantee is there that you can indeed have such a situation? Under what circumstances could you expect the flooding taking place?

MR. HENRY: The first and most important circumstance is that one must have water available to the top of the core. And these particular plants have the capability of putting water in at this point. The first point I made was that we assume that the bottom is totally blocked so there's no entry path to cool the core from the downcomer; that's the assumption. If the bottom is not totally blocked, then of course one can also put water in the cold legs, which allows you a continuous path from this way to remove the heat.

What I'd like to discuss with you is what provides a limitation in this countercurrent flow, and what available information do we have to give us some guidance.

Now, if one can't put the water in here, then of course your outcome is pre-determined. Obviously, you also must be able to remove the energy from the steam generators. You must put water into the steam generator, also.

MR. LEE: You should be able to remove quite a bit of heat then perhaps.

MR. HENRY: In the steam generator?

MR. LEE: Right.

MR. HENRY: The decay power only takes roughly a foot of direct contact in one steam generator, if you have that much water in there.

MR. LEE: How about the   to water that you have to put into your   ?

MR. HENRY: You theoretically don't have to put anymore water in once you get enough water in here to establish this path.

MR. LEE: Right, but in order to come to that point you'd have to start off with some amount of water. Otherwise, you may never reach this situation.

MR. HENRY: Oh, of course, you have to have a certain amount of water in there. If you want to take the amount of water that's required to fill this part, which is about 700 cubic feet, that's the number to start with. A few thousand gallons that you can put in, for instance.

All I want to point out here is that the operator, without going through a very specific sequence or set of sequences, I think it's important to realize that the aspects of this design which differ significantly from those which we've been discussing simply in the past year, that the operator would have some alternatives to remove the power within the vessel. I think that's a very important point and we don't want to forget about it.

MR. KERR: Please continue.

MR. HENRY: I'll just push on to the limiting criteria for cooling such a debris bed in a counterflow situation can be broken down into two different limitations. One which is the critical flux off the surface of the bed, and the area here is the total cross-sectional area of the core. We're dealing within the core. And the other is the ability for liquid to get down through the bed. Now, the model which was used in the utilities 60-day study merely says that the pressure gradient developed by the vapor flowing upward at this superficial velocity should not exceed the static head of the liquid, which is given here. Where this is the particle diameter, we'll get back to that in a second, and this is the velocity of the bed.

This describes the ability of the liquid to come in contact with the surface, and this describes the ability of the liquid to penetrate down through the bed in the presence of the vapor coming vertically upward through the bed. One has to satisfy both criteria.

Just to give you a feel for -- since the critical heat flux function is by value maximizes at about 7 mpa, I just want to put these up here to show you what kind of heat flux you could remove based on the cross-sectional area of the core which is about 12 square meters. At 150 bars you could remove about 37.4 megawatts, which is greater than 1% power, in this case, and 7 mpa can remove 50.6. In other words, this would not be -- unless you're talking about something very early in time, the critical heat flux, generally speaking, would not be a limiting criterion.

In terms of the particle models , the one which I showed you in the 60-day study, I just want to point out there that this is the model from the 60-day study. For very small particles, this can be reduced to this kind of formulation. The Sandia model is basically this, the Hardee and Wilson model, and you can see, since this is a very small term here, this is essentially the same as that except for this number right here, and that comes from whether or not you assume whose correlation you pick for the drag coefficient. And that by Catton and Dhir has the same kind of formulation which in this model is mechanistically based on the ability of the liquid to get down. The important thing here is to recognize that all these have the same functional dependence with diameter and porocity of the bed. Because that is the major uncertainty which one then has to address.

To give you some feel for the sensitivities, this is the model, again, from the 60-day study which shows you three different diameters; 500 microns, 1000 microns and 1500 microns, and various porocities, between 30 and 60%.

The heat flux corresponding to about 1% power is in this range here. The Sandia evaluation assumed the porocity of about 40% and particle size is less than 500 microns; about 300 microns, as a result of their steam explosion data. That's a fairly key assumption because you can see it's highly dependent upon the diameter that you pick, and it's also highly dependent upon the porocity that you pick.

MR. ETHERINGTON: Where did we start from? What is the equivalent porocity of the core?

MR. HENRY: The equivalent porocity of the core is somewhat greater than 50%. But you take uniform particles, then the minimum porocity you can have is about 37%. Available experimental data, most of which deals with fast reactor systems, but a fairly shallow bed as they start, about 55%; up to 60 to 55%

work their way down to about 47, as the beds become deeper.

On a 60-day study for our evaluation we had picked 50% as just a number to give some idea of the sensitivity. The Sandia evaluation ended up with something more like 40%, and as I say, a 300-micron particle diameter which they picked from their iron thermide paper explosion tests.

MR. SHEWMON: There were some in-pile work that Coats reported on yesterday from LMFBR's in which he could solidify the sodium and show that indeed, as I recall, they were appreciably greater than 60% porocity. Is there any way of getting the experimental data on this? What the actual porocity is? Or do you use that as a disposable parameter?

MR. HENRY: The data which I was discussing which showed values greater than 50% had been dried with sodium and cleaned and then looked at the porocity of the bed, and it was always realized that the drying and cleaning could obviously have changed the porocity and if anything made it tighter. I'm not aware of the most recent data which Sandia has which shows like 60%.

MR. SHEWMON: It was even higher than that, and apparently he could solidify the sodium and he was surprised, too, at how high it was.

MR. HENRY: I think, certainly from our evaluation, what appears to be an even more key parameter is this diameter. The diameter itself is dependent upon -- that they use, is 300 microns which comes from the vapor explosion data. When they don't get vapor explosions, they suggest that the diameter might be more like 5 millimeters. Five millimeters is coolable no matter which of those models you use. And at 300 microns and porocity of 40%, it depends on which model you want to believe. But as I'll show, it's difficult for us to see where very fine particles are going to stay in one spot within the containment to give you a bed which is representative of the total amount of core debris fragmented to that size.

I should also point out that we're talking about the total, like 100,000 kilograms, fragmented to that size in these evaluations.

To give you some idea --

MR. PLESSET: You don't expect any deleterious dependence on the depth of the bed; the bed is very deep.

MR. HENRY: These are all deep beds, that's a good point.

MR. PLESSET: And you think that that's not going to cause any trouble.

MR. HENRY: The models that we're using are all deep bed models. None of these models represent the kind of things that we're used to looking at in the LMFBR where we have shallow beds. These are all deep bed models and there's no dependence upon the depth of the bed.

MR. KERR: They could even be called water beds.

(Laughter.)

MR. HENRY: In terms of in-vessel cooling, to give you some idea of the kind of particles that we might talk about as being terminally coolable, again, this is the model that was in the 60-day study. Assuming that we had to remove 20 megawatts, and that the 20 comes from using at the 1% power level, 30 megawatts if you had melted the system, the more volatile fission products which represent about a third of decay power would have been removed from the melt themselves; it would be somewhere else in the primary system or be vented. So the kind of power that one would have to remove would be like 20 megawatts. The available cross-sectional area is something like 10.5 square meters, and if one assumes a porocity of 40%, then the coolable size of particles is pressure-dependent. As you can see, it goes from at 10 bars it's about 550 microns; at 7 bars you could take a fragmentation size down to 280microns.

If the bed is looser than that, of course, the particles are smaller. I only show this so that you have an appreciation for the sensitivities and kinds of particles that we would indeed be talking about.

MR. ETHERINGTON: What size particles do we get from fuel-burst experiments?

MR. HENRY: It depends on the amount of energy deposition that you have. If you've gone through fuel vaporization, of course, you get very, very fire dust. But if you stay short of fuel vaporization, you get fairly coarse pieces in the ballpark of --

MR. ETHERINGTON: In terms of microns how much?

MR. HENRY: Anywhere from 500 on up to a couple of millimeters.

MR. ETHERINGTON: So you're in the range of your curve, is that not true?

MR. HENRY: Yes. In other words, those kinds of particles would be coolable in-vessel. This is all in-vessel here.

MR. SHEWMON: What's a vapor explosion? Answer his question once more and see if I understand it this time.

MR. HENRY: I wasn't talking about vapor explosions. There are a lot of burst experiments that have been done in pile. For instance, in treat. And the particle size that you get is dependent upon the energy deposition that you've put into the system. If you've driven the system far past fuel vaporization, then you get very, very fine particles because what you've done is vaporize the fuel at the end of the experiments --

MR. SHEWMON: That's nice, but presumably, we have a vapor -- if it was vapor it would go up and condense on the lid. So it's not doing that, it's coming down, so it must be throwing a bucket or very large --

MR. HENRY: As long as you stay short of -- if you just go up, say, to fuel melting or just above melting, you get very coarse particles, especially through the water system.

MR. SHEWMON: Okay, thank you.

MR. HENRY: The same type of models were used to assess ex-vessel coolability, and again, I've merely shown these so that you'll get a feel for the size of particles which the model, which was in the 60-day study, would calculate as being coolable. The decay power is, again, using 1% and two-thirds of the 1% assuming that the one-third is more volatile, and means that we would be treating something like 19 megawatts at Zion and 16 at Indian Point 2 and close to 18 at Indian Point 3. The surface areas in the cavity are somewhat different between the reactors.

MR. KERR: Excuse me. Those decay powers are calculated decay heat for the whole core?

MR. HENRY: Yes, it's for the whole core at four hours into the accident, assuming that one-third has left the melt as more volatile fission products.

MR. KERR: Thank you.

MR. HENRY: Given this surface area in the cavity and the powers for the given reactor, this is the required heat flux to remove that heat, and this, of course, comes nowhere close to the heat flux for critical heat flux off the top of the bed. So the only limiting feature could indeed be the penetration of liquid down through the bed in a counterflow manner with the vapor coming up. I'm showing then the coolable particle size for pressures in the containment of 1 atmosphere, atmospheres and 5 atmospheres. Again you can see it's pressure-dependent. But the point here is that we're talking about pretty small particles as well, and that's why I'd like to come back to that part of it when we get to what we've done since the end of the 60-day study.

MR. LEE: May I come back to my old comment concerning the countercurrent flow? In a horizontal pipe, does countercurrent flow get readily? Vertical pipe perhaps a little bit more readily in some way, but you do have some horizontal sections between the hot leg and the steam generator.

MR. HENRY: The vertical flow that we're talking about here is merely within the core itself.

MR. LEE: Right, but I'm going beyond that now. That's why I said I'd like to come back to the other viewgraph that is related to how you can cool the core or maintain cooling of the core.

MR. HENRY: Let me see if I correctly understand your question. Are you asking me -- is your question whether there is a limitation, flow limitation, in this horizontal pipe?

MR. LEE: If there is any difference in the counter-current flow between a vertical pipe and a horizontal pipe.

MR. HENRY: There's certainly a difference in the cross-sectional area that it will occupy. But this condensate is such a small fraction of this pipe, which is like 36 inches in diameter, that for all practical purposes, the vapor and the liquid do not even interact within that pipe.

MR. LEE: If there is a small, a trap or something, what would happen?

MR. HENRY: A small trap? The, of course, you could fill the entire trap and there would be an interaction. It would have to be an oscillatory process. You could, indeed, fill that.

MR. HENRY: Then the reflux flow of liquid would be decreased in magnitude?

MR. HENRY: Yes. It would definitely be an oscillatory process. You would have to be able to build up enough pressure to clear the trap so that you could return the liquid back to the primary system.

MR. LEE: But there is no trap in the --

MR. HENRY: There is no trap in the hot legs. This is strictly in the hot leg and there is no trap here.

MR. LEE: Any obstructions, orifices, that you have to contend with?

MR. HENRY: Nothing of any consequence.

MR. PLESSET: I think he's supposing, I believe, that he has enough liquid along the walls to give him some hydrostatic head. Isn't that right? That is, if you look up into the steam generator, suppose he has a liquid layer all the way up, that gives him some hydrostatic head.

MR. LEE: Right.

MR. PLESSET: If he doesn't have that, then he doesn't get it, this countercurrent flow.

MR. LEE: Right. But I'm trying to understand how that hydrostatic head could be influenced by the presence of fairly long horizontal pipe section.

MR. PLESSET: That's right, he's got to worry about the friction and so on. But he does have a hydrostatic head that is driving the liquid.

MR. LEE: As long as you have a point.

MR. HENRY: We're talking about very small velocities.

MR. PLESSET: That's what I thought you were doing. Is that all right?

MR. HENRY: That's correct, yes.

MR. LEE: I understood that point. But I'm a little bit worried about the possibility of reduction in the liquid flow due to some orifices, some obstructions I should say, which would be inherently present in any piping.

MR. HENRY: There are no significant orifices or obstructions in this line, but as Professor Plesset said, you have to have sufficient dragging head here because the liquid returns and pushes --

MR. ETHERINGTON: I'm sorry. I can't see a driving head on a partially filled -- on one wall of a partially filled pipe. It will all come right down or it will be consumed in friction along the wall.

MR. HENRY: It balances, yes.

We're also talking about very low steam velocities and liquid velocities in these large pipes. If you look at the flooding limitation, there is no -- you don't even come close to the limitation in the veritical part of the pipe.

Let me skip to the -- I'd like to outline one aspect that we think is very important that has been developed since the end of the 60-day study. That relates to the dispersive characteristics of the stored energy within the system, again, which is scenario-dependent. It will be different for large breaks. What we're principally talking about here is dispersive characteristics of the stored high pressure steam hydrogen, whatever else may be in the primary system.

The kind of picture that has been drawn in the past is one of core materials in the bottom of the reactor which then falls into the reactor cavity.

The analysis I'd like to outline for you is, once this material falls into the cavity here, then you have the follow-on, the site pressure, steam, hydrogen, whatever, flowing down through this vessel breech. What does that do in terms of this amount of (?) material. Does that have a potential for redistributing within the primary system, within the containment?

Again, I'll try to quickly outline the sensitivities. In essence, what we're talking about is how large this velocity would be here if the material were accumulated down in this cavity and, of course, it vents up into the floor of the containment building. Using and training criteria, and for those of you who have worked with this you know that this has a Weber number of 12 built into it, taking a very simple picture of the critical flow rate out of a breech here, what I've done is just evaluate the breech required in terms of cross-sectional area giving me a velocity here in this cross-sectional area which solves the stability criteria. And this basically tells me the velocity required to levitate the fuel, to break up and levitate the fuel.

And the only thing I'd like to leave with you in that regard is the sensitivity to various transients. If you evaluate the minimum breech size required for dispersal of transients -- and I'm just talking in terms of initial pressure in the primary system, for TMLB 5 which approaches nominal operating pressures, one only needs a diameter of about 10 centimeters, like 4 inches, in order to disperse all the material which would be coming into the cavity. If you have some S2D, or S2 sequences, that might have a driving pressure somewhere between about 800 psi, 150 psi, one is talking about something which is only 20 centimeters up to maybe a foot and a half. So, 8 inches up to a foot and a half.

The dispersal characteristics of the high pressure gas coming out of the primary system are more than sufficient to move that material out of the cavity. So what one is really dealing with, I believe, is that the material could be dispersed not only here but on all horizontal surfaces; principally, on the basement or floor of the containment down here. So if one were really dealing with mitigating features, we would have to deal with the dispersal characteristics of the water and the core material because most accident scenarios have significant dispersal potential. If the core material remains within the reactor cavity and the quenching rate is large or if you have steam spike or steam explosion, the steam spike, for instance, would first limit it by the rate at which the water can get back down into the tunnel which is back down through this cavity, and secondly, by a critical heat flux on the surface. This tells you that the quenching rate might be in terms of several minutes for the core material at best.

If you had a steam explosion with fuel in the cavity, the steam explosion itself is quite dispersive; the dispersal forces would be large. Small debris, in terms of a few hundred microns that we're talking about, would be dispersed throughout the containment.

Conclusions, then. One is dealing with the mitigating features or concerned with mitigating features for severe accidents. Mechanical dispersion potential is large and should determine the final core deposition. What you'd like to have is water available on a continuous basis on all surfaces where significant fuel could collect, so that you don't begin to attack the concrete.

And the dispersed core is coolable; if you disperse it over that kind of range, in these systems, for instance, there is water of about 3 to 6 inches depth kept on that basement floor, and also for the sequences that we've been addressing, one would have water down in the cavity range as well. The dispersed core is coolable with no significant attack of the concrete.

That's a short summary of the work in the 60-day study and what we've done since then.

MR. KERR: Thank you. Are there questions?

MR. SHEWMON: I guess I'll ask the staff this question again later, but I'm still not very clear on whether there's a difference of opinion between you and the Sandia people with regard to the coolability of the particle size which it is thought will generate if this drops into a pool of water or something like that.

Now, I agree that we haven't got super-heated fuel and I suspect they would agree with that, too. But can you explain some of this difference, or is there a difference of opinion?

MR. HENRY: Let me give you my view on it, and, of course, they can respond, too. The difference comes from -- in the initial evaluation, we and the NRC consultants all assumed that the total amount of core material was in this reactor cavity; all the material which was 100,000 kilograms or more, and the kinds of particle sizes which they used which was a result of the steam explosion studies, would say that if you have all the core material down here you could not remove the decay heat. It would not be a permanently coolable bed.

MR. SHEWMON: And their steam explosion were to throw molten fuel which was liquid into water and that makes a steam explosion. Is that right?

MR. HENRY: It was an iron thermide mixture which they dropped into water and looked at the particle size after the event.

On the other hand, they also reported particle size if you have no steam explosion, which as I recall is like 5 millimeters. In their evaluation and ours, 5 millimeters in this cavity is coolable.

Now, there are two things that we feel are quite important here. The ability for core material to remain in this cavity

dependent upon the driving potential of stored energy in the system, high pressure steam, once the vessel fails. You, I think, almost have to talk about something which is like a large break LOCA before you would have the total amount of core material left here in the reactor cavity, because this represent a considerable amount of dispersive potential in itself.

MR. SHEWMON: Okay, I'm with you. You're saying if there's a steam explosion it's too fine and blows out, and if there's not a steam explosion it's so coarse it's coolable.

MR. HENRY: Right.

MR. SHEWMON: One other question. Would you have concern in advising somebody to keep water underneath that pressure vessel when you thought that the core might be coming out in a molten state?

MR. HENRY: I would highly encourage it.

MR. SHEWMON: Thank you.

MR. OKRENT: Where are the sumps in that building?

MR. HENRY: This is a very, very small sump here which is shown. During normal operation they do not like keep water down here. The major research sumps are up on this level, they run down from there back away from this cross-section.

MR. SIESS: Is that curb or on the cavity?

MR. HENRY: There's a curb here which is about six inches high.

MR. ETHERINGTON: In a loss of coolant accident, that sump would get filled with water, wouldn't it?

MR. HENRY: Not only the sump can fill with water, but with these systems you have to lose about 65,000 gallons from the primary system in order to begin to bare the core. That means that the curb would be completely full and you'd also have water laying down in here, about 10,000 to 20,000 gallons depending upon the system.

MR. ETHERINGTON: I didn't quite understand your thermide experiment.

MR. HENRY: The Sandia thermide experiments?

MR. ETHERINGTON: Yes.

MR. HENRY: It's a vapor explosion experiment where they were dropping I think 25 kilograms of iron thermide into cold water and measuring explosions at 1 atmosphere. Explosive releases. Vapor explosions. After the event is over, they look at the particle size distribution.

MR. ETHERINGTON: Those particles are largely metal, aren't they?

MR. HENRY: They're oxide.

MR. ETHERINGTON: Then I haven't quite understood the thermide. Thermide is usually aluminum and iron oxide which gives you iron.

MR. HENRY: It gives you iron and aluminum oxide, yes.

MR. ETHERINGTON: And aluminum oxide, so they're --

MR. HENRY: It's a combination of two types.

MR. ETHERINGTON: There could be iron as well as oxide, couldn't there?

MR. HENRY: Right.

MR. SHEWMON: Why do you call this a vapor explosion? Has the thermide reaction finished before it goes in?

MR. HENRY: It has. It's bringing in the contact -- two liquids, one at very high temperature, one at a low temperature, and the explosion results from a rapid production of steam. The thermide reaction has been completed.

MR. SHEWMON: You call that a vapor explosion because you vaporize water, not because the oxide is vapor at that time.

MR. HENRY: That's correct.

MR. SHEWMON: I misunderstood you earlier.

MR. HENRY: Most people in this industry call it steam explosions, not vapor.

MR. KERR: Earlier, he was vaporizing fuel and a steam reactor explosion.

MR. SHEWMON: But that's what I thought he always meant when he said a vapor explosion, and now he doesn't.

MR. HENRY: No. A steam explosion, or more generically, a vapor explosion, is one that's produced by two liquids at greatly different temperatures coming into contact, and the result --

MR. SHEWMON: But isn't that what happens when you've got molten fuel coming down out of the -- into liquid?

MR. HENRY: Yes. But Dr. Etherington had asked me about the particle sizes measured by energy deposition tests, the first test, in-pile, and that's a different beast.

MR. OKRENT: Are the sumps in any way subject to a problem from this dispersed core debris?

MR. HENRY: The only one I've had an opportunity to look at, Dr. Okrent, is that at Zion, and it appears that those would not be, because the intakes are sufficiently high above the bottom and with the velocity, it looks like it would not be sweeping the material into -- are you talking about plugging of the jets or whatever?

MR. OKRENT: Whatever.

MR. HENRY: The theory is that those would not be. I have not had a chance to look at the Indian Point sumps, whether they're different or the same.

MR. OKRENT: It would be worth looking at.

MR. HENRY: It's certainly something that has to be addressed, yes.

MR. KERR: This is so you can cool the core that's left? And then the vessel?

MR. OKRENT: No. You might be planning on using that water.

MR. KERR: For long-term cooling?

MR. OKRENT: Yes.

MR. HENRY: If you consider the full spectrum of accidents, this appears to be the more likely distribution. Anyway, that's certainly something that has to be addressed.

MR. OKRENT: And in regard to the question that I think it was Dr. Shewmon who asked you, about would you put water underneath, I must confess my inclination would be to agree with you, but I don't have a good basis for it. And I have the impression that the German design is such as to keep water out of the region under the vessel. Are you familiar --

MR. PLESSET: Dave, I looked into that. You stipulated me to do that and I'll tell you later why they do that.

MR. OKRENT: Let me ask Dr. Henry if he has any insights and I'll catch you during the break.

MR. HENRY: I'm not intimately familiar with why the German design is the way it is, but I know that there were designs investigated in this country where people attempted to preclude water accumulation below because of the steam explosion arguments. I don't think that the steam ex-`osion argument is one which should be governing such considerations.

MR. KERR: Are there other questions?

MR. LEE: In your opinion, is there a quantitative or a semi-quantitative model that can be used to explain the 150 psi apparent (?) or steam explosion? Other than saying that low bed pressure, a voiding would and so on.

MR. HENRY: Let me answer in the following sense. I'll give you my own opinion --

MR. LEE: That's what I want.

MR. HENRY: Let me also say that before I give you that, that there are certainly many other people who have differences of opinions in the community that has looked at sodium-fuel interactions and water-fuel interactions.

The 150 psi principally comes from experiments which were done at ESPRA with molten sodium chloride in water in large-scale experiments. Two kilograms of molten material. Before the experiments were run, we had to have a pre-test prediction of at what cutoff pressure we would see where explosions would not occur. At the time, there were two models, mechanistic models, if you will, that gave predictions of when this cutoff would occur. Both of those gave predictions that were very, very close to the 10 atmospheres, 150 psi, before the experiments were run. The experiments were run at 1 atmosphere and they got an explosion every time; they were run at 5 atmospheres and depending on which investigator you talked to, there was or was not an explosion; at 10 atmospheres they got nothing but slow vaporization and small over-pressure. So the answer to your question is yes, but if you're going to ask me if there's unanimity of opinion, no.

MR. ETHERINGTON: You used the model of the core melting through the bottom and running through an orifice. Supposing the model were a little different; supposing the bottom essentially collapsed and the whole mess went "plop." Would that radically change your particle size conclusions?

MR. HENRY: Those weren't particle size conclusions; those were just the dispersive potential of what's held within the primary system.

If you make this breech larger, it just means it becomes even more dispersive. I just gave you the minimum size which would have sufficient velocity -- the flow rate out of here would result in sufficient velocity in this cross-sectional area to remove everything up into here. If you made it bigger, more catastrophic, it would be more dispersive.

MR. SHEWMON: If it is more catastrophic, does that change your comment earlier with regard to my questions -- if pressures spike, that you would comment possibly on your containment?

MR. HENRY: The pressure spike -- I'm not quite sure --

MR. SHEWMON: The question was do we always want to encourage people to keep water underneath there in times of stress.

MR. HENRY: It doesn't change my answer. Certainly the water itself would be blown out as well. But also, there's only a curb here which keeps it from coming down in and you've already filled it up to the curb level, so it eventually will come back in, which I think is a very attractive feature.

One also must realize that when you do disperse it up into here, then you can have more rapid steam production, if you want, in terms of a steam spike. I think that's something that the containments can already take. That doesn't particularly bother me. What would bother me is if we had surfaces here which are unprotected. And I think water is a highly reliable protective mechanism.

MR. OKRENT: But, if we can carry it along, if we have a situation like Mr. Cybulskis was talking about before with the hydrogen burning, for whatever reason, comes right at the same time as the large loss of heat from the fuel to water, you do have then the potential for the combined pressure pulse which you might not have if it were dry beneath or pretty dry, so that that fuel took a longer time, had a longer time constant to give up its heat. So I'm just saying if you want to play the game, usually you can find something on either side.

MR. HENRY: I think that particular part will be addressed in the next talk, and that also, in terms of the ultimate strength of the containments, does not appear to be a problem.

MR. KERR: Other questions? I have a commitment, I think, for 30 more minutes of presentation.

MR. PEOPLES: Yes, sir, Mr. Chairman, we're prepared to give our remarks in 30 minutes or less.

MR. KERR: I'm sorry we have to compress it that much. I'm ready for the next presentation.

MR. LYPARULO: My name is Nick Lyparulo. I'm going to discuss some of the containment calculations we've done with our 60-day study, discuss the hydrogen burn model and our present program. I'm not going to go through all the slides in the handout. Let's skip ahead and talk about Slide 3 and talk about our approach to the calculation of the problem.

The approach was as follows. When the 60-day study started, we didn't feel we had time to go back and redo March calculations for the Zion and Indian Point plants, so we took presently available March code results for a typical four-loop, 17 by 17 plant, and used them as boundary conditions of the problem.

We fed these energy releases into our Westinghouse containment code, COCO. We modified the Westinghouse code to include a vent, non-condensables such as hydrogen and CO_2 and CO, and we put a hydrogen burn model. The code already had in it containment structure, heat transfer and safeguards.

We looked at a number of scenarios. These were scenarios that were available and that's how they were chosen, but it turned out when we went back and did our mini-WASH-1400, these scenarios were also -- these two scenarios, were also representative risk contributors.

The first scenario we looked at was AD, that's a large break LOCA with active ECCS systems assumed not to operate. However, you do have containment safeguard systems operating. Their sequence is STD, that's a small break LOCA, again without active ECCS systems but we do have containment heat removal capability.

Another scenario we looked at, which turned out not to be a representative risk contributor was a TMLB[1] sequence This is a loss of all AC power, no break. You have no active heat removal systems operating. Any active heat removal system driven by DC power not operating. If you have a diesel-driven spray pump, for instance, it would assume that that is operating. The Zion plant does have a diesel-driven spray pump. Containment systems which are run on AC power were assumed not to operate.

I want to look a little bit about our hydrogen burn model next. First I want to discuss just some rules of thumb on hydrogen. These are characteristics of hydrogen burn in dry air. If you have a hydrogen concentration of 4%, you reach the first point at which hydrogen can burn. At 4%, hydrogen can burn only upwards. At 6%, it can burn upwards and sideways, and at 8½% it can burn in all three directions.

Up to 18% you have what's called a deflagration, and that's basically a slow burn. Above 18% to 59%, you can have a detonation. The important point to note right here is that below 8½%, you have a relatively benign burn, since you can't burn in all three directions and the pressure rises are rather slow, and you don't get a significant percent reaction. But above 8½%, up to about 59%, you can have a significant pressure rise due to the hydrogen burn.

This is illustrated in the next two slides. This is all from Bureau of Mines data. The first slide shows you this threshold where slightly above 8½%, the rate of reaction, percent completion, percent reaction, takes off.

The next slide is the pressure rise for those experiments. It shows the same thing.

We didn't concern ourselves, therefore, with burns below the 8½% number.

MR. PLESSET: Is there any data on the effect of moisture in the air?

MR. LYPARULO: The effect of moisture in the air is taken into account in our model, which I'll present next, but there isn't a lot of good data on high concentrations escape.

MR. SHEWMON: Is the effect of the heavy sprays on the combustion also taken into account?

MR. LYPARULO: No, we don't take that into account. I can elaborate on it, what I think the effect would be, if you wish.

This is the criteria, then, that we used to develop our hydrogen model. The first criterion is that a chemical reaction exponentially depends upon temperature. Therefore, for a chemical reaction to occur, some critical temperature is needed for the   to proceed. This critical temperature can be backed out from that data that I presented on the previous two slides, and if you work from the 8½% number where you have a significant pressure rise, you find this critical temperature is 710°C. This is a temperature at which the rate of reaction takes off and you get a non-benign pressure rise.

You can calculate this temperature and you can include the effect of pollutants just by constant pressure calculation. If you have a sphere burning proceeding in the containment calculating the temperature just on   , you can compare that temperature to this critical temperature, and if this temperature is above your critical temperature, then you can have a significant burn. Is that clear? I have the equations if you want to go through them.

MR. KERR: Is that clear to you?

MR. PLESSET: Not quite. When you're above T critical, you get an explosion really, don't you?

MR. LYPARULO: No, it's a deflagration.

MR. PLESSET: It's a deflagration still, no matter what the temperature relative to T critical?

MR. LYPARULO: This critical temperature isn't like the critical temperature in a steam, for instance. It's a critical temperature in the depths of temperature at which the hydrogen reaction can be a non-significant pressurized. The definitions are slightly different.

MR. SHEWMON: You're saying that the pressurized is -- what you're saying is that the burn is faster and faster as the temperature goes up which also fits in with your kinetics, and above some value you choose to call it critical in rate.

MR. LYPARULO: That's sort of what I'm saying. Below 8½%, you do not see a significant percent reaction. Then, if I just treat an 8½% by volume hydrogen mixture, knowing the amount of heat that would be liberated by that reaction, I can calculate the temperature at 8½%. That's what we call a critical temperature. That works out to be 710°C.

MR. ETHERINGTON: Is that what you've called the flame front temperature?

MR. LYPARULO: That is also the flame front temperature, that's right.

MR. ETHERINGTON: As a matter of information, do you figure the products of combustion specific heat from constant volume or constant pressure?

MR. LYPARULO: Constant pressure. As long as you're consistent it doesn't really matter, but the reaction is really occurring on constant pressure.

MR. ETHERINGTON: What temperature do you consider the temperature to continue to combustion?

MR. LYPARULO: About 710°C. That's our clave temperature criterion. That corresponds to that calculation you do at 8½% by volume, assuming 100% reaction.

I can give you a copy of the equations.

I just wanted to mention real quickly about our COCO code. This is the code we use to do all of our calculations. It's a one volume code, two systems modeled to sump water and containment atmosphere, and we modeled all the heat removal systems in the containment. We can handle the burning of hydrogen and an event in the containment.

I want to mention some of the assumptions in the present analysis. We took our percent zirc water reaction right out of March, this boundary condition. In all of our calculations we assumed 100% burn of all the hydrogen when we burned it. We utilized March calculations that were available for a 17 x 17 plant. It turns out that with Indian Point and Zion, there's a good deal less zirconium in the core than the boundary conditions we used; therefore, you expect a good deal less hydrogen generated. You burn the hydrogen over 20 seconds but we didn't account for any stainless steel water reaction.

MR. OKRENT: Have you estimated whether that's significant

MR. LYPARULO: We're looking into it. I don't have a number for you today.

MR. LEE: What is the difference in zirconium come from?

MR. LYPARULO: The 17 x 17 core has a different clouding technique than the 15 x 15 core.

MR. ETHERINGTON: On your previous slide you had a percent zircwater reaction given by March. Do you know what that number was?

MR. LYPARULO: That's 100%.

MR. ETHERINGTON: Why did you use March for that?

MR. LYPARULO: It's all that was available. We had a 60-day study and given the time we had, we didn't have time to develop it.

This is how we do a containment calculation for a Class 9 accident. For a given scenario, we do the calculation assuming no hydrogen burns. We calculate what we call our flame front temperature and compare that flame temperature to our temperature criteria of 710°C. By making that comparison we can tell if the containment atmosphere is flammable with the dilutents present. If it is, we run a second calculation where we burn the hydrogen at the worst point in the transient, so it's a two-step calculation.

I'll show you some representative results. This is for the Zion plant. This is our 710°C temperature criteria. Here is calculated flame temperature versus time, depending upon mixture conditions. When you cross this point right here for a short period of time, that tells you that you could have a non-benign burn. We re-ran the calculation burning the hydrogen at this point. Here's the results for both calculations in terms of containment pressure. The dotted line is a no-burn case and the solid line is the case where we burned the hydrogen. As you can see, it gets somewhere between 40 to 50 psi pressure spike when you burn hydrogen at that time.

The reason the difference made in time is you actually reduce the model mass in the containment atmosphere when you have a hydrogen burn because you have two molds going to one mold. So you have a conduction in mass which causes a reduction in pressure.

MR. PLESSET: Can you tell me again the difference between what you call a benign and non-benign?

MR. LYPARULO: A benign burn from hydrogen wouldn't give you a significant pressure rise in a short period of time. For instance, if I had hydrogen going into the containment, let's say at 1 pound mol per second. I have two options; I could burn as it comes in and get a very insignificant pressure rise, or I could burn it all at once. If I burned it all at once above a certain concentration, I would have a non-benign burn.

MR. OKRENT: And you took uniform distribution.

MR. LYPARULO: Right, uniform distribution in all our calculations.

MR. OKRENT: Are you evaluating how good an assumption that is as part of your follow-on?

MR. LYPARULO: We're looking and getting to it. We do know that the Battelle Frankford test, which was run in a full-sized containment, they introduced a little bit of hydrogen into the bottom of the containment with no heat removal systems on. So it was a quiescent atmosphere. When you have a temperature distribution with the hot air at the bottom and the cold air at the top, you have good mixing is what the test results show. That would be the situation we had here. If we had our safeguard systems on we would just about insure a good mix.

MR. PLESSET: Did you determine the width of that pulse in the non-benign burn? You had it on the slide.

MR. LYPARULO: For a non-benign burn, we assume it doesn't burn.

MR. PLESSET: You've got a pulse of almost 100 psi.

MR. LYPARULO: Yes, we calculate pressure as a function of time throughout the whole transient.

MR. PLESSET: So what was the width of that peak, how long?

MR. LYPARULO: I'll say approximately a minute. It was long enough --

MR. PLESSET: So it was a long pulse.

MR. LYPARULO: When I say pulse, I meant in terms of the (?) grapl , right. It would -- containment would see it as a quasi-steadystate pressure.

MR. PLESSET: So it was about a minute.

MR. LYPARULO: Somewhere in that range. It's long enough that the containment would see pressure.

MR. OKRENT: If I can come back to the hydrogen distribution, it's a factor of roughly 2 between the beginning of your non-benign burn and the beginning of your detonation range.

MR. LYPARULO: That's right.

MR. OKRENT: It seems to me, since it has potentially important ramifications for what the containment would do, it might warrant a reasonable amount of study, and I don't see how one or even a dozen tests, whatever, would of itself give you the general kind of information you'd like to have in that regard.

MR. LYPARULO: Well, there are two sides to the problem. When you have your containment heat system is on; fan coolers, sprays. I feel that the containment would be well mixed. In fact, correct me if I'm wrong, I believe that Zion and Indian Point have mixing systems to preclude the buildup of hydrogen in containment. And that would be for the AD and S2D sequences.

For those sequences which were representative risk contributors and therefore you would expect a well mixed containment --

MR. OKRENT: But you just put in some groundrules that say we'll consider risk contributors that have well-mixed hydrogen won't burn in such a way, therefore, -- to damage the containment. Which might mean that some other scenario where the hydrogen might conceivably accumulate locally, could damage the containment and now it becomes a dominant risk contributor. At least if you haven't looked I don't know why that --

MR. LYPARULO: We are going to look into it. It's a very difficult problem, though.

MR. OKRENT: I agree. I heard around the ACRS table a man who lived with hydrogen quite a bit express doubt about it always being well mixed. At least in his experience that wasn't the case; he felt --. In any event, as I tried to indicate, you have to watch whether your original boundary conditions don't lead you down a predetermined path.

MR. LYPARULO: I understand. Why don't we move on to the TMLB[1] sequence, get to S2D sequence.

Again, we have our comparison between our flame temperature calculated versus time in our criteria, and it shows that the hydrogen never reached a flammable mixture condition for this accident. Therefore, we didn't have to run a case where we burned hydrogen. To get into trouble we didn't have to.

This is the pressure versus time. You can see where you see the failure pressure at about 6.6 r's. This is slightly different from Pete Cybulskis' calculations because as my slides mention, we assume a cobalt(?) debris bed at all times in the transient. Pete in his calculations began to have some concrete interaction here and that actually slows the rate of rise of this curve.

If you could get some safeguards on at about 6.6 r's is about when you can see design to failure pressure. If you get your safeguards on at 6.6hours, the current comes down; later on you have a hydrogen burn and it goes out, but again, you never do see failure pressure. In fact, if you have a one-fan cooler on for the entire transient you don't even see the failure pressure. So all this is really showing you is that in order not to fail the containment for TMLB[1] scenario, you need to have some sort of heat removal.

MR. ETHERINGTON: But don't the cold surfaces of the containment tend to quench that --

MR. LYPARULO: Right, we have those models. We have the containment structure with heat sinks modeled in these calculations.

MR. ETHERINGTON: Is that one that you show on the screen now?

MR. LYPARULO: Yes, in this calculation we have containment with structure heat sinks modeled, in all the calculations. We do use conservatively low heat transfer coefficients.

MR. OKRENT: Presumably, this is one in which a filtered venting system would have time to be useful.

MR. LYPARULO: Right, or some other means. I think just about -- you don't have to take a lot of heat out, because decay heat is really low at this point in time. But you do need some means of heat removal besides structures.

MR. OKRENT: Or pressure relief.

MR. LYPARULO: Or pressure relief. That's taking mass out. But you have to be very careful because you could get into a situation where if you began to relieve pressure at the wrong time, you get yourself in the mixture where you have a flammable mixture in the containment. Then you have a hydrogen problem. Right now we don't have a hydrogen problem. Steam inerts the atmosphere.

MR. KERR: I'll urge that you not answer questions that Dr. Okrent didn't ask.

(Laughter.)

MR. LYPARULO: I think it's important so let's talk a little bit about March. As we mentioned, the March computer code was developed as a probabilistic analysis and not a design analysis, and while the code does preserve integrals, rates are not well substantiated.

Going through the March review we did, the code is a one-node model. Because it's a one-node model, you can get system effects such as break flow, and we're looking into that and finding out whether we have to add additional nodes or what the effect of that is. The code right now has a fuel rod-clad interaction with no gaps. If you have no gap, you have at least an early fuel melt, when you have the zirconium water reaction.

The gap provides a buffer. The hydrogen release calculations, we don't model an inside reaction between the zirc/clad and fuel; thus, we get -- we over-predict the hydrogen release calculations, and as Don mentioned this morning, we over-predict the amount of energy put into the system.

The molten zirconium that falls from the melting fuel is assumed to react when it falls in the water fuel lower plenum. That indicates that that is not true.

The stainless steel water reaction isn't accounted for. The core melt sump model -- the present models were only scoping. We looked at a number of scenarios in which the core could melt.

Total non-sequential failure

are assumed. This addresses one of Dr. Okrent's questions that he brought up during Bob Henry's talk. In the March studies, once the core goes, is sitting along the lower support structure it allows it to calculate the time for this plate to fail and it forms a mixed puddle with all this water in the lower plenum. As you can see, what we have to do is improve the modeling because there are several plates that the core has to go through before you can allow this kind of mixing. That's one of the major reasons for a pressure spike in the Sandia calculations. The failure to model those support structures.

MR. KERR: It also seems to me it's going to be difficult to tell whether you have improved the model or changed it.

MR. LIPARULO: Another thing that should be improved is the fuel debris/coolant interaction. Right now the limiting heat processes like debris bed critical heat processes aren't calculated.

As Bob Henry talks about, the dynamic effects of the water in the lower reactor cavity aren't modeled, and as a result, the particle sizes which are user input could be inconsistent. You could have a particle size to correspond to a steam explosion, and yet assume a steaming rate. If you have a steam explosion, you blow the particles out of the reactor cavity.

The MARCH hydrogen burn model burns hydrogen over typically about 6 second intervals. Data indicate that in 20 to 60 seconds, the burn completion is usually 100 percent. We have seen data that indicate that that is off slightly.

The effect of sprays on flammability are not accounted for, and you have a limited data base.

Summarizing, we have performed containment calculations for the AD, S2D and TMLB sequences. Mass and energy releases were taken from MARCH. We have developed a capability to calculate the bulk combustibility of a hydrogen/air/steam/containment atmosphere. For the AD and S2D sequences which were representative risk contributors, if the hydrogen was assumed to burn continuously or not to burn at all, we don't have a problem with present containment designs. If the hydrogen is assumed to accumulate to a combustible mixture and then burned, while we do get a rapid pressure spike, the containment is not calculated to fail at this time.

DR. KERR: Thank you, sir.

MR. LIPARULO: I have passed out a calculation comparing the flame temperatures calculated in the TMI hydrogen burn compared to what our model predicts, and it provides a verification for the model. If you have any questions, you can contact me.

DR. KERR: And you are going to get some equations from Dr. Glassick (?).

MR LIPARULO: Yes, I will get some equations from Dr. Glassick (?).

DR. KERR: Are there questions? Mr. Shewmon.

MR. SHEWMON: How much water do you have to have suspended in the air before the vaporization and content of the water equals the heat of combustion of the flame for that volume of gas?

MR. LIPARULO: It depends upon the volume of gas that you assume and the temperature.

MR. SHEWMON: I have got unit volume of gas, whatever it was when you started your burn, and I am trying to get some idea what this more than sprinkle coming out of the containment sprays does. My question was, in that unit volume, how much water do you have to have as droplets before its heat of vaporization equals the heat of combustion for that unit volume?

MR. LIPARULO: I haven't done that calculation, but my feeling is it probably wouldn't have to be too much. If you assume that --

DR. KERR: You can just say you don't know if you don't know.

MR. LIPARULO: I don't know.

MR. SHEWMON: I have heard others say that they don't think it would be too much either, yet given that, if it wasn't too much, then it certainly would influence the hydrogen burn because it touches the flame.

MR. LIPARULO: That is right. One thing you have got to be careful of, though, is you are assuming complete interaction during the burn. Don?

MR. PADDLE (?) Don Paddle from Westinghouse. Dr. Bernard Lewis, who has been our hydrogen consultant for the last six months, indicated that about .05 volume percent water suspended as droplets of about 40 or 50 micron size would be effectively inert (?).

MR. SHEWMON: Thank you.

DR. KERR: Mr. Peoples, were you about to say something?

MR. PEOPLES: I was going to comment that we are going to be studying that question in experiments later on, and I could address that briefly.

DR. KERR: Mr. Seale, did you have a question?

MR. SEALE: Yes. I understand that according to your plot there, there is about a factor of 3 difference between the most benign burn of hydrogen, that is, the concentration at which you begin the burn, and the place where you get to the rather --

MR. LIPARULO: A wispy fire, right.

MR. SEALE: I have also heard it suggested that there are a large number of initiators, typically relays and whatever else, in a containment building, and that one might be fairly frustrated if he tried to get a mixture up around 12 percent or so.

Are you looking into that aspect; indeed, in line with Dr. Okrent's line of suggestions, is that number a function of what particular accident sequence you happen to be in when you are looking at hydrogen concentrations?

MR. LIPARULO: I will see if I can answer the first part of your question. If you want a guaranteed burning of hydrogen at 4 percent, let's assume I have an ignition source. How will it burn? The hydrogen mixture will burn as a wisp, straight upward. So in order to guarantee a significant burn, I would need an infinite number of sources.

So while you could have a burn of 4 percent, it is not going to do much for you on decreasing your hydrogen concentration.

MR. SEALE: At 6 or 8 it might, though.

MR. LIPARULO: At 6 or 8 it might, but again, if you look at the plot, you would get a very low percent reaction even at 8 percent based upon the test data of the Bureau of Mines, where there was a spark in the center of the test facility. At about 8.5 percent, you would get a good deal of completion of reaction, but that also gives you the pressure rise. They are tied together.

You don't attempt to guarantee ignition of the hydrogen. Right now our calculation is just burn at the worst time. If we could guarantee ignition at different times, you would get better answers.

DR. KERR: Mr. Etherington.

MR. ETHERINGTON: Have you ignored the iron/water reaction because you think there is no credible probability of it being significant, or because you haven't gotten around to it yet?

MR. LIPARULO: The data I have seen says that it is not a significant generator of hydrogen, and that is why we have ignored it.

MR. ETHERINGTON: Of course, the data depends on what you assume physically with regard to the mixture and the temperature of the iron and on the steam around, if you have got enough iron there for it to be significant.

MR. LIPARULO: It is something we will have to look into, but my feeling is right now that it will not be significant, based upon other reports I have read on that subject. I haven't done any work myself on it.

DR. KERR: Are there other questions?

Mr. Peoples, did you want to make some conclusions?

MR. PEOPLES: We have two very brief presentations on containment structural response by our architect-engineers. I really think they are quite worth hearing because they talk to the work that they have conducted to assess the containment capability. I would like to ask them to come up now, and then I will summarize in about a minute and a half with concluding remarks after that.

MR. WALSER: Mr. Chairman, ladies and gentlemen, I am Adolf Walser from Sergent and Lundy in Chicago. The purpose of my presentation is to give you a brief summary of the Sergent and Lundy study which was designed to determine the upper limit of the Zion containment capacity of pressure loads as may be experienced during a hypothetical Class 9 accident.

The objectives of the studies were to determine the ultimate pressure capacity, to assess the effects of high temperature, to identify modes of failure, suggest possible remedies to reinforce the weak areas, assess the effects of the rate of pressure rise and rate of temperature rise.

The evaluation of the containment was performed by both hand calculations and computer analysis. The hand calculations were performed to evaluate typical areas and to find their capacities, and the state of stress and strains of the materials. The computer analysis was performed to evaluate the entire containment and to locate the weak link.

In the next slide we see a picture of the analytical computer model. The program we used is based on the Wilson-Goshe (phonetic) program, and is modified and validated by Sergent and Lundy. Each finite element consists of several layers which represent concrete, prestressing tendons, reinforcing steel, and the steel liner on the inside.

The materials are represented in this computer model by bilinear stress-strain curves, and this permits to represent the cracking of the concrete and the yielding of the steels. The material properties are determined from actual material test reports obtained from the Zion project.

The pressure load was applied in several discrete steps of increasing magnitude, and the material stresses and strains were evaluated at each step.

I would like to show now a few of the results we found during the study. The hand calculations indicated that the weak link of the containment are the tendons in the hoop direction. This, by the way, was, of course, confirmed by the computer model. We show here the pressure in psia at which the hoop tendons would yield, and this is the factor times the design pressure for easy reference.

In the next slide, we show a graph indicating the relationship between the hoop tendon stresses and the pressure load. This graph is based on the computer analysis. The study was conducted considering the liner to be fully effective, and also without the liner. The liner can be ineffective if it is really hot.

For the purpose of this study, failure was considered to be the yielding of the hoop tendon at the tested tendon yield at 1 percent elongation, 1 percent extension.

Next slide. This graph shows the relationship between the containment wall, radial displacement, and the pressure load, and we see that the radial displacement at the failure node defined before is approximately 4 inches. The containment, of course, is capable of carrying somewhat more load, except the deformations increase rapidly without any substantial increase in pressure.

This graph shows the stress-strain curves of the steel materials, stress versus strain, for prestressing, reinforcing steel and liner. The solid lines indicate the extent to which the material was used, let's say, in this study, up to the defined containment capacity. We also can see that we have still a substantial margin left for a kind of factor of safety to strain failure.

MR. SHEWMON: You said there was some yielding at 120 on the hoop tendons, yet that looks elastic.

MR. WALSER: Here is where the yield of the hoop tendon is defined at 1 percent extension. After this, the tendon can carry more load, except the deformation increases very rapidly.

MR. SHEWMON: But in your table you had the tendons yield at 120 ksi.

MR. WALSER: 120 psi pressure load in the containment.

MR. SHEWMON: Thank you.

MR. WALSER: This table lists the margins available in noncritical parts of the containment at the pressure load which brings the hoop tendons to yield at 120 psi gauge. The margins, as you see, are all greater than one. For instance, for something like shear loads in the containment, that is with the enforcement at the equipment hatch and reinforcement at the containment.

The equipment hatch was re-evaluated because this factor of 1.0 didn't look very good. We re-evaluated the equipment hatch using actual material stresses based on test results rather than the minimum specified values, and we find that the margin, instead of 1.0, should be 1.14.

DR. SIESS: These are margins with respect to what?

MR. WALSER: To our defined failure, which is equivalent to the tendon yielding, for instance. A failure, if you want to call it that --

DR. SIESS: Margin here is used differently than it was in the other figure. That was the multiple of the design load to failure.

MR. WALSER: That is correct. In other words, we could increase the pressure --

DR. SIESS: The load would have to be increased by that amount to --

MR. WALSER: That's right. To bring the areas to failure.

DR. SIESS: What was your definition of failure for shear?

MR. WALSER: For shear it was the containment code equation, except the built-in load factor, capacity reduction factor was eliminated, which is a very conservative approach. But since it is a brittle failure, we didn't want to be --

DR. SIESS: This was on a principal stress or a --

MR. WALSER: Radial shear.

MR. ETHERINGTON: But with respect to leakage, it seems to me that is very unconservative because you have stiff regions in the containment around locks and others which are perhaps not designed right up to the same limit. To assume that you are not going to have any seams cracking in liners as you go to twice the design capacity doesn't seem to me --

VOICE: (Inaudible)

MR. ETHERINGTON: Yes, 2 percent strain in the tendons.

MR. WALSER: And, essentially, in the liner.

MR. ETHERINGTON: I am talking about if everything was designed so everything stretched uniformly, then this would be correct. But it is not designed like that.

MR. WALSER: Except in the areas --

DR. SIESS: Put the stress-strain curves back on, will you please, because on the liner you have got a spot on there marked "membrane and bending."

MR. WALSER: Y s.

DR. SIESS: Is that free field membrane or is --

MR. WALSER: This is free field membrane. This is membrane plus bending due to liner buckling, and even cramping (?) due to the pressure load being applied to a buckled panel.

DR. SIESS: But nothing for localized stresses around a penetration. Isn't the liner thickened at the penetration?

MR. WALSER: That is correct, yes.

DR. SIESS: And do you look at the transition between that thicker plate and the other, if there is a weld there, or there may be local stresses, which I think is what Mr. Etherington is concerned about.

MR. WALSER: Yes. The penetrations have generally higher load factors because in those areas we have added reinforcing, which reduces the deformation. The weld between the liner panels is at least as ductile or is ductile enough to carry strains of this magnitude.

DR. SIESS: What kind of strains can the liner take with biaxial tension?

MR. WALSER: Based on our information using ASME information, we would have to discount the uniaxial liner strain by about 70 percent.

DR. SIESS: How much?

MR. WALSER: To about 70 percent, to about 70 percent, to account for biaxial loads.

MR. SHEWMON: To 70 from what?

MR. WALSER: From the uniaxial strain, which in this particular case is about 23 percent to about 16 percent.

DR. SIESS: Now, we essentially have to assume here that if we do get a break in the liner, we do have a significant path to the environment, don't we?

MR. WALSER: That is correct, yes.

DR. SIESS: The concrete is well cracked.

MR. WALSER: That is correct, yes. The assumptions we took to determine the limit of the containment pressure capacity is such that we have a really high confidence that we have no failure, not even local failures. At the pressures we have calculated, we are very certain that we have no leakage.

DR. SIESS: I don't have any trouble accepting that when I think in terms of structural failure, but I don't see from what is being presented -- I'm not saying you haven't done it -- but I don't see the calculations that would tell me that I could rely on the integrity of the liner up to this same level of load, and particularly a deformation, with all the local type of discontinuities that are in that liner in all sorts of places.

MR. WALSER: Well, we shouldn't forget that the liner in this containment under normal operating load is essentially in compression. We have to go to substantial pressure loads of something like 100 psi before we even get the liner into tension. So the liner strain only builds up into tensile strain from then on.

DR. SIESS: I just have no feel now for how localized strains might get around the penetration or at some discontinuity. The liner is very ductile. It can take 15, 20 percent strain, probably. I guess I have some difficulty visualizing strains that large except over very, very short lengths, but that will tear a well, will tear a liner. I don't get the comfort on leak tightness from this that I get from structural integrity, but it is not structural integrity that is the problem.

MR. WALSER: We have looked at the liner very carefully, and we have determined that the maximum strain we see in the liner itself due to membrane plus bending action, due to buckling and ceasing due to pressure load is in the area of 6 percent.

MR. ETHERINGTON: But isn't that still a gross regin of the membrane and not the local --

MR. WALSER: Yes, but we have not been able to determine that we have added strains in local regions.

MR. PEOPLES: Adolf, if I am correct, as I understood at a much longer presentation before the NRC back in Washington, the analysis did try to look at penetrations in some detail, and that they did take a look at the local strains, and in particular at the equipment hatch because it is the single largest opening into the containment and represents the largest opening compared to the circumference of .ne (inaudible word), and evaluated that in some detail.

It is on that basis that we have the analysis from CB&I with respect to the hatch itself and looked within that region to local strains in that region. It was determined through the analysis that that was n c limiting and that, in fact, the hoop stress was the limiting feature in the containment design.

DR. OKRENT: You mentioned that you had a high degree of confidence that you wouldn't have a leak up to this point. I don't know what degree of confidence you mean: one sigma, two sigma, 99 percent? Are you able to quantify it for me in any way?

MR. WALSER: There are too many probability experts around here for me to make a statement like that.

DR. OKRENT: Oh, I mean just your judgment. I am trying to see what you mean by a high degree; when you say high degree, what it means, that's all.

MR. WALSER: Ninety percent.

DR. OKRENT: Now, how do you factor into that the possibility that there may have been some kind of a design error or fabrication deficiency that wouldn't have shown when it was proof tested at the original test.

MR. WALSER: Well, I have that confidence because I am not even approaching the strains which the material is capable to carry.

DR. OKRENT: Have you looked to see what kind of local failures, I will call it that, could lead to local strains that exceed the failure point of the liner and judge that these can't occur? I can recall back in Indian Point, for example, they had a surprise once. There was something having to do with the steam line that led to a problem on the containment liner. It came as a big surprise.

MR. WALSER: I am familiar with that incident.

DR. OKRENT: I am a reactor physicist by vocation, let's say, and I just sit here and listen to these engineers being surprised at a thing like that happening. Why should I assume that we don't have something like this here?

MR. WALSER: Exactly that incident shows you that despite those deficiencies, the liner didn't leak.

DR. OKRENT: Well, you know, you can always look at at the bright side.

(Laughter.)

MR. SHEWMON: That is why you design things out of steel instead of concrete.

DR. OKRENT: Yes, but you also put a factor in which you are now trying to use up almost fully.

MR. SHEWMON: No, he is using hardly any of it.

DR. SIESS: The stress-strain curve for the liner there, I assume, is for the liner material.

MR. WALSER: That is correct.

DR. SIESS: What would the ductility be for the weld or for the heat effectiveness of it?

MR. WALSER: For the weld it will have an elongation at least as much as the liner. The heat effect and so on will be somewhat reduced, there is no question about it. That is why we are not going to this limit in our acceptance criteria, staying way back. The weld axial strain, of course, is not any more than the membrane strain, except, of course, we have fiber strains. But don't forget, all the welds, for instance, in Zion, were really carefully tested, not only by visual and vacuum box, but also by 100 percent dye penetrant testing, 2 percent radiograph.

DR. KERR: Are there any other questions?

MR. WALSER: I just have one more slide. That is the conclusion. We consider that the ultimate pressure capacity of the containment is 120 psi gauge without the liner as a load carrying member. It is 134 psi gauge with the liner as a load carrying member, which is the likely case because the temperatures are not that high. The failure mode is hoop tendon yielding.

The temperature effects are probably not significant. Possible remedies are not recommended because it would be impractical. The effect of rate of pressure rise, of rate of temperature rise is probably not significant.

DR. OKRENT: If I can come back to the question on confidence, the very point you mentioned, that 2 percent of the welds were radiographed, I think if you were to feed that into a fault tree and put in some probability that there was a defect in the weld that didn't show up on the dye penetrant test or was ignored, either of which could have occurred, you might end up with some probability that isn't negligible at a pressure less than the one you are counting.

This is the kind of thing. That is only one of the paths that can get you to a leak. I just want to note that one wants to be a little bit cautious about being --

MR. WALSER: Yes.

DR. KERR: I should point out, Mr. Walser, that reactor physicists do very elaborate calculations, and then they have elaborate critical facilities because those calculations don't come out very well. That is the reason Dr. Okrent is skeptical of calculations.

(Laughter.)

DR. OKRENT: I have seen enough of my own come out wrong.

DR. KERR: Mr. Etherington.

MR. ETHERINGTON: Did any cracks develop in the liner during the original proof testing of the containment?

MR. WALSER: No, sir. The test did not indicate such cracks.

MR. ETHERINGTON: Thank you.

DR. KERR: Other questions or comments? Yes, sir. Very short.

MR. NOYES (?): Larry Noyes, Philadelphia Electric Company. It is my understanding that some in some the WASH scenarios, they assumed that when the containment failed, that it effectively lost structural support capability, and therefore other tightening such as steam water (?) tightening might be lost. Would you comment on that aspect of it?

MR. WALSER: The equipment stands entirely on the basemat, and the basemat deformations are very small.

DR. KERR: Thank you, sir.

Did you say one more presentation?

MR. PEOPLES: Yes, to just talk to Indian Point briefly on the same topic.

MR. TOLAND: My name is Richard Toland. I am with United Engineers and Constructors of Philadelphia. United Engineers was asked to address the Indian Point Units 2 and 3 containment vessels for their capability under a condition representative of a Class 9 accident.

This was done. We performed an evaluation on a realistic basis, allowing for such factors as the actual material properties, inclusion of the liner as a strength element. Such factors ordinarily are not allowed in the design of these containments.

The definition of capability. We defined capability as that combination of temperature and pressure which would produce a general yield state in the reinforced concrete structure itself. It is an upper bound of the elastic response of that structure. It is not failure. We addressed failure modes represented the same as Adolph's here, which showed that we could achieve the pressures associted with the actual yield state in the concrete structure.

This serves as a lower bound on the actual capability of the structure. The capability will be higher. In the conclusion of it, the Indian Point Unit 2 and 3 containments are shown to be able to withstand pressures of 126 psi gauge, which is 2.7 times the original design accident pressure 4.7. This is independent of the temperature.

The analyses were based on hand calculations with classical shell theory, with foundation analogies, and was justified on our experience and design analysis of these containment vessels and our understanding of the behavior of rdhneorbdc boncrete structures, and the agreement between prior hand calculations and computer solution from previous analyses.

The regions of the containment that we evaluated included the membrane region of the dome and cylinder, the discontinuity region at springline, at the base of the cylinder, the basemat, the large penetrations, including the equipment hatch and the personnel airlock, typical small penetrations, the liner and the liner anchorage system.

The latter ones we found to be all not limiting. They did not govern in our conclusion of 126 psi gauge. It was the membrane region in the cylinder which did limit us.

The next slide. This is a sketch of the reinforcement in the membrane region. These are hoop reinforcements which resist the PR membrane hoop forces, meridional reinforcement of the liner. It is these hoop reinforcements which are yielding across this section which is limiting our capability.

Next slide. Our hoop reinforcement is at this point. Prior to that pressure we are below this point and we are behaving elastically. We are behaving in the small deformation regime. There is linear elastic behavior. Beyond this point it is inelastic. We are starting to get large deformations. The strength capability of the rebar is much greater to alternate, but we are not trying to take account of that because it incurs very large displacements in the cylinder.

The displacements associated with the cylinder in this region are about 2 inches out of 840 inches on the radius.

Next slide. How did we get 2.7 times the design pressure? It is by the conservatisms in the design process itself. The original design pressure was multiplied by a load factor of 1.5. We have used the minimum strength of the materials, so instead of using the 71 ksi actual yield strength, we used 60, but then we had to incorporate capacity reduction factors of .9.

The strength of the liner was not allowed for. The seismic rebar was not considered.

Next slide. You factor these all together times 47 and it gives you 126 psi. This capability, again, is the limit of the elastic response. We are not delving into the inelastic, large displacement response. The limit region of the containment is one of high ductility located away from the discontinuities. The discontinuity regions of the containment have at least the conservatism of the membrane region. The original design was based on the ACI 318-63 code, which mandates additional conservatism in regions of low ductility. Shear, anchorage and compression do not govern the design.

I will answer any questions.

DR. KERR: I guess you can perhaps comment on Dr. Okrent's question about how uncertain you are about errors in fabrication or inspection have not invalidated the results of your analysis.

MR. TOLAND: My response to that is that that is one of my principal concerns. It is for that reason that we limit ourselves to the maximum of the small displacement response. It is when we get into the large displacement associated with yielding of this rebar that I am uncertain about where the weakest link will be, and I don't know if anyone can, because that is where the uncertainties associated with fabrication will come into play. I do not think that they will come into play prior to that point.

DR. SIESS: Would you put the last slide back up, please? I hope you did not rely on the additional conservatisms required by the ACI code for shear, and that you did check shear. Did you?

MR. TOLAND: We did check shear. We considered three aspects of shear. First of all, the code says that under the high membrane tensions that we have here, we have zero capacity in the concrete itself, and therefore the steel has to take all of it. We reviewed that aspect of it.

A second point is that the base shear, which is the one I am principally concerned about, is one which increases but it increases at a lesser rate than the actual pressure increases because the concrete is cracking and you are getting a softening of the structure there. That is a conservatism that you don't really mention.

In addition, in the design of this containment, we brought the hoop reinforcement all the way down to the base. That is totally redundant. It is not stressed under normal service. But as you start to increase the displacements, you can never displace them beyond what the actual membrane displacements would be. It is not a part of the shear capability, but the first two are.

MR. ETHERINGTON: Did you have to make any lining repairs following the original proof test?

MR. TOLAND: Sir, I can't really address that in detail. I know, in fact, that there was no liner tearing. The liner did not tear. What they did do was tear some of the anchorages from the liner. It would be predictable if they had predicted the liner actually being hit with that kind of temperatures. You can predict locally, very localized temperatures. It was repaired. It was put back in service and they had some insulation --

DR. SIESS: Which plant was that?

MR. TOLAND: It was Indian Point.

DR. SIESS: The incident at Midland gives you a little confidence in the toughness of the liner, too. There were very large deformations, and I don't believe the liner cracked clear through anywhere. Are you familiar with that?

MR. WALSER: I am familiar with a similar incident in Zion where by accident we blew a piece of liner away from the concrete and displaced it over a relatively small area, and there was no failure.

DR. KERR: Other questions?

Mr. Peoples.

MR. PEOPLES: Thank you. In the interest of time, Mr. Chairman, I would ask that the concluding remarks be entered in the record as though read, and that I would summarize just a couple of points from the concluding remarks.

DR. KERR: I am in agreement with that procedure.

(The remarks referred to follow:)

ENTER INTO RECORD [D L Peoples] 7/2/80

Zion/Indian Point Presentation
to
ACRS Subcommittee on Class 9 Events
July 2, 1980

IV. Scope ~~With~~ of Utility Effort

Thus far in the presentation we have provided details concerning the short term mini WASH-1400 study, the longer term probabilistic risk assessment of Zion and Indian Point, and the research activities related to severely degraded core behavior and containment structural response. I would now like to outline the scope of utility and industry studies which are being conducted to further define our knowledge of a core melt incident and mechanisms (Lights Down) 4 → for the mitigation of risk of such an incident. The utility effort is proceeding along two parallel paths. The first path is a probabilistic risk assessment study which will enable us to determine in a quantitative, comparable fashion those sequences which contribute most significantly to risk. Identification of contributors to risk may allow for action to be taken which will effectively prevent or mitigate severely degraded core accidents. The second parallel path is one of development of technology related to mitigation of the effects of a core melt accident.

The Pickard, Lowe & Garrick detailed probabilistic risk assessment of Zion and Indian Point Stations is well underway. In addition to the excellent administration by Dr. John Garrick, we have retained three senior consultants to help direct and evaluate the study: Dr. Ian Wall--EPRI, Professor Norman Rasmussen--MIT, and Mr. Saul Levine--NUS Corp. Westinghouse is performing sensitivity studies of various computer codes used to predict severe accidents and investigating phenomenology related to predicting severe accident transients. Commonwealth Edison, Consolidated Edison, and the Power Authority of the State of New York, NSAC, and EPRI are coordinating efforts to conduct additional research in the areas of core coolability, hydrogen control, corium-concrete interaction, and containment structural response. Potential consultants for these studies include Argonne National Laboratory, Dr. Robert Henry with Fauske & Associates, Westinghouse Electric Corporation, and Combustion and Explosives Research, Inc.

As examples, the following two research projects have been authorized and will be conducted by Westinghouse Electric Company. Laboratory tests will be performed to measure particle bed cooling including dryout correlations and effect of particle signs for the range of power density and particle size expected in LWR melt sequences. Also, a series of tests will be performed with the aid of Dr. Bernard Lewis to verify the "flame temperature criterion" over a range of initial temperature and pressure conditions and steam and hydrogen concentrations. Burn velocity, degree of completeness of burn, and the effect of containment spray will also be determined.

Architect-engineers for the utilities have defined the containment structural capability in detail.

The utilities, with the aid of Westinghouse and architect-engineers, are considering various mitigating features. Conceptual design work is proceeding. Continued phenomenological studies are necessary to define rational functional requirements. However, design concepts can only be evaluated consistently after the PL&G work is completed. Then this probabilistic risk assessment study will be used to test the impact on risk of any changes to systems or the addition of new features. Decisions related to the installation of a feature or other alternative action should compare the calculated risk, with and without the feature, to a relative safety goal.

(5) The magnitude of the utility effort is indicated in this slide. During the time period December, 1979, through June, 1980, the following man-months of effort have been expended: Utilities - 53, Consultants - 167. Projecting our activities through December, 1980, shows the following additional man-months expended: Utilities - 59, Consultants - 84. Significant computer time has been utilized in our studies. The total of historical and forecast costs are shown on this slide.

(6) The utility program plan has three basic objectives. One, to complete the technical assessment related to probabilistic risk assessment, hydrogen behavior, containment capability, sensitivity studies, and conceptual designs of mitigating features. Two, to achieve a common technical base with the NRC, and three, to be (7) prepared for degraded core rulemaking. This program plan requires accomplishment of the following tasks by utilities and the NRC: (1) utility technical assessment, (2) utility probabilistic risk assessment study, (3) NRC technical assessment, and (4) definition of a safety goal.

Upon completion of these four items, joint technical review is needed to reach agreement and to prioritize alternatives for further investigation. Then, in order of priority, detailed risk evaluations of features and value impact analyses can be completed so that rational decisions can be made. Decisions related to significant mitigative or preventative features must be made in the context of adequate technical information and in relationship to a safety goal.

(8) From our studies on Zion and Indian Point Stations we have gained insight into the work that must be accomplished to conduct a degraded core rulemaking. This slide shows a functional diagram of the principal elements related to a degraded core rulemaking. Listed in the top lefthand corner is the definition of a safety goal as the basis for decisions. Such a safety goal is needed if one is to determine the need for mitigative or preventive features. It is our current understanding that responsibility for such a safety goal has been assumed by the NRC Commissioners. In parallel with the definition of a safety goal are the following: probabilistic risk assessments of representative plants and definition of risk dominant sequences, phenomenological studies of core melt behavior, containment integrity, and fission product removal, and conceptual designs of additional mitigative features and additional preventive devices. All of these inputs are necessary if one is to decide if additional features should be considered.

At this point one must also consider the interrelationship of plant design to emergency planning and siting as all three impact public safety. If additional mitigative or preventative features are warranted, the next step is to define functional requirements, vis-a-vis the safety goal, and to define criteria and standards for construction. A Rule in the format of functional requirements and defined criteria, would be subject to review and subsequent issuance as appropriate.

(SLIDES OFF)
(LIGHTS UP)

It is our conclusion, based upon the original design review and the mini WASH-1400 study, that Zion and Indian Point Stations are safe for continued operation. We fully expect that the longer term probabilistic risk assessment currently being conducted will confirm our earlier work. It is on this basis that we believe that any decisions related to the installation of additional mitigative or preventative features at Zion or Indian Point be deferred until after the industry-wide degraded core rulemaking has been conducted.

As we close our presentation, I would like to reiterate the actions which we would ask ACRS to consider: (1) encourage development of a safety goal, (2) support PRA as an appropriate means of evaluating reactor safety, and (3) support the realistic analysis of Class 9 accidents.

We appreciate the opportunity to present to this ACRS Subcommittee a summary of the work which we have conducted related to Class 9 accidents. At this time I would be happy to take any questions which you may have.

MR. PEOPLES: With regard to the probabilistic risk assessment that is being carried on by Pickard, Lowe and Garrick, we have retained three senior consultants to help direct and evaluate the study. These include Dr. Ian Wall, now at EPRI; Professor Norman Rasmussen at MIT; and Mr. Saul Levine with NUS Corporation.

We have hired these gentlemen to help us learn from WASH-1400 and to do as thorough a job as we can do in this work.

In addition to this activity, Westinghouse is performing sensitivity studies of various computer codes used to predict severe accidents and investigating the phenomenology related to predicting severe accident transients. The three utilities, NSAC and EPRI are coordinating efforts to conduct additional research in the areas of core coolability, hydrogen control, corium-concrete interaction and containment structural response.

Two particular examples that we are putting our money up front on are to be coordinated by Westinghouse Electric Company, and these include laboratory tests to measure particle bed cooling, including dryout correlations and effect of particle sizes for the range of power density and particle size expected in light water reactor melt sequences.

Also, a series of tests will be conducted with the aid of Dr. Bernard Lewis to verify the flame temperature criterion over a range of initial temperature and pressure conditions, and steam and hydrogen concentrations. Burn velocity, the degree of completeness of burn and the effect of containment spray will also be determined, and that may answer a question you had earlier, Dr. Shewmon.

So we are going to be trying to look at that in an experimental fashion.

From our studies on the Zion and Indian Point stations, we have gained insight into the work that must be accomplished to conduct a degraded core rulemaking. There is one slide I would like to put up very briefly. This slide shows a functional diagram of the principal elements related to a degraded core rulemaking. Listed in the top left-hand corner is the definition of a safety goal as a basis for decisions. We keep coming back to that.

It is our current understanding that the responsbility for such a safety goal has been assumed by the NRC commissioners. In parallel with the definition of a safegy goal are a number of other elements, including probabilistic risk assessments at representative plants, and definition of dominant risk, dominant sequences, phenomenological studies, core melt behavior, containment integrity, and fission product removal, as well as conceptual designs of additional mitigative features and additional preventive devices.

At this point one must also consider the end relationship of the plant design to emergency planning and siting, as all three affect public safety, showing that they have to interrelate. I have also indicated that other elements of concern to the Commission, such as the Indian Point hearings, have an impact because they relate to this work. They cannot necessarily go on independently if we are to come out with a consistent product.

If additional mitigative or preventive features are warranted, the next step is to define functional requirements vis-a-vis the safety goal, to define criteria and standards for construction. A rule in the format of functional requirements and defined criteria would be subject to review and subsequent issuance as appropriate.

I would like to thank everybody today for hearing what we have done and offer that if we can answer any more questions, we would be happy to do so.

DR. KERR: Are there any questions?

DR. OKRENT: Yes, I have a question. What do you recommend for the safety goal the NRC should use?

MR. PEOPLES: We have been working on that with Ed O'Donnell in the AIF. I think Ed has a basically sound approach at this point in time. It is a damn tough question, I will admit, and he has come up with a logic that

I have gotten myself through, at least, and feel more comfortable with now than when I first started with it, but I think that is an approach.

MR. STRATTON: We don't know what that is, though.

MR. PEOPLES: We presented it yesterday. I'm sorry.

VOICE: It was a presentation yesterday. We would be glad to drop you a copy.

DR. KERR: Are there other questions?

VOICE: Is that satisfactory? Do you have a follow-on question?

DR. OKRENT: I would rather say it is adequate for today.

(Laughter.)

VOICE: Was it adequate for yesterday? That's the question.

(Laughter.)

MR. SHEWMON: To the layman, it sort of seemed similar to what the ACRS was putting out, or some parts of it.

DR. KERR: If you believe the agenda, we have two additional sets of presentations for today, and I believe the agenda, but I don't believe the schedule.

I planned to schedule a ten-minute break at this point and then go to the presentation by NRC on the consideration of the Class 9 accident. I think Mr. Bernero wants to make a brief presentation, and then we are going to discuss budgets. I would guess that we might be finished by 8:00 or 8:30, so you can make your plans accordingly if you plan to participate in the rest of the session.

(Brief recess.)

DR. KERR: Please go ahead.

MR. MEYER: My name is Jim Meyer. I am a member of the Nuclear Regulatory Commission. The purpose of my presentation this afternoon is to bring the Subcommittee up to date on the Zion/Indian Point action, to respond to certain comments and concerns that the Subcommittee shared with us at the last meeting, May 9th, and also to give some general idea of where NRR is heading in terms of the Class 9 rulemaking.

I would like to start out by presenting very briefly an update on the goal and the approach that we are presently taking on the Zion/Indian Point program.

The program, as you are probably aware, is divided into two parallel programs. The first is the actual mitigation feature study that we reported on on May 9th, and the second is a concurrent program on the Zion/Indian Point risk analysis. What I have tried to do here is give you some idea of the program logic, with a decision late this fall.

The mitigation feature study, again, as you are probably aware, is divided into two semi-independent studies: one, the utility study that you heard about in some detail this afternoon, and the NRC study. We have completed five technology exchange meetings that have been held over the past several months that deal with the technological aspects of mitigation features and accident sequences that dominate risk and sequences that we want to protect against in considering mitigation features.

The utilities study will culminate in a final report that will be issued sometime in late summer, as I understand it, the month of September. The NRC study will have two aspects to it. Still this month we are going to issue some requirments and criteria on mitigation systems for comment, and I will have more to say about that in a few minutes, and we will be preparing a staff report, our recommendations on the mitigation features program that we have been conducting.

That report will be based on our own work as well as the work performed by the utilities. Paralleling this is the work on the risk analysis. You heard brief overviews of the OPS mini-WASH-1400 that was completed in February as part of the 60-day study, and you also heard an outline of the much larger probabilistic risk assessment program by Picker, Low and Garrick, that report to be complete, again, in late summer. I think they mentioned September.

At the May 9th meeting there was some concern about NRC not having an appropriate program in place in order to perform an equivalent study on Zion and Indian Point in the area of risk analysis. At that time we had intended to have an IREP, that is, a reliability assessment program analysis, completed for Zion and Indian Point in this time frame.

However, due to two facts, one being the constraints of budget and other priorities on the probabilistic analysis staff, and the other being that the utilities have a major program under way, it was decided that there would be no IREP program for Zion and Indian Point per se at NRC.

As an alternative, NRC will become very closely involved in following the probabilistic risk assessment analysis being performed for Zion and Indian Point, and will also do an assessment of the mini-WASH-1400 report that has already been published. This relationship now has been set up where NRC staff people will be very closely following the Picker, Low and Garrick work.

When the report is complete, then NRC will complete its review and evaluation of that risk assessment and it will basically try to answer two questions. The first question is are the probabilistic risk assessment report or reports okay; are they basically acceptable reports with adequate assessments of the risk of design in Indian Point plants?

If the answer to that question is yes, then the further question will be asked of based on these two studies, is their undue risk from Zion or is there undue risk from Indian Point? If the answer to that question is no, there is determined to be no undue risk from Zion or Indian Point, then it will be the staff recommendation that Zion and Indian Point action be folded into the rulemaking: that is, that they will be no longer singled out with specific requirements later on in the year for implementing mitigation features.

When the staff completes its report on the mitigation features, the following question, hopefully, will be answered. Do the features that have been considered sufficiently reduce risk? If that question is yes, together with a yes response that there is undue risk from Zion and Indian Point, then the staff will recommend that mitigation features be required on either Zion or Indian Point.

If, on the other hand, it has been determined from this assessment of the risk analysis that there is undue risk from Zion and Indian Point but it is also the staff's feeling that the features under consideration do not adequately reduce risk, then, as I have indicated with a box with a question mark, it is not clear what the next approach will be.

The other decision that I did not cover here in this diagram is that if it is felt that the probabilistic risk assessment work is not adequate, then there will be some action on the deficiencies of that report. I would like to emphasize that this box, "Require Mitigation feature on Zion and Indian Point," will be in the form of an NRR recommendation.

There are, of course, other inputs to that particular decision. It will go as a recommendation to the commissioners and the commissioners will consider that recommendation and, I am sure, also the recommendation of the special task force that the commissioners have established and that Bob Bernero will be talking about after I complete my presentation. They will also, I am sure, be taking into consideration the advisement of the ACRS.

As I indicate in the time schedule, these decisions are hopefully to take place yet late this fall.

DR. KERR: Does late this all mean about the middle of December?

MR. MEYER: December 20th, yes.

(General laughter.)

MR. MEYER: If there are no questions on this present strategy, I can move on to the next agenda item.

DR. KERR: Please do.

MR. MEYER: There were a number of questions at the last subcommittee meeting, centering around what our bases are for the development of the Zion/Indian Point mitigation features, and in particular, the appropriate approach for determining the requirements and criteria for such features.

I would like to first present a very brief Vu-graph that runs through the basic strategy, and then if you would want to get into some of the details of that logic, we could move on to the accompanying Vu-graph.

The first element is perhaps self-evident. The basis for developing mitigation features and the corresponding requirements and criteria are based on desire to reduce the risk from the particular plants. We have in the past used an order of magnitude as a yardstick for an appropriate risk reduction.

This would come about by preventing containment failures by mitigation features that would otherwise occur. We are concentrating on containment failures by over-pressurization, and also considering failures by basemat melt through. Once we understand the containment failure modes, pressures, then we move on to defining functional requirements for the system which will prevent the containment failure.

This, then, leads to a first try, anyway, at defining and designing a system that meets those functional requirements Once this is done, then we assess the consequence mitigation capabilities of that particular system, using the CRAC code that we have heard about earlier today.

Another important ingredient in developing requirements and criteria is to assess the reliability of the system. That are, what are the negative aspects, the competing risks that may make what would appear to be a very attractive system at first glance less attractive due to opening up other possibilities for release of radiation from the containment.

DR. KERR: I am surprised at that definition because I would have thought that by the reliability of the system you would mean the likelihood that it would work. You seem to be saying that reliability means have I possibly introduced unexpected systems interactions which may be deleterious.

Am I misunderstanding you?

MR. MEYER: You are quite right. The word "reliability" is too specific to one particular aspect that should be considered, namely, how reliable is the system, what are the probabilities that it would fail, and thereby provide a release path for radioactive materials. Perhaps that item 6 would have better read to assess interactions with other systems, assess competing risks that that system would introduce.

DR. KERR: Do you propose to design toward some reliability of the mitigation system, or are you going to design the mitigation system and then analyze it to see what reliability it has? Or have you decided?

MR. MEYER: We know it will be an important aspect in considering the requirements and criteria, but we have not taken it much beyond that in terms of specifics. Our goal is not, of course, to design a system. Our goal is to use possible design concepts as an intermediate step in arriving at appropriate requirements and criteria which then would be used by the licensee, who is much better qualified to do the actual design.

DR. KERR: I asked the question because a literal interpretation of 2 and 3 would lead me to think that containment failure was going to be, the probability was going to be made zero, and I don't imagine you mean that literally, or do you?

MR. MEYER: No, I do not mean that literally.

DR. KERR: I thought perhaps you did have in mind ultimately specifying some reliability goal for your mitigation system.

MR. MEYER: We would look at it in the context that by having a feature that will accommodate certain severe accidents, we can reduce the consequences from those accidents sufficiently enough that the overall risk reduction will be approximately a factor of 10, and that overall risk reduction incorporates all the other concerns regarding the system interactions effects, the competing risk effects.

So we do not look for a system that will guarantee zero failure at the containment.

I would like to spend a few minutes being a little bit more specific about the process involved. What I have done here is tried to portray graphically the process, the logic by which we intend to arrive at this important box, which is the definition of the requirements and criteria for mitigation features.

But before we get to a consideration of specific mitigation features, which is outlined in the dotted large box, it is important to step back and consider what ingredients go into taking [illegible] a consideration of a specific mitigation feature. This is how it developed historically for Zion and Indian Point.

The first question that was asked was do we have plant specific risk assessment that is complete, that would tell us what the dominant action sequences are for Zion and Indian Point? In the case of our work we felt that that was not complete, so we went ahead and determined what we felt were best estimate sequences that were major contributors to risk for the Zion and Indian Point plants.

If you had this information, and now we do, or at least it is starting to come in, then we could determine the specific risk dominant sequences for the Zion and Indian

Point plants. In any event, from either set of sequences or from a combination of the two, we select the accident sequences that for the reactor system as it is presently designed would be those sequences that would be major contributors to risk from overpressurization of a containment, and possibly from melt through of the basemat.

From this there is an initial calculation of the containment loadings, and here the code we heard about from P. Cybulskis is used, the MARCH code, as an indication of the containment loadings based on those accident sequences. From those sequences we could get a basic idea of what the more important functional requirements should be for any mitigation system.

Of course, the dominant one is to maintain containment pressure below the failure pressure of the containment. Once these have been determined, then this information plus a determination of what our goal is in this whole program -- that is, the determination of the risk reduction requirements plus selection of a specific mitigation featue -- are the three inputs that allow us to start thinking about the determination of requirements and criteria.

I will just walk through those very briefly with the example that you heard a lot about over today and the meeting May 9th, the TMLB' that has the large pressure spike. If we would look at that as an important controlling sequence, then we would specify requirements, one of which would be to accommodate the steam pressure spike, and go ahead and do a very preliminary conceptual design of a feature that would do that.

In this case let's talk about a filtered vent system. For the initial TLMB' results, the opening, for example, in the containment was determined to be so large that it was impractical and not feasible, so it was a matter of going through where basically you downgrade the initial requirements and consider requirements that would accommodate most of the other accident sequences but perhaps not this steam spike; to go through, do a conceptual design of a feature, again ask the question is it practical and feasible, keeping in mind that we are thinking in terms of a backfit to Zion and Indian Point, and then proceeding to calculate the containment loading with that feature present: that is, doing a MARCH-type analysis and estimating the pressure at which the filtered vent would open up the amount of flow through the filtered vent and the amount of radiation that would finally be released from the filtered vent system.

Then you would do another CRAC analysis, or an initial CRAC analysis without the system, and then an additional CRAC analysis with the system, do a comparison, and determine whether this meets our risk reduction requirements.

If it doesn't meet the risk reduction requirements, then we attempt to go back and upgrade the requirements so that this particular mitigation feature by itself can meet the risk reduction requirements. If it does meet the risk reduction requirements, then all that has to be determined is whether this is a favorable mitigation feature relative to others that are being considered. If it is favorable, then there is the NRC recommendation to incorporate that mitigation feature.

If it is not a favorable one relative to another feature that has been analyzed, then there is no further need to consider it. There is the possibility that a combination of mitigation features will be practical in terms of back fitting the Zion and Indian Point, and will together meet the risk reduction requirements.

Therefore, there is also the possibility that even though you do not wish to upgrade these requirements any further, you can go through again the same process by combining it with another mitigation feature, hydrogen control, for example, where the hydrogen control plus a filtered vent might be adequate and feasible, adequate to meet the risk reduction requirements, and feasible from a backfit point of view.

Eventually what we would like to have is a system that does meet the requirements. Once this has been achieved, then whatever the basic functional requirements were, together with what the specific requirements and criteria were, will help to define those requirements that we would recommend in our staff report for the mitigation feature.

Are there any questions on our thinking along these lines?

DR. KERR: The box on the left talked about determining the risk reduction requirements. Does that imply that you have not yet determined them?

MR. MEYER: The overall risk reduction requirement, as I mentioned before, the number that is being considered is about an order of magnitude. If it could be demonstrated with sufficient confidence that the risk reduction was going to buy you about an order of magnitude, then we felt that that would be a sufficient risk reduction requirement. Of course, anything better than that would be better.

The factor of 10 also includes consideration of what I referred to earlier, namely, the consideration of interaction with other systems and competing risks. So the specific risk reduction requirement for this particular mitigation feature would have to be considerably more than that because you would want to fold in all the so-called negative aspects of this feature to give you a net reduction in risk of a factor of 10.

DR. KERR: I would assume the factor of 10 is based on someone's assumption that the actual risk at these sites is perhaps ten times some goal, and that therefore the risk ought to be reduced toward this goal. Suppose one discovers, after the detailed analyses are completed, that this estimate was in error and that the actual risk is 100 times that at the site.

Would one then set as a risk reduction goal a risk reduction of 100?

MR. MEYER: Since that was the initial logic that set up the factor of 10 then a logical follow-up would be to try to have a factor of 100 improvement in the overall risk.

MR. ETHERINGTON: Suppose the determination were made that the original estimates were too pessimistic and you don't need to do anything? Will you buy that?

MR. MEYER: Yes. That is what I tried to present in the earlier Vu-graph. In fact, that is what will be part of the consideration this fall when this particular analysis and assessment is combined with the NRC review of the risk reports.

DR. LEE: How would you go about assessing if certain mitigating features would indeed need the requirement for reducing risk? What do you start off with?

MR. MEYER: Well, the key calculation in determining whether in fact we have met this risk reduction goal would be the comparative consequence analysis indicated here. This type of comparative consequence analysis has been reported on in the Sandia, Zion and Indian Point study, where they have taken an initial look at conceptual designs and actually done CRAC analyses with and without the filtered vent to see what the benefits of that system are in terms of early fatalities, latent cancers and property damage.

Then this is considered together with an assessment of the competing risks that are associated with this particular design in order to make a determinaton regarding whether the overall risk matches the initial requirements that were established.

DR. LEE: Would an IREP program be involved at some point in that assessment process?

MR. MEYER: Well, it is actually the substitution for the IREP program, which is the evaluation of the utility risk analysis, but yes, it is involved. I indicated that on the initial Vu-graph. A part of the NRC study is taking information that comes out of this review and evaluation and incorporating it in the NRC study.

For example, it was mentioned earlier that AB Burn is a sequence that NRC has considered in order to assess the functional requirements for a filtered vent. The initial results from this report indicate that AB burn is a very low probability event. Well, that would be then factored into the assessment by, for example, removing that sequence as a sequence that would define the functional requirements.

So yes, there is input from the risk analysis in the determination of the appropriateness of mitigation features.

DR. LEE: I guess I probably didn't pose my question properly. Suppose we decide to go for filtered vented contained system, and somebody has come out with a certain design with certain features identified and so on, and then somebody has to go through some kind of risk assessments again with that particular system incorporated?

MR. MEYER: That is correct, yes.

DR. LEE: And that is based on design features that you have to pass judgment before actual manufacturing, anything is done.

MR. MEYER: Yes, that would have to be done. Hopefully, it would be a minor addition to the overall risk analysis to incorporate that assessment into the overall risk analysis and a determination of the overall risk reduction, but that would be done.

DR. PLESSET: Do you have a great deal of confidence in your ability to make these comparative assessments?

MR. MEYER: I could answer that in two parts. We can certainly do what has been done to date in terms of comparative consequence evaluations. It is obvious from discussions today and at other meetings that there is considerable engineering judgment or -- well, I guess you would call it engineering judgment --regarding the accuracy of the models and the appropriateness of, for example, using MARCH CORRAL, which was never meant to be a design tool, to be part of this analysis

But we can do the comparisons as they have been done in the past, and we feel that in the general sense we can have a pretty good handle on order of magnitude benefits from these systems.

DR. PLESSET: It is an order of magnitude that you are after. Your error is an order of magnitude. Either way you may not be doing what you think you are doing.

MR. MEYER: That is correct. If our error is that big, then that consideration would have to be taken into account in our recommendations to the Commission.

DR. KERR: Are there other questions? Please proceed.

MR. MEYER: I would like to briefly run through sample criteria and requirements for a filtered vent system. These are very preliminary and I have only put down numbers in order to focus in on a specific system and to stimulate some discussion regarding numbers.

First I should correct one item in this list of sample criteria and requirements. I indicated down here a system characteristic being a suppression pool with submerged gravel. That, of course, is not a criteria or requirement. That is a design as response to a criteria or requirement. I will comment briefly on why that is there.

The three most important basic criteria are indicated by the first three bullets. The pressure for venting initiation is given as 100 psia. This is considerably higher than what was initially considered appropriate, but as was discussed earlier, the containments appear to be considerably stronger than we had first given them credit for, so it was felt appropriate to increase the pressure from venting initiation to that value.

The flow rate exiting containment is indicated at 150,000 cfm. This, I should emphasize, is a very soft number. Depending on the outcome of research and assessment on the pressure spike, that number could change anywhere down to 18,000 cfm and on up to higher than 150,000 cfm.

MR. STRATTON: How big a valve or hole in the containment does this require?

MR. MEYER: The diameter hole that I believe is required for this is under 3 feet in diameter, which --

DR. SIEGEL: How much?

MR. MEYER: Under 3 feet in diameter.

MR. STRATTON: Does that mean greater than 2-1/2 feet and less than 3?

MR. MEYER: The reason I don't have the specific values is that the specific values are normalized to 60 psia, and this is for 100. I could get those for you.

MR. STRATTON: It's close enough.

MR. MEYER: In any event, there are existing penetrations of this size on these containments.

DR. OKRENT: On the pressure for venting initiation, were you proposing that the only mechanism would be a relief valve that was automatic; in other words, it went at this pressure?

MR. MEYER: That's correct, yes.

DR. OKRENT: And it couldn't be initiated manually at a lower pressure?

MR. MEYER: Well, the reason why later on here I say that the system is passive is because we feel at this time that operator control or intervention may introduce more negative aspects to the system than positive, and that there would be certain overrides, provisions that would probably be built into it. But initially as it is envisioned it would be a pressure-relief type as opposed to a rupture-disc type that would reset at some lower pressure value, say 70 psia.

But our initial thinking is that it would be passive, that it would not require electrical power, that it would not require operator action. It could perhaps be overridden by operator action.

DR. OKRENT: I would suggest you parameterize your criteria with regard to the pressure, the system for pressure relief, and whether or not it automatically closes; not choose a single path -- do you understand what I am saying -- so that you have a chance to look at some alternate designs and their pros and cons.

I think it would be a mistake to try to guess too early that we knew which was an optimum way.

MR. MEYER: I didn't want to leave the impression that we feel that this is optimum --

DR. OKRENT: Again, what I am suggesting is don't at this stage -- I suggest that instead of trying to find one best, I would try to find three or four likely combinations and have them all looked at.

DR. KERR: It also seems to me that one might consider the possibility that one uses them in some as yet unforeseen situation. After all, it is going to be expensive and you have it, and I would therefore be reluctant to make it entirely automatic.

MR. MEYER: Those considerations are --

DR. PLESSET: You don't want to exceed the speed of sound in that discharge rate, either, I think.

MR. STRATTON: It is marked four-tenths.

DR. PLESSET: I made a very rough calculation that went a little higher than that.

MR. STRATTON: Okay.

DR. PLESSET: And that is getting up there.

MR. STRATTON: It's a good wind tunnel.

VOICE: It appears to me you should also leave open at least consideration of operator action, I think, from what Mr. Kerr said, and partly just to take advantage of the flexibility that you are given.

MR. MYER: Well, I agree with that. There would be situations where advantage could be taken of that feature. However, there is the real concern that there could be an inadvertent of the system when it was inappropriate, anticipating, for example, a large accident, opening the system, and then it turning out there was, in fact, no large pressure rise in the containment.

We haven't touched on the decontamination factors. These are, again, presented as samples, decontamination factors that are felt readily achievable with existing technology, that achieving these decontamination factors isn't terribly expensive, and make for easier retrofit capability.

The Seismic Category 1 is included there because of the consideration that there may be a seismic initiator to an accident, and thereby you would want to have a filtered vent system, for example, that would be able to withstand that seismic event.

The last bullet, as I mentioned, is not a criterion. One of the obvious criteria would be that there would have to be the size space available at the sites to accommodate this filtered vent system, and it is considered by the people studying it at Sandia that a suppression pool with submerged gravel would be the route to go in order to minimize the size.

DR. KERR: Is there a report or something that maybe ACRS already has that gives the background for arriving at these criteria?

MR. MYERS: The report that ACRS already has is the rather extensive Sandia study on filtered vent systems that considers many systems, both --

DR. KERR: No, I am asking how one arrives, for example, at 100 psia. I realize these are firm, but you had to go through some process to arrive at these. Is there something written that describes --

MR. MYERS: There is nothing written down that gives the background justification for these. They are --

DR. KERR: Nothing at all.

MR. MYERS: Nothing that is written down. They can be inferred from the report, many of these particular criteria requirements.

DR. KERR: You mean one can look at the report and just sort of pick these off.

MR. MYER: No. They were determined by talking to the Sandia people that are conducting this program, and that is how they were arrived at.

DR. KERR: I would assume that before one arrives at some final criteria, there will be a formal analysis of some kind of documentation.

MR. MYER: That's correct. In fact, that will be the substance of the staff report I referred to earlier. That will contain the recommendations on the mitigation features. The recommendations will be recommendations for requirements and criteria and the bases for those requirements and criteria.

DR. KERR: Thank you.

DR. LEE: Do you have some estimate of how large the suppression pool system could be or had to be?

MR. MYER: I don't have the details in front of me, but there is a considerable amount of information in the --

DR. KERR: Well, it is probably bigger than a bathtub. Is it the size of a containment?

MR. MYER: Perhaps Charlie Kelber could correct me, but I think it is 150,000 cubic feet, a typical volume in a suppression pool with submerged gravel type of filtered vent system to accommodate an integrated heat load that one would expect from Zion or Indian Point.

DR. KELBER: It is about 150,000 cubic feet of liquid plus gravel, water plus gravel, and plus some air space on the order of about 100,000 cubic feet or so of air space above that.

DR. KERR: That's close enough. Thank you.

DR. PLESSET: I have heard of suppression pools before, and there is some problem with them, too.

MR. MYERS: Yes.

DR. PLESSET: Oh, okay. You know about that.

MR. MYER: We are aware of those problems. We feel that there are less problems with the suppression pool as a heat sink as opposed to a dry gravel bed, for example, although that has also been looked into.

For the sake of time, I would like to skip the other example that I had in your handout unless you have some specific questions regarding it. These were criteria that were suggested by the staff at the fourth technology exchange meeting, and there was considerable discussion regarding those criteria. Unless you have any further specific comments, I would like to move on to the next topic.

DR. KERR: Any other questions or comments? Please move on.

DR. LEE: I would like to raise a question. If you want a flow rate of 150 cubic feet per minute and would like to fill 100 cubic feet of air space, you have to fill that space, essentially, in a minute, essentially on that order of magnitude, a minute or half-minute or something like that. Do you think the present technology exists to accommodate a fairly rapid influx of air mixed with certain things without failure or without undue concern?

DR. KERR: I'm not sure what you mean by undue concern. There have been conceptual designs that have been studied, and the judgment of those who have studied them is that they are feasible in an engineering sense.

DR. SHEWMON: It is Seismic Category I, so that would take care of that.

DR. LEE: It is possible, too, to do that. Is that the consensus of the people who performed the feasibility design calculations? That is all I would like to know.

MR. MYER: Yes.

MR. BERNERO: Excuse me. Bernero from NRC. I think he is asking the question is it filling that 150,000 cubic feet in a minute or so. It starts out with a pool full of water with submerged gravel and just this gas going.

DR. LEE: Right.

DR. OKRENT: What question are you asking, Dr. Lee? What is your concern?

DR. LEE: The air space has to be filled with the exiting gas in a minute or half a minute or something like that.

MR. MEYER: The non-condensables are only a fraction of that that is being released from the containment.

DR. LEE: But something is flowing out at that flow rate, right?

MR. MEYER: Right.

DR. LEE: The gas, presumably.

MR. MEYER: The non-condensables and the water vapor.

DR. LEE: But the Seismic Category I, does it guarantee the transient effect can be also contained?

MR. MEYER: No, I didn't say that the Seismic Category I would guarantee that.

DR. LEE: I'm sorry.

MR. MEYER: All I said is that the dynamic loadings on that system have been considered, and it was determined that it is a feasible system to design.

DR. OKRENT: I think it is probably a small flow rate compared to what you have in the large LOCA, isn't it?

DR. LEE: So it is a sort of secondary containment that we are considering, and that would presumably not be built to the same specifications the primary containment has been built.

DR. SHEWMON: There certainly will be impulse loadings at --

MR. OKRENT: Well, one reason for gravel was if you are worried about losing the fluid, gravel has advantages in that direction.

DR. KERR: I would suggest that we not design the filters and containment today. We now have the problems and we can go home and design.

DR. KELBER: If I might just pose a comment, the Sandia report, NRC 1410, I think it is, does contain a number of typical design variables for pressure drops, loadings and so on. The pool with submerged gravel is actually an invention of Bob Hilliard up at Hettle (?), and there have been a number of tests up there with similar types of problems but with sodium-type loads.

DR. KERR: I can't really believe that he was the first man to design a pool with submerged gravel, but go ahead.

DR. KELBER: I understand it is an invention.

MR. MEYER: I would like to continue with an update on the Zion and Indian Point mitigation features study. Several of these points we have already touched on in the first Vu-graph. The technology meetings which started May 7th and 8th have been concluded. The mitigation feature requirements and criteria we intend to issue for comment sometime yet this month.

The staff report on the mitigation features, as I mentioned, is due in late fall. The licensee report on the same program is due --

DR. KERR: Excuse me. The mitigation feature requirements and criteria are the kinds of things we saw on an earlier Vu-graph, and you told me at least with that group of things there is no written justification. So by July 1980, which is today, we are going to have criteria and justification therefor in publishable form?

MR. MEYER: The intent of issuing criteria for comment is different than the intent of the final report in late fall in terms of it being a definitive report.

DR. KERR: But surely you aren't going to issue something for publication for which you don't have a fairly signficant analysis and in which you have some confidence, are you?

MR. MEYER: That's true. I didn't mean to --

DR. KERR: I guess I am puzzled. Go ahead.

MR. MEYER: The intent has been and was in the schedule that we showed you in May that we would be issuing something in July that reflected what we felt were appropriate requirements and criteria. Perhaps they will be more functional requirements as opposed to specific design criteria, but we are committed to issue this for the purpose of response from the ACRS, from the licensees and from the public.

DR. KERR: MR. SHEWMON.

DR. SHEWMON: I guess I am reading for the first time, maybe, that this is mitigation features, not safety goals or things of that sort. You are going to say there will be these features?

MR. MEYER: Are you referring now to the staff report?

DR. SHEWMON: The two, yes, mitigation feature requirements and criteria.

MR. MEYER: On the first Vu-graph, I indicated that we would be in the fall making a recommendation whether we felt that the mitigation features on the consideration would, in fact, reduce the risk by, say, a factor of 10 at Zion and Indian Point. This, then, will only be part of the consideration of whether it would be appropriate to go ahead with these features.

The other part of it is more related to your concern of matching it against a risk goal, and that is to compare the risk analyses that are being conducted and making --

DR. SHEWMON: Tell me again what this July document is going to do, then. It specifies mitigation feature requirements and criteria for consideration?

MR. MEYER: The intent of this July report is simply to get on the street our present thinking on functional requirements, oth r key requirements for these mitigation features, so as to invite comment from the licensees, from EPRI, from the public, and to allow us to incorporate those comments and considerations into our late fall staff report.

DR. SHEWMON: Okay. And the safety goals will be defined when?

MR. MEYER: The safety goal is this question right here.

DR. SHEWMON: That was December 20, give or take.

MR. MEYER: This is December 20, give or take. And that judgment, as you notice, will be independent of the mitigation features work that we are doing. It will be dependent on the NRC review evaluation of the probabilistic risk assessments that are being conducted by Pickard, Lowe and Garrick and the mini-WASH-1400 report that was completed by Offshore Power.

DR. KERR: Can assume, then, that the mitigation feature has the ability and multiplicity so that, for example, if one mitigation feature will reduce the risk by a factor of 10, two would reduce it by a factor of 100, or maybe whatever the number is, so once you determine the characteristics of a mitigation feature, it remains to determine how many of them you need to put in series or parallel or something like this?

(Laughter.)

MR. MEYER: That would not necessarily be the case. In fact, if a filtered vent would accommodate all the credible hydrogen burns, then there perhaps would be no need for requiring hydrogen control measures, which is another mitigation feature. So no, they are not additive in any sense.

MR. SEALE: Just to clarify something again, maybe ad nauseum, this document you are going to put on the street is going to say you want to reduce particulates by 100 and iodine by 100, or it is going to say you want a vented containment system? And if it says that, is it going to say that you want it to initiate at 100 psia and so on? Exactly what level of specificity if this three-week report you are going to write going to have in it?

MR. MEYER: It will certainly contain the first level of functional requirements. By that I mean the requirements for pressure to be relieved or the system to be vented at a given pressure, taking into account the containment strength and that type of thing. It will contain requirements regarding decontamation factors that are consistent with risk reduction goals. It will contain requirements, probably functional requirements of integrated heat load that the system will have to accommodate.

It gets more specific after that in terms of site requirements or reliability requirements, or the list of criteria could be very long and extensive.

MR. SEALE: You are not going to specify that they meet this goal by using the suppression pool with gravel.

MR. MEYER: No.

MR. SEALE: That is up to the --

MR. MEYER: In fact, there would be various mitigation options open.

DR. LEE: May I follow up once more? In the next two to three weeks that we are talking about, do you expect that we can add substantially new information or input from the probabilistic risk assessment study for Zion and Indian Point specifics more than we do have now?

MR. MEYER: The July report that I am referring to is part of this arm of the parallel study.

DR. LEE: Sure, but you might need some input, according to your dashed line.

MR. MEYER: Yes. There will be, for example, the considerations of those accident sequences that are appropriate for Zion and Indian Point.

DR. LEE: My question is do you expect to get substantially more new information by that time, by the end of July, than what we have now?

MR. MEYER: I don't think there will be much more information than what we learned, for example, earlier today from Dean Walker when we gave the presentation on the mini-WASH-1400 study.

DR. LEE: So you have to make the decision based essentially on what we have today.

MR. MEYER: Yes.

DR. LEE: Then according to the presentation we have heard, it is concluded there is no need for additional mitigating features whatsoever, unless I misunderstood the presentation.

MR. MEYER: Are you asking the question?

DR. LEE: Well, so what do we do? I mean if the information we have has to be utilized essentially to reach the conclusion on the risk reduction requirement and so on, what should you do, or what do you propose to do?

MR. MEYER: There are sequences from the utility risk analysis that fail the containment, and those would be the dominant risk contributors. Those would be incorporated into the considerations of functional requirements.

DR. LEE: So you still approach, move along this parallel path and say that although the utilities study indicated there is no need for further risk reduction, we will assume that there is something like a factor of 10 reduction necessary and will try to see what are the dominant mechanisms and try to design mitigating features to contain those dominating sequences?

MR. MEYER: That is the approach that the utilities presently assume.

DR. LEE: And you are taking that approach, too?

MR. MEYER: We are combining that information with the approach we have been using over the past several months. I believe that Mel Ernst might have a comment.

MR. ERNST: Malcolm Ernst, NRC. Perhaps you may have lost sight of the fact that this is a parallel effort in this time frame.

DR. LEE: Yes, I understand.

MR. ERNST: We are taking a look at the probabilistic approach to analysis of risk at the same time we are taking a look at several mitigating schemes that might or might not be usefully imposed. The decision process is at the end of that time, not in the July time frame.

DR. KERR: But how can you expect to get any very meaningful comments unless the people who comment know what it is you have in mind for the mitigating features to do? Until you have decided on what it is you want to accomplish, I don't see how you can write criteria that have any very specific significance.

I grant you you are trying to do something on a schedule which will accomplish more than if you had to do things in sequence, but how does one do it?

MR. ERNST: It does make it a lot more difficult than if you are doing it in a series.

MR. MEYER: I think that one of the results of the presentation that D. Walker presented -- I don't know if he is still here -- is that his list or the list from your study of appropriate dominant sequences, sequences that should be considered in establishing requirements for mitigation features, those aren't much different than the ones that we are considering.

The only one that I think stood out was the AB burn, and as I commented, that sequence will probably be removed from our considerations in defining functional requirements.

DR. LEE: I think I appreciate that point, but still I am puzzled whether you could say a factor of 10 requirement of reduction is what you would like to strive for now even in this period of time, or two orders of magnitude reduction. Don't you have to decide on that, at least, right now, essentially?

MR. MEYER: You are talking about the risk reduction requirement of a factor of --

DR. LEE: Otherwise, how can you set the criteria for the mitigating systems?

MR. MEYER: For example, decontamination factors are directly related to the reduction in consequences when you do the CRAC analyis. You establish those decontamination factors based on what you feel is feasible and practical. As I indicated in the diagram on establishing requirements, you also establish decontamination factors in order to meet that risk reduction goal.

So you don't indicate functional requirements on decontamination factors unless you have a handle on what the ultimate effect of those decontamination factors is on reducing risk.

DR. LEE: Let me try one last time. I will give up after that. Suppose you have identified a dominating sequence. Let's assume that. Would you like to reduce the consequence of that particular dominating sequence by a factor of 10 or a factor of 100, or as best as we could? Those are the three, let's say, alternatives. How would you like to do it?

MR. MEYER: You must reduce that, assuming that as you reduce it, no other sequence is -- well, you want to reduce that enough so that when you do your integrated risk assessment, you meet your goal of reducing the risk by a factor of 10. Now, that might mean for one particular sequence, in doing the comparative consequence analysis, that you reduce the consequences by a factor of 100.

DR. OKRENT: I would like to comment here. It seems to me that there has been presented a fairly reasonable approach. This is the first time I have seen it. They have outlined a path to try to go along two aspects, not make the decision until they have the information in both of these in December.

I think one can proceed along each of these two paths in parallel. I am not at all bothered by that. Now, what I am bothered about is the following. There seems to be pressure on the staff in this case to be rather more specific about what it is these mitigation features should be, what is the quantitative goal it should achieve, and what is the reliability and so forth.

I would suggest that if you were to look at the containment building and ask yourself what is the goal it should achieve and has it achieved it and so forth, you would have a bit of a problem. If you were to look at the emergency cooling system and try to apply these same kinds of questions, you would really have a bit of a problem.

In fact, if you would look at almost anything, if you looked at things that are normally non-safety and ask did they have negative aspects. You know, there is a PORV in the thing to help meet some kind of a transient condition, yet it can open up into a leak.

I would like to suggest that maybe there is an undue degree of, oh, I guess you might say searching out the criteria and sort of insisting at the beginning that we know exactly that it is going to be good, perfect or whatever it is. But we haven't done this and we are not doing it on lots of other things.

I would suggest, in fact, this emphasis on is it going to produce a factor of 10 or 100 on the dominant sequence is not the way we should be pushing the staff. I tried to indicate that earlier. My guess is we don't know what the sequence is that it will be useful for, if there is one and if it is useful. It is likely not to be the one that is now thought to be the dominant one.

In fact, the best you can get from these kinds of studies is some handle on what are some possible sequences where it might be helpful. And then you will find you can't design for all of them in any practical way, as we have heard, so you are going to have to limit the scope of what is practical. After you look at that, you are going to have to arrive at some engineering judgment as to does it seem to provide an increase in safety.

If you are going to try to insist it must be a factor of 10 and I'm going to have to be able to show it, or why is it a factor of 10 and not a factor of 100 or vice-versa, that is beyond the realm of what we can do. I think we would be kidding ourselves if we really thought we could show a factor of 10 or not a factor of 10.

So in the end, one is going to have to look and say does it on balance seem to provide a significant increase part of the time, and does it not seem to have any substantial negative features, or whatever. In other words, you can only play this game of a factor of 10 and the sequence that is dominating and so forth so far, after which I think you can lead yourself down the wrong path, almost.

DR. LEE: I thought I had posed the three alternatives that --

DR. OKRENT: And I was suggesting there was a fourth alternative that wasn't even in your three.

DR. LEE: Right, but as much as possible, one of the three alternatives. I had taken the answer from Dr. Meyer as being the third alternative. If that is the criteria you would like to propose, it is certainly acceptable to me personally. I wasn't necessarily suggesting --

MR. MEYER: Well, it is impractical. Your third alternative was zero, wasn't it?

DR. LEE: I mean assuming that there is some problem with our present containment system, try to do something about it in the best and as practical a way as we know now, perhaps. That is what I meant.

MR. MEYER: That is the basic motivation behind the program.

DR. LEE: Okay.

DR. KERR: Dave, I would agree with your analyis if I thought that our end result was the installation of filtered vented containment. Indeed, I have been trying to ask questions because I am trying to determine what it is that we are attempting to do. If we have decided we have to install filtered vented containment because we think it has some merit, and we design one and install it with the hope that it can be useful, that is one thing.

What we have been talking about, however, is risk reduction, and this is a way, presumably, of getting risk reducjion. It is not the only way. I am not even sure it's the best way. And it does seem to me that some of the questions are relevant.

For example, if one talks about a decontamination factor of 100, unless the system has a reliability of at least 99 percent, that decontamination factor doesn't have any meaning. That is one of the reasons I asked if one was going to specify reliability. It seems to me a reliability specification is fairly important if what one is trying to do is achieve risk reduction.

If what one is trying to do is achieve the installation of a filtered vented containment, then that is another story and we will talk about that.

DR. OKRENT: I thought they showed a set of paths where they would try to ascertain as best they could what were the levels of risk from these two plans, and what were the areas, in fact, where they might put in preventative features, incidentally, if they found any what are called outlyers, and they would also try to see what kind of features could be designed for mitigation, and if you did have a design of these, how much might you gain and how much might you lose, what was the net, and then arrive in late fall or sometime thereafter at a decision. They were not arriving at a decision now.

It may well be that out of all of this program, the thing that is of most value is they find some weak point from the point of view of what might cause an accident. I am not going to prejudge. But I don't find anything particularly wrong with the logic. I think if I were going to try to lay out a program, I would try to do it concurrently rather than sequentially, frankly. I think the problem is important enough that that is the way you should do it unless you can do each of them in a month, which you can't.

So that doesn't give me any problem at all. I think they have already said that for whatever reason, a factor of 10 is worth shooting for. Also, from previous studies they have looked at population and so forth, and in fact, something we may hear later today if we get to it, there are some aspects of this site demographically that are roughly a factor of 10 different.

So there are various reasons why you might arrive at the judgment that a factor of 10 is something of interest. Now, I think the staff, the applicants and the ACRS are all unable at this time to answer the question this is the level of risk that you should be seeking at Zion and Indian Point, that above this it is unacceptable, and below this it is acceptable.

We are not able to answer that question, I think. Why should we press the staff to the bitter end to answer it, is part of what I'm saying. I don't think I have heard anybody as a group -- there may be individuals here brave enough -- but there has been no group that has come forth and said this is the answer and it should be done this way.

DR. SHEWMON: No, but we have gone through all this PRA stuff as if we believed it. At least that is what I thought we were doing yesterday. We are into this Class 9 problem at least partly because there are accidents of this sort which contribute most to risk. And now you say, gee whiz, pick the worst sequence, do something positive and forget all about cost-benefit or risk reduction.

DR. OKRENT: Now, who said that?

DR. SHEWMON: That is my paraphrase of what I heard you say before Bill commented.

(Laughter.)

DR. OKRENT: I think in the end what you should do is read the transcript to see if I said that.

(Laughter.)

DR. SHEWMON: That is what you said to me.

DR. OKRENT: What I said is look at their flow diagram, and in fact, it says really nothing like that.

DR. SHEWMON: I know it doesn't. I am talking about what you said.

DR. OKRENT: Well, Paul Slovic has a theory that people tend to hear what reinforces their previous conviction.

DR. KERR: I am going to assume that my daddy can whip your daddy, and we are going ahead with it.

(Laughter.)

DR. SIEGLE: This is my first participation in the activities of this committee, and this morning I recognized that I was a neophyte. It is now 6 o'clock and I think I 'm a veteran, so I am going to --

(Laughter.)

DR. SIEGLE: I want to make some comments that really disturb me. We have been talking about mitigation of risks to the health and safety of the public, which might arise in the event of a Class 9 accident. One of the things I thought we were supposed to pay a great deal of attention to is what we learned from TMI.

There we learned that the exposure of the public to a radiological hazard was really quite small, and perhaps a person might ultimately have a latent cancer. The exposure of the public to apparently serious psychological hazards I don't believe can be neglected, at least in considering a Class 9 accident.

There apparently is evidence that people were subjected to psychological stresses there, for the set of circumstances that exist there, which should not be ignored, at least in considerations such as these. Certainly our knowledge and belief about what constitutes health is not restricted solely to physiological effects nowadays, but includes mental effects also, and there were many legal examples of this.

If I imagine the situation where the mitigation is the thing which gets most of the attention but the Class 9 accident is, as it says on one of these preliminary criteria, designed for full core meltdown, if that is the situation I think we may well alleviate the hazard to the public of exposure to radiological materials, but we may substantially exacerbate the psychological stresses to which they are exposed, for reasons that we can't easily identify. And because we can't solve those problems or even identify them well, we adopt the usual physicist's practice of solving the easy problem. The hard problem is to hard to handle, so we will do the easy ones first.

What concerns me is that the approach that is apparently being pursued is one that may not help mitigate the dangers, the challenges to the health and safety of the public, and that the program is quite unbalanced in its allocation of attention and effort.

The important place to allocate effort is to the Class 9 accident itself, because it seems to me it is this -- whether or not the radiological consequence occurs later -- which exposes the public to the psychological stress which has been observed to some limited degree and which I believe would be much greater if a Class 9 accident actually took place, in spite of the fact that the public assured there was a better building, there was a gravel bed and God knows what else will be the final outcome of this effort.

I think from what I have heard today, there is a significant possible misallocation of effort, and it arises because of an inadequate definition of what is the risk to the public. There is always a hanging qualification when we use risk. Risk of what? Apparently we learned from TMI, and we should have known it before, the risk is not necessarily a risk of radiological exposure, but a risk of a different kind, and which has associated with it property losses, loss of productivity of all kinds of activities in that area, which can be given financial values.

If I have a recommendation, it is that much more attention be given than is apparent at this point to reducing the likelihood of a Class 9 accident, rather than mitigating the consequences after it has occurred.

DR. KERR: Thank you. Do you want to continue, Jim?

DR. MEYER: I suppose so, yes.

DR. KERR: Now that we have given you all of this guidance.

DR. MEYER: Well, I indicated before that making visible suggested criterion requirements were for the purpose of getting feedback, and so I think that process has started today. And I think some of it has been very valuable to me.

If I can, and just as a very brief aside, we are considering reliability. It is listed as an important design criteria to consider in there. There are seven items in the reliability that will be considered when these systems are taken on in some detail.

The last item on the update is that the research in NRR programs are proceeding through the summer on key issues, and that introduces me to another question that you asked, what are the NRR needs in the area of Zion/Indian Point program, and I would like to touch on those briefly.

Divided the needs into two parts. The one being the immediate needs for Zion and Indian Point, and then I will present a viewgraph on the long-term needs.

We have heard a lot about the steam spike phenomena. There is an experimental program being conducted at Sandia using the FITS facility that in the near term we feel can tell us considerably more about the rate of rise of a steam pressure spike and thereby answer some questions that have been going back and forth regarding how rapid and vigorous the molten material-water interaction really is.

We have a definite need in this area, and as I understand the program, the program at least in part through a series of possibly four or five tests will start meeting that need.

The other item that is very important in determining the accident sequences is the characterization of the debris bed fragmentation. Here we have a need for better characterization of the fragmentation and also of the dispersion of the fragmented material. And we are, for example, asking research at Sandia to inquire further about what the steel industry and the coal industry can tell us regarding the interaction of slag with water and whether this can possibly tell us something about the fragmentation characteristics over and above what we know now.

The third item is not near term. It is long term. That is, we look at it more in terms of the 15-month response. Is the problem of core melt concrete and core melt refractory material interactions. There is an experimental program that I believe the subcommitee is familiar with at Sandia that is addressing this particular need.

An extension of this would be introducing a third material into these tests; namely, a core melt concrete but adding water into the interaction. Again looking to answering questions regarding the effect of core melt in the reactor cavity with the presence of water.

The filter and vent containment system program that has been referred to in the Sandia report referred to earlier is being continued, and the areas that they are presently concentrating on for Zion/Indian Point are the areas of failure modes and effects, the possible interactions of and competing risks. What are the negative aspects of these particular features based on conceptual designs that they have put together.

So we feel that, at least in part, the Sandia program is meeting an important need in the mitigation feature, the filter and vent mitigation feature.

There is also a cooperative program between NRR, NRR consultants and research in trying to answer the question of whether a core retention device can be backfit onto either Zion or Indian Point. What are the practical problems that are associated with the backfit of a core retention device? What would a core retention device look like, this type of thing?

Another area that we feel is important and I think was highlighted as an important issue at the technology exchange meetings was some questions related to the hydrogen control systems.

We still feel that there is an awful lot that we can learn from industries outside the nuclear industry in the area of hydrogen control, hydrogen migration during fronts, and we look for research help in this area in the near term.

The final area is in the area of containment failure modes. At the fifth technology exchange meeting there was a report of the work that LASL and Sandia had done paralleling the work that you heard earlier today.

The conclusions of that work were that the failure pressures were quite similar to the failure pressures that were arrived at by the contractors to the utilities. We are interested in some followup work related to that; in particular, a better understanding of the failure modes themselves. And we would hope to have that program continued through the summer for resolution of certain specific issues.

That is the basic list of what our needs are in the short term. Unless you have any questions on that, I can --

DR. KERR: Is there some way, and a sensible way, to distinguish between those that we need completed in the four months and those in the fifteen months?

DR. MEYER: Well, I define the four-month as being near term and the fifteen-month being long term.

DR. KERR: No, but of the ones that are there which do you consider to fit in -- oh, you have them in parentheses.

DR. MEYER: Yes, I have them in parentheses around each one.

DR. KERR: Okay.

DR. MEYER: And I think that it is --

DR. KERR: Okay, I am with you. All right. Thank you.

DR. MEYER: I think with the rulemaking in mind we also have a list of long-term needs, which is on the next viewgraph. And here we are defining the completion as being within two to three years. I believe that the research report is in the timeframe of three or four years for most of this. But we feel that an accelerated program would be appropriate based on the needs of the rulemaking.

It may be perhaps obvious these are the continuation and expansion of the programs that were started in the design of Indian Point needs. Those are generic basic problems that would need further, we feel need further work as the rulemaking continues.

In addition, we believe that there will be the need for work in these three areas indicated -- hydrogen mitigation and burning, core melt accident progression, and in the area of radiological source terms.

For the core melt accident progression I think it is clear from what we heard today that there is very little known about the progression of the accident from its initial configuration into melting and its progression as it melts through to the lower head of the vessel and also the various vessel failure modes are also uncertain at this time.

DR. KERR: Have you given some thought to how much you need to know about that?

DR. MEYER: Right now --

DR. KERR: I am not asking you to tell me how much you need to know, because you know it is a fairly complicated process and it is not necessarily repeatable from one experiment to the other. Have you given some thought to the magnitude of a program that is likely to be able to tell you what it is you think you need to know in core melt accident progression?

DR. MEYER: We are presently -- well, the severe accident phenomenon mitigation research program is in draft form, and we have not received it officially. We are reviewing this, and --

DR. KERR: Well, then you are just sort of depending on RES to tell you what you need to know about that?

DR. MEYER: No, we are depending on them to give us some feeling as to what can be accomplished over a two to three year timeframe.

I think we will know much more about what we need to know upon the conclusion of some sensitivity analyses that we are presently performing at Brookhaven National Laboratory where we are bounding the unknowns in this initial area to see what the sensitivity of those uncertainties are to the final loading of the containment, and I think that will be a big help to give us some guideline as to what kind of improvement will be important to give us the type of information we need in terms of containment loading for further assessment of the need or lack of need of mitigation features.

DR. KERR: What is meant by hydrogen mitigation? Mitigating against the consequences of --

DR. MEYER: Mitigating the -- well, the hydrogen mitigation features that have been proposed are in two categories. One is control burning of hydrogen by various means such as sparks, spark plugs and open flames, and the suppression of burning through a number of means available. And that is what is meant by hydrogen mitigation.

DR. OKRENT: I would have thought that in your long-term needs with regard to rulemaking -- I mean defining it in that way -- that you might have included the same kind of studies on containment failure modes, either filtered, vented containment systems or other mitigating systems, for other containment designs, as you are doing for the Zion/Indian Point type. Okay, that first item would pick that all up, would it?

DR. MEYER: Yes. That would pick it up to --

DR. OKRENT: All right, I am sorry.

DR. MEYER: -- particularly the ice condenser and the boiler containers.

DR. OKRENT: I see, thank you. I missed that as covering that.

DR. MEYER: If there are no further questions --

DR. KERR: Are there questions? I see none.

DR. MEYER: -- I can go to my last viewgraph.

In this last viewgraph, in a very abbreviated fashion, gives the program plan for NRR's approach to severe accidents, Class 9 accidents. As you are probably aware, there are four areas of proposed regulatory changes that are being considered or in the process of being implemented at this present time.

I would like to keep my remarks to the design for severe accidents, the design component of those four. The other three that are presently under consideration -- the area of siting, emergency planning, and environmental impact.

In the area of safety the way it is presently envisioned is to have four major components. Yesterday we heard an awful lot about the safety goal, and this will be a key component in the program.

Inputting to the safety goal considerations will be as indicated, the ACRS, EPRI, Atomic Industrial Forum, public and other regulatory agencies. Some of the items that would come under the safety goal category would be what are the criteria for determining the need for a given feature, whether that feature be a mitigation feature or an accident prevention feature.

Another area would be criteria for choosing between mitigation or prevention features. There are several that believe that we have saturated on preventing and having features that will prevent Class 9 accidents and that the appropriate emphasis should be on mitigation, but there are others that feel otherwise. And one of the derivative aspects of the safety goal --

DR. KERR: Does the NRC staff have a position on that question?

DR. MEYER: I am not aware of any formal position on that question.

DR. KERR: Please continue. I was just curious.

DR. OKRENT: I have heard Mr. Denton say to the ACRS that it might be hard to get a major reduction in the probability of an accident that would severely damage the core. And that is why he thought it was worth looking hard at what are here called the mitigation features.

I don't know if that met with formal position, but he did say something like that to the ACRS.

DR. MEYER: Well, one of the important ingredients in determining the safety goal will be the role of probabilistic risk assessment in that determination, again an item that we heard a lot about yesterday.

A parallel important activity is the activity of plant specific probabilistic risk analysis. There was a list presented yesterday of these activities going on presently within NRC and also being conducted by the utilities. And it is our anticipation that this type of activity would continue to support considerations of the role of the probabilistic risk assessment in determining a safety goal and also in aiding and better defining mitigation features.

The research program that will parallel these two activities will be activities in phenomenological studies.

DR. KERR: Does that last statement imply that ultimately in the mitigation features there may be some probabilistic specifications, because it seems to me in your short-term mitigation systems the probabilistic consideration is, at the risk of incurring Dr. Okrent's ire, strangely missing?

DR. MEYER: Well, I wouldn't say that it is missing. It is certainly not a major ingredient.

DR. KERR: Well-hidden, then?

DR. MEYER: Well, if I drew all the arrows I would have had nothing here but arrows.

(Laughter.)

DR. SIESS: Let me ask a question slightly differently. There is a box that says role of PRA and the comments you made on it. Does that imply that there are still some question as to what the role of probabilistic risk assessment is in relation to a quantitative safety goal?

DR. MEYER: Yes.

DR. SIESS: But you think it might be possible to come up with a safety goal that is not based on probability or risk assessment?

DR. MEYER: I think that that has to be considered.

DR. SIESS: Okay, thank you.

DR. MEYER: It is a very tough question, and one of the reasons that I singled it out was to highlight the fact that the role in licensing a probabilistic risk assessment is going to be key to establishing the ultimate safety goal. We have had safety goals in the past that have been totally independent of this type of risk assessment, and it is conceivable to proceed under those old methods of doing things.

DR. SIESS: We have had criteria in the past. Do you really think we have had goals in the past other than reasonable assurance that there is no undue risk? That is the only goal.

DR. OKRENT: But the Congress writes laws and puts words in like "use best available technology" or --

SPEAKER: As well as practical --

DR. OKRENT: -- "not unreasonable risk" you know. People do give qualitative guidance frequently.

DR. MEYER: We have already discussed some of the key areas that we feel are important in terms of the phenomenological studies. Those studies will continue, and there will be similar studies conducted by the utilities.

Then there is the area of the study of features

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where both mitigation features and what I refer to as prevention features, those features that would be just the probability of a core melt, will be addressed.

In addition to this, the other elements of the Class 9 changes -- the siting, emergency planning, and environmental impact -- will also be input to a consideration of a safety goal.

The ultimate goal then would be out of this whole process to determine the safety goal and to determine the role of achieving that safety goal of probabilistic risk analysis, the requirement or lack of need for mitigation features based on functional requirements that are established.

This is a very preliminary overview of the program plan meant for discussion purposes, and certainly this type of strategy will be fine-tuned and revised based, among other things, on comments from the subcommittee. But this is a summary of our basic thinking in this area.

DR. KERR: Thank you, Jim. Are there questions?

Mr. Shewmon?

DR. SHEWMON: Yes, at the prevention aspect for a minute. One major effort in that area, or if not the major effort then, is the -- you employ the effort to try to identify for their plants the weakest links or the failures which would give the highest probability result or the highest risk result and that that would be your main input on -- or their input would be the main input you will have with regard to what prevention features might be most useful.

DR. MEYER: That would be an important ingredient if containment isolation failure turns out to be an important contributor. Then the effort would be directed to preventive measures to accommodate that problem.

DR. SHEWMON: But if that failure was due primarily to the unreliability of the core spray units -- I am sorry, the containment spray or the containment cooling, then another option might be available to you?

DR. MEYER: That is correct.

DR. SHEWMON: Okay, but you don't have any efforts of your own in the prevention area, is that it?

DR. MEYER: We do have an effort that we have been trying to get underway to take a look, for example, at dedicated heat removal systems, both -- well, I think dedicated heat removal is the best description. You can also consider them in terms of bunkered safety systems.

DR. SHEWMON: Let me make one other comment. I am interested to hear you say that you will look at the steel business. I think it is an interesting idea since they granulate, give or take, a million tons a year of slag in the water. It would be very interesting to see just what particle sizes they do get.

DR. KERR: Mr. Okrent.

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DR. OKRENT: I realize that the heading on top is program plan for accidents Class 9, and so maybe the lefthand column which is marked areas of proposed regulatory changes somehow is felt not to include the question of what regulatory changes might be made in existing or future plants from the point of view of making it less likely to have a Class 9 accident.

You proceed, comments made -- -- other people, and undoubtedly -- staff, for example, that the single failure criterion may not be adequate and that is just one kind, and you just mentioned another kind of thing that might enter into a box on that lefthand side which you might call development of new general design requirements or something.

It seems to me that it is an important topic, that instead of being possibly included in some of your units over to the right that it be then culled out as an item receiving its own focus.

DR. MEYER: I am not the one to respond to that comment. I think it is a good one, but --

DR. KELBER: Of course the committee, or various members of this committee have heard of a range of activities in response to the task action plan, both within NRR and RES, that are aimed at that sort of thing.

I think you are going to have to bear with the staff of both NRR and RES until we get our program straightened out and well organized.

DR. KERR: It is not our responsibility to be sympathetic with the staff. We are absolutely ruthless.

DR. KELBER: (overlapping conversation).

DR. KERR: That is not our job.

DR. KELBER: It may not be your job, but since I am asking you --

DR. KERR: Impatience is what we are supposed to exhibit.

Mr. Peoples.

DR. PEOPLES: We made a distinction, included this morning in our presentation. It has been so long I have almost forgotten myself. We draw a distinction in the prevention mitigation area in the sense of the use of words, and in my own line of thinking at least it is useful to me to put prevention and to use that word as it relates to preventing a core melt accident occurring, and I stop it there.

That means that really my basic ECCS systems allow me to cool that core inside the vessel, thus preventing -- --. And then I look at mitigation as what I can do to reduce the effects of having failed all of those I end up with a severely degraded core.

And now I might be able to consider certain restoration of ECCS that allows me to cool that debris bed within the vessel or I can certainly consider containment spray, the containment itself as mitigated devices in the sense that they prevent release to the environment and subsequent exposure of personnel to radiological effects.

So from that viewpoint I think that Dr. Meyers might really consider in specifying criteria for "mitigated devices" not to be as specific as describing the solution in terms of filter vented containment systems or in terms of core retention devices but in terms of reducing the risk of radiological exposure, which for example would suggest that possibly ways to cool that material within the containment, prevent overpressure by your steam or prevent overpressure by ..gen control, which can take a variety of forms, may give more range for imagination to be used and end up really with ultimately better results, because in my own -- I am fully convinced that what we want to do is keep that stuff in the containment. We don't want to start spewing that stuff out even through filters -- --

DR. KERR: Well, I bet you if you could come up with a good alternate scheme which would be a lot better than filter vented containment that you could persuade Dr. Meyer it will work back.

DR. PEOPLES: But I am suggesting that the definition of the criteria should be broad in that sense too though, and that that is the way to aim at that rather than doing it specifically.

DR. KERR: And if we can prevent the core melt to begin with, I want to go on record as agreeing wholeheartedly with Mr. Siegle, I am in favor of preventing them.

DR. PEOPLES: And we certainly are too from both the public viewpoint and from our strictly economic viewpoint. And obviously that is the direction to go.

DR. KERR: But there is a problem. How does one demonstrate that one can prevent such a low probability incident? That it seems to me is the problem that faces us. And I don't think we stop trying, but it makes one have to look at --

DR. SIEGLE: Apparently it is not a problem for physicists because we like to do easy ones.

DR. KERR: Have you heard enough philosophical discourse, or do you have some more for us?

Thank you.

DR. MEYER: I believe that Bob Bernero is going to give us a brief --

DR. BERNERO: I will try to be briefer than my predecessors here.

Late last year there was a widespread perception that the risk of reactors, light water reactors in the United States, societal risk, was dominated by two reactor sites, Indian Point and Zion, that the four reactors at those two sites were the predominant societal risk of water reactors in the United States.

There was a petition formerly filed to the NRC to shut down the Indian Point reactors because of that high societal risk, the large population near it.

Harold Denton, as Director of Reactor Regulation, acted and gave a bunch of orders, that in the short term the owners had to do something and in the long term they had to do something. Zion got caught up in it, and in essence Harold decided that we don't have to shut the reactor down, the reactors -- Indian Point-2 and 3 -- don't have to shut them down in the interim provided these short-term fixes are made.

The Commission then found itself required by its own regulations to provide technical oversight of what Harold Denton did, and they needed an independent basis to judge whether he did right or wrong.

So they turned to the Office of General Counsel and the Office of Policy Evaluation and said form a task force and evaluate a number of things. And that task force received its order from the Commission on May 30th with the requirement that the report be filed by June 12. It was not as phony as it sounds because we knew behind the scenes ahead of time, at least four weeks ahead of time what we were supposed to do.

There are three parts to that task force report. There is a part on accident risk assessment. There is a part on the need for power, you know, how many megawatt hours or whatever in the New York power grid. The report is confined to the Indian Point reactors. And lastly, there is an analysis of the petitioners and other people's comments. What I will just give you a rundown on is the accident risk perspective which the probabilistic analysis staff prepared for the task force, and if you have the paper -- I distributed this SECY paper, 80-283 I think is the number of it.

DR. KERR: We do have it.

DR. BERNERO: And the part we are responsible for, and I am addressing is the Section 1, the front-end, and the two little appendices that go with it.

Basically what we did was a parametric analysis to give comparative risks. What we are really after is a way to tell the Commission here is how the apparent risk of Indian Point compares with the risk of other reactors.

In order to do that, we found it necessary to do a quick rebase lining of the WASH-1400, and you will find that covered in Appendix B of the report, right at the tail-end.

It explains how the WASH-1400 analysis was rebase lined in order to be able to compare it better to the other things we were doing. And then we proceeded to do a parametric analysis trying to hold everything else constant and varying only one thing at a time.

The first variable was site. The second variable was design. And the third variable was public protection action, evacuation or shelter and that sort of thing.

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The vehicle of comparison is the CCDF, the complimentary cumulative distribution function or risk profile. I won't bother explaining that. There is material to be found to explain it, but it is basically the risk curve and I will show you enough of them so that you become familiar with them.

The first parametric variation, site variation, we took the WASH-1400 pressurized water reactor, SURRY, artificially boosted it to 3,025 megawatts thermal, which happens to be the power output of Indian Point-3, the larger of the two reactors there, and then we varied only the site.

We had one set of public protection measures for all these sites. We chose six sites -- four populous sites -- Indian Point, Zion, Limerick, and Fermi near Detroit, Limerick near Philadelphia. And we took one out of the population tables; we went in and took Palisades in western Michigan and said that is a typical site give or take. You know, it is good enough for a reasonable choice. And we took one remote site on the same basis. We didn't do any statistical analysis. We just picked one. That is Diablo Canyon above Santa Barbara on the California coast.

We analyzed for four measures of risk -- immediate fatalities, early injuries, latent cancer fatalities, and property damage.

The immediate fatalities are in essence people who receive doses on the order of 300 rem or higher. Early injuries

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are people who receive doses of 50 rem or higher. Latent cancers are calculated as the reactor safety study calculated it, the linear hypothesis with a fudge factor. And the property damage is really a population model. It just models property damage according to the population density in direct proportion. It has no way to deal with things like the New York port or unique facilities, abnormal things like that.

I said before the Commission the property damage model of WASH-1400 is probably very optimistic. That is, it probably significantly underestimates the costs involved.

The result of that first, or site comparison, here is the result -- don't bother reading the legends. Every figure in that report has the Pontius Pilate basin. There are large uncertainties with the absolute values. We wash our hands on every single absolute value.

(Laughter.)

We are dealing in comparative analysis here. So keep that in mind. Here on this risk curve the probability of exceeding the consequence on the X axis here, curve number one, Indian Point, curve two is Zion, three is Limerick, four is Fermi, five is Palisades, the typical site, and six is Diablo Canyon.

In the report there is a presentation that calculated expected values which lend themselves, you know, the interval -- it lends itself to direct numerical calculation. But roughly, on the fatalities there is an order of magnitude difference twice. The populous sites here about tenfold lower to the typical site and tenfold lower to the remote site.

If you turn -- now remember again this is SURRY, the SURRY reactor. If you turn to early injuries you see the populous sites pretty well go together into this long slope here and the typical site and the remote site sort of come together themselves, especially at the lower probability end.

Remember now you are dealing with 50-rem doses, and as a matter of practicality, early fatalities are found less than ten miles from the reactor, in fact even less than five miles from the reactor.

These doses can be achieved or reached out as far as 50 miles away. So when you start getting 50 miles away from a lot of places in the United States they begin to look alike.

The next measure, latent cancers, you see that even more so. The latent cancers in effect are a measure of the population out to about 200 miles.

And here the sites are essentially the same. The uncertainty out at this end is far greater than the uncertainty at this end. And so for latent cancers this is another way of saying that a 200-mile radius of any U. S. reactor is not that much different from the 200-mile radius of any other. There just aren't that many differences.

You pick up Los Angeles with Diablo Canyon and so forth.

Now the property damage I will show you just briefly. It too -- it somewhat resembles the latent cancers. It is sort of a hybrid between the latent cancers and the early injuries, and I discount its value because of the suspicion of the model being too optimistic.

It is possible. I have said this before. It is possible that if you really went after that with a rigorous model and went in with dollar per man-rem figures that are believed credible, you might find yourself saying that the dominant risk for reactors is financial, not public health. It is a rather interesting thing.

Well, if we use those previous curves, here is the formula that was really the basis of that perception that Zion and Indian Point dominate societal risk. This is the four curves you just saw, and this table is a reiteration of something that was buried in the work of Jeremy Sprung at Sandia. He published a report on population distribution using the reactor safety study model back in 1978. You saw one of the curves in one of the talks yesterday. It was a Zion curve with a funny dog leg in it, down at the low probability end.

But basically the equation says if SURRY is a prototype of all reactors and you move it from site to site, that SURRY at Indian Point or Zion is ten times worse than SURRY at a typical site, you can set out this crude equation

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that says there are 20 units of risk at Indian Point and 20 units at Zion and all other sites are typical, so using, say, 68 other sites, you can show that 40 percent of the societal risk is at those two sites.

DR. SHEWMON: This is your short-term deaths, is that it, or your --

DR. BERNERO: Well, if you go through the comparison, it would be -- it is a hybrid comparison of short-term deaths and injuries basically. The differences fade as you go to the property damage and latent.

But that equation holds only if SURRY is that prototype. So we look at the design and to -- I use the shorthand word "design," meaning design and operation, including the operator procedures and human errors and that sort of thing, and the human error can contribute on the order of half of that risk.

So this is not just hardware design; it is hardware and human design. But for this purpose we stablize on the Indian Point site, and the same public protection measures we used in the previous comparison.

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So this is not just hardware design. It's the hardware and human design.

But for this purpose we stabilized on the Indian Point site and the same public protection measures we used in the previous comparison but now we used five reactors of the same power level. We used the rebaselined Surry, really the reactor safety study reactor, a rebaselined Peach Bottom, the Sequoyah reactor -- which is a four-leaf (?) Westinghouse ice condenser reactor -- and then we did, for the purpose of this study we did, a very short-term independent risk assessment of the two Indian Point reactors. And we did the Indian Point before the January short-term fixes. These are the things that Harold Denton ordered in February, whatever the date was, I forget. And we did Indian Point after those fixes.

Now, we did all the parameters. I'll show you just one, one curve, the one for early fatalities. And here it is. Curve number two here, which is Surry rebaselined, is curve number one from the previous set. It was Surry at Indian Point. So here's Surry at Indian Point again. Here is Peach Bottom at Indian Point. Here is Sequoyah at Indian Point. This one is Indian Point before the fix and this one is Indian Point after the fix -- about a factor of three difference from the fix.

We could find no substantial risk-significant difference between Indian Point 2 and 3 in that assessment that was done.

So you see that the swing from what I called the design

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variable.

Now, public protection was pursued at some depth with the Commission, because just the week before they had been discussing the emergency planning rule and I had had the occasion to tell them that I wasn't terribly happy with the emergency planning rule that was going forward. In fact, we wouldn't concur with it.

What we tried to do here in public protection is, we used the benchmark reactor, back to Surry again, used the Indian Point site, and then varied the public protection. Now, I'd point out to you, in the report you have, that Sekke (?) paper. there is the coverage of some public protective action; we did further work, which is presented in the slides, on public protection variation. Basically, the stuff in the report to some would seem overly pessimistic; to others, realistic. It in essence says nobody beats the cloud.

If you look in the legend under each curve, or the text, it says no matter what the areas of evacuation, the subjects of evacuation suffer full cloud exposure plus four hours' ground exposure before they leave. And consequently it sort of stacks the deck against evacuation and you just -- you just won't achieve anything.

So all we varied was either shelter or evacuate and varied the radii of evacuation. In the presentation I cover different early warning and different -- we also looked at different evacuation rates, which I cover verbally.

Now let me show you some of those, those figures. But before I do, I'd like to emphasize one point. It's a functional character. When you do a reactor risk assessment and then go to use it in emergency planning, many people tend to think that a 12-hour core melt containment failure sequence gives you 12 hours' warning for evacuation -- not necessarily so. For every accident sequence there is a characteristic path or time line. And here I just pull them out of the air -- two sequences.

The Ts is the time of start. That's when you have the transient or you have the LOCA or whatever it is. Then there is a period of time and then a certain time which I call time of diagnosis, T-sub-d. A smart operator, using the instruments, using his head, using his training, knows he's on the path to perdition; he knows he's going to melt the core and fail to contain it. And that's the first time he can really call up and say, "Do something, folks. We're going to blow it."

If after some hours, six hours in this case, only an hour and a half in that case, there is the time of release -- the release, of course, may be a high, high puff, a low puff, a long one, a short, you know, that sort of thing -- so if you do a risk assessment for emergency planning, in order to analyze the impact on the public, you have to consider each one of the time of diagnosis and have a weighted consideration of whether they promptly notify the public and get them moving. The earlier assumptions I spoke of, everyone sees the cloud, assumes that the release takes place and then you bumble around and tell people to leave, and if you do it that way they will see the cloud. And the smart thing to do is tell them just stay indoors.

But if emergency planning were done effectively and the decision-making process was there to activate prompt notification and removal of people at least within the first few miles, where you can save lives, you could use these time of diagnoses to accomplish something effective; but you won't do it just by saying warn people in a hurry -- you've got to think it through.

DR. OKRENT: Can I make a comment?

DR. BERNERO: Yeah.

DR. OKRENT: I'm not completely sure that you should leave the impression, inadvertently or intentionally, that the time that you would choose to evacuate people out to some distance would be the one that corresponded to the time when the operator knew that, to use your words, he was on the way to perdition, because if I look at what happened in Canada recently, where there was a derailment, I guess it was, yes, if I followed your logic, there would have been no evacuation because the thing didn't fail in the way that they were -- they were concerned. Am I correct?

DR. BERNERO: Oh, I disagree with you totally. They had a huge bonfire.

DR. OKRENT: Yeah.

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DR. BERNERO: They were releasing poisonous chemicals. They blew one tank car 2,222 feet.

DR. OKRENT: I see.

DR. BERNERO: It was -- they were at time of release.

DR. OKRENT: Were they at time of perdition? Had it gone the worst that it could?

DR. BERNERO: Oh, yes. Well, the only thing that could have changed was the meteorology.

DR. OKRENT: I see. Okay.

DR. BERNERO: They were at T release right -- right in the accident.

DR. OKRENT: Okay, my error in that regard.

DR. SHEWMON: Yeah. And they removed people out because there were other tanks there which could also go.

DR. BERNERO: Oh, yes.

DR. OKRENT: All right.

DR. BERNERO: It was -- they were trying to patch, and, you know, they were trying to prevent the thing. But the evacuations --

DR. OKRENT: So that's a bad example, because it's a mixed case. It was my impression, was that they had many others that could have made it much worse, and that was the basis for --

DR. BERNERO: Are you suggesting that precautionary evacuation is the --

DR. OKRENT: Well, that's all; in other words, I think there could well be precautionary evacuation.

DR. BERNERO: Okay, I would just counter, if you -- if you will go back to this time, no one is suggesting that. That's every time the plant trips, practically.

DR. OKRENT: Right.

DR. BERNERO: On the other hand, if you look at emergency plans -- and we do -- and you look at the myriad documents that guide, they typically tell you things like before you make the recommendation there shall be a high radiation reading in the reactor building and at least one or two additional confirmatory things, like high pressure in the building and something else. They are skewed, the instructions and agreements are skewed, to have public protection measures start around the time of release, not around the time of diagnosis.

DR. SIESS: Well, could you skew them the other way?

DR. BERNERO: Yes, you could. But there's a lot of homework that would have to be done. And that's the very point I am trying to make.

DR. SIESS: You've got a sort of a worst-case scenario.

DR. BERNERO: Yeah. What I have right now is quite consistent with the emergency planning documents that exist, which, in effect, tell you, "Don't move until you're sure it's happening." Three Mile Island was a dramatic example -- high radiation in the building but no high pressure: so they took a sample over there, and Goldsboro (?) said, "Well, it's not ten R per hour like it theoretically could be. So we won't evacuate."

DR. OKRENT: Well, okay. Go ahead. Just --

(Pause)

DR. BERNERO: So if you look at the evacuation -- or what -- the -- here's -- here's the -- if you go back, this curve here is curve number one of Figure 1 that you saw: it's Indian Point -- or, it's Surry at Indian Point with a ten-mile evacuation. It is also Surry at Indian Point with a 25-mile evacuation, a 50-mile evacuation, or sheltering, no evacuation at all. And curve number four, which is not a whole lot higher than it, is no evacuation for one day. That means everybody walks out of the house, stands there looking at the cloud and looking at the ground and getting zapped, and then leaves.

You know, this is a real bungle. You'd really have to bungle the thing to do that. But this is a sensitivity analysis, a comparison. So you go up there. That, that tells you that if you've got bungled evacuation it doesn't make a whole hell of a lot of difference what you did.

DR. SHEWMON: Is sheltering in your own house?

DR. BERNERO: Yes. Sheltering in their own house.

Now, then what we did is, we said what if we got clever and on the release time we're able to use diagnosis on the average effectively to get advanced notice, if you could get -- what we analyzed was -- one hour's advanced notice, three hours' advanced notice, and five hours' advanced notice of the release, and we evaluated evacuations -- now, mind you, this is a simple plume model, it just goes out on a radius -- and we said move the people out at one-mile-an-hour net velocity or ten-miles-an-hour net velocity and what this'd do to the early fatality risk curve. We did it both for early fatalities and early injuries, which are the only ones that change.

I'll just overlay the two Vu-graphs here. You've got them Xeroxed separately as a slide.

And what it is: this is a one- and five-hour delay; if you have one hour's advance notice -- now, wait a minute, I said that wrong. It's a one-hour -- it's a one-hour delay from diagnosis, so I am saying it backwards.

DR. SIESS: From diagnosis.

DR. BERNERO: From the time of diagnosis, if you can get people moving within one hour and then they move at velocity V, where "V" is either one-mile-an-hour or ten-miles-an-hour, if you can do it within one hour, promptly, you do this -- you move the curve from up here to down here. One hour, ten-miles-an-hour. If it's one hour and one-mile-an-hour, you're right back in the bag. If it's three hours or five hours, you stay right here.

So it says, effective, quite effective notification and movement can save lives, but if you bungle it you won't. Our present guidance will just bring you right back to the -- to the same thing as standing around waiting for the cloud.

DR. SHEWMON: Do you try to do anything with how many people get hurt in the process of just evacuating? Or --

DR. BERNERO: Well, I'm inclined to think that many of us in the nuclear field exaggerate that risk.

I think Missasauga, which was a quarter of a million people moved rather rapidly out of that city, had virtually no one hurt. I have yet to hear of an injury in that.

DR. SIESS: They started pretty late, though.

DR. BERNERO: No, not really. That was a mis-report. I went over it with Brian Bryans (?), and he has a -- a participant in that study, he bought a part of the postmortem study, and they were moving people within an hour or so. They were moving in blocks. You know, it was graded -- they were like following the windrows to get people out.

DR. SIESS: And the other people didn't try to move till they --

DR. BERNERO: I think they did get a lot of movement -- you know, people saw the thing coming.

So now if I just go in, you've seen what the emergency planning does, let us go back and remind you this curve here is that sort of reference curve, Surry at Indian Point, let me put the band of variability of the other parameters on there, so you get a feel for the risk sensitivity.

What does the site do? These two heavy lines. The site variation -- of course, one is it's the same curve; this was

Indian Point -- or Surry at Indian Point, so it should come out there. The Surry at Diablo Canyon is this one down here. So that's the variation of site. And you see the site variation is greater than the significant variation of public protection.

What does design do? The design spread put on there -- and mind you, this is only for the limited population of designs for which we have a figure of merit -- here is the upper one and here is the lower one, the dashed lines.

Now, we only -- we have a very limited population of reactors. There's a lot more uncertainty with that than there is in the analysis of siting, because, you know, there are different analysts and different sequences and different length of analysis and so forth. So it's as big as the site, and I contend is the larger variable.

And lastly, because it was of interest in Indian Point, what if we derated the reactor? We did a curve -- the dots here are -- the ones with the circles, those aren't data points, they are just circles to distinguish the curve. We tried to do a quantitative analysis of how does the risk go down with reduction in power level. If you do the isotopic analysis, just remember roughly half the risk comes from iodine, which will follow the equilibrium power level, because it's an eight-day half-life or less. The other half of the risk comes from longer-lived stuff like cesium and strontium, so it's going to track core burn-up, it is not going to track equilibrium power level. So unless you limit core burn-up you aren't going to knock down that factor of risk. However, if you derate, if you force a reactor to operate at 50 percent power or something like that, your fuel is colder, your decay heat curve is lower, the whole thing is more sluggish -- it takes longer to reach melting temperature, it takes longer to boil water away.

You've got a fuzzy benefit there. I can't quantify it. I don't have a figure on it. But what I said was: I will just assert that although the isotope risk doesn't drop in proportion to power, the design sensitivity drops greater than the proportion to power, and I will assert that a 50 percent power drop is a 50 percent risk drop.

And I plot it here just to give a perspective. It's a waste of time. It's not a significant variable. It's a factor of two. And it is a small difference compared with the other differences in risk.

DR. OKRENT: On that curve where it says "number one, ten-mile evacuation," that occurs after the cloud you said and after four hours, right?

DR. BERNERO: Yes, the very first curve I laid up here. All of the evacuations in the report have that. If you look in the footnotes, whatever the radius, it says a cloud plus X hours and then (WORDS UNINTELLIGIBLE).

DR. OKRENT: Thank you. Right.

DR. KERR: Mr. Bernero, I don't want to cut your

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presentation short --

DR. BERNERO: Oh, this is --

DR. KERR: -- but about how much --

DR. BERNERO: Yeah, this is it. I'm just going to show you the -- the final thing: We said to the Commission that, based on this, this is what we conclude. We weren't supposed to make a recommendation to them. They specifically said that. We said the Indian Point site is worse than a typical site, the Indian Point reactor is about as much better than typical as the -- than a typical reactor, at least what we know of one, as the site is worse, and we conclude Indian Point is not a dominant societal risk; and we emphasized that design and operation is the least certain and the most significant variable of all and that that's where attention ought to be focused.

We suggest that this sort of technique, this sort of comparative risk assessment, is a valuable tool for weighing the siting, rule-making and all that kind of stuff.

DR. OKRENT: Now, before you run, I gather that an important reason for your -- one of your conclusions, that Indian Point is not a dominant risk, is that you believe that the probability of an accident which severely damages the core or core melt is less at Indian Point than for the average reactor. Is that correct?

DR. BERNERO: No, it's more to the point that the probability of an accident which damages the core and causes a substantial off-site release --

DR. OKRENT: Yeah?

DR. BERNERO: -- really if you look at the Indian Point reactors after having looked at the Surry reactor and other PWRs, what you find is, the Indian Point reactors have the most forgiving type of containment -- the large dry; and wherever, almost wherever you found a significant risk contributor in Surry, like event B, you look at Indian Point and you find the decay heat removal system is inside containment, they do have checked out inspection and, you know, they've knocked down that risk contributor. You look at the station blacked out and they have diesels, more DC buses, they've got these gas turbine generators -- they have reduced that risk contributor.

It's as if someone went in and fixed a typical PWR -- or, you know, what we assert is typical, Surry.

So what they've done, it's not so much that they've reduced the probability of a core melt occurring: they've mitigated the apparent consequences of a core melt.

Now, we also looked at and recognized -- it's very difficult to quantify -- they have learned a lot, you know, from TMI: they've put on -- they've got a Westinghouse rep' on shift as well as the shift technical advisor; they've got two senior reactor operators; they have a lot of things like that, that are very difficult to quantify, that would generally tend to reduce the probability of even having a core melt.

But the significant things for risk are that the dominant risk sequences from what we know of reactors are dealt with in that design.

DR. OKRENT: Well, all right, but your estimate of the probability of severe core damage for Indian Point is a factor of six lower than for Surry and a factor of 20 or 30 less than some of those others, and so forth -- and, in fact, a factor of four less than Diglas (?) and so forth. So your -- now, I agree there is a large dry containment, but we have a lot of PWRs with large dry containments, so that part is fairly common, although not universal. It may well be that they have a better than average power system; I don't know. On the other hand, they may have a worse than average protection against some things, like fires, possibly, or -- or earthquakes; I don't know. And in any event, implicit in your arriving at a conclusion that this, for example, is 20 times worse than the two loopers (?) you mentioned, is that they're -- these others things that are not included, like seismic and sabotage and so forth, in fact, aren't a contributing factor that's bigger than your one times ten to the minus five.

DR. BERNERO: Yeah, we -- they aren't dealt with. We don't know what they are.

DR. OKRENT: Well, now, I agree that in what you wrote you had this big no on every page which tended to make it lose a little bit of its significance at the end, that there are large uncertainties in the absolute values. But I didn't find in this a statement of -- let's say, summarizing the kinds of points I just made, saying there could be some systematic omissions that in fact change the relative ranking on your page 27, making it in fact not nearly what it is. It could make them all right. And it seems to me if you were advising the Commissioners it would have appropriate at least to alert them to the fact that that could be the case. And I don't find that in this document.

DR. BERNERO: Your point is well taken. In general, we do say, in explanations to the Commission, in follow-up to receipt of the document, we have explained that if you reduce down to the level where Indian Point is, or appears to be, you are probably in the range where these other things are significant, we don't know enough about it to say.

I would like to go back and emphasize, though, that table which ranks probability of core melt is not an index of risk. That's an index of core melt probability. The risk curve is the better -- I thought you were comparing them.

But your point, I accept that, that that is correct: it would have been better if we'd said it right in the report.

DR. KERR He understood you immediately. I'm not sure I quite did. What you're saying is, he may have left some things out.

DR. BERNERO: Seismic risks, for example.

DR. OKRENT: Or sabotage. He's listed a group of things

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and Indian Point has ended up being the lowest probability of severe core damage on this table. And all I was saying is, this is based on an incomplete set of sequences and you could well have some other sequences that lead to a larger risk. And I indicated earlier --

DR. KERR: What do you mean by "well have"?

DR. OKRENT: Well, I think the SSE, for example, is probably a one-in-a-thousand at Indian Point, I'd guess.

(Several speak at once)

DR. KERR: That's designed to withstand an SSE.

DR. OKRENT: Yeah, right. Right. But let me finish, let me finish the thought.

If the SSE is about a one-in-a-thousand -- I'm pulling a number out of the air -- then I'll assume roughly twice the SSE, maybe about a one-in-ten-thousand. I don't have a very high degree of confidence now, or any basis for it, that at twice the SSE, for this plant, which is, in fact, a really old plant, that everything you'd need for safe shutdown, heat removal, will be there.

So -- but, if, in fact, at twice -- using my crude numbers, I can get a number from that one source which is larger than the one times ten to the minus five.

DR. KERR: That would be the same for all plants.

DR. OKRENT: And it might be the same for many plants and tend to be an equalizer.

JO-17

DR. BERNERO: Or it might be quite different. Or it might move Indian Point to the top of the risk heap.

DR. SIESS: But even if it just makes all plants the same, instead of the seismic, which is site-related, you could postulate sabotage -- about which we know nothing -- and say that that was one order of magnitude more probable. Then all plants are alike. And then the only differences are the sites.

DR. OKRENT: Or the mitigating features.

DR. SIESS: Or the mitigating features.

DR. OKRENT: In that case the containment difference might be significant. I was looking in this -- again, I'm just trying to say, in the same way that I was urging quality assurance on the representative from the group of utilities, I think it behooves the NRC staff no less -- in fact, all the more -- to be careful that they have properly qualified what their -- they may still have a certain conclusion, but I think it behooves them to do this, and especially if they're advising the Commissioners. And I think --

DR. KERR: They weren't advising them.

DR. OKRENT: They weren't advising them. But this can't help being advice.

DR. SIESS: Well, they can confuse them better.

DR. OKRENT: Well. And, you know, I guess I've indicated, I would be reluctant to guess that there was this factor of 20 between those two plants without looking at these other

JO-18

contributors. It may be there, but at the moment I would be unwilling to say I have a basis for assuming it.

Now, if you had one of the plants where you have a good reason to think, gee, this is a one-in-a-hundred thing, clearly stands out at the high end, then, you know, that's unequivocal, that's different. But when you get down to the low end, I think you run into this kind of problem.

DR. KERR: Have you finished?

DR. BERNERO: Yes.

DR. KERR: Other questions?

Thank you, sir.

I am now going to relieve the reporter of further responsibility. And I'm going to take about five minutes for a stretch, before we listen to Mr. Gill (?).

END TAPE 13

(Thereupon, at 7:18 p.m., the reported part of the meeting ended.)

CERTIFICATE OF REPORTER

This is to certify that the attached proceedings

before: NUCLEAR REGULATORY COMMISSION

In the matter of: ACRS - Subcommittee

Name of Proceeding: Meeting on Class 9 Accidents

Docket No.:

Place: Inglewood, California

Date: July 2, 1980

were held as herein appears, and that this is the ORIGINAL transcript thereof for the files of the Department.

Reporter

DEE WALKER

TABLE 1

WASH-1400 SHORT TERM STUDY

SYSTEM UNRELIABILITY ESTIMATES FOR DOMINANT ACCIDENT SEQUENCES

CONTAINMENT FAILURE MODE PROBABILITY ESTIMATES

ACCIDENT SEQUENCE - CONTAINMENT MODES ASSIGNED F.P. RELEASE CATEGORY

CRAC CALCULATIONS FOR CONSEQUENCES ESTIMATES

CUMULATIVE RISK PLOTS FOR INDIAN POINT AND ZION

INPUTS

1) INDIAN POINT AND ZION SYSTEMS DESIGN
2) WASH-1400 & PWR ICE CONDENSER EVALUATIONS

INPUTS

1) WASH-1400 & PWR ICE CONDENSER EVALUATIONS

INPUTS

1) INDIAN POINT & ZION METEORLOGICAL DEMOGRAPHIC DATA
2) WASH-1400

UTILIZATION OF WASH-1400 IN Z/IP MINI-STUDY

GENERAL

- METHODOLOGY
- STARTED WITH WASH-1400 LIST OF DOMINANT SEQUENCES

ACCIDENT SEQUENCE PROBABILITIES

- INITIATING EVENTS, USED SAME PIPE BREAK PROBABILITIES
- GENERALLY UTILIZED WASH-1400 COMPONENT FAILURE DATA BASE

CONTAINMENT FAILURE MODES AND PROBABILITIES

- UTILIZED THE 5 WASH-1400 FAILURE MODES
- UTILIZED ISOLATION FAILURE AND MELT-THRU FAILURE PROBABILITY VALUES

UTILIZATION OF WASH-1400 IN Z/IP MINI-STUDY

FISSION PRODUCT SOURCE TERM

- UTILIZED CORE INVENTORIES
- UTILIZED SPRAY WASHOUT ASSUMPTIONS
- UTILIZED CONTAINMENT RELEASE ASSUMPTIONS
- UTILIZED SAME 7 FISSION PRODUCT RELEASE CATEGORIES

CONSEQUENCE CALCULATIONS

- UTILIZED CRAC CODE
- UTILIZED WASH-1400 EVACUATION MODEL

ACCIDENT SEQUENCE IDENTIFICATION

- STARTING POINT

  WASH-1400, APPENDIX 5, TABLE 3-14

  CUTOFF PROBABILITY OF 1×10^{-6} / YEAR

- SEQUENCES ADDED

  AHF

  S_1HF

  S_2HF

- SEQUENCES DELETED

  S_2C

  TML

  TKQ

  TKQM

ACCIDENT SEQUENCE SUMMARY

SEQUENCE	INITIATING EVENT	FAILED FUNCTIONS
AD	LARGE LOCA (A)	ECCS INJECTION (D)
AH	LARGE LOCA (A)	ECCS RECIRCULATION (H)
AHF	LARGE LOCA (A)	ECCS RECIRCULATION (H) + SPRAY RECIRCULATION (F)
S_1D	INTERMEDIATE LOCA (S_1)	ECCS INJECTION (D)
S_1H	INTERMEDIATE LOCA (S_1)	ECCS RECIRCULATION (H)
S_1HF	INTERMEDIATE LOCA (S_1)	ECCS RECIRCULATION (H) + SPRAY RECIRCULATION (F)
S_2D	SMALL LOCA (S_2)	ECCS INJECTION (D)
S_2H	SMALL LOCA (S_2)	ECCS RECIRCULATION (H)
S_2HF	SMALL LOCA (S_2)	ECCS RECIRCULATION (H) + SPRAY RECIRCULATION (F)
V	INTERFACING CHECK VALVE FAILURE (V)	
TMLBB'	TRANSIENT LOSS OF OFFSITE POWER (T)	ONSITE AC POWER + AUXILIARY FEEDWATER + LONG-TERM NON RECOVERY OF POWER
TMLBB"	TRANSIENT LOSS OF OFFSITE POWER (T)	ONSITE AC POWER + AUXILIARY FEEDWATER + RECOVERY OF SOME POWER

SUMMARY OF WASH-1400 DIFFERENCES

1. CONTAINMENT FAILURE FROM STEAM EXPLOSION

   - REDUCED BY 10 FOR "A" SEQUENCES

   - REDUCED BY 100 FOR "S_1", "S_2", "TMLB" SEQUENCES

2. OPERATOR ERROR PROBABILITIES

   - ELIMINATE FAILURE TO SHIFT TO HOT LEG RECIRCULATION

   - REDUCE INJECTION-TO-RECIRCULATION ERROR PROBABILITY BY 10 FOR S_1HF AND S_2HF

3. INTERFACING CHECK VALVE CALCULATION

4. LOSS OF OFFSITE POWER PROBABILITY (ZION)

5. CONTAINMENT OVERPRESSURE FAILURE PROBABILITY

   - USED 0.1 FOR ALL SEQUENCES EXCEPT TMLB

6. DIESEL-GENERATOR COMMON MODE FAILURE

   - REDUCED PROBABILITY BY 100

COMPARISON OF CHECK VALVE FEATURES

PLANT FEATURE	ZION	IP-2	IP-3	WASH-1400
CHECK VALVE TEST CONNECTIONS PROVIDED?	YES	YES	YES	NO
PERIODIC TEST INTERVAL	NOT DONE(*)	15 MOS. (*)	9 MOS. (*)	—
LOW PRESSURE SYSTEM PIPING INSIDE CONTAINMENT?	NO	YES	YES	NO
CHECK VALVES ISOLATED BY NORMALLY CLOSED VALVE?	NO	YES	NO	NO
NUMBER OF PATHS TO LOW PRESSURE PIPING ISOLATED BY CHECK VALVES	4	4	4	3
NUMBER OF CHECK VALVES IN EACH PATH	3	2	2	2

(*)TESTING IS PRESENTLY PERFORMED AT EACH RCS PRESSURIZATION.

COMPARISON OF ACCIDENT

SEQUENCE PROBABILITIES

SEQUENCE	PROBABILITY (PER YEAR)			
	ZION	IP-2	IP-3	WASH-1400
AD	5×10^{-7}	6×10^{-7}	5×10^{-7}	2×10^{-6}
AH	2×10^{-8}	1×10^{-7}	2×10^{-9}	1×10^{-6}
AHF	4×10^{-7}	3×10^{-7}	3×10^{-7}	1×10^{-10}
S_1D	5×10^{-7}	2×10^{-6}	1×10^{-6}	3×10^{-6}
S_1H	1×10^{-6}	1×10^{-6}	1×10^{-6}	3×10^{-6}
S_1HF	5×10^{-7}	1×10^{-7}	1×10^{-7}	4×10^{-10}
S_2D	5×10^{-7}	5×10^{-6}	4×10^{-6}	9×10^{-6}
S_2H	4×10^{-6}	4×10^{-6}	4×10^{-6}	6×10^{-6}
S_2HF	2×10^{-6}	4×10^{-7}	4×10^{-7}	1×10^{-9}
S_2C	N/A	N/A	N/A	2×10^{-6}
V	7×10^{-8}	3×10^{-10}	5×10^{-8}	4×10^{-6}
TMLBB'	1×10^{-8}	8×10^{-9}	8×10^{-9}	3×10^{-6}
TMLBB"	1×10^{-8}	8×10^{-9}	8×10^{-9}	3×10^{-6}

CONTAINMENT FAILURE MODES

FAILURE MODE	THIS STUDY	WASH-1400
IN-VESSEL STEAM EXPLOSION (ALPHA)	10^{-3}, LARGE BREAKS 10^{-4}, SMALL BREAKS & TRANSIENTS	10^{-2}
FAILURE OF CONTAINMENT ISOLATION (BETA)	2×10^{-3}	2×10^{-3}
OVERPRESSURE FAILURE (GAMMA + DELTA)	0.1 0.8 FOR TMLB'	0.1 TO 0.2 FOR NO SPRAY CASES 0.8 FOR TMLB $< 10^{-2}$ OTHER CASES
MELT THRU (EPSILON)	CASE 1: RESIDUAL ASSUMING TOTAL FAILURE PROBABILITY OF 1.0 CASE 2: 10^{-2}	RESIDUAL ASSUMING TOTAL FAILURE PROBABILITY OF 1.0

CATEGORIZATION OF SEQUENCES INTO RELEASE CATEGORIES

1a	1b	2	3	5	6	7
STEAM EXPLOSION + NO SPRAY						
LOW PRESSURE	HIGH PRESSURE	OVERPRESSURE NO SPRAY	STEAM EXPLO. WITH SPRAY	OVERPRESSURE WITH SPRAY	MELT-THRU NO SPRAY	MELT-THRU WITH SPRAY
AHF	S_1HF	AHF	AD	AD	AHF	AD
	S_2HF	S_1HF	AH	AH	S_1HF	AH
	TMLBB' (IP)	S_2HF	S_1D	S_1D	S_2HF	S_1D
		V	S_2D	S_2D	TMLBB'(IP)	S_2D
		TMLBB'(IP)	S_1H	S_1H		S_1H
			S_2H	S_2H		S_2H
			TMLBB'(ZION)	TMLBB'(ZION)		TMLBB' (ZION)
			TMLBB"	TMLBB"		TMLBB"

SUMMARY OF PROBABILITY ESTIMATES FOR INDIAN POINT UNIT 3 (CASE 1)

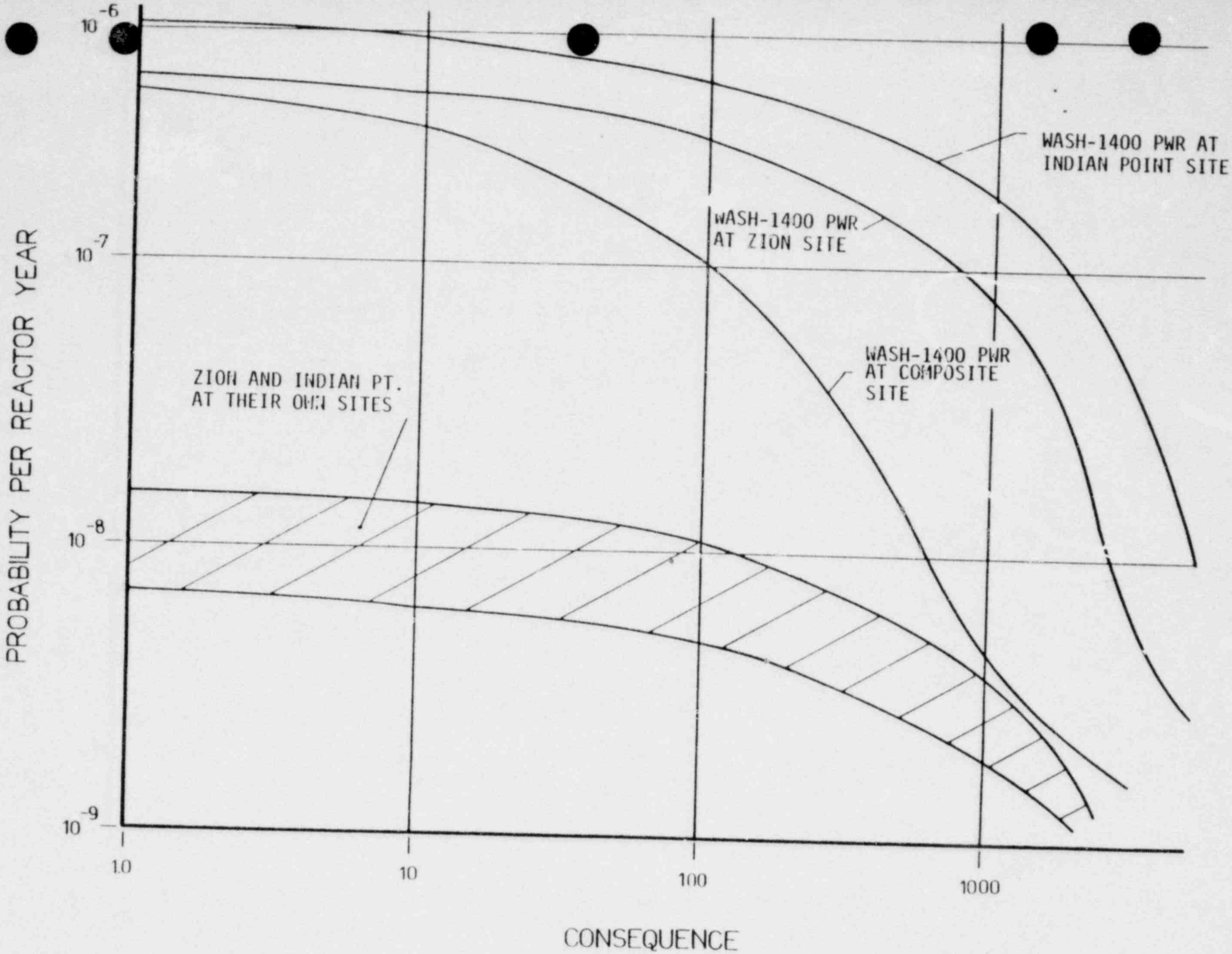
Sequence \ Release Category	1	2	3	4	5	6	7
AD			5.4(-10) α		1.1(- 9) β 5.4(- 8) γ		4.9(- 7) ϵ
AH			1.6(-12) α		4.8(-12) β 1.6(-10) γ		1.4(- 9) ϵ
AHF	3.0(-10) α	3.0(- 8) γ			9.0(-10) β	2.7(- 7) ϵ	
S_1D			1.4(-10) α		2.8(- 9) β 1.4(- 7) γ		1.3(- 6) ϵ
S_1H			1.2(-10) α		2.0(- 9) β 1.2(- 7) γ		1.1(- 6) ϵ
S_1HF	1.1(-11) α	1.1(- 8) γ			1.8(-10) β	1.0(- 7) ϵ	
S_2D			3.6(-10) α		7.9(- 9) β 3.6(- 7) γ		3.2(- 6) ϵ
S_2H			4.0(-10) α		6.7(- 9) β 4.0(- 7) γ		3.6(- 6) ϵ
S_2HF	3.6(-11) α	3.6(- 8) γ			6.0(-10) β	3.2(- 7) ϵ	
V		5.2(- 8)					
TMLBB"			8.0(-13) α		1.9(- 9) γ		6.1(- 9) ϵ
TMLBB'	8.0(-13) α	1.9(- 9) γ 4.5(- 9) δ				1.6(- 9) ϵ	
Total Category Probability	3.3(-10)	1.4(- 7)	1.6(- 9)		1.1(- 6)	6.9(- 7)	9.7(- 6)

SUMMARY OF RELEASE CATEGORY
PROBABILITY ESTIMATES (FOR CASE 1)

PLANT \ RELEASE CATEGORY	1	2	3	4	5	6	7
INDIAN POINT - 2	3.5(-10)	8.5(-8)	1.9(-9)	-	1.3(-6)	7.0(-7)	1.1(-5)
INDIAN POINT - 3	3.5(-10)	1.4(-7)	1.6(-9)	-	1.1(-6)	6.9(-7)	9.7(-6)
ZION	6.4(-10)	3.2(-7)	1.2(-9)	-	6.8(-7)	2.2(-6)	6.1(-6)
WASH-1400 PWR	2.8(-8)	4.6(-6)	2.3(-6)	-	3.8(-8)	1.4(-7)	2.9(-5)

PRINCIPAL DESIGN DIFFERENCES
IMPORTANT IN THE DOMINANT SEQUENCES

DESIGN FEATURE	PLANT APPLICABILITY			
	ZION	IP-2	IP-3	WASH-1400 PWR
DIESEL SPRAY PUMP	X			
CONTAINMENT FAN COOLERS	X	X	X	
PARALLEL LOW PRESSURE RECIRC. SUBSYSTEMS		X	X	
THREE VS TWO DIESELS	X	X	X	
GAS TURBINES		X	X	
CHECK VALVE TEST CONNECTIONS	X	X	X	
CTMT. SPRAY RECIRC. SEPARATE FROM ECCS RECIRC.				X



PROBABILITY PER REACTOR YEAR
10^-6
10^-7
10^-8
10^-9
1.0
10
100
1000
CONSEQUENCE
WASH-1400 PWR AT INDIAN POINT SITE
WASH-1400 PWR AT ZION SITE
WASH-1400 PWR AT COMPOSITE SITE
ZION AND INDIAN PT. AT THEIR OWN SITES

SUMMARY OF MAN-REM CONSEQUENCE
AND RISK RESULTS BY RELEASE CATEGORY

		1a	1b	2	3	5	6	7	TOTAL
INDIAN PT.2	CONSEQUENCE PER EVENT	4.8(7)	2.1(7)	2.9(7)	3.0(7)	2.9(6)	2.7(5)	4.4(3)	
	RISK PER YEAR	1.4(-2)	1.1(-3)	2.5	5.7(-2)	3.8	1.9(-1)	4.8(-2)	6.5
INDIAN PT.3	CONSEQUENCE PER EVENT	4.8(7)	2.1(7)	2.9(7)	3.0(7)	2.9(6)	2.7(5)	4.4(3)	
	RISK PER YEAR	1.4(-2)	1.0(-3)	[illegible].1	4.8(-2)	3.2	1.9(-1)	4.3(-2)	7.5
ZION	CONSEQUENCE PER EVENT	1.8(7)	1.2(7)	1.6(7)	1.9(7)	2.6(6)	3.2(5)	5.9(3)	
	RISK PER YEAR	7.7(-3)	2.5(-3)	5.1	2.3(-2)	1.8	0.7	3.6(-2)	7.6
WASH-1400	CONSEQUENCE PER EVENT	2.8(6)		3.1(6)	1.4(6)	7.0(4)	7.5(3)	1.3(2)	
	RISK PER YEAR	7.8(-2)		1.4(1)	3.2	2.7(-3)	1.1(-3)	3.8(-3)	17.3

SUMMARY

IMPORTANT CONTRIBUTORS TO RISK FOR INDIAN POINT & ZION

	TYPE OF SEQUENCE - CONTAINMENT FAILURE MODE CONTRIBUTION	REPRESENTATIVE SEQUENCES
MAJOR	• CONTAINMENT OVERPRESSURE FAILURE WITHOUT SPRAYS. FAILURE RESULTING FROM PRESSURE SPIKES (HYDROGEN BURN OR RAPID STEAM GENERATION)	AHF-GAMMA S_1HF-GAMMA
INTERMEDIATE	• CONTAINMENT OVERPRESSURE FAILURES WITH SPRAYS • INTERFACING CHECK VALUE FAILURES (PERHAPS) • TMLB' WITH OVERPRESSURE FAILURE (PERHAPS)	AH-GAMMA AD-GAMMA S_1D-GAMMA S_2D-GAMMA V(?) TMLB(?)
MINOR	• INTERFACING CHECK VALVE FAILURES (PERHAPS) • TMLB' WITH OVERPRESSURE FAILURE (PERHAPS) • ALL STEAM EXPLOSION SEQUENCES • ALL CONTAINMENT ISOLATION FAILURE	V(?) TMLB(?) AD-ALPHA AD-BETA

SUGGESTED SEQUENCES

FOR

DESIGN

LARGE BREAK	AD OR AHF
SMALL BREAK	S_1D OR S_2D
	S_1H OR S_1HF
TRANSIENTS	• SMALL CONTRIBUTOR TO RISK
	• CONTINUE TO EVALUATE IN DESIGN STUDIES WHILE LOW RISK ESTIMATE IS CONFIRMED

GARRICK

Presentation

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS SUBCOMMITTEE ON CLASS 9 ACCIDENTS

Los Angeles, California
July 2, 1980

Prepared by
PICKARD, LOWE AND GARRICK, INC.
Irvine, California

ZION/INDIAN POINT
PROBABILISTIC RISK ASSESSMENT

- STATEMENT OF PURPOSE
- CONCEPT OF RISK
- METHODOLOGY
- PROGRESS AND SCHEDULE

ZION/INDIAN POINT PROBABILISTIC RISK ASSESSMENT

PURPOSE

- QUANTIFICATION OF RISK
  - — HEALTH AND SAFETY
  - — PROPERTY DAMAGE
- QUANTITATIVE BASIS FOR EVALUATING THE IMPACT ON RISK OF
  - — PLANT MODIFICATIONS
  - — EMERGENCY PLANNING
- TRAINING
  - — ANALYSIS
  - — OPERATIONS AND MAINTENANCE

WHAT IS A PROBABILISTIC RISK ASSESSMENT?

ANSWER TO:

(1) WHAT CAN HAPPEN?
(WHAT CAN GO WRONG?)

(2) HOW LIKELY IS IT?
(WHAT IS ITS FREQUENCY?)

(3) WHAT ARE THE CONSEQUENCES?
(WHAT IS THE DAMAGE?)

FORMAT OF THE ANSWER:

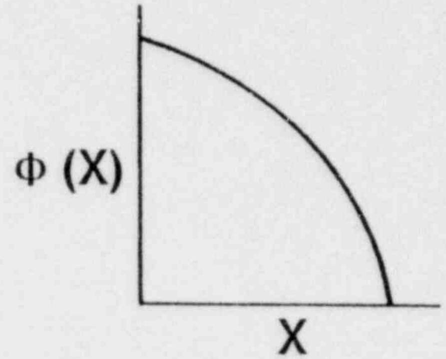
(1) TABULAR:

SCENARIO	FREQUENCY	DAMAGE
s_1	ϕ_1	X_1
s_2	ϕ_2	X_2
•	•	•
•	•	•
•	•	•
s_N	ϕ_N	X_N

(2) ANALYTIC:

$$R = \left\{ \langle s_i, \phi_i, X_i \rangle \right\}$$

(3) GRAPHICAL:



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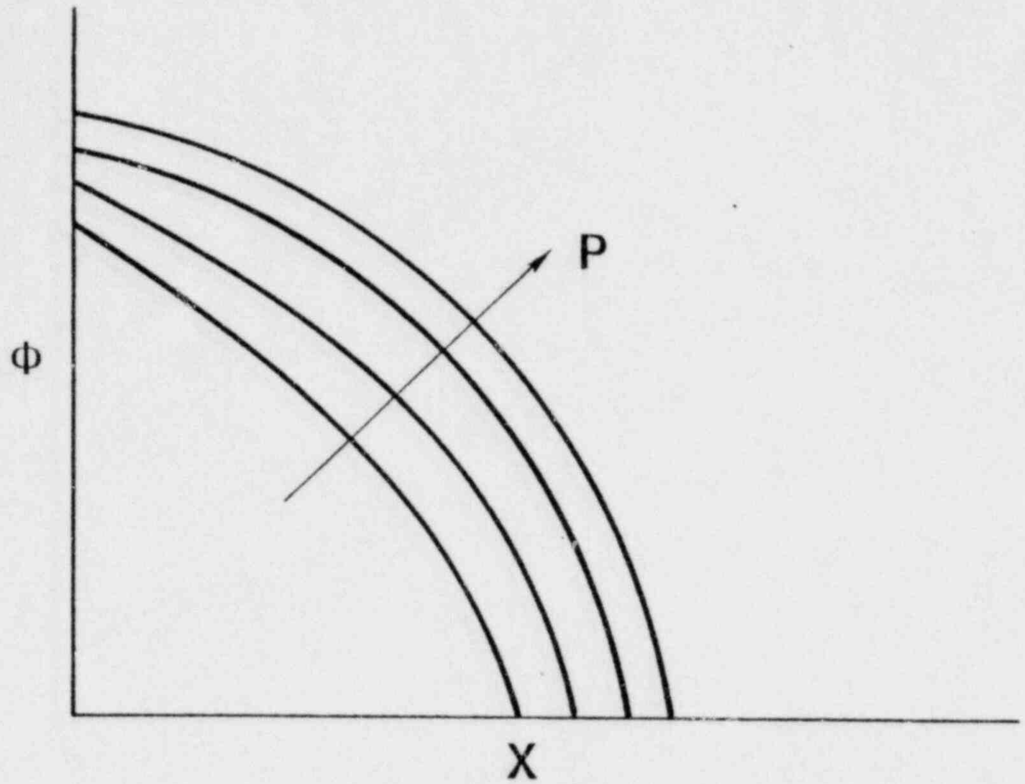
INCLUDE UNCERTAINTY

$$R = \left\{ < s_i, p_i(\phi_i), q_i(x_i) > \right\}$$



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INCLUDE "OTHER" CATEGORY

	SCENARIO	FREQUENCY	DAMAGE
	s_1	ϕ_1	X_1
	•	•	•
	•	•	•
	•	•	•
	s_N	ϕ_N	X_N
(OTHER)	s_{N+1}	ϕ_{N+1}	X_{N+1}

LIST IS NOW LOGICALLY COMPLETE

STRUCTURING THE SCENARIO LIST

STRUCTURING THE SCENARIO LIST
BASIC PINCH POINT

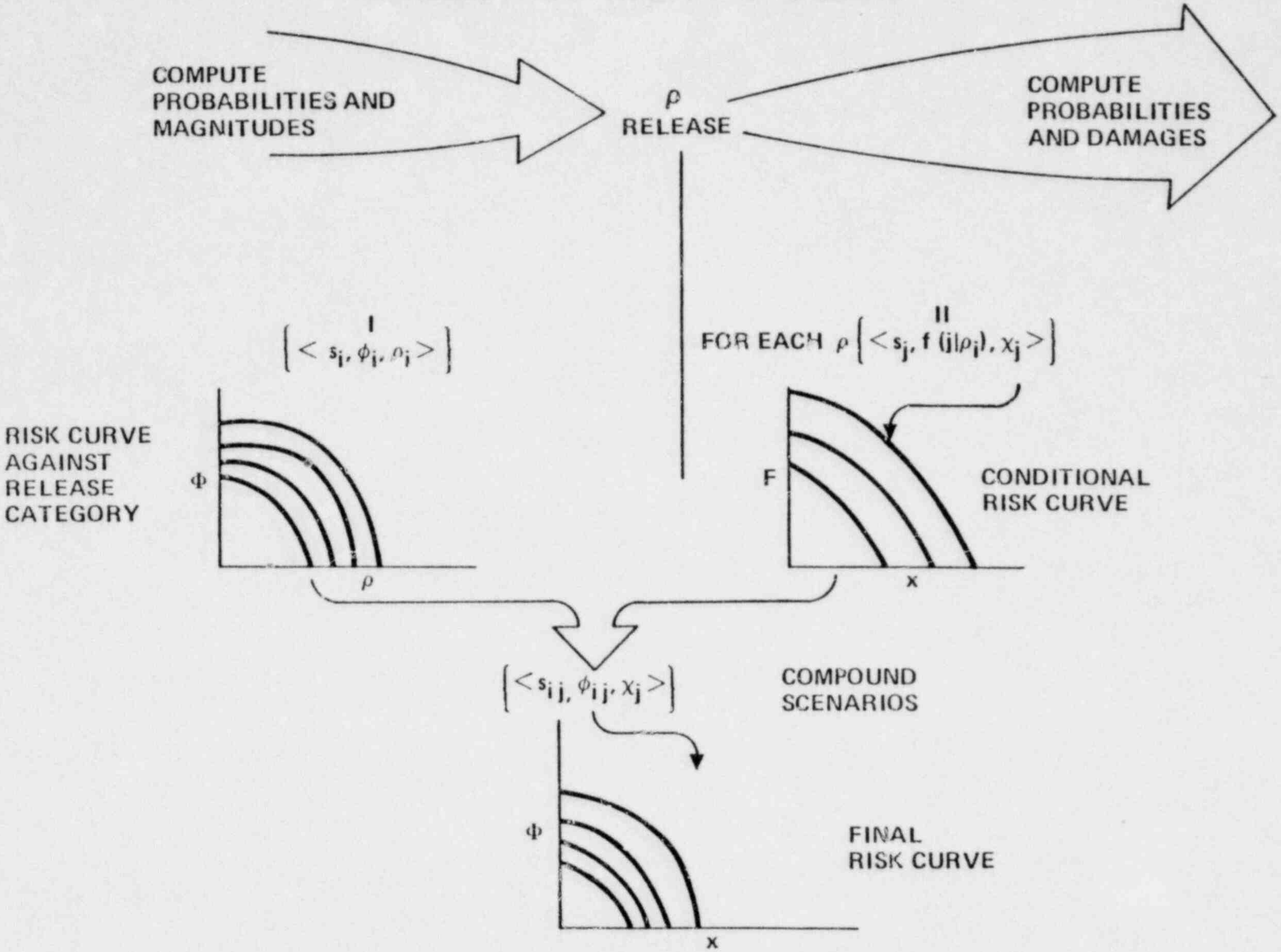

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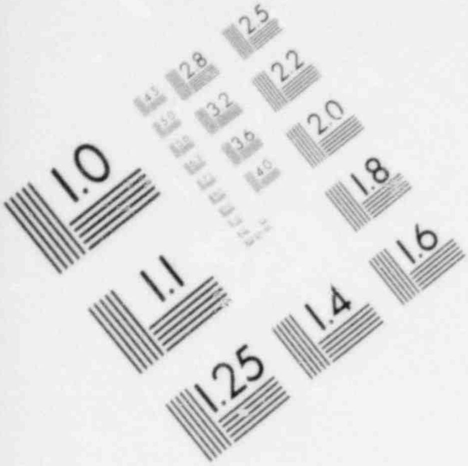
IMAGE EVALUATION
TEST TARGET (MT-3)

6"

MICROCOPY RESOLUTION TEST CHART



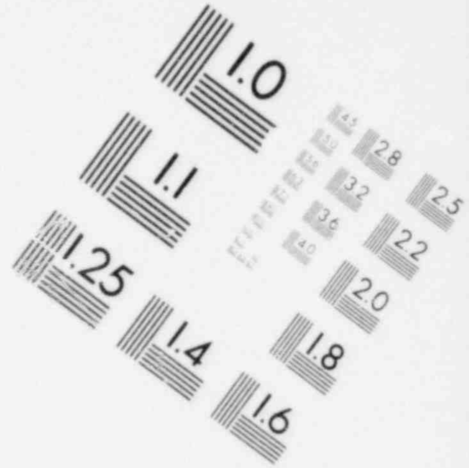
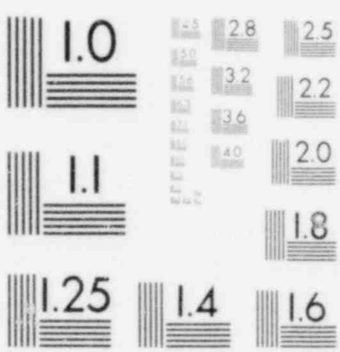
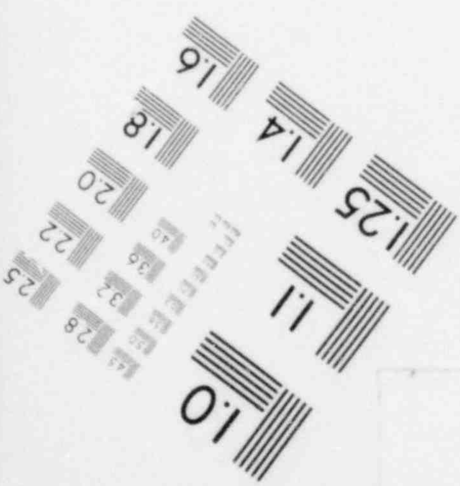


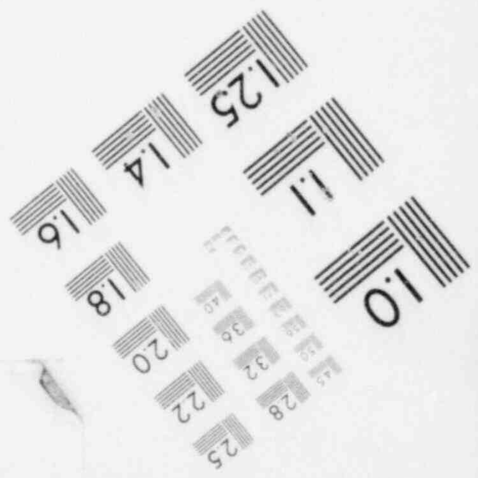
IMAGE EVALUATION
TEST TARGET (MT-3)



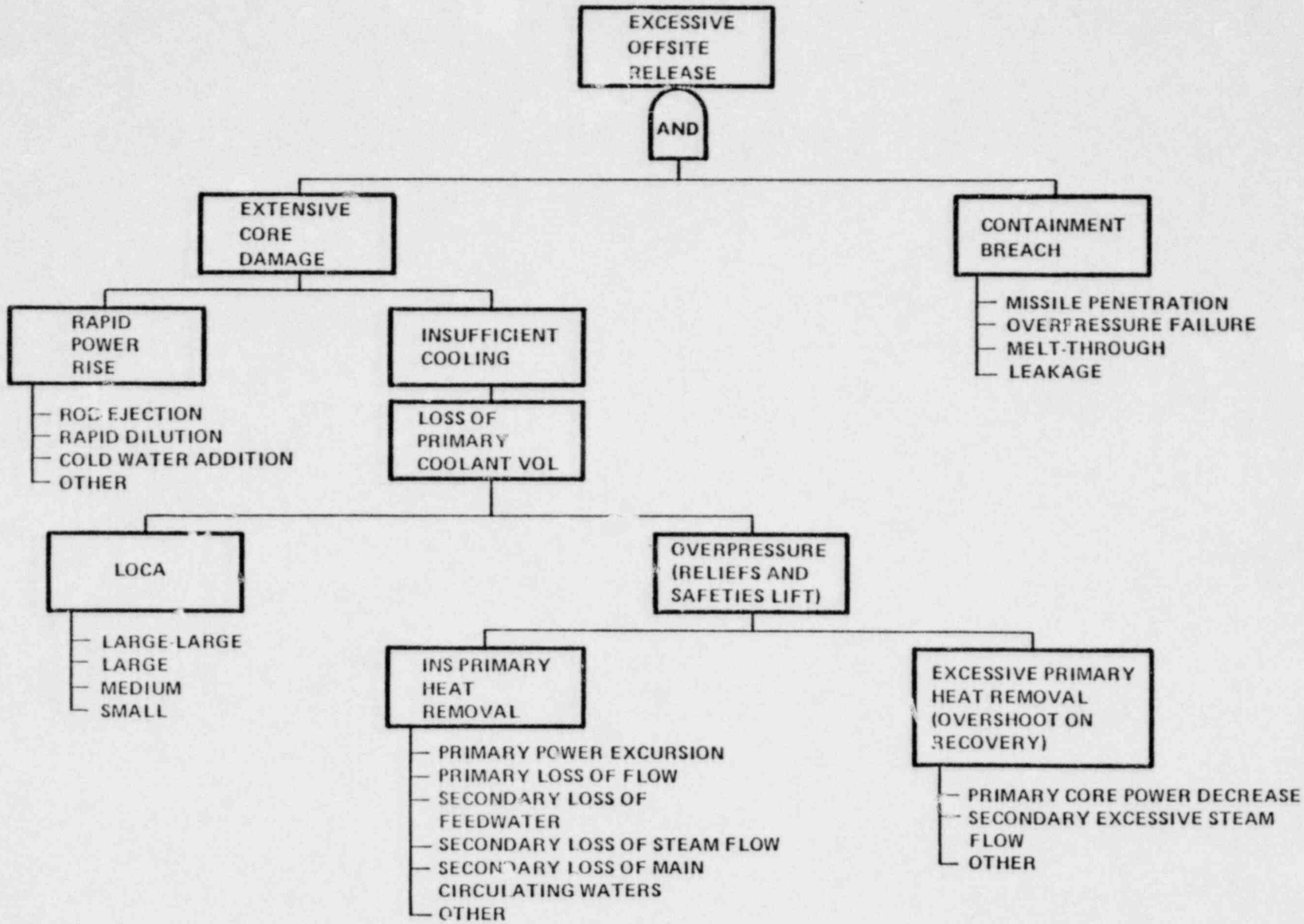
6″

MICROCOPY RESOLUTION TEST CHART





ZION/INDIAN POINT MASTER LOGIC DIAGRAM



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STRUCTURING SCENARIOS — MODELS

SYSTEM MODEL

- COMPONENT FAILURE MODE TO SYSTEM FAILURE

PLANT MODEL

- INITIATING EVENT TO SYSTEM AND HUMAN INTERACTION TO FUEL DAMAGE

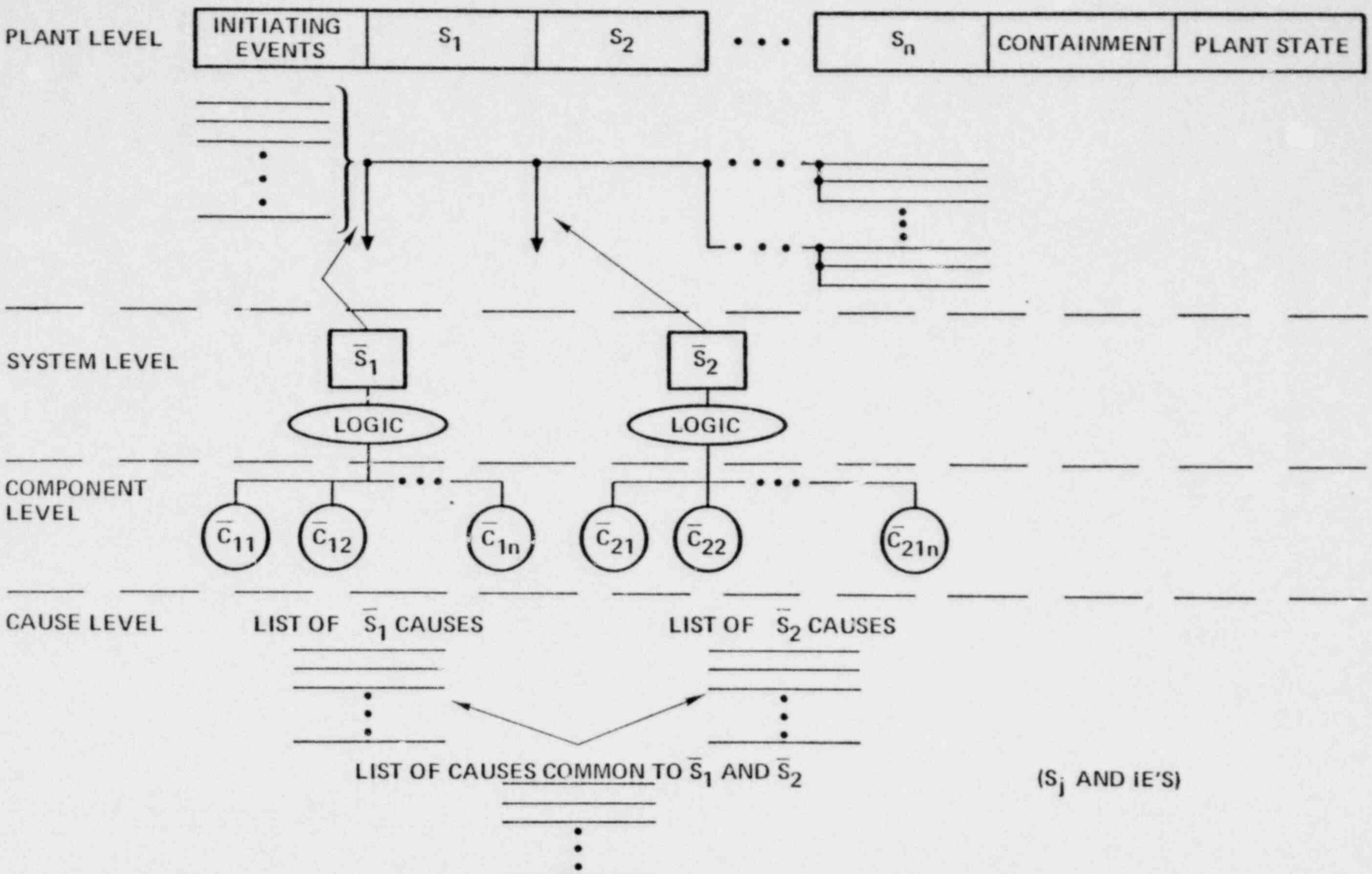
CONTAINMENT MODEL

- FUEL DAMAGE TO CONTAINMENT RELEASE

ATMOSPHERIC DISPERSION AND HEALTH EFFECTS MODEL

- CONTAINMENT RELEASE, WEATHER SCENARIOS, HEALTH EFFECTS, AND PROPERTY DAMAGE

STRUCTURING SCENARIOS



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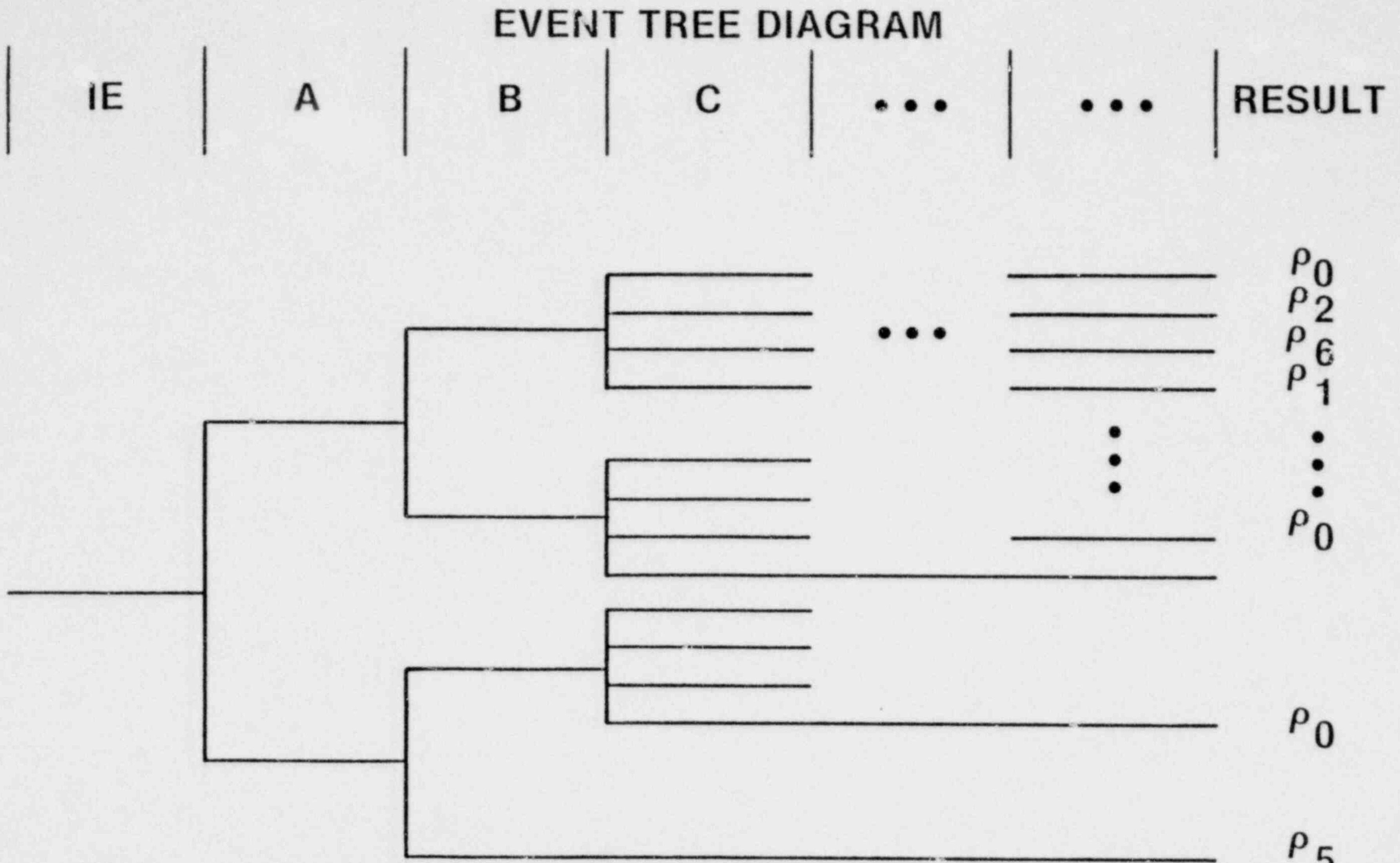
STRUCTURING THE SCENARIO LIST



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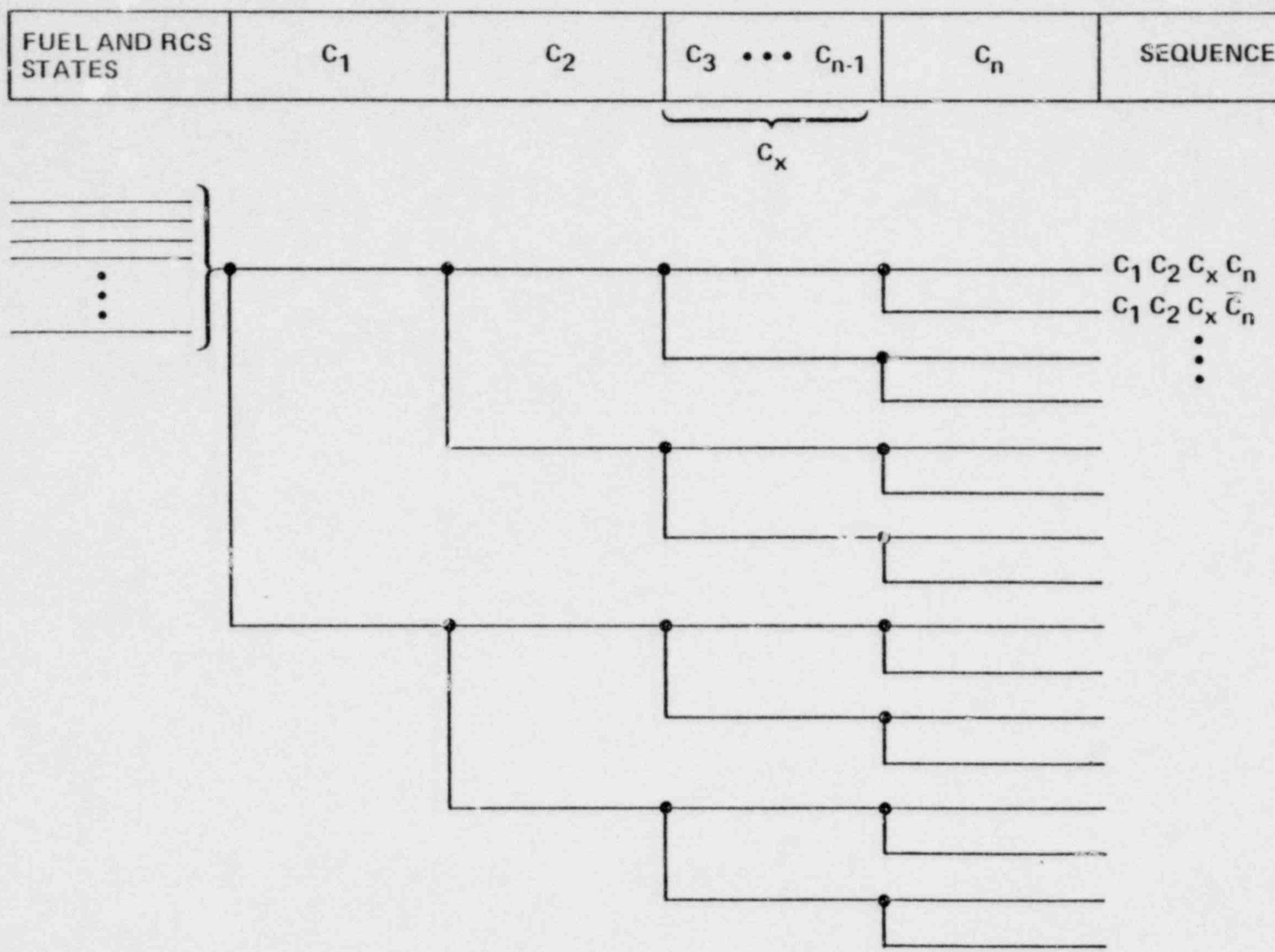
CONTAINMENT SCENARIOS



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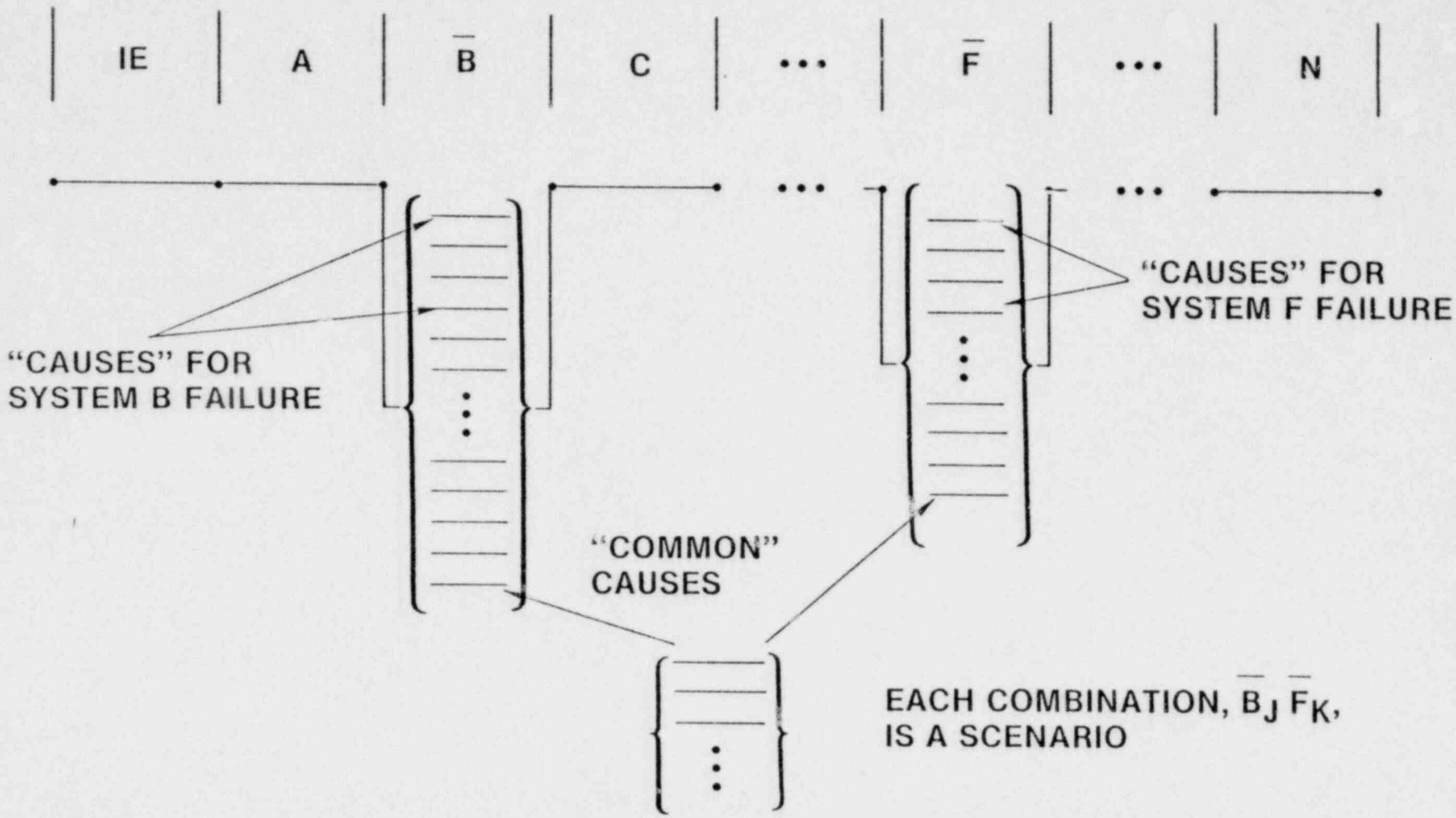
STRUCTURING THE SCENARIO LIST

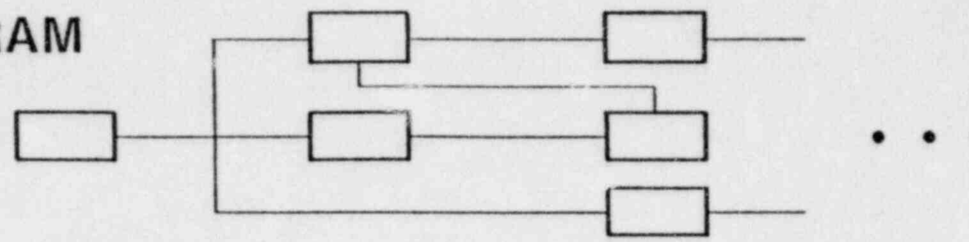
SCENARIOS INCLUDING SYSTEM FAILURE MODES



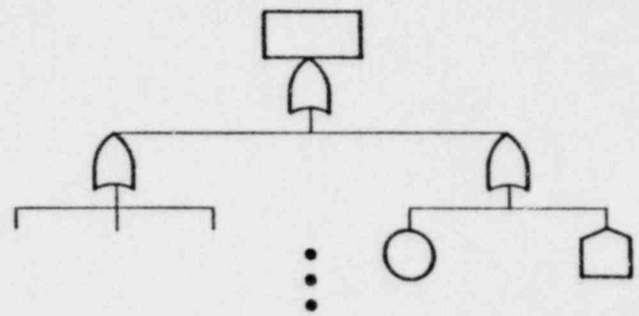
FORM OF A SYSTEM ANALYSIS

I. SYSTEM LOGIC

(1) BLOCK DIAGRAM



(2) FAULT TREE



(3) FAILURE SETS (CUTSETS)

(A, B) (H, K, M) ...

II. CAUSE LIST

CAUSE OR CAUSE SET	FREQUENCY	RESPONSE	RESULTS
⋮	⋮	⋮	⋮

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CAUSE TABLE

SYSTEM: ______

CAUSE	ϕ	RESPONSE	ψ	RESULTS			
				COMPONENTS	SYSTEM	OTHER SYSTEMS	IE
CRFs							
T&M + CRFs							
HUMAN ERRORS							
DESIGN ERRORS							
ENVIRONMENTAL FACTORS							
HE + EFs							
• • • • ∞							

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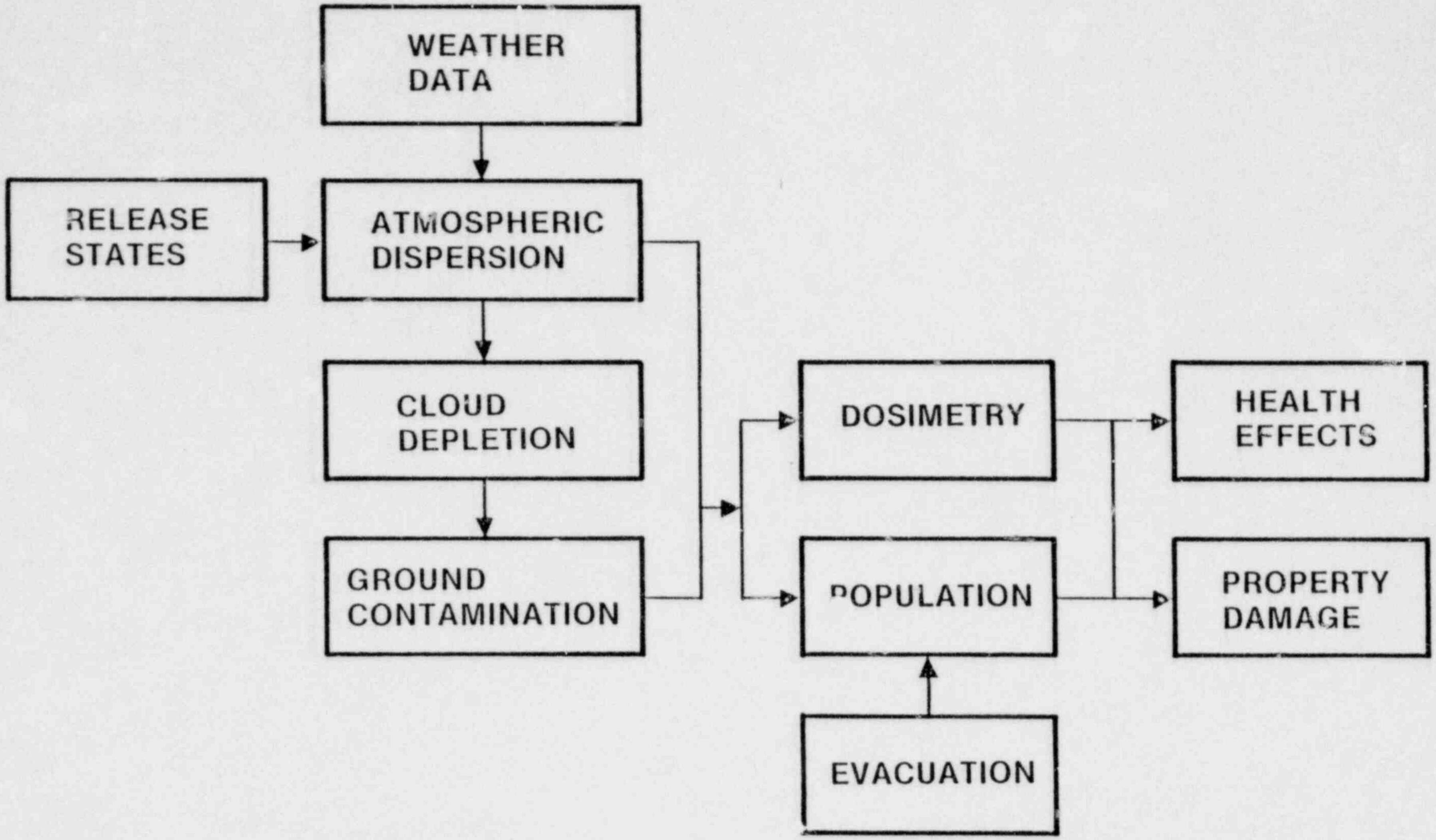
CAUSE TABLE
LOW PRESSURE INJECTION SYSTEM

SYSTEM EFFECTS OF PIPE FAILURE

PIPE SECTION	DIAMETER	SYSTEM FAILURE	POTENTIAL FOR OTHER SYSTEM IMPACT	INITIATING EVENT (LOCA)	COMMENTS
SI-006 (SUCTION, RWST)	12"	YES – COMMON SUCTION LINE	NO	NO	
RH007 (DISCHARGE PUMP A)	10"	YES, THROUGH CROSSOVER	YES–CVCS; SI, CS	NO	CAN BE ISOLATED FROM THE CONTROL ROOM
RH008 (DISCHARGE PUMP B)	10"	YES, THROUGH CROSSOVER	YES–CVCS, SI, CS	NO	CAN BE ISOLATED FROM THE CONTROL ROOM
RH010 (CROSSOVER)	8"	YES	YES–CVCS, SI, CS	NO	CAN BE ISOLATED FROM THE CONTROL ROOM
SI004 (SUPPLY TO SI, TRAIN A)	10"	YES, THROUGH CROSSOVER	YES–CVCS, SI CS	NO	
SI005 SUPPLY TO CVCS, TRAIN B	10"	YES, THROUGH CROSSOVER	YES–CVCS, SI, CS	NO	
SI129 THROUGH SI131	2"	NO	YES – SI (SINGLE INJECTION LINE ONLY)	NO	
SI123, 124, 125, 127	8"	NO	YES – SI (SINGLE INJECTION LINE ONLY)	YES – LARGE LOCA	

EXPLANT CONSEQUENCE SCENARIOS



DETERMINING FREQUENCY OF ELEMENTAL EVENTS (HANDLING FAILURE DATA)

DATA HANDLING

1) FOCUS ON:

SPECIFIC EQUIPMENT / FAILURE MODE m (OR IE)

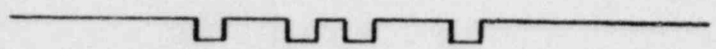
2) DESIRE:

λ_m (a) FAILURE RATE
OR OCCURRENCE FREQUENCY

3) AVAILABLE INFORMATION:

(i) BASIC KNOWLEDGE
ENGINEERING, DESIGN, MANUFACTURE
T&M PRACTICE, GEOLOGY, ETC.

(ii) HISTORY OF SPECIFIC EQUIPMENT (PLANT SPECIFIC)



(iii) HISTORY OF SIMILAR EQUIPMENT

4) COMBINE INFORMATION (i), (ii), (iii)

BAYES' THEOREM

$$p(\lambda \mid E) = p(\lambda) \left[\frac{p(E \mid \lambda)}{p(E)} \right]$$

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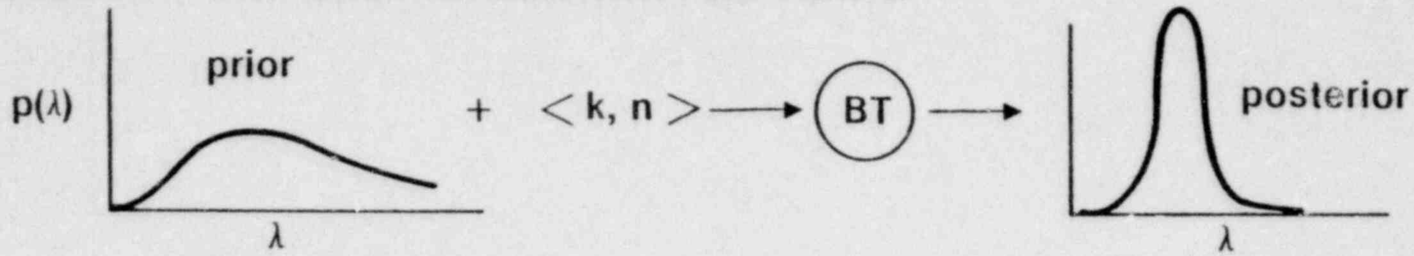
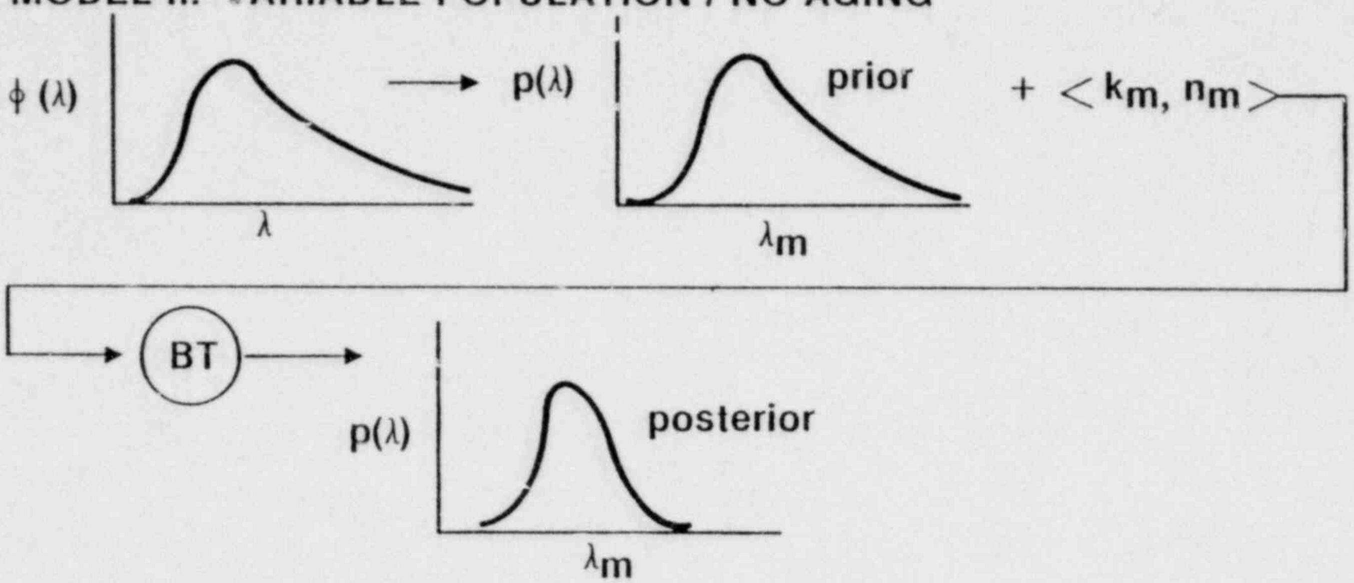
MODELS FOR DATA HANDLING (1)

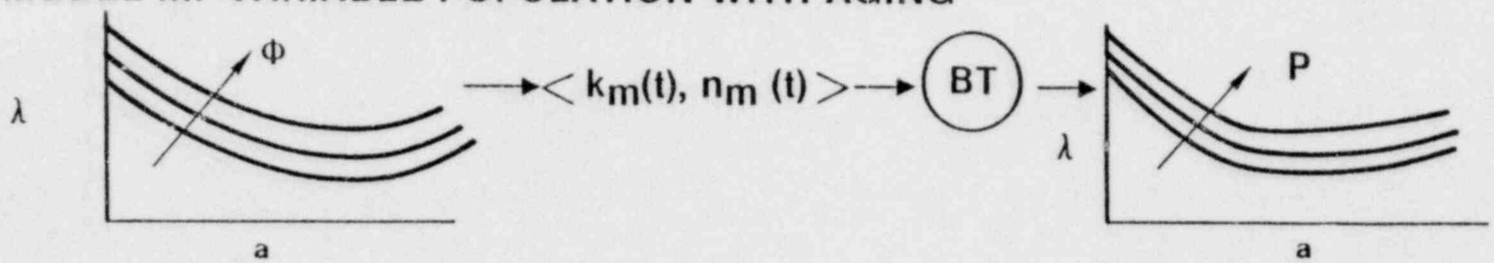
MODEL I: UNIFORM POPULATION / NO AGING



MODEL II: VARIABLE POPULATION / NO AGING



MODEL III: VARIABLE POPULATION WITH AGING



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MODELS FOR DATA HANDLING (2)

MODEL IIA:

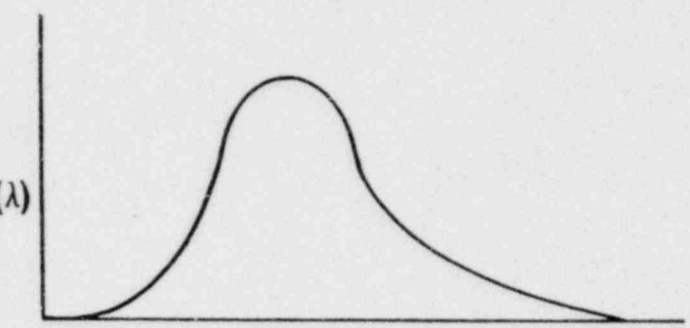
GIVEN $\phi(\lambda)$ (FROM MEASUREMENT WASH—1400, ETC.)

MODEL IIB:

USE ACTUAL EXPERIENCE DATA:
(FROM NUREG, NPRDS, ETC.)

$$\left\{ \begin{array}{c} \langle k_1, n_1 \rangle \\ \langle k_2, n_2 \rangle \\ \vdots \end{array} \right\}$$

$$\left\{ \begin{array}{c} \langle k_1, n_1 \rangle \\ \vdots \end{array} \right\} \longrightarrow \text{BT} \longrightarrow \phi(\lambda)$$



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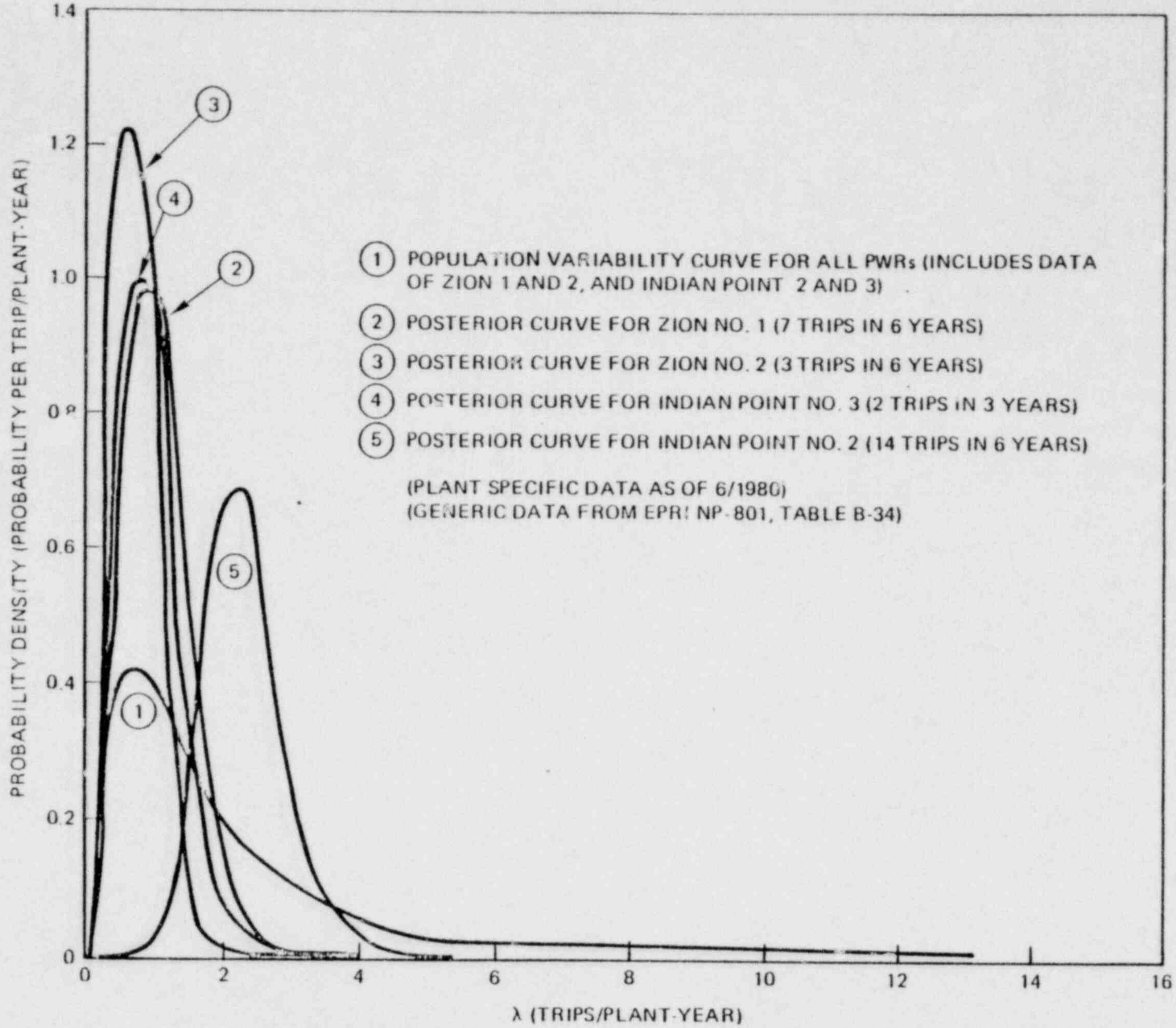
MODELS IIIA, B ANALOGOUS

FREQUENCY OF TURBINE TRIPS



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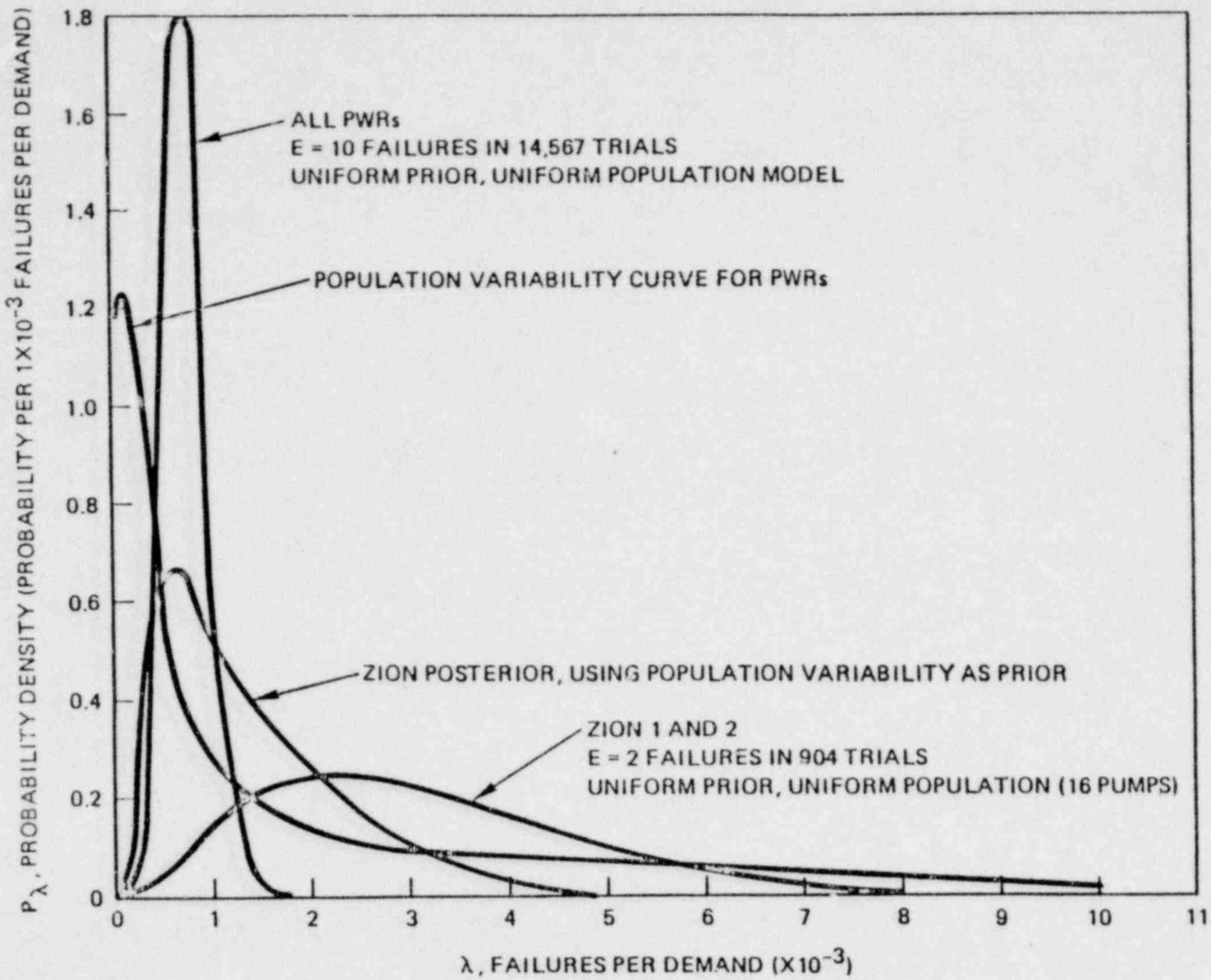
STATF OF KNOWLEDGE CURVES FOR STANDBY PUMPS MOTOR DRIVEN, FAILS TO START



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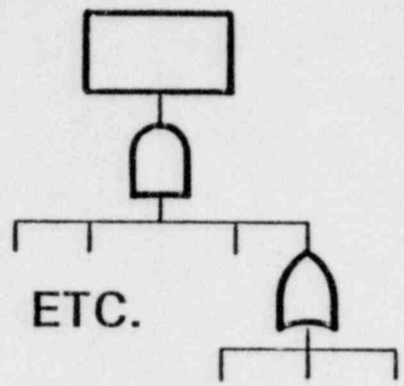
EARTHQUAKE METHODOLOGY
(SIMILAR FOR OTHER EXTERNAL CAUSE)

1. PLANT MODEL: $\{\mathscr{S}, S, \mathscr{L}_p\}$

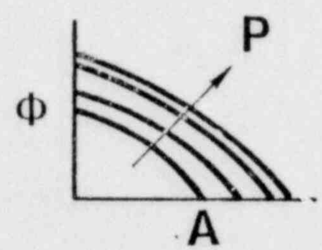
IE	A	B	•••	N

2. SYSTEM MODELS:

SYSTEM (OR IE) $= \{C, \mathscr{L}_s\}$,



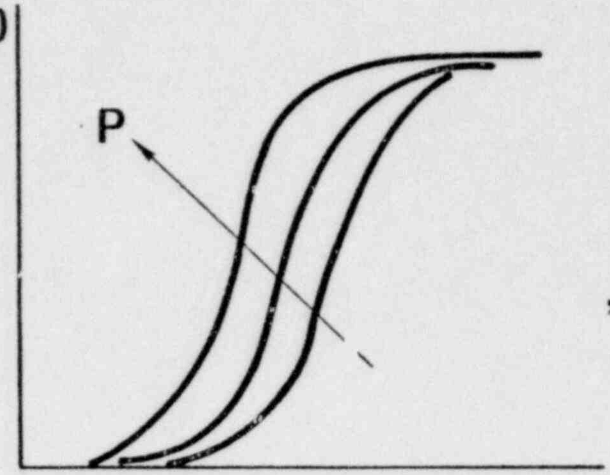
3. SEISMICITY:



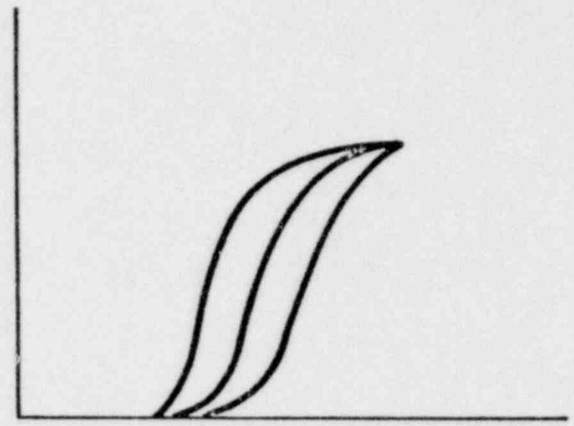
, $\{< P_I, \Phi_I(A) >\}$

EARTHQUAKE METHODOLOGY (CONT.)

4. FRAGILITY:

(FOR COMPONENT C) F  , $\{< Q_j^c, F_j^c(A) >\}$

5. SYSTEM (OR IE) FRAGILITY:

$\{F_j^c(A)\} \rightarrow \{\mathscr{L}_S, \mathscr{D}_S\} \longrightarrow F_j^s(A) \longrightarrow$ 

SYSTEM DEPENDENCY RULES

FOR SYSTEM S

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PROGRESS AND SCHEDULE ZION/INDIAN POINT PROBABILISTIC RISK ASSESSMENT JUNE 1980

SPECIFIC TASKS	PERCENT COMPLETE	PERCENT OF EFFORT
EVENT/FAULT TREES	92	30
QUANTIFICATION	20	10
INPLANT CONSEQUENCES	20	5
EXPLANT CONSEQUENCES	78	25
EXTERNAL CAUSES	35	15
REPORT PREPARATION	15	15
		100
OVERALL	~58% COMPLETE	
SCHEDULED COMPLETION	SEPTEMBER 1980	

Pickard, Lowe and Garrick, Inc.

PADDLEFORD

DEGRADED CORE COOLING CALCULATIONS

FOR

TMI-2

D. L. BURMAN

L. E. HOCHREITER

S. E. JACOBS

J. E. OLHOEFT

D. F. PADDLEFORD

S. L. SHELL

H. C. YEH

BEST ESTIMATE CORE COOLING CALCULATIONS WERE PERFORMED FOR THE PRESIDENT'S COMMISSION ON THREE MILE ISLAND CONSIDERING FOUR CORE CONFIGURATIONS:

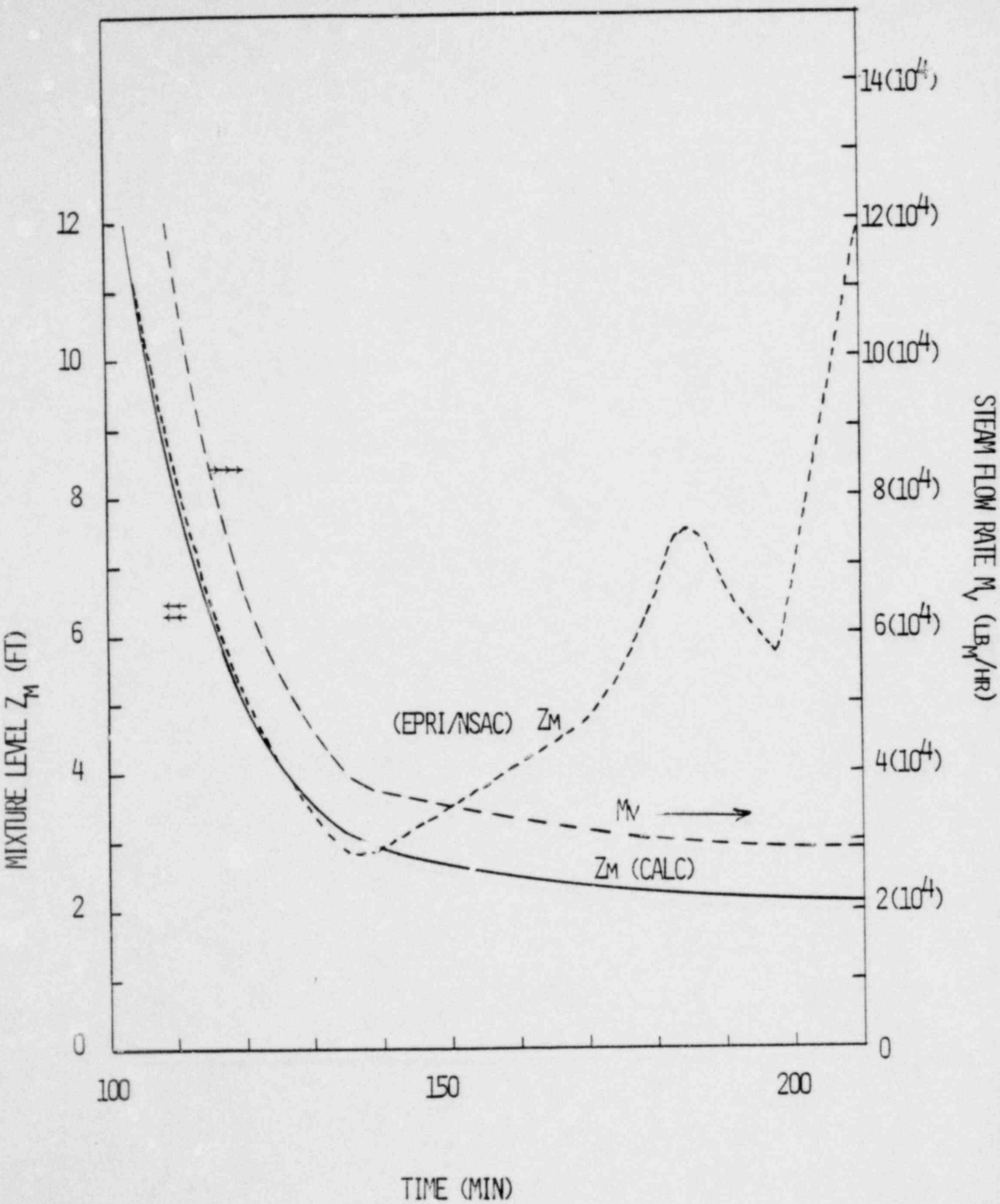
- COOLABLE CORE WITH INTACT GEOMETRY
- COOLABLE CORE AS A WATER COOLED PARTICLE BED
- COOLABLE CORE AS A MOLTEN POOL
- COOLABLE CORE AS A DEBRIS BED IN THE CONTAINMENT

THE INITIAL AND BOUNDARY CONDITIONS FOR THE CALCULATIONS WERE:

- CORE COVERED AT 100 MINUTES
- BLOCK VALVE WAS OPEN, NEVER CLOSED
- RCS PUMPS OFF, NO SI
- NET MAKE-UP FLOW OF 41 GPM
- NO ACCUMULATOR DISCHARGE

HYDRAULIC CALCULATIONS WERE PERFORMED FOR THE CORE TO OBTAIN THE TWO-PHASE FROTH HEIGHT AND CORE STEAMING RATE.

- YEH VOID MODEL WAS USED
- MAKE-UP FLOWS WERE ESTIMATED FROM EPRI/NSAC REPORT
- 41 GPM OF COLD WATER WAS AVAILABLE TO CONDENSE STEAM
- STEAMING FROM THICK METAL LOWER INTERNAL AND REACTOR VESSEL WAS CONSIDERED



MIXTURE LEVEL Z_M (FT)
STEAM FLOW RATE M_V (LB_M/HR)
TIME (MIN)
100
150
200
0
2
4
6
8
10
12
$2(10^4)$
$4(10^4)$
$6(10^4)$
$8(10^4)$
$10(10^4)$
$12(10^4)$
$14(10^4)$
(EPRI/NSAC) Z_M
M_V
Z_M (CALC)

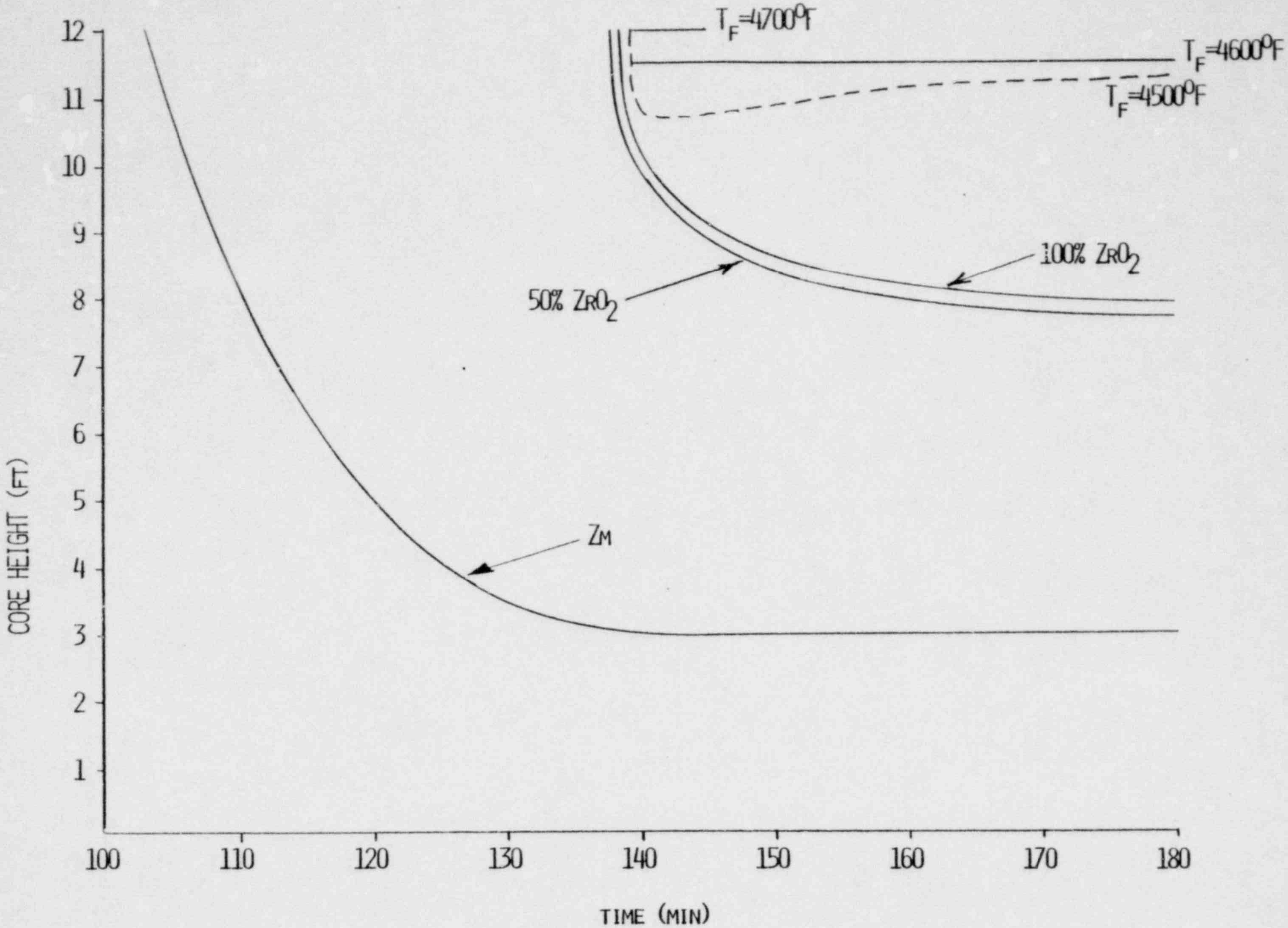
COOLABLE CORE WITH INTACT GEOMETRY ASSUMPTIONS:

- BEST ESTIMATE ZIRC/WATER REACTION
- 40% ENERGY FREE INSIDE REACTION
- MELTING POINTS OF 4900°F FOR UO_2 AN ZrO_2
- RADIATION TO STEAM
- BEST ESTIMATE TMI DECAY CURVE
- VOLATILE FISSION PRODUCTS ASSUMED TO ESCAPE AS FUEL TEMPERATURE INCREASED
- TMI-2 MEASURED AXIAL AND RADIAL POWERS WERE USED

CORE HEAT-UP CONDITIONS WERE PERFORMED USING INPUT HYDRAULIC CONDITIONS:

RESULTS INDICATED:

- UPPER FOUR (4) FEET OF CORE WOULD COMPLETELY REACT
- ZIRC/WATER ENERGY COULD MELT THE CLAD (T_C>4900°F) AT THE TOP
- FUEL/CLAD GAP PREVENT FUEL MELT (T_F = 4724°F)
- LOWER ELEVATIONS WOULD REACT SLOWER
- BLOCKAGE DUE TO CLAD MELT WAS SMALL



CORE HEIGHT (FT)
TIME (MIN)
T_F=4700°F
T_F=4600°F
T_F=4500°F
100% ZrO_2
50% ZrO_2
ZM
12
11
10
9
8
7
6
5
4
3
2
1
100
110
120
130
140
150
160
170
180

COOLABLE CORE AS WATER COOLED PARTICLE BED IN VESSEL

ASSUMPTIONS:

- NON-VOLATILE DECAY HEAT AT 5 HOURS
- 41 GPM MAKE-UP AVAILABLE
- HARDEE-NILSON CORRELATION APPLICABLE
- LOWER PLENUM FILLED WITH WATER

IN VESSEL PARTICLE BED CALCULATIONS INDICATE:

- FOR 1MM PARTICLE SIZE AND A BED VOID FRACTION OF .3, BED CHF WAS NOT LIMITING
- BED COOLABILITY WAS LIMITED BY THE MAKE UP FLOW, 41 GPM COULD COOL 20 - 40% OF THE BED
- AT 38 HOURS, THE ENTIRE BED WAS COOLABLE WITH 41 GPM

COOLABLE CORE AS A MOLTEN POOL IN VESSEL

ASSUMPTIONS:

- NON-VOLATILE DECAY HEAT AT 5 HOURS
- REACTOR VESSEL CAVITY REMAINS WATER FLOODED, WITH NO STRUCTURAL IMPEDIMENTS TO WATER FLOW

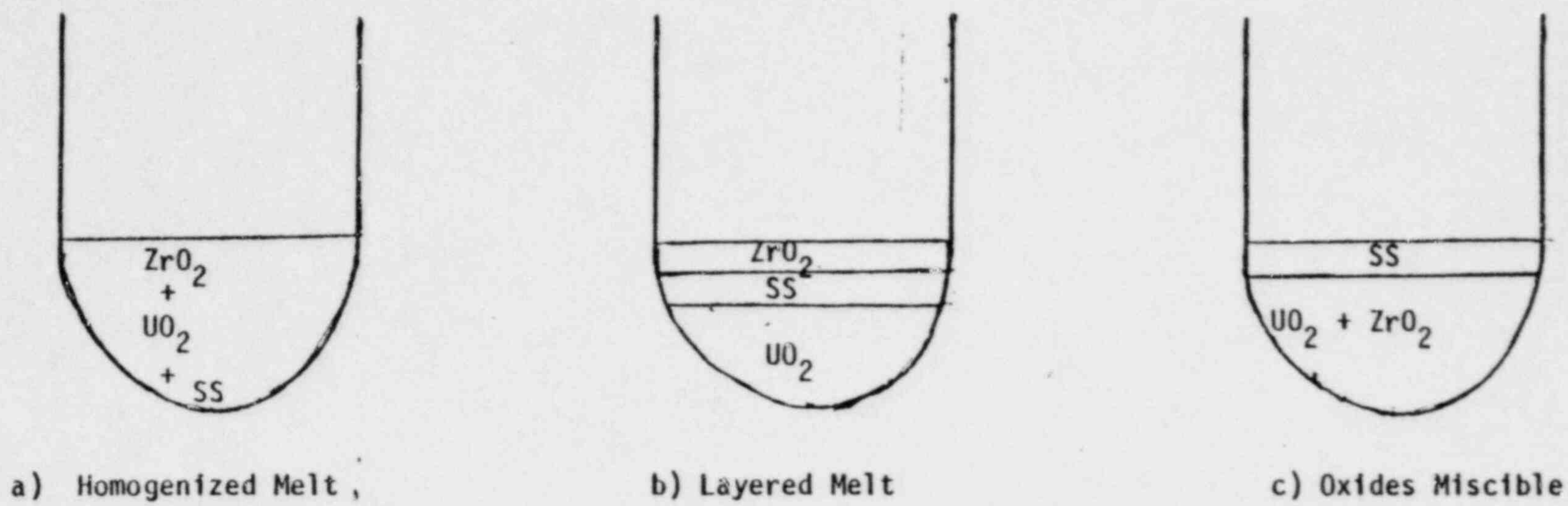
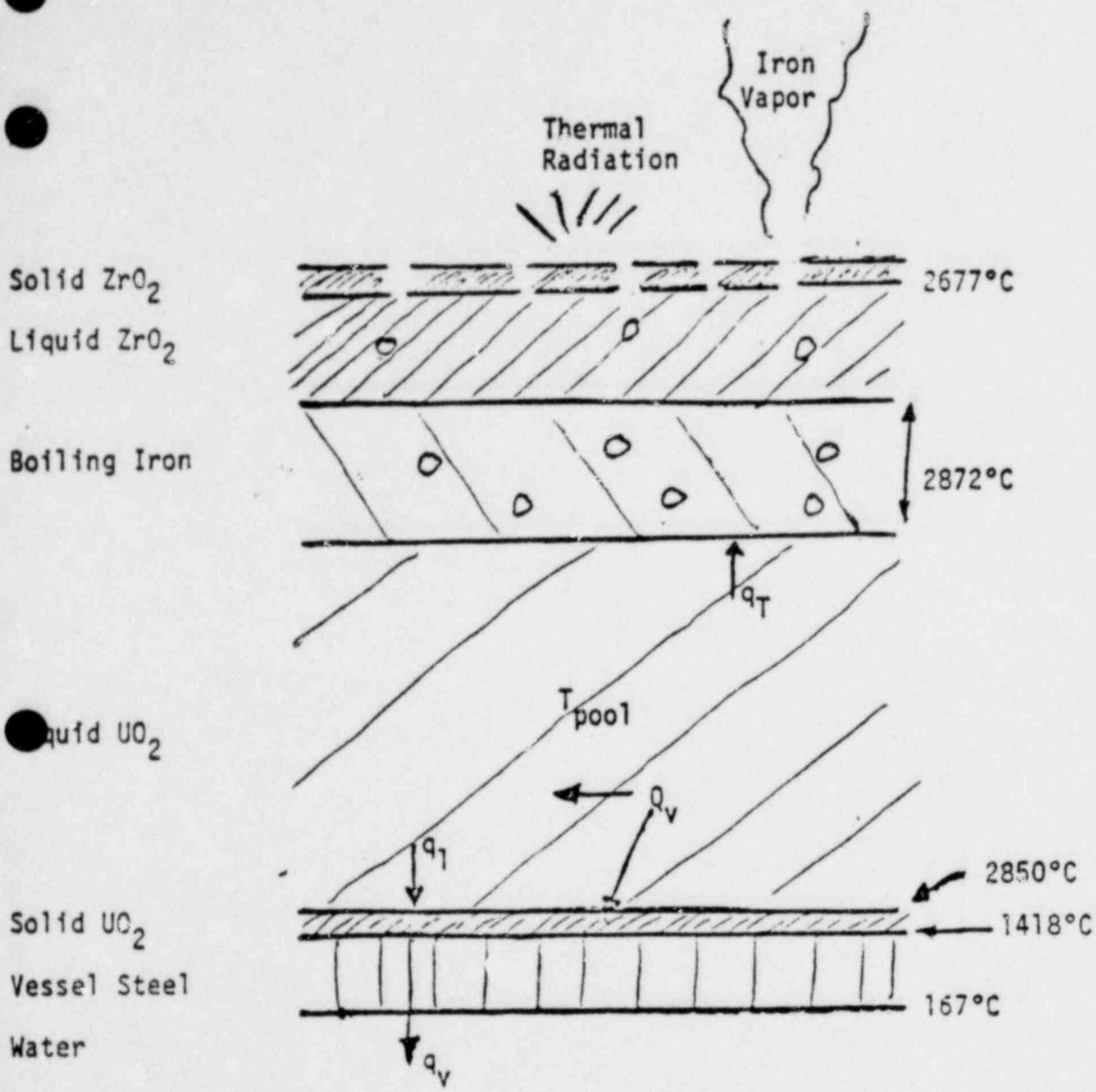


FIGURE 1 Possible Melt Configurations

FIGURE 2 Melt Heat Balance Relations



$$q_T = h_T (T_{pool} - 2872)$$

$$q_v = q_1 + L \cdot Q_v = (q_1^2 + q_2^2)^{1/2}$$

$$q_1 = h_v (T_{pool} - 2850)$$

$$q_2 = (2K(2850 - 1418)Q_v)^{1/2}$$

$$Q_{DECAY} = q_T A_T + q_v A_v$$

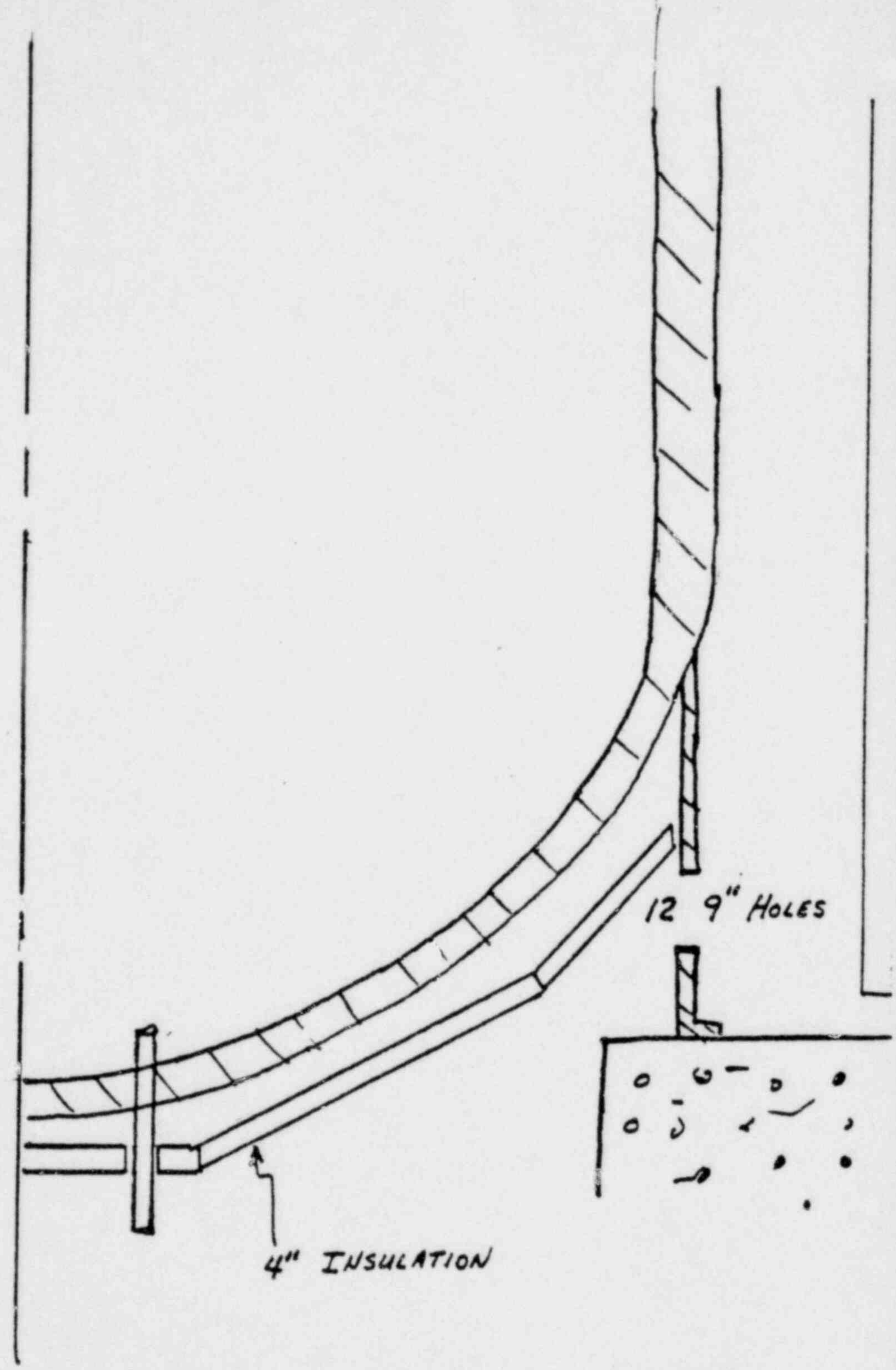
IN VESSEL MOLTEN POOL CALCULATIONS INDICATE:

- FOR A HOMOGENEOUS MELT THE VESSEL WALL WILL MELT TO AN AVERAGE 4.8 - INCH THICKNESS, 2.4 - INCH MINIMUM, BUT WOULD CARRY THE CORE WEIGHT

- THE VESSEL SURFACE HEAT FLUXES ARE BELOW SATURATED BOILING CHF, THUS THE CORE IS COOLABLE IN THIS CONFIGURATION

- EXTERNAL VESSEL STRUCTURES WHICH TRAP STEAM LIMIT CORE COOLABILITY

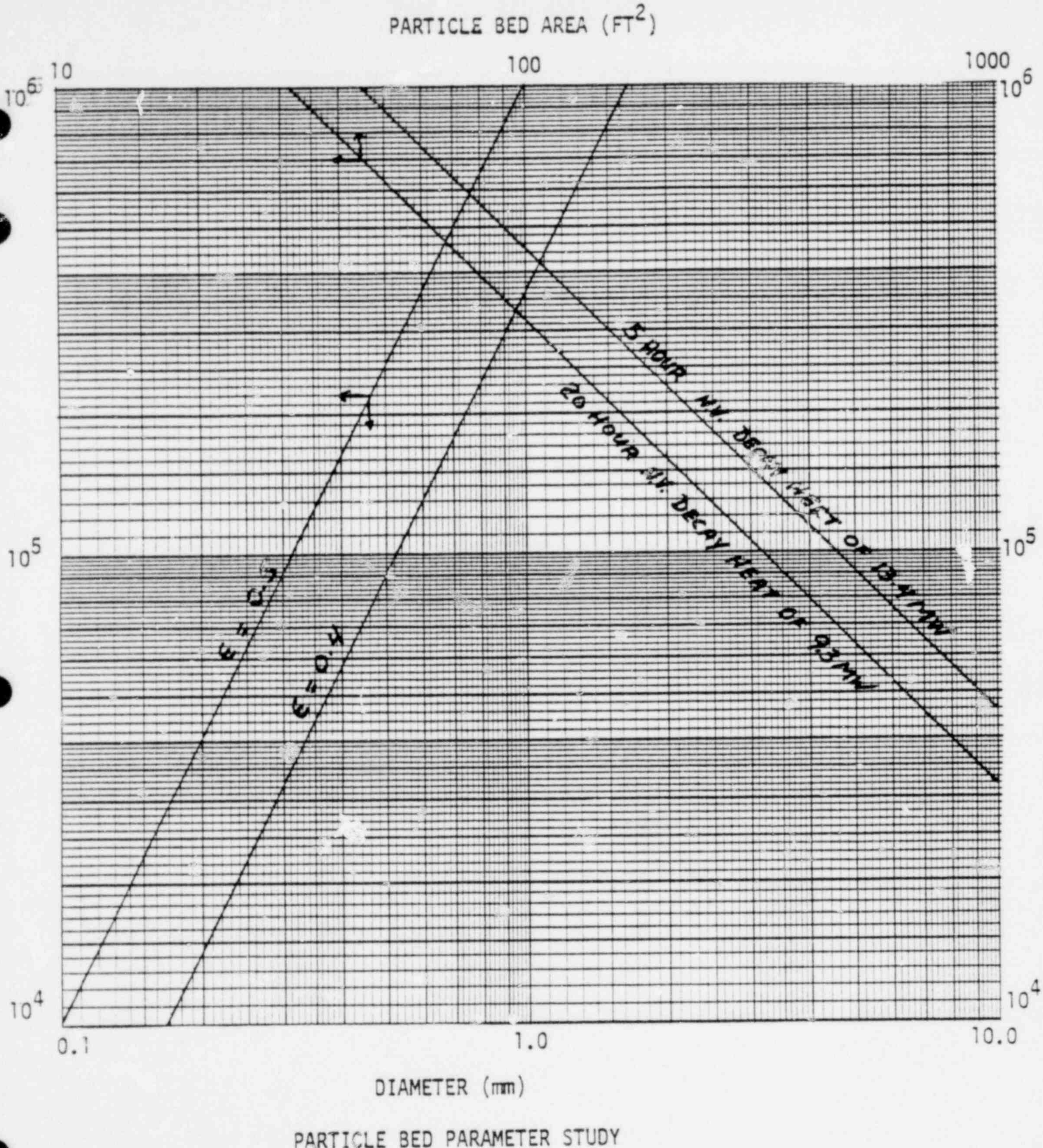
- CONSIDERING THE TMI VESSEL SUPPORT STRUCTURE 10% OF THE CORE COULD BE COOLED AS A MOLTEN POOL



SUPPORT SKIRT AND INSULATION ARRANGEMENT

COOLABLE CORE AS A DEBRIS BED IN CONTAINMENT ASSUMPTIONS:

- WATER FLOODED REACTOR VESSEL CAVITY
- 25 FEET OF WATER HEAD
- NON-VOLATILE DECAY POWER AT 5 HOURS
- CAVITY AREA OF 200 SQUARE FEET
- HARDEE-NILSON CORRELATION APPLICABLE



PARTICLE BED PARAMETER STUDY

EXCESSEL CORE DEBRIS BED CALCULATIONS INDICATE:

- FOR PARTICLES GREATER THAN 1MM CORE IS COOLABLE AS A DEBRIS BED WITH A VOID FRACTION OF 0.35
- FOR SMALLER PARTICLES, LESS THAN ENTIRE CORE WOULD BE COOLABLE

CONCLUSIONS

- THE REACTOR SYSTEM AS DESIGNED, PROVIDES FOR ALTERNATE COOLABLE GEOMETRIES WITH A MINIMUM OF REQUIRED ECC WATER
- MAJOR CORE DAMAGE IN TMI IS POSTULATED TO OCCUR AT THE TIME THE BLOCK VALVE WAS CLOSED
- NO FUEL MELT WAS CALCULATED FOR TMI

GARRY THOMAS

Mitigation of Small-Break LOCAs in Pressurized Water Reactor Systems*
(Summary of Report NSAC-2, March 1980)

Garry R. Thomas

Nuclear Safety Analysis Center
Electric Power Research Institute
Palo Alto, California

Presentation of ACRS Subcommittee Meeting
on Class 9 Accidents

July 2, 1980

*Three Mile Island Unit 2 Used as Reference Case

SMALL BREAK ACCIDENT MITIGATION

THEME:

- Provide Perspective on Ability to Mitigate Progress of Small-Break LOCAs in PWR Systems
- Provide Assurance that Resulting Threat of Containment Breaching Can Be Greatly Reduced or Eliminated with Active Mitigating Responses

OBJECTIVES:

- Define Primary Observables Indicating Small-Break LOCA
- Review Primary Automatic and Operator-Initiated Responses Available for Mitigating Small-Break LOCA
- Demonstrate Resiliency of System with Enhanced Man-Machine Interfaces (e.g., Safety Panel) for Mitigating Small-Break LOCA
- Provide Real-Time Bases for Assurance of Proper Emergency Planning Capability

GRT/js
7/2/80

SMALL-BREAK LOCA MITIGATION

ULTIMATE QUESTION OF POSSIBLE CONTAINMENT DAMAGE DEPENDS ON TWO PRIMARY FACTORS

- ASSURANCE THAT SMALL-BREAK LOCA CONDITION IS RECOGNIZED AND APPROPRIATE AUTOMATIC ENGINEERED SAFETY FEATURES ARE ACTIVATED AND/OR OPERATORS APPROPRIATELY RESPOND
- ASSURANCE THAT WATER AND SOME PUMPING SOURCE CAN BE PROVIDED EVEN IN EVENT OF LOSS OF ALL NORMAL AND INSTALLED BACKUP SUPPLIES

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7/2/80

SMALL-BREAK LOCA MITIGATION

OPERATING SPACE INVOLVES CONCEPT OF INTEGRATED USE OF:

- TIME AVAILABLE TO REACT
- OBSERVABLES THAT DEFINE SYSTEM STATE AND TRENDS
- OPTIONS AVAILABLE FOR COUNTERING ACCIDENT PROGRESSION
- MAGNITUDE OF RESPONSES REQUIRED FROM AVAILABLE OPTIONS

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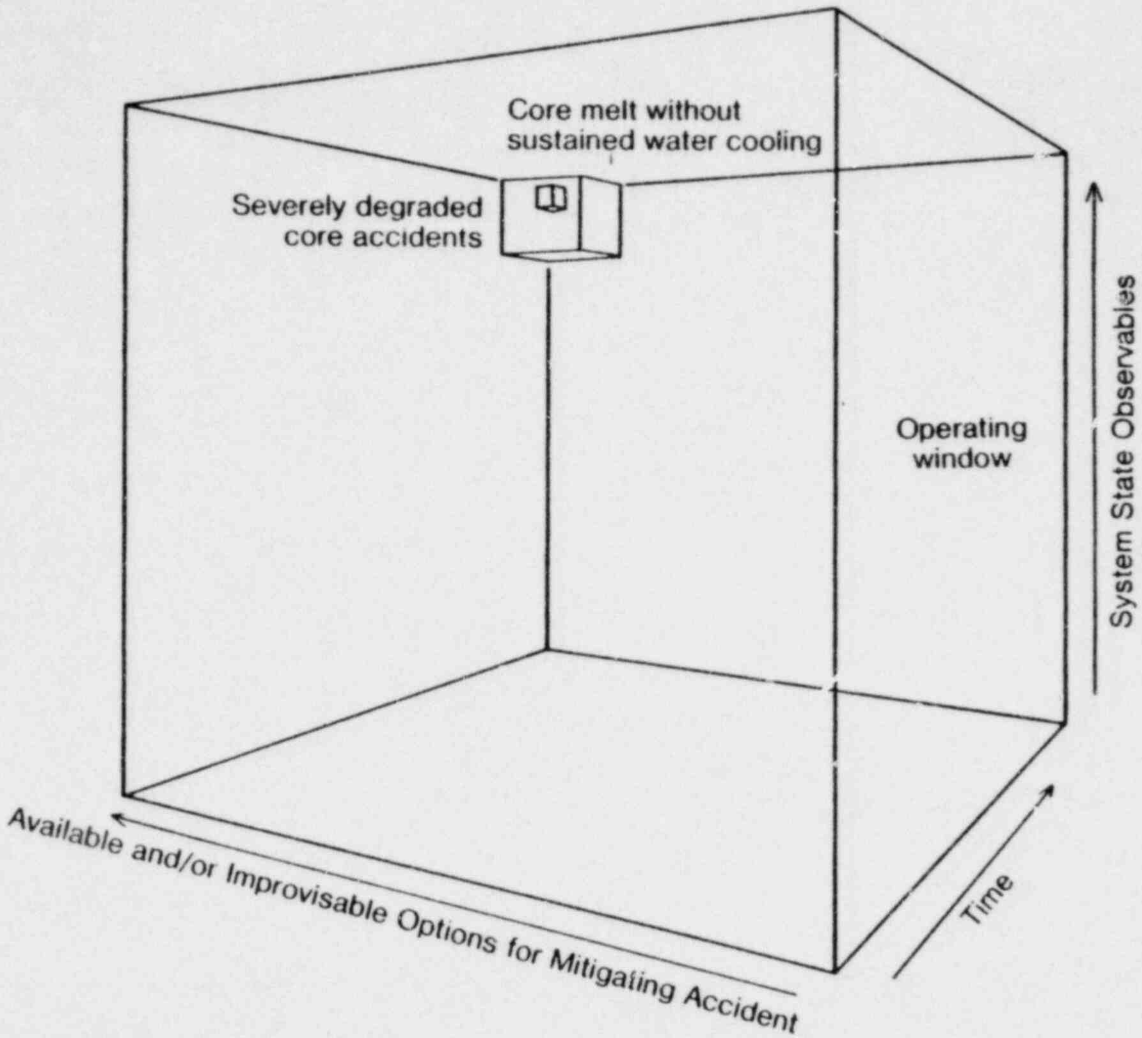
LWR ACCIDENT OPERATING SPACE



SMALL-BREAK LOCA MITIGATION

- Deviations from Normal System Parameters and Heat Sink Capabilities during a Small-Break LOCA Provide Abundant Observalbe Conditions (Observables) Indicating Both Current Accident State and Trends

- Scope and Time Scale of these Observables Permit Rational

    - Selection of Effective Countermeasures

    - Bases for Conservatively Projecting:

        - Potential Public Danger
        - Emergency Planning Actions

    - Selection of Set-points for Objectively Determining Whether or Which Public Alarms or Emergency Responses are needed

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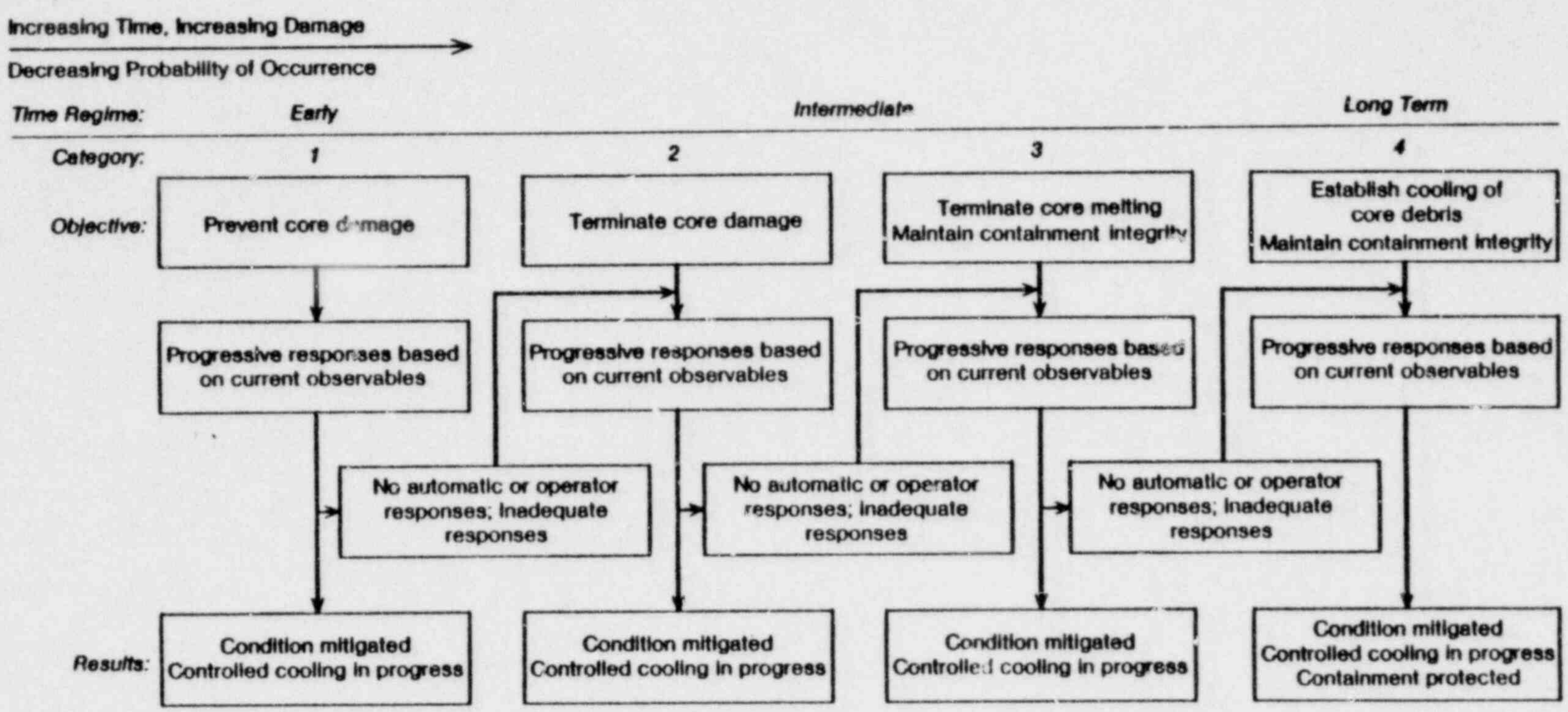
SCHEMATIC PWR SMALL-BREAK LOCA MITIGATION



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SUMMARY OF PWR SMALL-BREAK LOCA OBSERVABLES FOR DIRECTING MITIGATION EFFORTS

Increasing Damage Due to Small-Break LOCA $\longrightarrow$

Decreasing Probability of Occurrence

Time Regime	*Early*	*Intermediate*		*Late*	*Long Term*
Initial Condition	*Initial Core Uncovering*	*Core Uncovering*	*Core Melting*	*Reactor Vessel Failure*	*Core Debris Being Cooled*
Category	*1*	*2*	*3*	*4*	
Objective	Prevent Core Damage	Terminate Core Damage	Terminate Melting	Establish Cooling of Core Debris	Maintain Containment in Integrity
Primary indicators of increasing damage potential (assumes no earlier actions)	1. $P_P \downarrow$; pressurizer level $\uparrow$ or $\downarrow$ 2. HPI actuates 3. $P_C \uparrow$; $T_C \uparrow$ 4. Deviation in source-range monitor signals 5. $T_{sat} \rightarrow T_P$ 6. Boiling noises/coolant pump vibrations 7. $\Delta T_{sup} > 0$	1. $\Delta T_{sup} > 0$ 2. P_P low or $\downarrow$ 3. Radiation signals $\uparrow$ 4. H_2 in containment 5. P_C and/or T_C high 6. Containment spray and/or coolers actuate	1.–6. Same as previous but of increasing severity 7. Core flood actuates 8. LPI actuates	1. $P_P \rightarrow 0$ 2. P_C and T_C suddenly $\uparrow$ 3. Radiation signals large $\uparrow$ 4. Core flood actuates 5. LPI/RHR actuates 6. Pressurizer dumps 7. Containment spray actuates 8. Containment coolers actuate	Responding to previous conditions and actions

Where:

P_P and T_P = Primary pressure and temperature

P_C and T_C = Containment pressure and temperature

T_{sat} = Primary saturation temperature

ΔT_{sup} = Primary superheat temperature difference

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SMALL-BREAK LOCA MITIGATION

MINIMUM COOLANT FLOWRATE FOR REMOVING DECAY HEAT

Decay Power (% Full)	Time (Hours)	Minimum Core Inlet Flow (GPM) Subcooled	Saturated
2%	0.24	340	540
1%	2.6	170	270
0.5%	22	85	135
0.25%	113	43	68

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SMALL-BREAK LOCA MITIGATION

- NSAC-2 Report Is Not a Contradiction of WASH-1400 Reactor Safety Study but a Preliminary Extension of WASH-1400 Methodology.

- WASH-1400 Has Several Major Conservatisms

  - Core Temperature = 2200^{o}F Equivalent to Core Melt
    - Actual Melt Temperatures Range from ~3500^{o}F to > 5000^{o}F
    - Core Melt Progression Would Tend to be very Non-coherent, Possibly Self-Limiting, and Reversible with Added Cooling

  - Containment Failure Probability and Environmental Release of Fission Products Essentially 1.0 if Core Melts
    - Core Melt Should be Reversible with Added Cooling
    - Many ESF and Improvisable Systems Potentially Available to Protect Containment Building

  - Accident Mitigating Systems Not Ever Available if 1st Attempt at Use Fails

  - No Consideration for Positive Use of Time Aspect in Actual Accident Sequence
    - Increasing Accident Time = Increasing Opportunity to:
      - Understand Accident Progress
      - Take Positive Actions Involving Installed Systems
      - Improvise New Mitigating Systems and Actions

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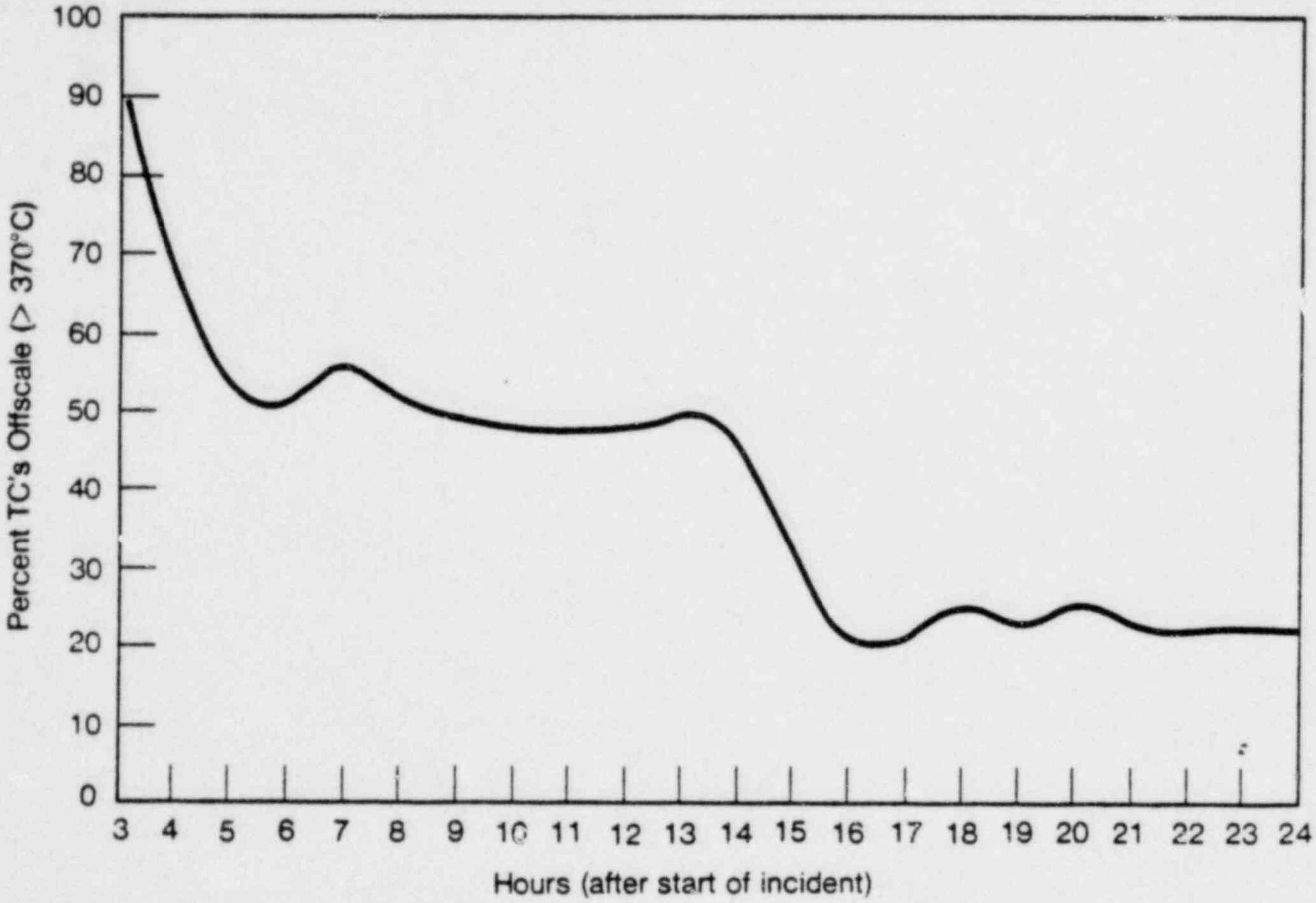

TMI-2 Core exit thermocouple time history.

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LIPPORELO

AGENDA

I. INTRODUCTION

II. HYDROGEN BURN MODEL

III. CALCULATED CONTAINMENT RESPONSE FOR SELECTED SCENARIO'S

IV. REVIEW OF MARCH CODE

V. W MARCH CODE PROGRAM

VI. FUTURE WORK

VII. SUMMARY

PURPOSE OF THIS TALK

1) DISCUSS THE HYDROGEN BURN MODEL DEVELOPED

2) TO DISCUSS CONTAINMENT CALCULATIONS WHICH WERE PERFORMED AS PART OF THE 60 DAY STUDY

3) DISCUSS THE MARCH COMPUTER CODE (BCL CORE MELT CONSEQUENCE ANALYSIS CODE) WHOSE RESULTS WERE USED AS BOUNDARY CONDITIONS TO THE W CONTAINMENT ANALYSES

- 60 DAY STUDY ON CORE MELT FOR ZION AND INDIAN POINT

  - PROGRAM INCLUDED

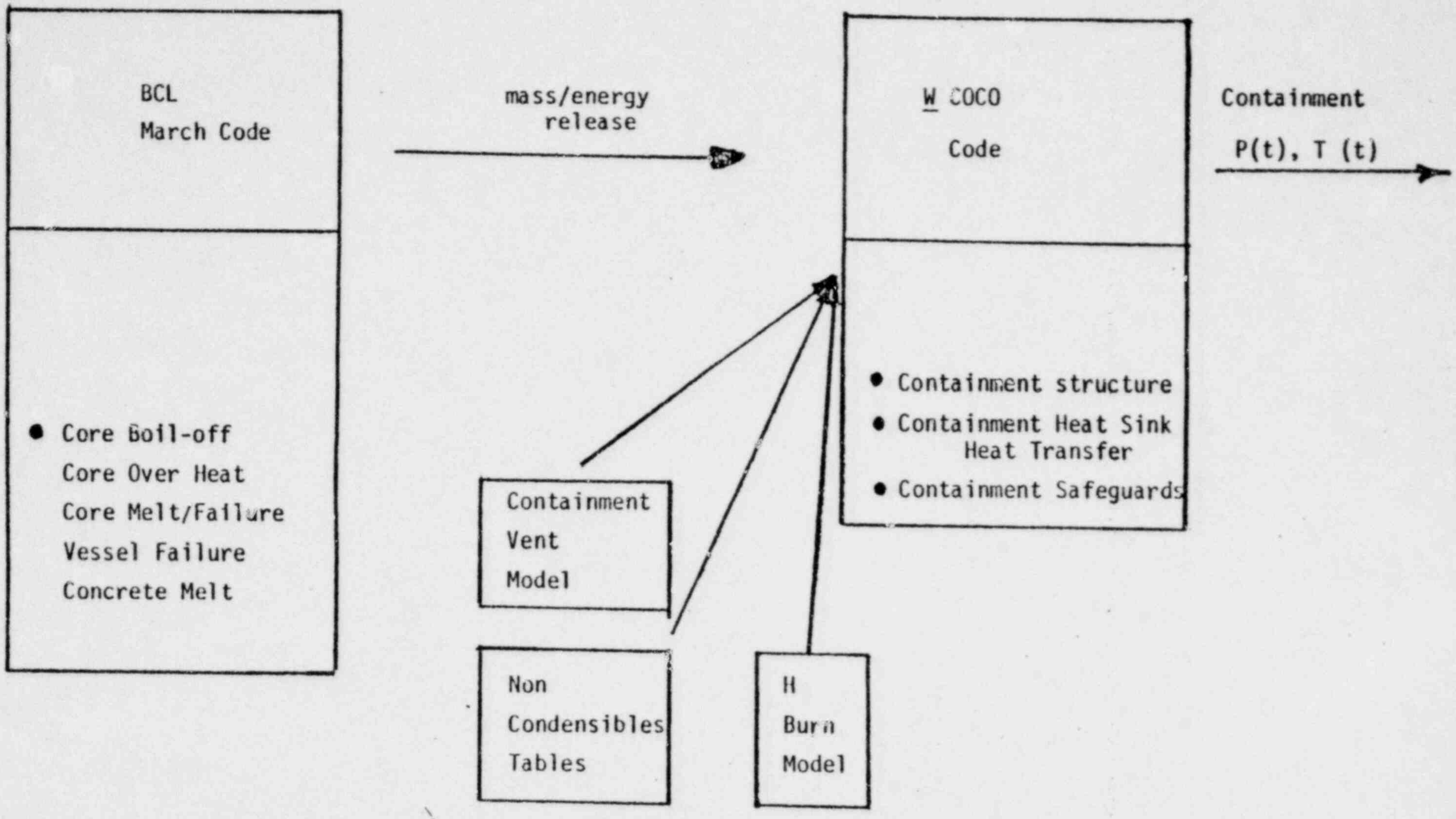
    1) MINI REVIEW OF WASH 1400

    2) BASE RISK STUDY

    3) ENGINEERRING REVIEW OF PLANT SYSTEM MODIFICATIONS

    4) CONTAINMENT RESPONSE TO SCENARIO'S WHICH LEAD TO CLASS-9 EVENTS

Calculational Approach



MINI WASH 1400 STUDY INDICATED THAT THE FOLLOWING SEQUENCES WHICH WERE AVAILABLE WERE REPRESENTATIVE RISK CONTRIBUTORS.

- AD
    1) LARGE BREAK LOCA
        A) ACTIVE ECCS SYSTEMS ARE ASSUMED NOT TO OPERATE
        B) CONTAINMENT SAFEGUARDS (I.E. SPRAYS AND FAN COOLERS) ASSUMED TO OPERATE

- S_2D
    1) SMALL BREAK LOCA
        A) ACTIVE ECCS SYSTEMS ARE ASSUMED NOT TO OPERATE
        B) CONTAINMENT SAFEGUARDS (I.E. SPRAYS AND FAN COOLERS) ASSUMED TO OPERATE

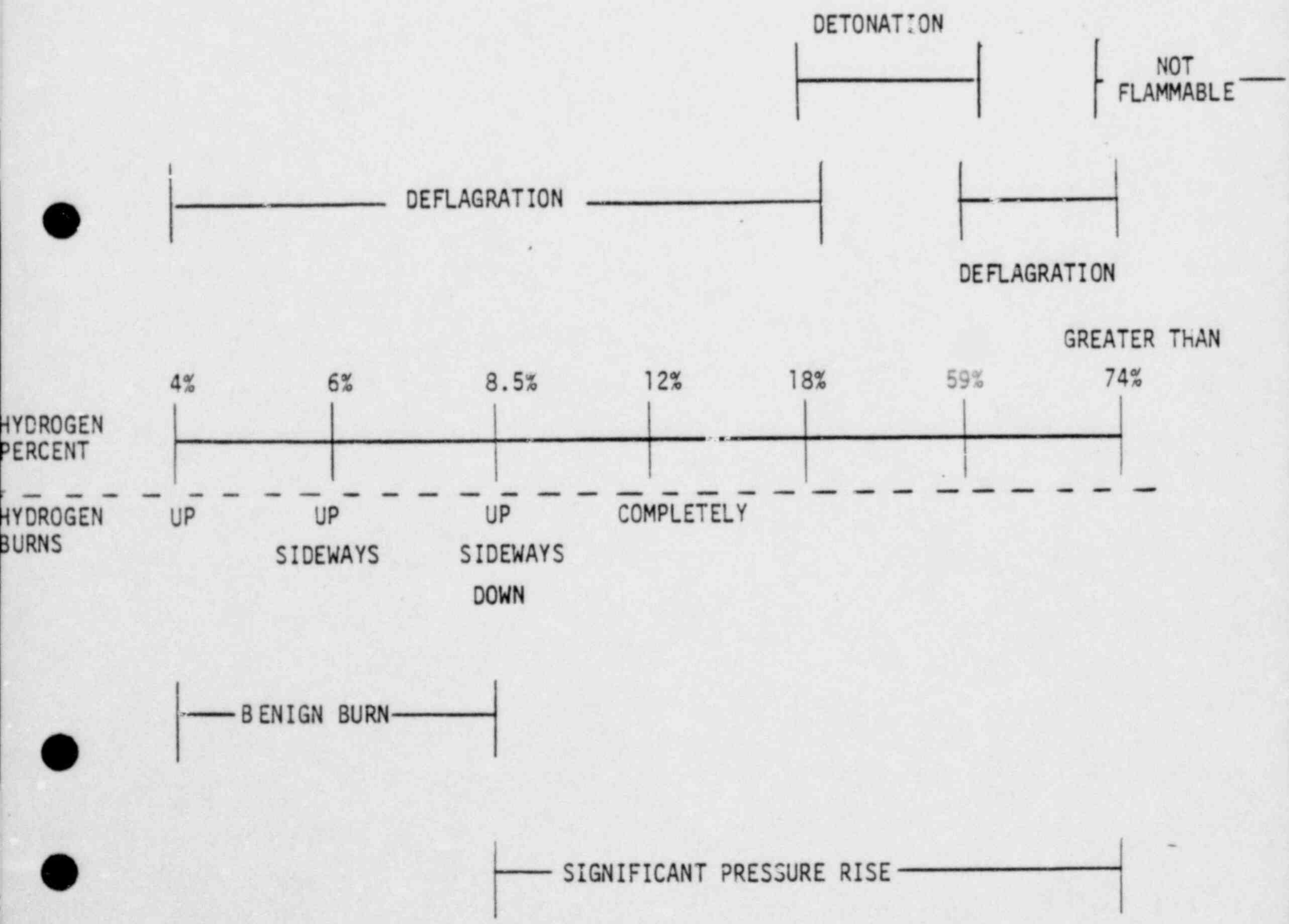
SINCE AN ADDITIONAL [redacted] SEQUENCE WAS AVAILABLE IT WAS INCLUDED

- TMLB'

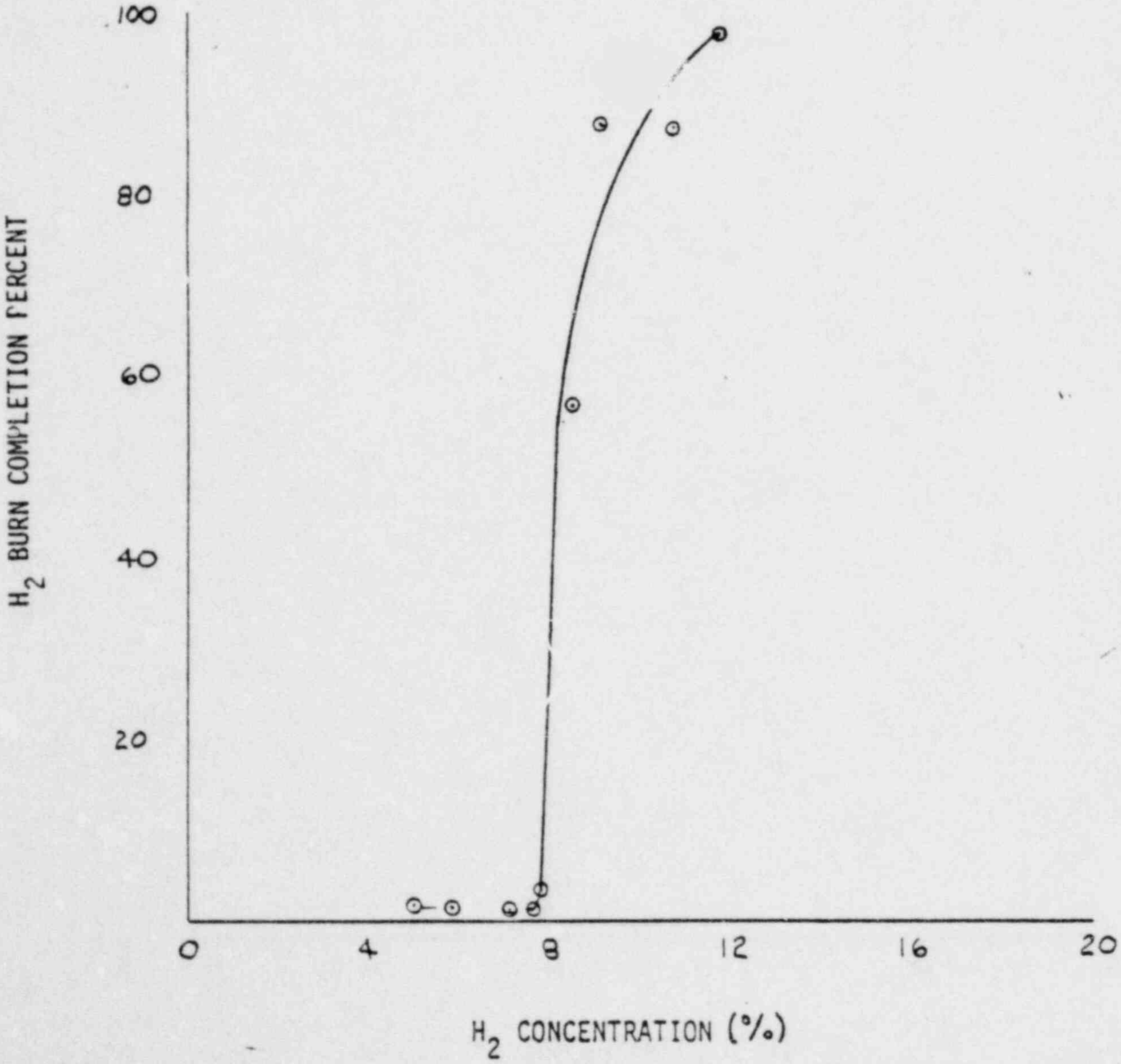
1) LOSS OF ALL AC POWER
   A) ACTIVE HEAT REMOVAL SYSTEMS ARE ASSUMED NOT TO OPERATE
   B) CONTAINMENT SAFEGUARDS WHICH ARE RUN FROM AC POWER ARE ASSUMED NOT TO OPERATE. THOSE WHICH ARE DIRECT DESEL DRIVEN ARE ASSUMED TO OPERATE

DRY AIR

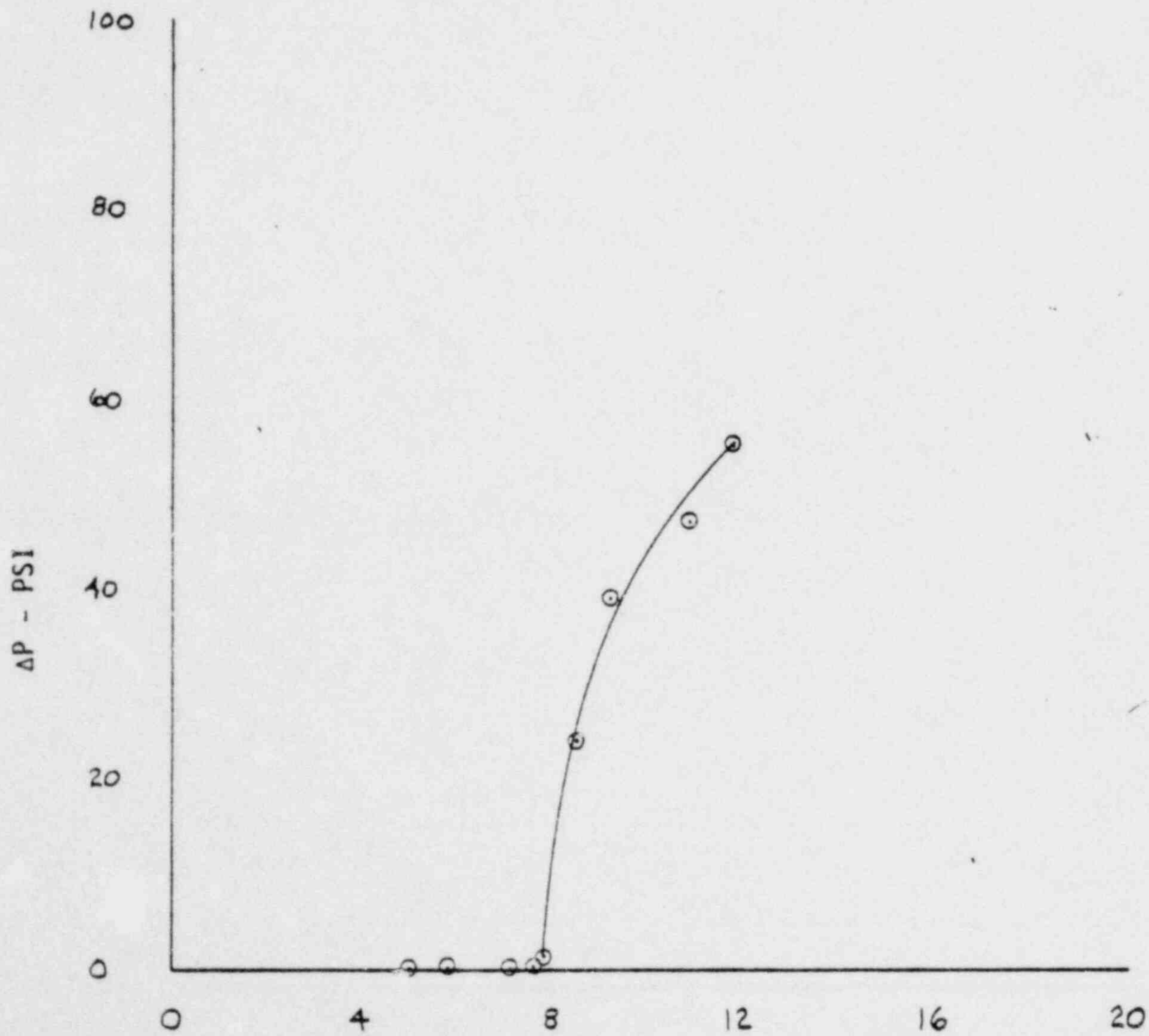
HYDROGEN BURN CHARACTERISTICS



PERCENT H_2 BURN REACTION VS H_2 CONCENTRATION



H_2 BURN PRESSURE RISE VS H_2 CONCENTRATION



H_2 PERCENT OF TOTAL CONTAINMENT

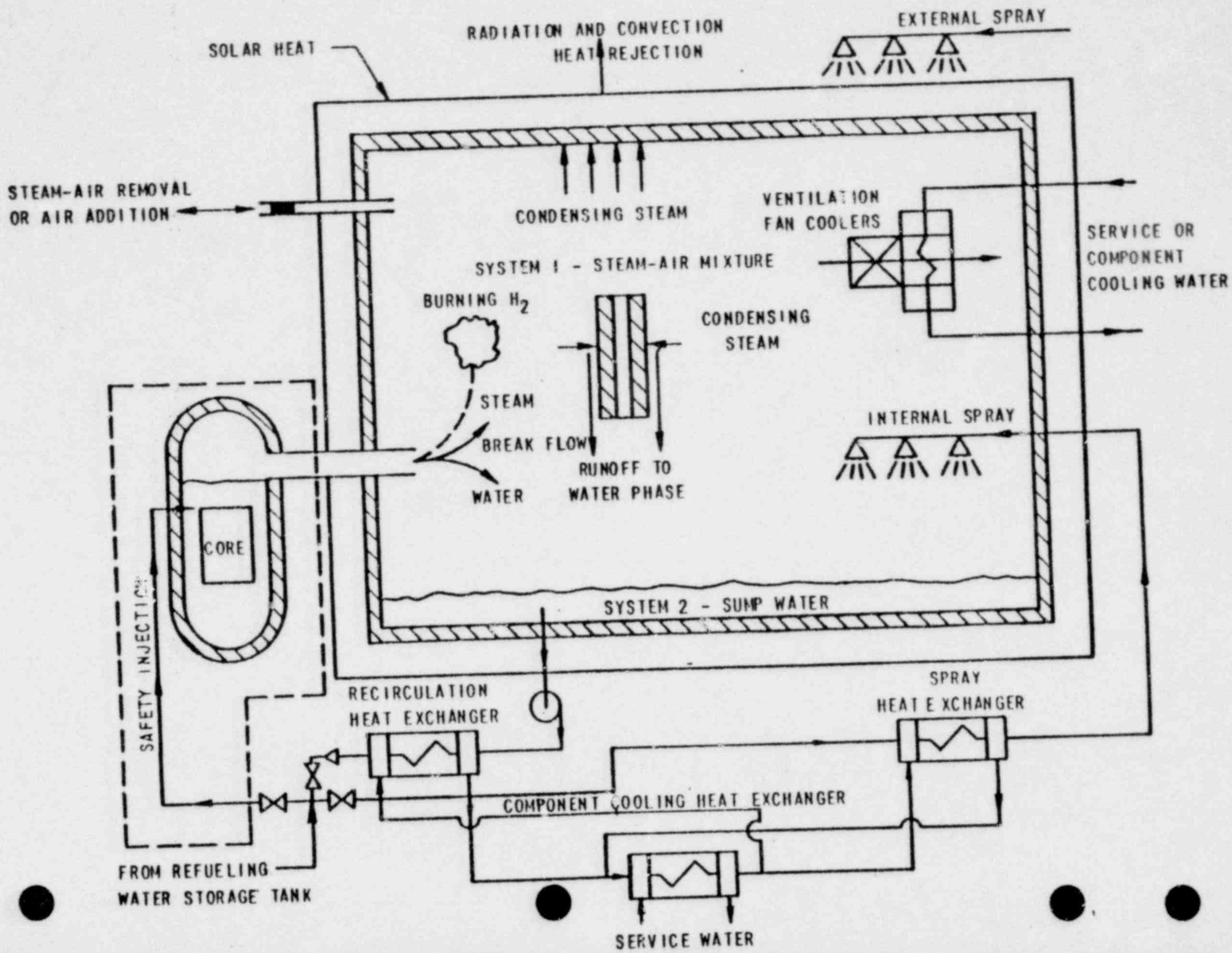
HYDROGEN MODEL CRITERIA
(SIGNIFICANT PRESSURE INCREASE)

- THE RATE OF CHEMICAL REACTION HAS AN EXPONENTIAL DEPENDENCY ON TEMPERATURE. THEREFORE BELOW SOME CRITICAL TEMPERATURE A FLAME FRONT WILL NOT PROCEED.

- THIS CRITICAL TEMPERATURE FOR THE BULK COMBUSTION LIMIT (SIGNIFICANT PRESSURE INCREASE) OF HYDROGEN-AIR AT 8.5% H_2 IS CALCULATED TO BE 710^{o}C. THIS TEMPERATURE IS DEFINED AS THE TEMPERATURE CRITERIA (T_{CRIT}).

- BY COMPARING A CALCULATED FLAME FRONT TEMPERATURE WHICH INCLUDES THE EFFECT OF DILUENTS TO THE T_{CRIT}, IT CAN BE ESTABLISHED WHETHER OR NOT A MIXTURE IS COMBUSTIBLE.

W COCO CONTAINMENT MODEL



PLANTS ANALYZED

- INDIAN POINT (INT)
- ZION (CWE)

SCENARIO'S CONSIDERED

- AD
- S_2D
- TMLB'

MASS AND ENERGY RELEASES TAKEN FROM SANDIA MARCH CALCULATIONS FOR A TYPICAL W 4 LOOP 17 X 17 PLANT BUT ASSUMING A COOLABLE DEBRIS BED

CONTAINMINET ANALYSIS

- GEOMETRIC AND FLUID SYSTEMS DATA PROVIDED BY UTILITY
- CALCULATIONS PERFORMED UTILIZED MODIFIED VERSION OF WESTINGHOUSE CONTAINMENT MODEL (COCO). BASICALLY THE SAME MODEL AS USED IN LICENSING ANALYSIS.

HYDROGEN MODEL ASSUMPTIOUS IN PRESENT ANALYSIS

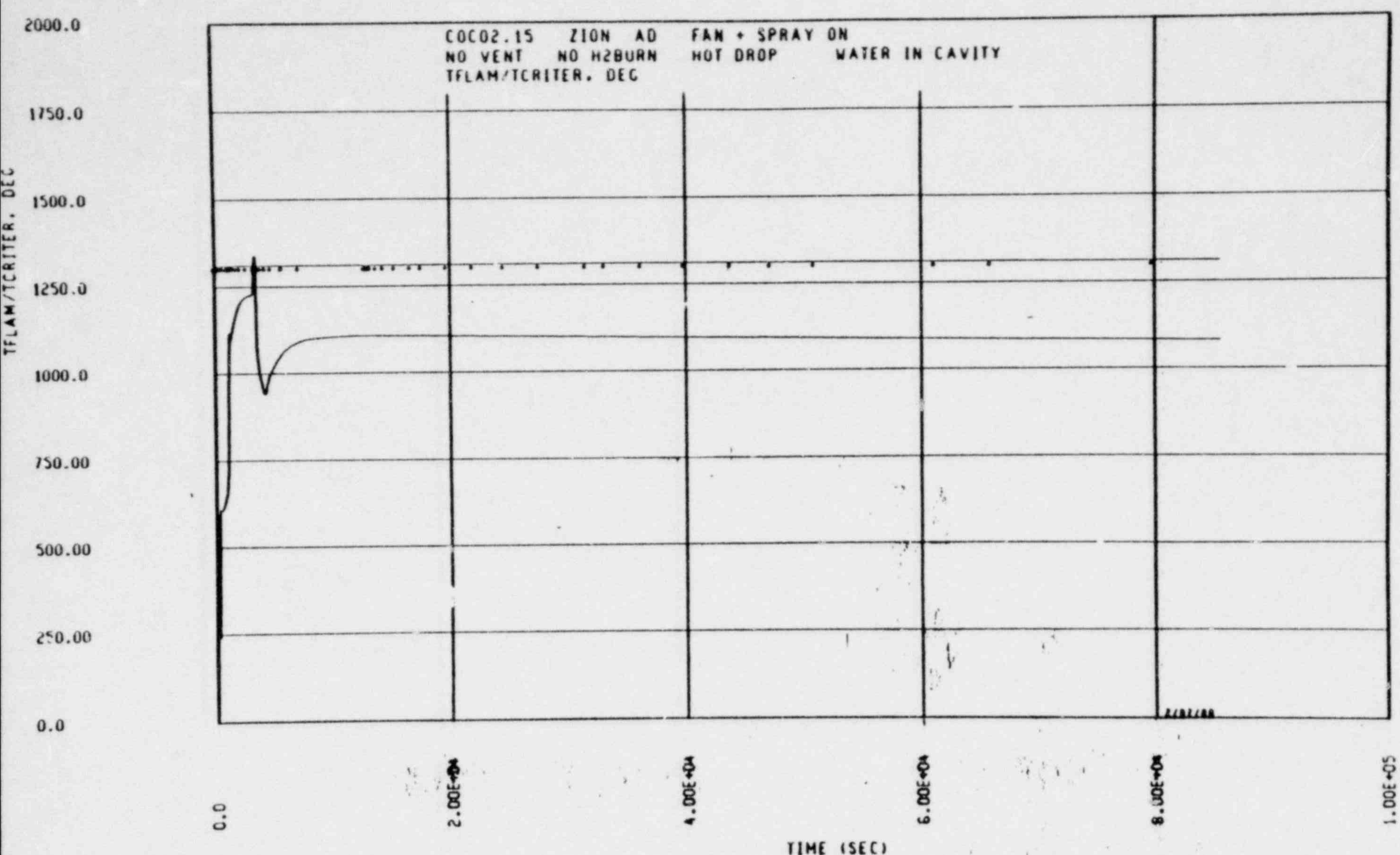
- % ZIRC/WATER REACTION AS GIVEN BY MARCH
- 100% BURN OF HYDROGEN
- UTILIZATION OF HYDROGEN GENERATION CONSISTENT WITH A ZIRCONIUM MASS FOR 17 X 17 PLANT
- BURN OF HYDROGEN OVER A 20 SEC TIME PERIOD
- STAINLESS STEEL/WATER REACTION NOT ACCOUNTED FOR

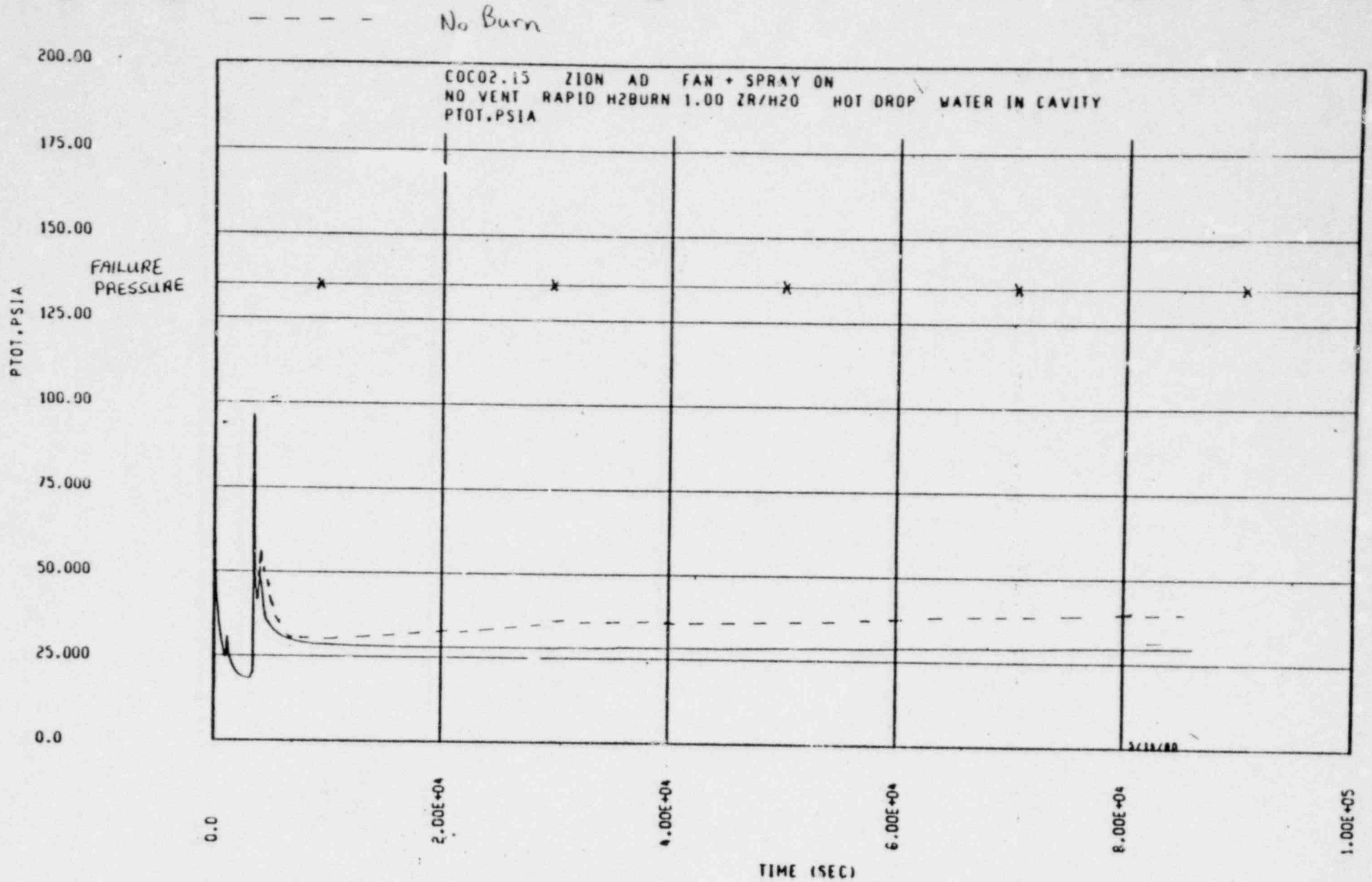
COCO HYDROGEN BURN ANALYSIS

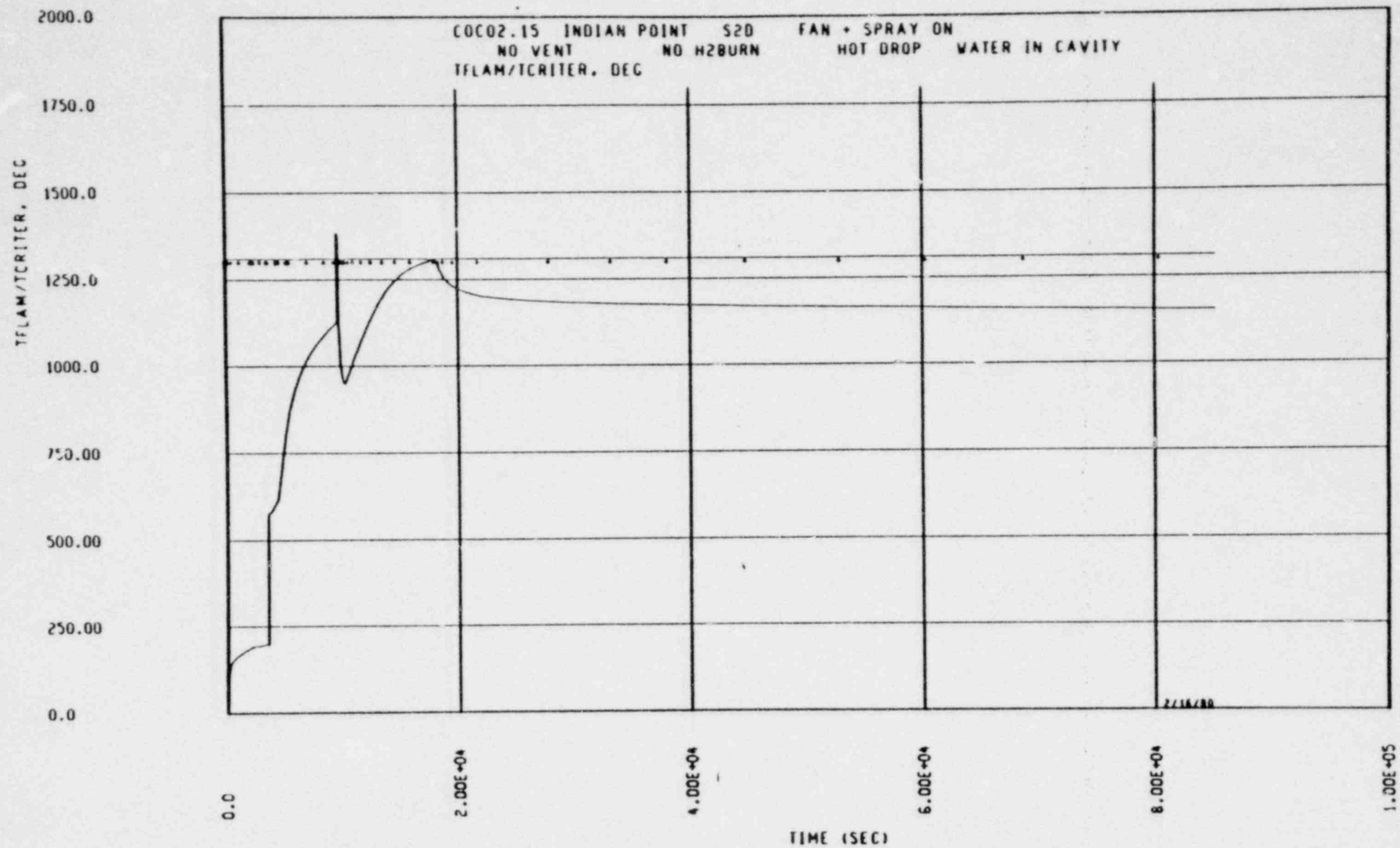
TWO STEP ANALYSIS

1. FOR A GIVEN SCENARIO A CALCULATION IS MADE WHICH ASSUMES NO HYDROGEN BURN
   - FLAME TEMPERATURES ARE CALCULATED DURING THE TRANSIENT
   - IF THE CONTAINMENT ATMOSPHERE IS CALCULATED TO BE COMBUSTIBLE THE RESULTANT PRESSURE FOR AN ADIABATIC 100% HYDROGEN IS CALCULATED.

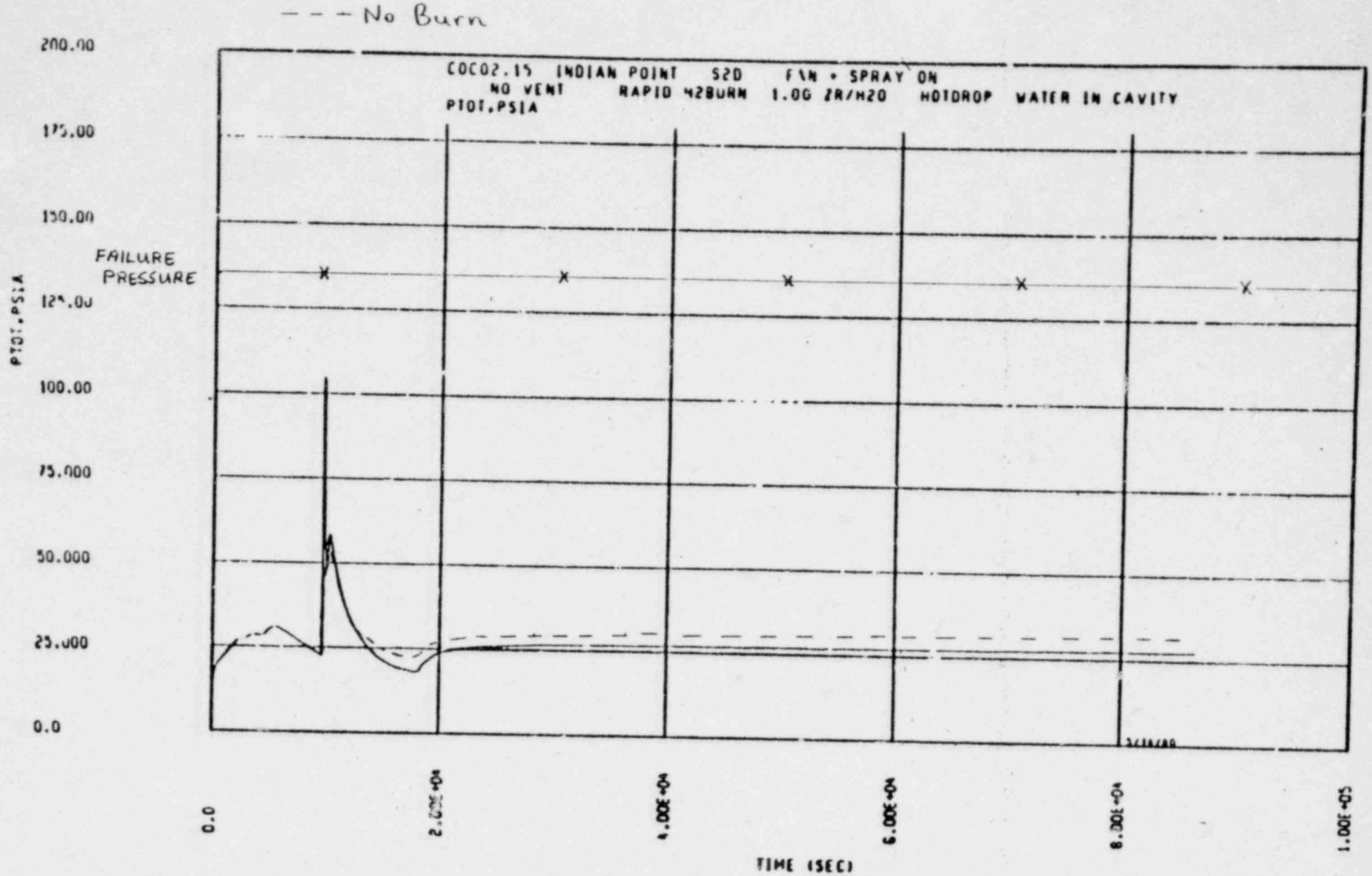
2. IF THE CONTAINMENT CONDITIONS FOR A GIVEN SCENARIO ARE CALCULATED TO BE COMBUSTIBLE THEN A SECOND CALCULATION IS PERFORMED BURNING A GIVEN AMOUNT OF HYDROGEN. THE IGNITION IS ASSUMED TO OCCUR AT THE TIME CORRESPONDING TO THE MAXIMUM ADIABATIC BURN PRESSURE.


COCO2.15 ZION AD FAN + SPRAY ON
NO VENT NO H2BURN HOT DROP WATER IN CAVITY
TFLAM/TCRITER, DEG
TFLAM/TCRITER, DEG
2000.0
1750.0
1500.0
1250.0
1000.0
750.00
500.00
250.00
0.0
0.0
2.00E+04
4.00E+04
6.00E+04
8.00E+04
1.00E+05
TIME (SEC)

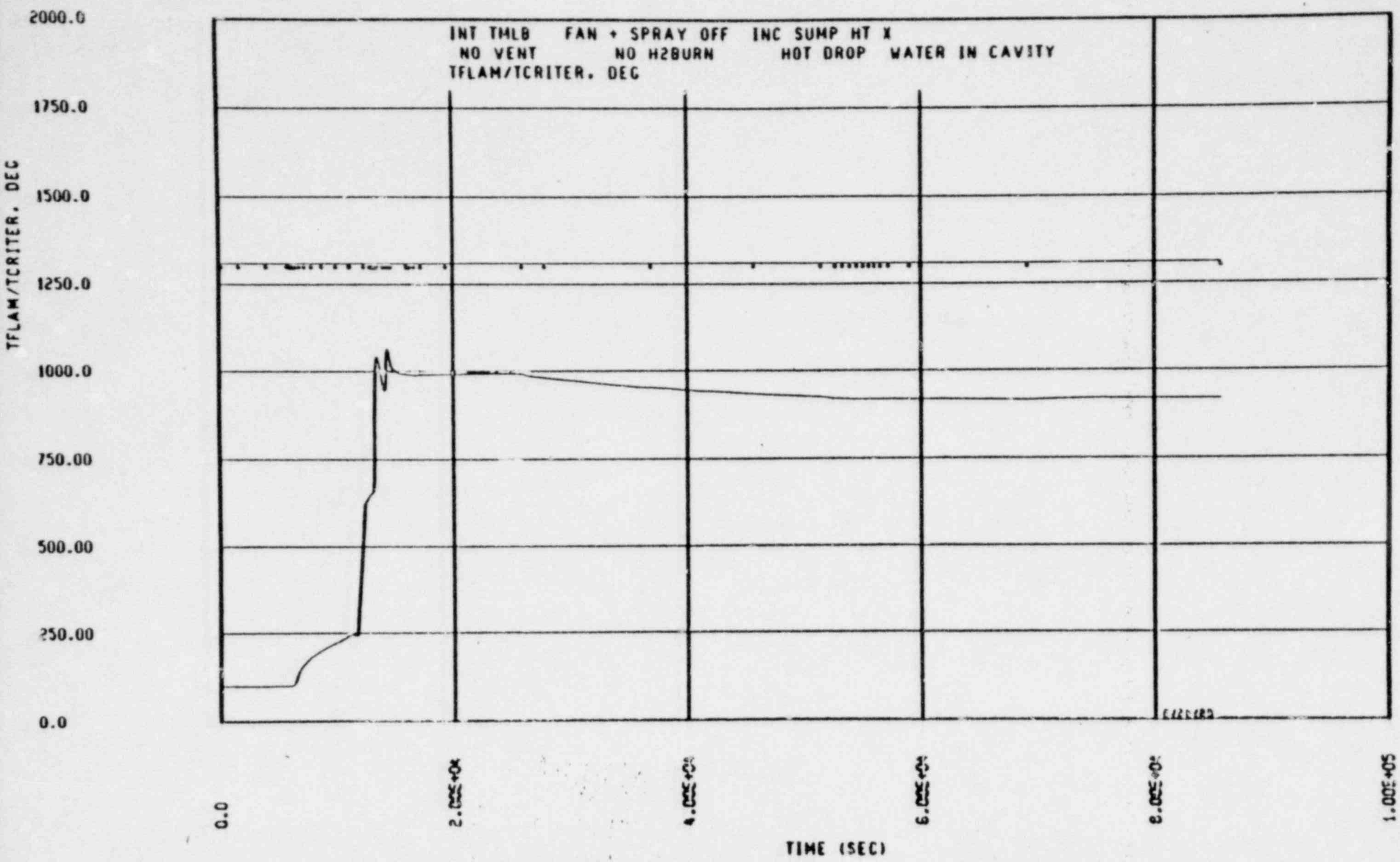

- - - - No Burn
COCO2.15 ZION AD FAN + SPRAY ON
NO VENT RAPID H2BURN 1.00 ZR/H2O HOT DROP WATER IN CAVITY
PTOT.PSIA
FAILURE PRESSURE
PTOT.PSIA
200.00
175.00
150.00
125.00
100.00
75.000
50.000
25.000
0.0
0.0
2.00E+04
4.00E+04
6.00E+04
8.00E+04
1.00E+05
TIME (SEC)


COCO2.15 INDIAN POINT S2D FAN + SPRAY ON
NO VENT NO H2BURN HOT DROP WATER IN CAVITY
TFLAM/TCRITER, DEG
TFLAM/TCRITER, DEG
2000.0
1750.0
1500.0
1250.0
1000.0
750.00
500.00
250.00
0.0
0.0
2.00E+04
4.00E+04
6.00E+04
8.00E+04
1.00E+05
TIME (SEC)

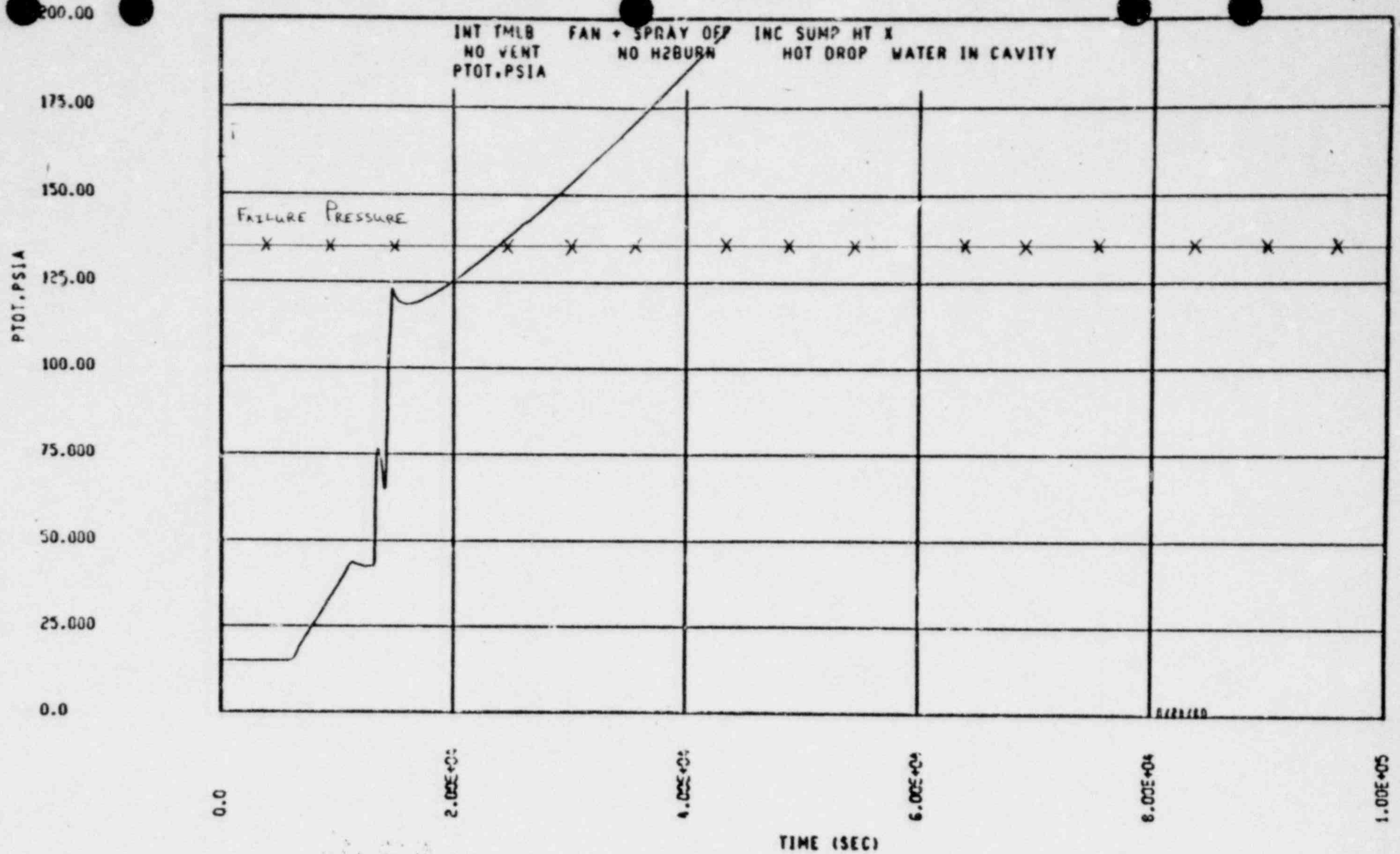
245/165



--- No Burn
COCO2.15 INDIAN POINT S2D FAN + SPRAY ON
NO VENT RAPID H2BURN 1.0G ZR/H2O HOTDROP WATER IN CAVITY
PTOT,PSIA
FAILURE PRESSURE
PTOT,PSIA
200.00
175.00
150.00
125.00
100.00
75.000
50.000
25.000
0.0
0.0
2.00E+04
4.00E+04
6.00E+04
8.00E+04
1.00E+05
TIME (SEC)



INT TMLB FAN + SPRAY OFF INC SUMP HT X
NO VENT NO H2BURN HOT DROP WATER IN CAVITY
TFLAM/TCRITER, DEG
2000.0
1750.0
1500.0
1250.0
1000.0
750.00
500.00
250.00
0.0
TFLAM/TCRITER, DEG
0.0
2.00E+04
4.00E+04
6.00E+04
8.00E+04
1.00E+05
TIME (SEC)



INT TMLB FAN + SPRAY OFF INC SUMP HT X
NO VENT NO H2BURN HOT DROP WATER IN CAVITY
PTOT,PSIA
FAILURE PRESSURE
PTOT,PSIA
200.00
175.00
150.00
125.00
100.00
75.000
50.000
25.000
0.0
0.0
2.00E+04
4.00E+04
6.00E+04
8.00E+04
1.00E+05
TIME (SEC)

PRESENT ANALYSES

- BURN ALL THE HYDROGEN PRESENT IN THE CONTAINMENT ATMOSPHERE IN 20 SEC
  - NEW CALCULATIONS INDICATE THAT LESS THAN A HUNDRED PRESENT BURN WOULD PROBABLY OCCUR, AND THIS BURN WOULD OCCUR OVER APPROXIMATELY 30 SECONDS.

- THE PRESENT CALCULATIONS USE MASS AND ENERGY RELEASES TAKEN FROM CALCULATIONS PERFORMED FOR A TYPICAL 4 LOOP PLANT (17 X 17 FUEL ASSEMBLY). INT AND CWE HAVE 15 X 15 FUEL ASSEMBLIES AND THUS HAVE LESS ZIRCONIUM IN THE CORE

MARCH COMPUTER CODE

- DEVELOPED FOR WASH 1400 PROBABILISTIC ANALYSIS NOT A PWR DESIGN ANALYSES
- CODE PRESERVES OVERALL MASS AND ENERGY BALANCES, BUT RATES AT ANY GIVEN TIME ARE NOT WELL SUBSTANTIATED

CONTINUING EFFORTS

- MARCH CODE
  - PRESENTLY OPERATIONAL AT W
  - MARCH MODEL SENSITIVITY STUDIES ARE BEING PERFORMED
    - MELT MODEL
    - PARTICLE SIZE
    - FUEL/COOLANT INTERACTION
  - PHENOMENOLOGICAL MODEL ASSESSMENT
  - COMPARING MARCH CONTAINMENT MODELS TO W CONTAINMENT CODE (COCO)
  - PERFORM PLANT SPECIFIC CALCULATIONS FOR RISK DOMINATED SEQUENCES
  - INTEGRATE R. HENRY'S DEBRIS BED COOLABILITY WORK
- CONTINUE TO PARTICIPATE WITH NRC/NATIONAL LABS TO
  - IMPROVE ANALYTICAL AND EXPERIMENTAL DATA BASE
  - INCORPORATE IMPROVED MARCH MODELS AND USE TO DEVELOP SPECIFICATIONS FOR FUNCTIONAL REQUIREMENTS

MARCH MODEL IMPROVEMENTS

- QUANTIFY EFFECTS OF ONE NODE MODEL OR ADD ADDITIONAL NODES
  - LOOP FLOWS
  - SYSTEM EFFECTS
  - BREAK FLOW
- IMPROVE MODELING OF FUEL ROD/CLAD INTERACTION
  - PRESENT MODEL LEADS TO EARLY FUEL MELTING
- HYDROGEN RELEASE CALCULATIONS
  - INSIDE REACTION BETWEEN ZIRC CLAD AND FUEL IS NOT ACCOUNTED FOR
  - ALL MOLTEN ZIRC IS ASSUMED TO REACT COMPLETELY IN EITHER THE WATER FILLED LOWER PLENUM OR THE WATER FILLED LOWER REACTOR CAVITY. TEST DATA ON MOLTEN ZIRC WIRES DROPPED INTO WATER INDICATES ONLY PARTIAL REACTION.
  - STAINLESS STEEL/WATER REACTION IS NOT ACCOUNTED FOR
- CORE SLUMP/MELT MODEL
  - PRESENT MODELS ARE ONLY SCOPING, PHYSICS ARE NOT MODELLED
  - TOTAL (NOT SEQUENTIAL) FAILURE OF ALL LOWER CORE SUPPORTS ARE ASSUMED
  - TOTAL LOWER HEAD FAILURE IS ASSUMED
- FUEL DEBRIS/COOLANT INTERACTION
  - HEAT TRANSFER LIMITING PROCESSES ARE NOT MODELLED (I.E. DEBRIS BED CHF LIMIT IS NOT CALCULTED)

- LOWER REACTION CAVITY AND LOWER PLENUM COOLANT DYNAMIC BEHAVIOR IS NOT MODELLED

- PARTICLE SIZE UTILIZED ARE USER INPUT, AND THE PHYSICS OF THE PARTICULARIZATION IS NOT MODELLED

- MARCH HYDROGEN BURN MODEL

  - PERIOD OVER WHICH H2 BURNS IS INPUT

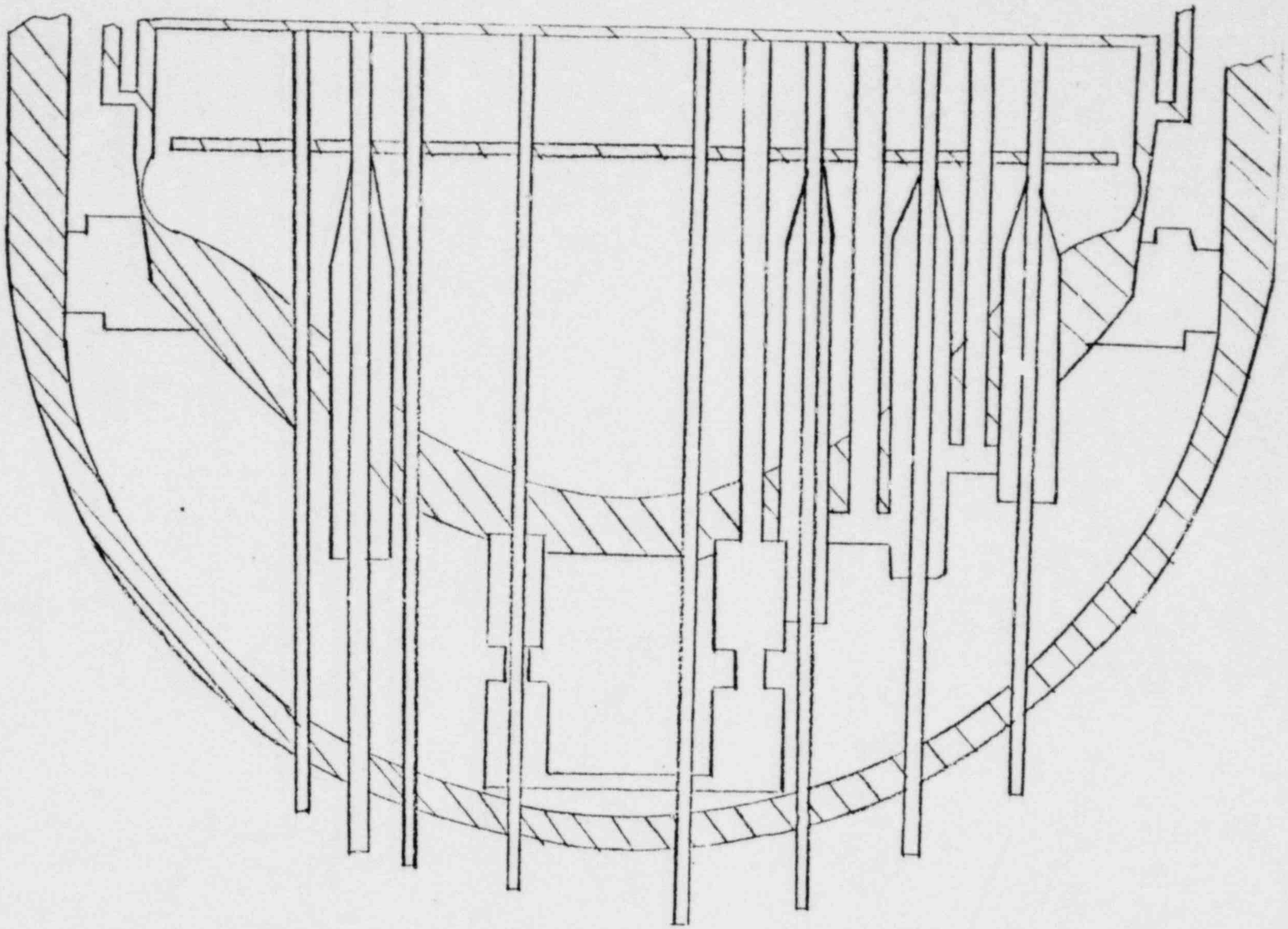
(TYPICALLY 6 SECONDS) DATA INDICATES THAT H2 BURNS SHOULD TAKE 20 TO 60 SECONDS.

  - BURN COMPLETION IS INPUT NOT CALCULATED (USUALLY 100%)

  - EFFECT OF SPRAYS ON FLAMMABILITY IS NOT ACCOUNTED FOR

  - DATA BASE UTILIZED IS LIMITED

REACTOR VESSEL LOWER ASSEMBLY DRAWING



CONCLUSIONS

- CONTAINMENT CALCULATIONS WERE PERFORMED FOR THE AD, S_2D, AND TMLB' SCENARIO'S WHICH LEAD TO CORE MELT.
  - MASS AND ENERGY RELEASES WERE TAKEN FROM TYPICAL MARCH CALCULATIONS AND MODIFIED TO INCLUDE THE EFFECT OF A PERMANENTLY COOLABLE DEBRIS BED
- THE CAPABILITY TO CALCULATE THE BULK COMBUSTIBILITY OF A HYDROGEN/AIR/STEAM/CONTAINMENT ATMOSPHERE WAS DEVELOPED AND IMPLEMENTED INTO OUR CALCULATIONS
- FOR THE AD AND S_2D SEQUENCES WHICH WERE REPRESENTATIVE RISK CONTRIBUTORS ONE IMPORTANT PARAMETER WAS THE ASSUMED BEHAVIOR OF HYDROGEN IN CONTAINMENT
  - IF THE HYDROGEN IS ASSUMED NOT TO BURN OR TO BURN CONTINUOUSLY CONTAINMENT RAILURE WAS NOT CALCULATED TO OCCUR WITH THE PRESENT CONTAINMENT
  - IF THE HYDROGEN IS ASSUMED TO ACCUMULATE TO A COMBUSTIBLE MIXTURE AND THEN BURNED, A RAPID PRESSURE SPIKE WOULD OCCUR. THIS PRESSURE SPIKE IMPOSES SIGNIFICANT PRESSURE LOADINGS ON THE CONTAINMENT STRUCTURES, BUT THE CONTAINMENT IS NOT CALCULATED TO FAIL

TOLAND

EVALUATION OF CAPABILITY OF INDIAN POINT CONTAINMENT VESSELS - UNITS 2& 3

PURPOSE OF EVALUATION - TO MAKE A CONSERVATIVE ASSESSMENT OF THE CAPABILITY OF THE INDIAN POINT CONTAINMENT VESSELS - THE CAPABILITY WAS EVALUATED BASED ON CONDITIONS REPRESENTATIVE OF A CLASS 9 EVENT

- THE EVALUATION WAS PERFORMED ON A REALISTIC BASIS
- ACTUAL MATERIAL PROPERTIES WERE USED
- THE STRENGTH OF THE LINER WAS INCLUDED IN THE EVALUATION

DEFINITION OF CAPABILITY - THE MAXIMUM COMBINATION OF TEMPERATURE AND PRESSURE TO PRODUCE A GENERAL YIELD STATE. (ESSENTIALLY THE LIMIT OF ELASTIC RESPONSE)

THIS IS A CONFIDENT LOWER BOUND OF FUNCTIONAL CAPABILITY WITHOUT ACCOUNTING FOR ADDITIONAL AVAILABLE STRENGTH DUE TO STRAIN HARDENING - THE ACTUAL CAPABILITY IS HIGHER.

CONCLUSIONS - INDIAN POINT UNIT 2 AND 3 CONTAINMENTS CAN WITHSTAND A PRESSURE = 126 PSIG OR 2.7 TIMES THE DESIGN ACCIDENT PRESSURE.

METHOD OF EVALUATION - HAND CALCULATIONS

JUSTIFICATION

- EXPERIENCE IN DESIGN AND ANALYSIS OF CONTAINMENT VESSELS

- AGREEMENT BETWEEN HAN[illegible] CALCULATIONS AND COMPUTER SOLUTIONS FROM PREV[illegible]S ANALYSES.

REGIONS OF CONTAINMENT EVALUATED

- MEMBRANE
  DOME & CYLINDER

- DISCONTINUITY REGION AT SPRINGLINE

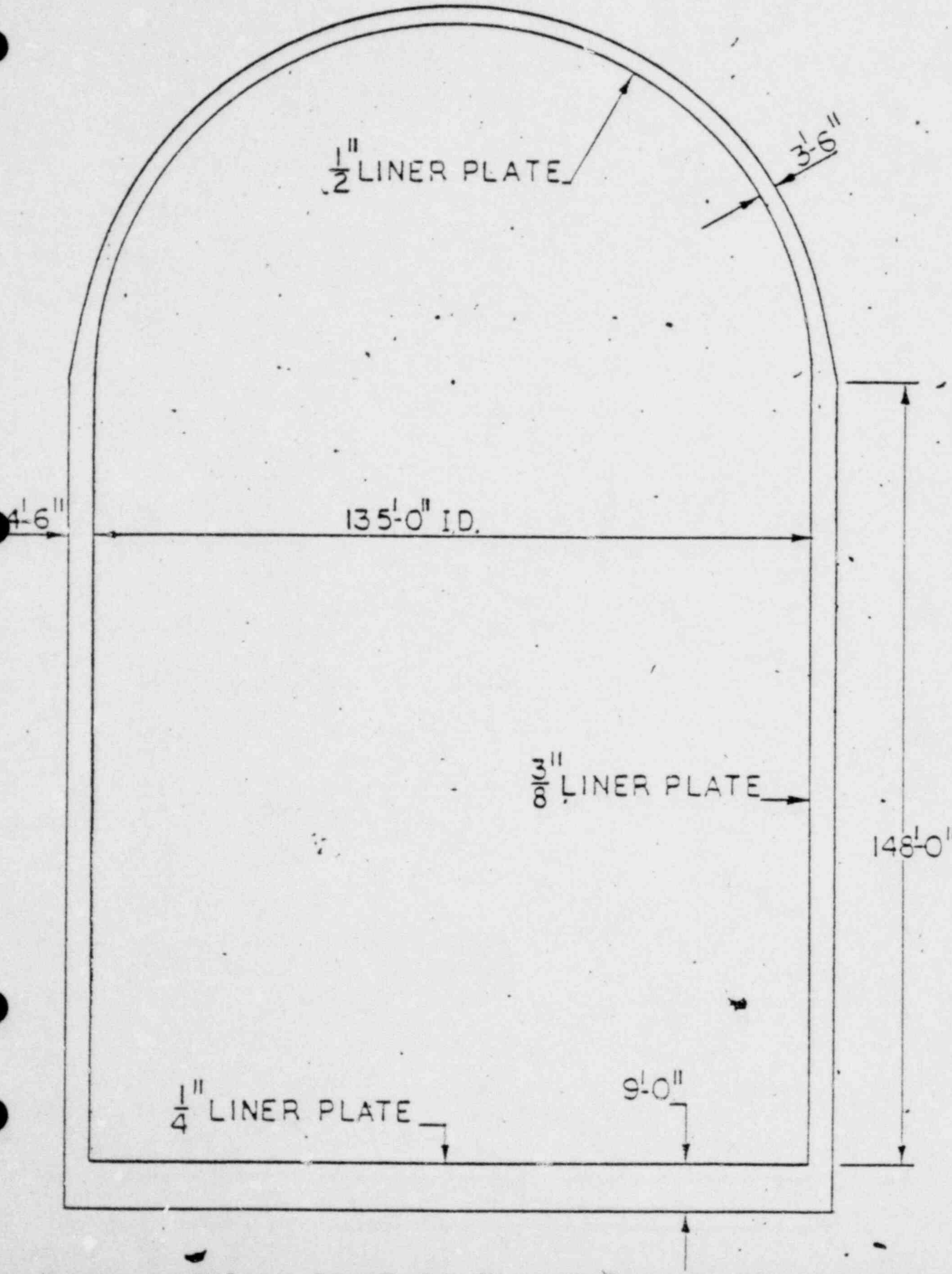
- DISCONTINUITY REGION AT BASE OF CYLINDER

- BASE MAT

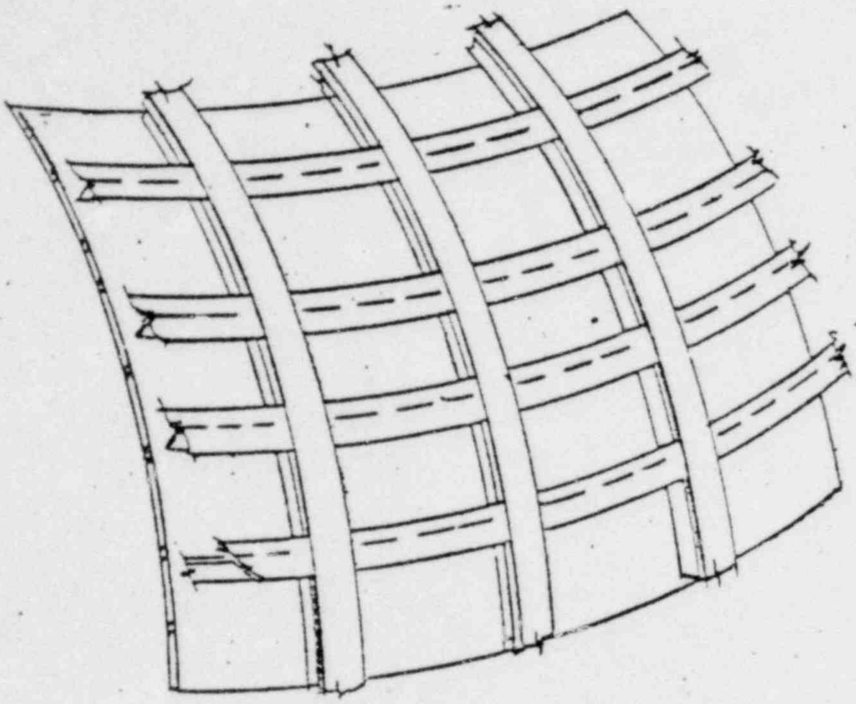
- LARGE PENETRATIONS
  EQUIPMENT HATCH
  PERSONNEL AIRLOCK

- SMALL PENETRATIONS - TYPICAL

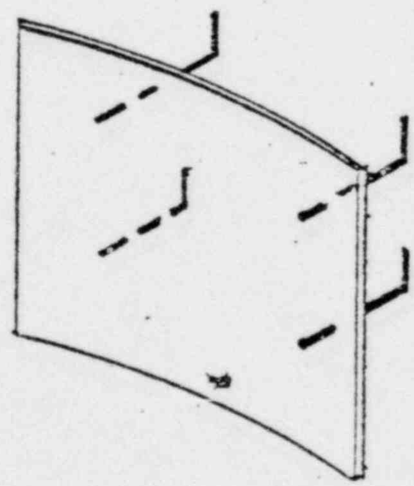
- LINER



INDIAN POINT CONTAINMENT BUILDING

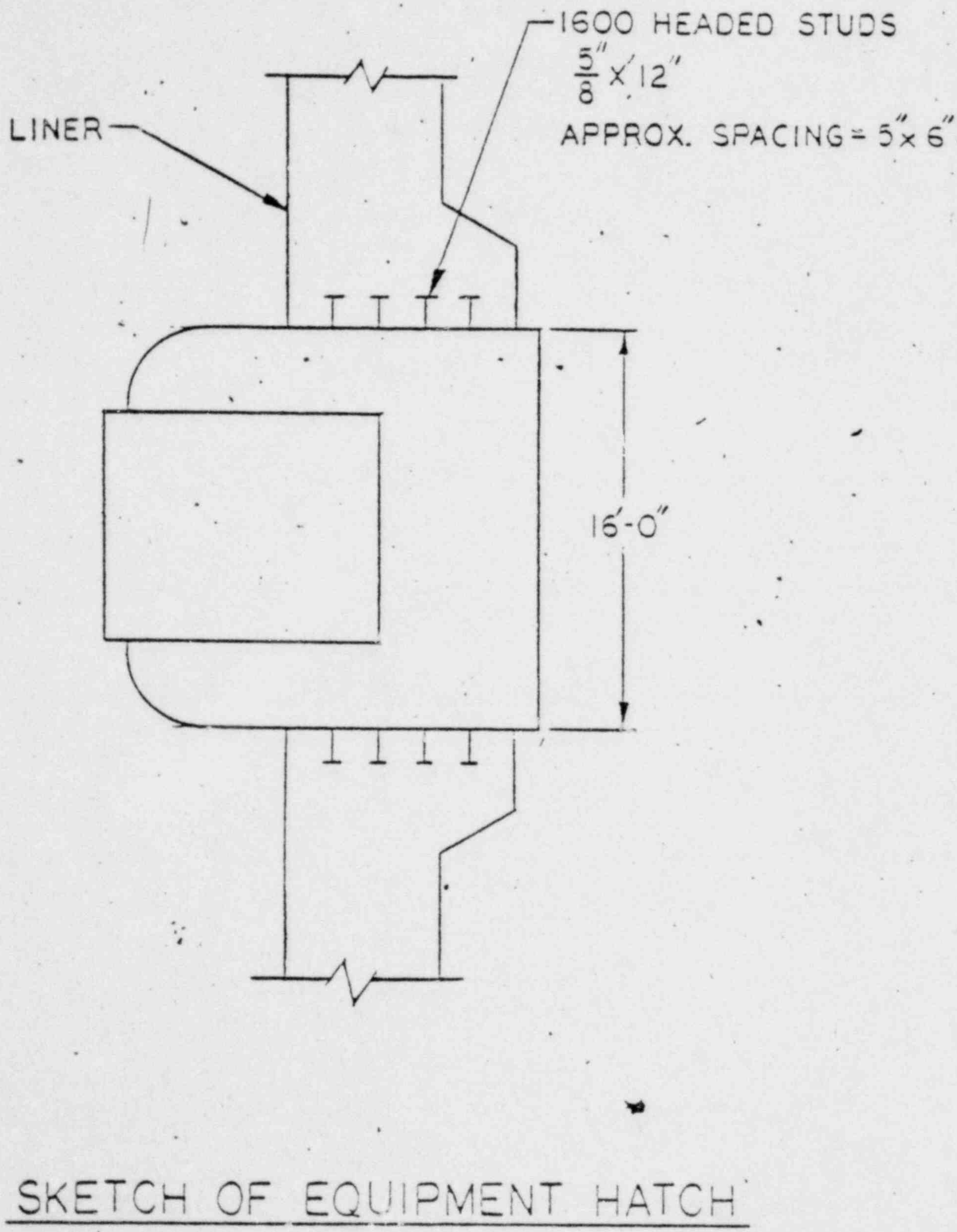


ATTACHMENT TO DOME

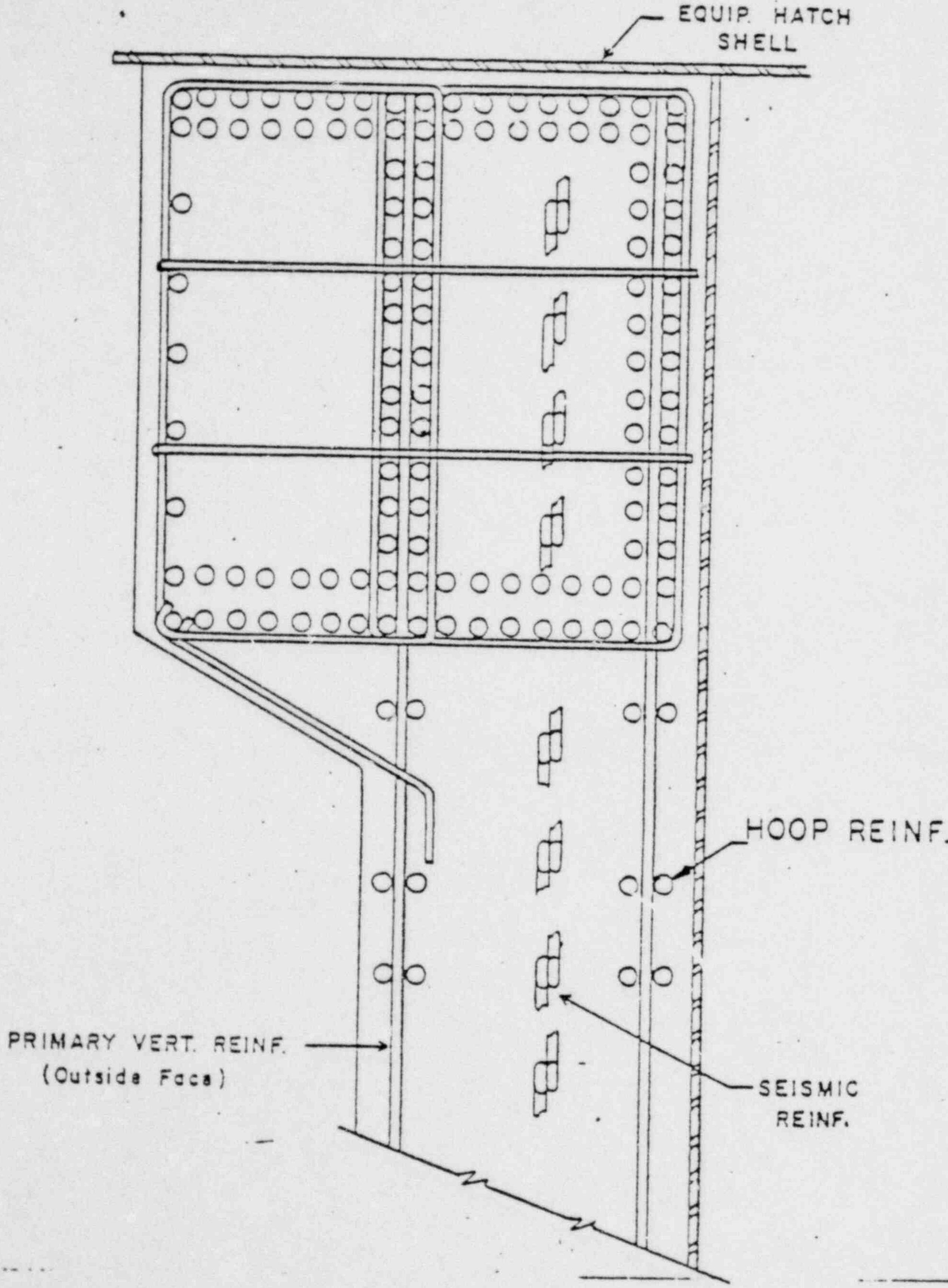


ATTACHMENT TO CYLINDER

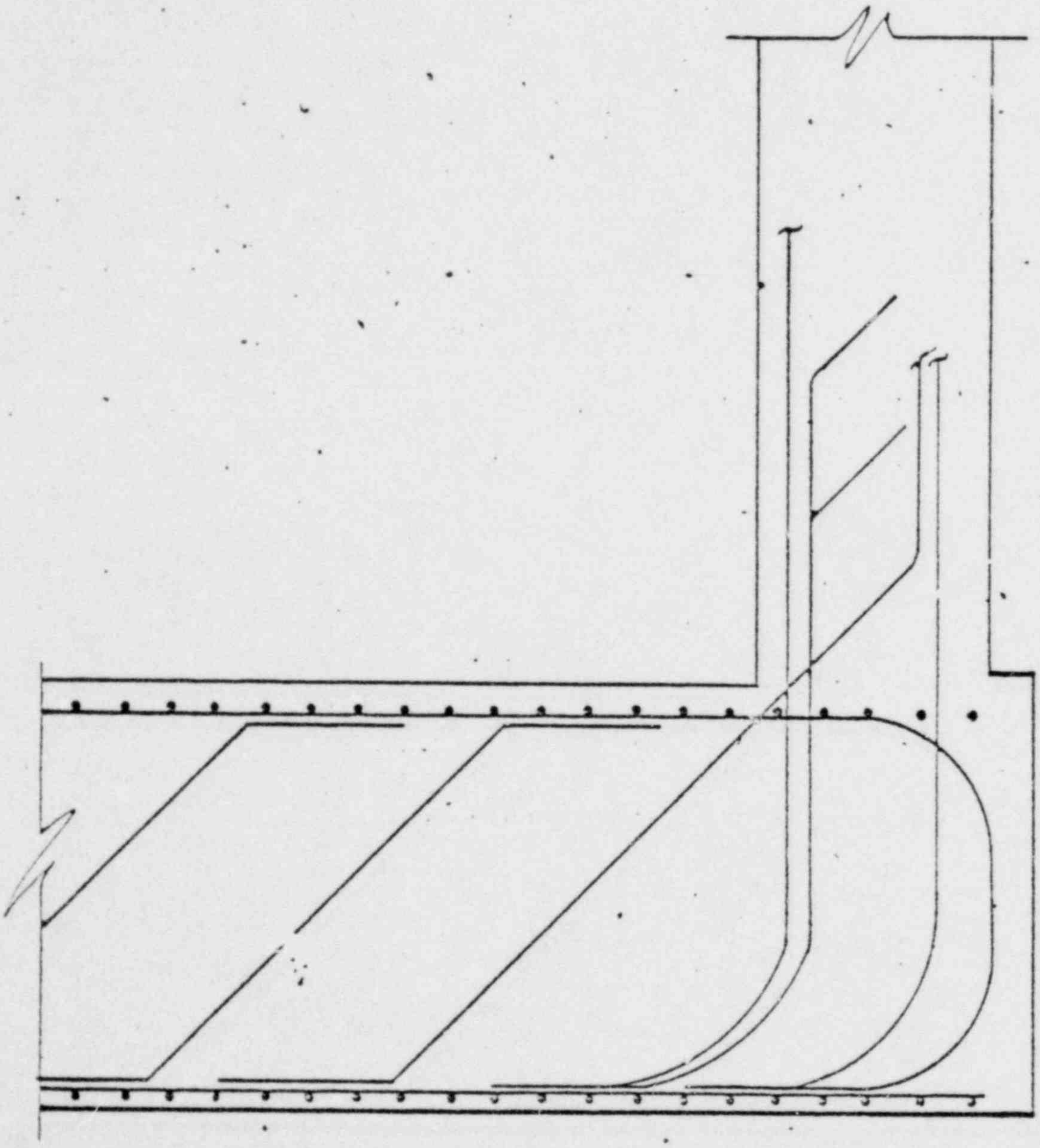
ATTACHMENT OF LINER TO CONCRETE



SKETCH OF EQUIPMENT HATCH

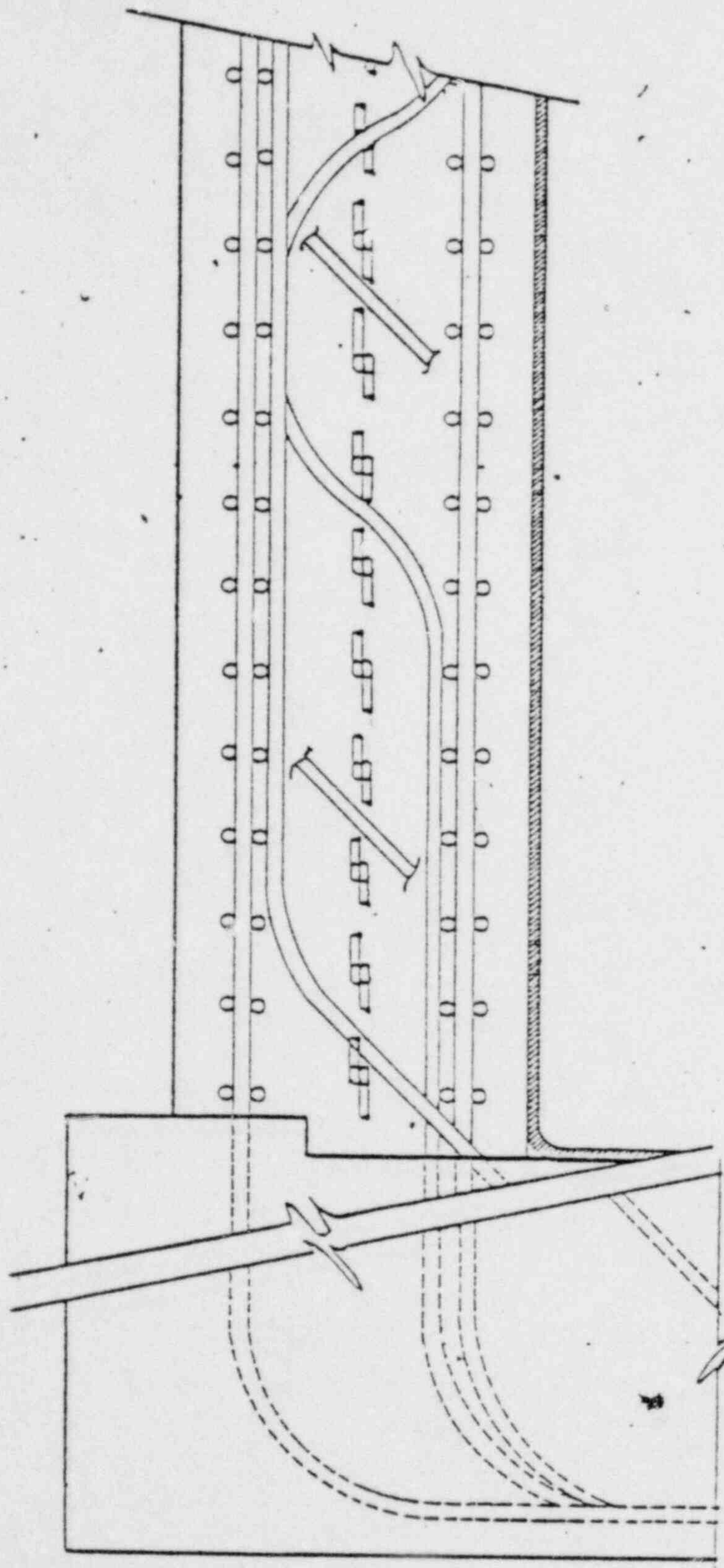


SECTION THROUGH DISCONTINUITY
REGION AROUND EQUIPMENT HATCH

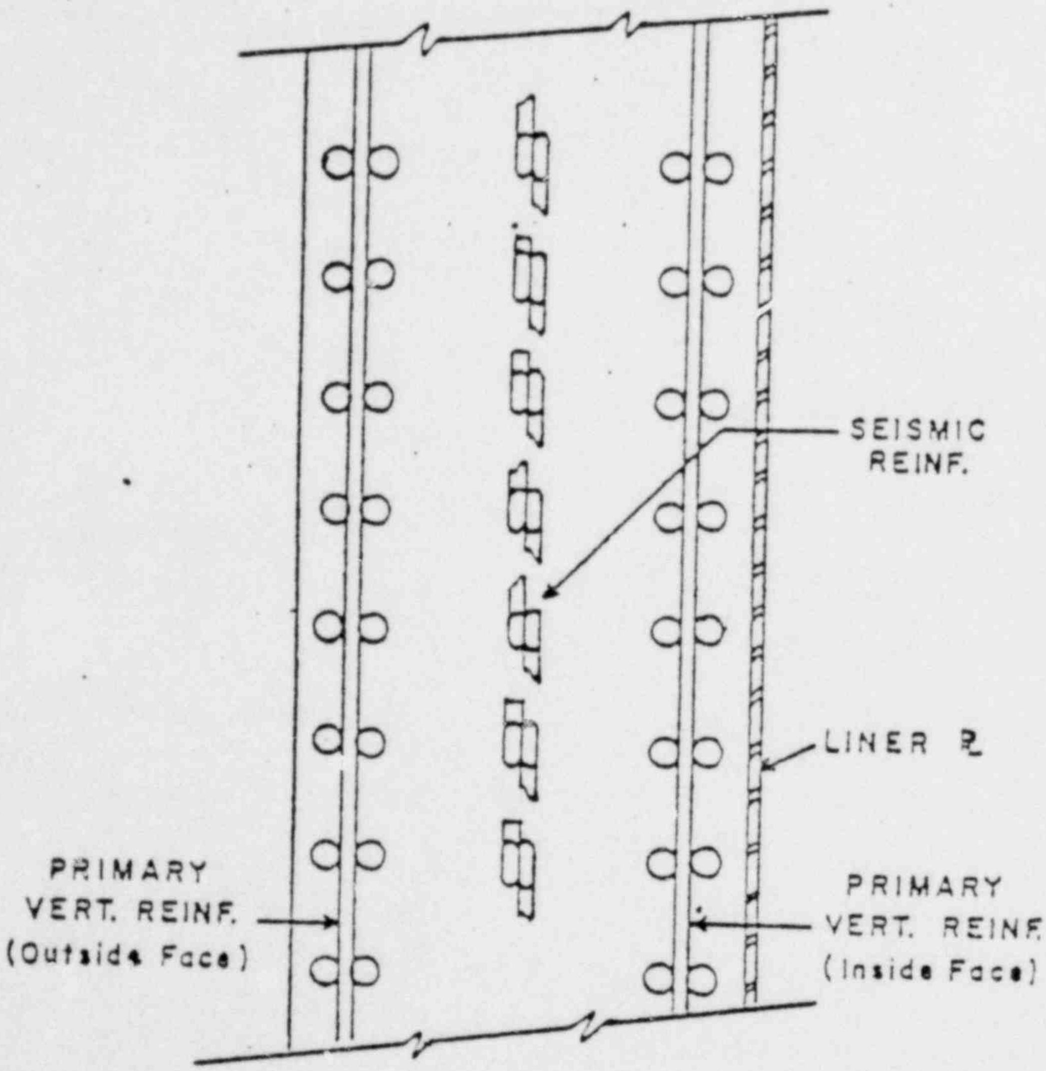


SECTION THROUGH MAT BELOW

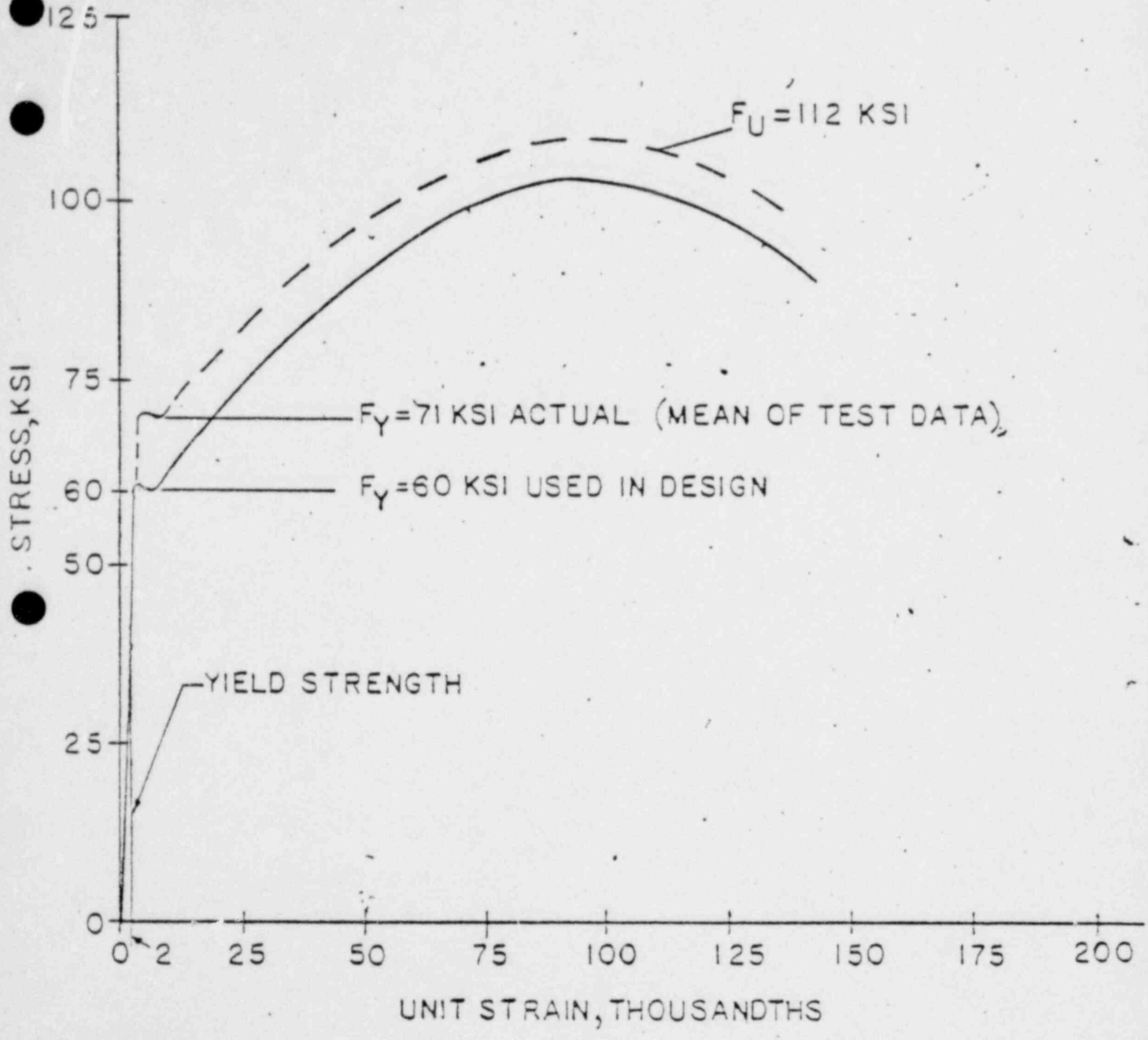
CONTAINMENT WALL



DISCONTINUITY REGION AT
BASE OF CONTAINMENT SHELL



SECTION OF CYLINDRICAL SHELL
I.P. UNIT 2



STRESS-STRAIN CURVE OF ASTM A-615-60 STEEL

INDIAN POINT CONTAINMENTS UNITS 2 & 3
CONSERVATISMS IN ORIGINAL DESIGN

- PRIMARY CONSERVATISMS APPLIED IN ORIGINAL DESIGN OF THE GOVERNING REGION (BELOW SPRINGLINE) OF THE CONTAINMENT SHELL.

  1. APPLICATION OF LOAD FACTORS (1.5)
  2. APPLICATION OF CAPACITY REDUCTION FACTORS (1.11)
  3. STRENGTH OF LINER NOT ACCOUNTED FOR (1.15)
  4. MINIMUM STRENGTH OF MATERIALS CONSIDERED (1.18)
  5. SEISMIC REBAR RESISTING LOCA LOADS (1.12)
  6. DESIGNER CONSERVATISM (1.06)

- OTHER CONSERVATISMS APPLICABLE TO REGIONS WHICH DO NOT GOVERN

  1. SEISMIC LOADS COMBINED WITH LOCA LOADS (MAXIMUM EFFECT AT BASE OF SHELL)
  2. SEISMIC LOADS CONSERVATIVELY DETERMINED - EXAMPLE SSE UNIT 2 DAMPING - 2%
  3. REDUNDANCY AT BASE PROVIDED - SHEAR FORCE AND BENDING MOMENT ARE SECONDARY AND EXIST ONLY BECAUSE OF BASE CONSTRAINT - THEY ARE NOT REQUIRED FOR EQUILIBRIUM.

P_A (DESIGN) = 47 PSIG

P (CAPABILITY) = 47 * (PRODUCT OF FACTORS ABOVE)

P (CAPABILITY) = 47 * 2.7

P (CAPABILITY) = 126 PSIG

WHERE CAPABILITY IS THE LIMIT OF ELASTIC RESPONSE

NOTE THAT THE LIMITING REGION OF THE CONTAINMENT IS ONE OF HIGH DUCTILITY LOCATED AWAY FROM DISCONTINUITIES.

DISCONTINUITY REGIONS OF THE CONTAINMENT HAVE AT LEAST THE CONSERVATISM AS THE MEMBRANE REGION. THE ORIGINAL DESIGN WAS BASED ON THE ACI 318-63 CODE WHICH MANDATES ADDITIONAL CONSERVATISM IN REGIONS OF LOW DUCTILITY. SHEAR, ANCHORAGE AND COMPRESSION WILL NOT GOVERN DESIGN.

PRIMARY CONTAINMENT ULTIMATE CAPACITY OF ZION NUCLEAR POWER PLANT

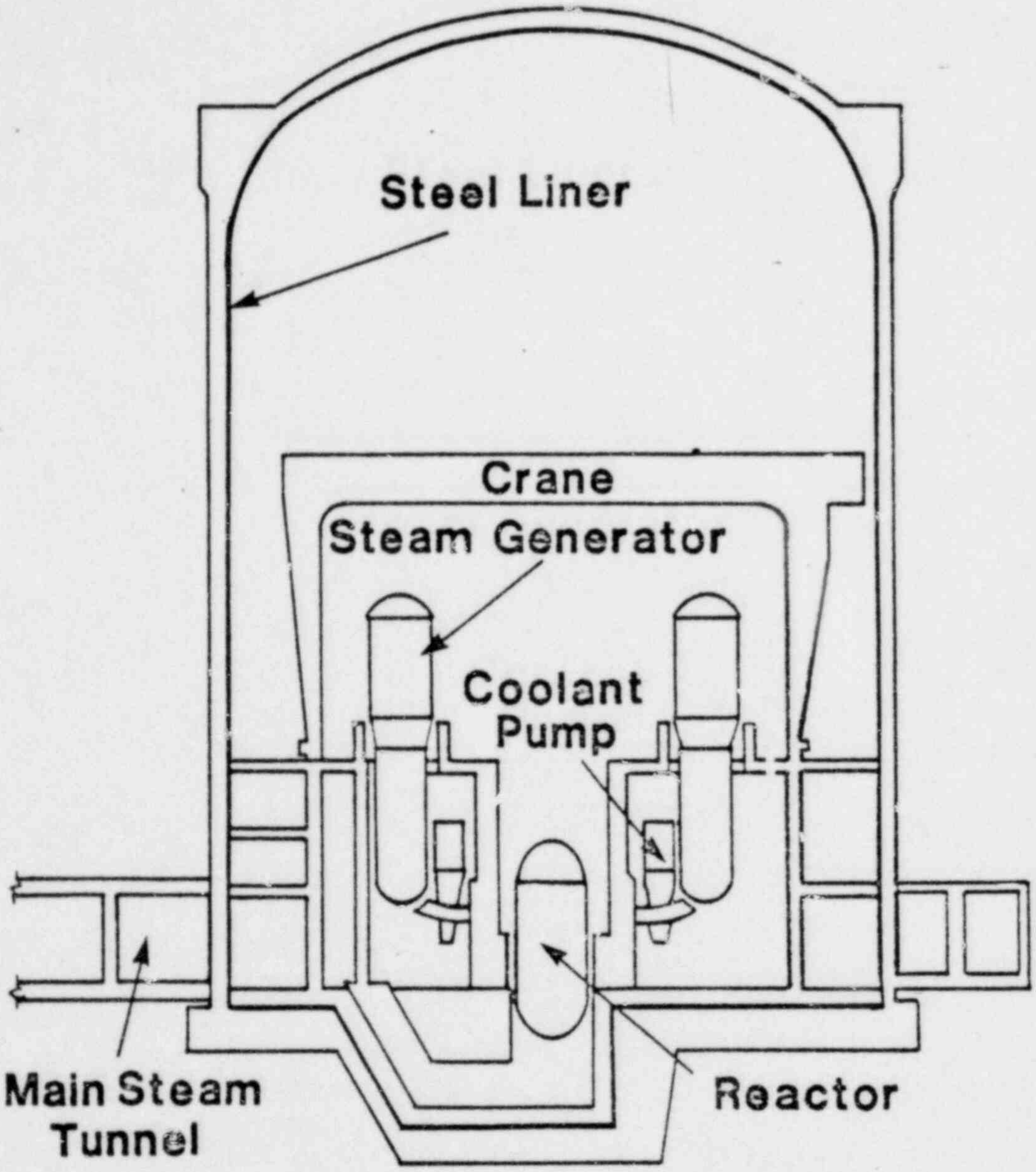
A STUDY PREPARED FOR

COMMONWEALTH EDISON COMPANY

CHICAGO, ILLINOIS

JUNE 16, 1980

SARGENT & LUNDY
ENGINEERS



Steel Liner
Crane
Steam Generator
Coolant
Pump
Main Steam
Tunnel
Reactor

OBJECTIVES

1. Ultimate Pressure Capacity.
2. Effect of High Temperatures
3. Identify Failure Mode(s)
4. Possible Remedies If Meaningful
5. Effect of Rate of Pressure/Temperature Rise

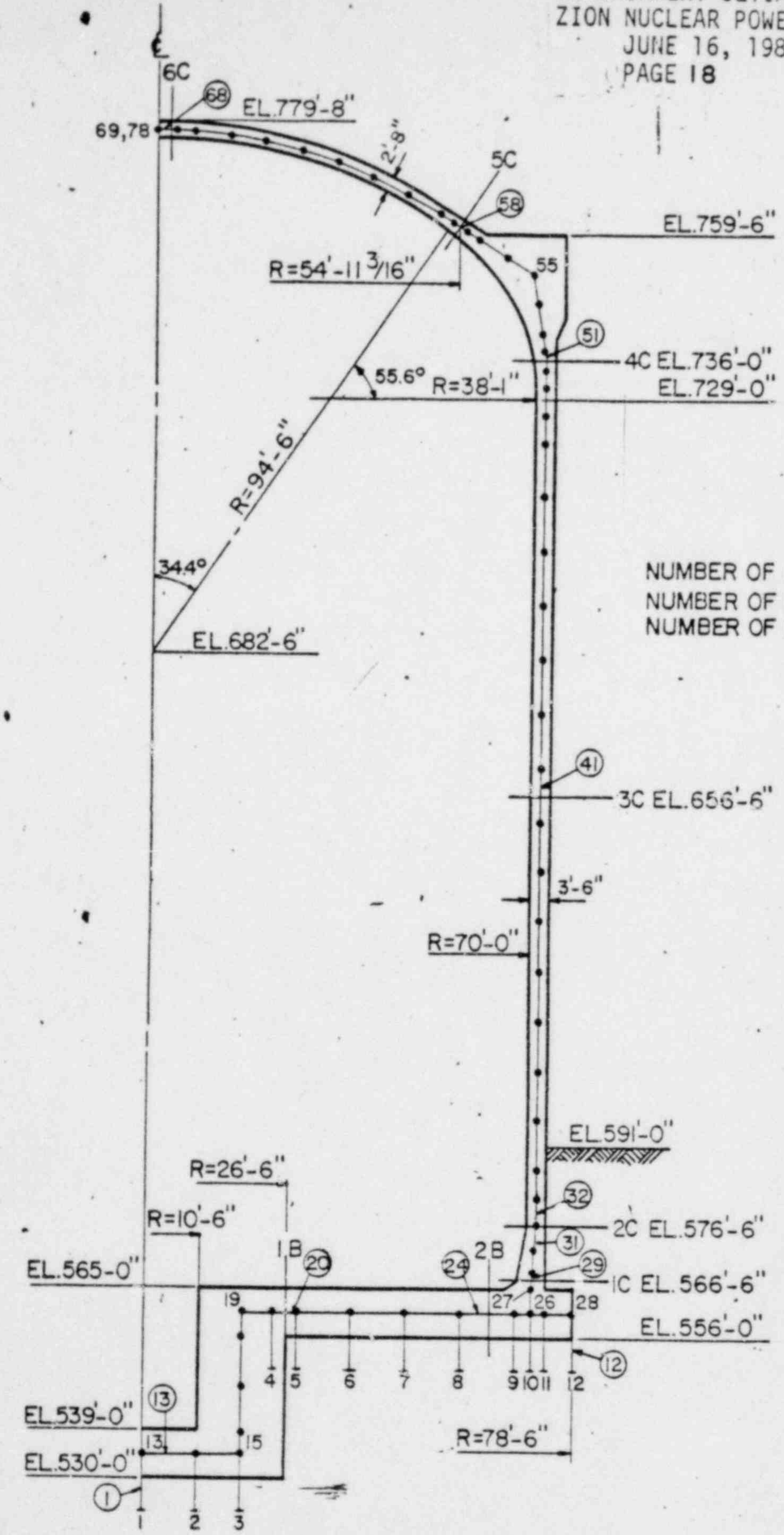
POTENTIAL FAILURE MODES

- TENDON YIELDING : HOOP, VERTICAL, DOME.
- SHEAR FAILURE : DISCONTINUITIES.
- LINER FRACTURE : STRAINS.
- REINFORCING BARS : STRAINS.
- PENETRATIONS : EQUIPMENT HATCH.
- SOIL FAILURE.

ANALYSIS

- COMPUTER ANALYSIS
  - TENDON YIELDING : HOOP, VERTICAL, DOME.
  - SHEAR FAILURE : DISCONTINUITIES
  - LINER : MEMBRANE STRAINS
  - REINFORCING BARS : STRAINS
  - SOIL FAILURE

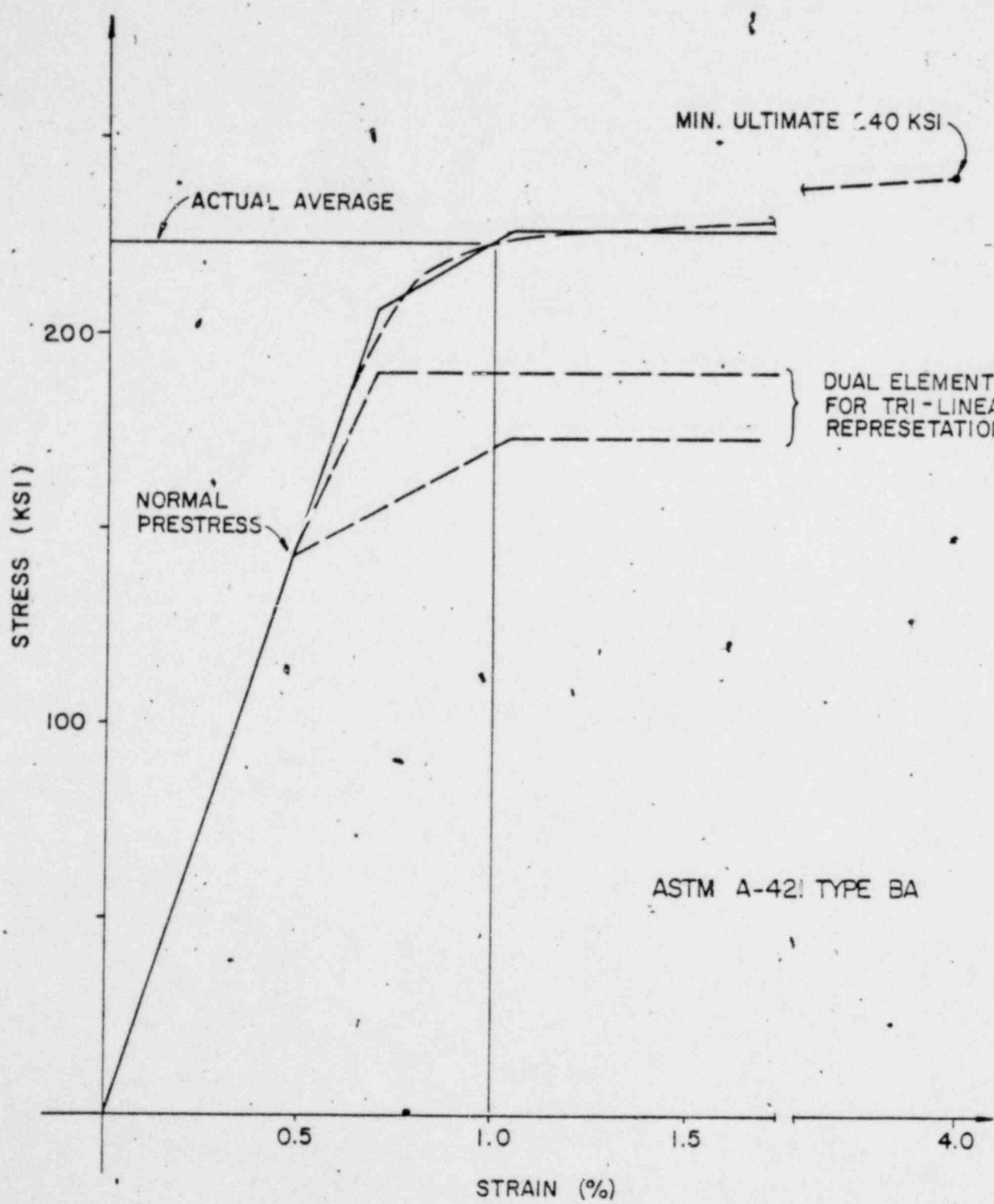
- HAND CALCULATIONS
  - TENDON YIELDING : HOOP, VERTICAL, DOME
  - LINER : FLEXURAL STRAINS - BUCKLING
  - PENETRATIONS : EQUIPMENT HATCH



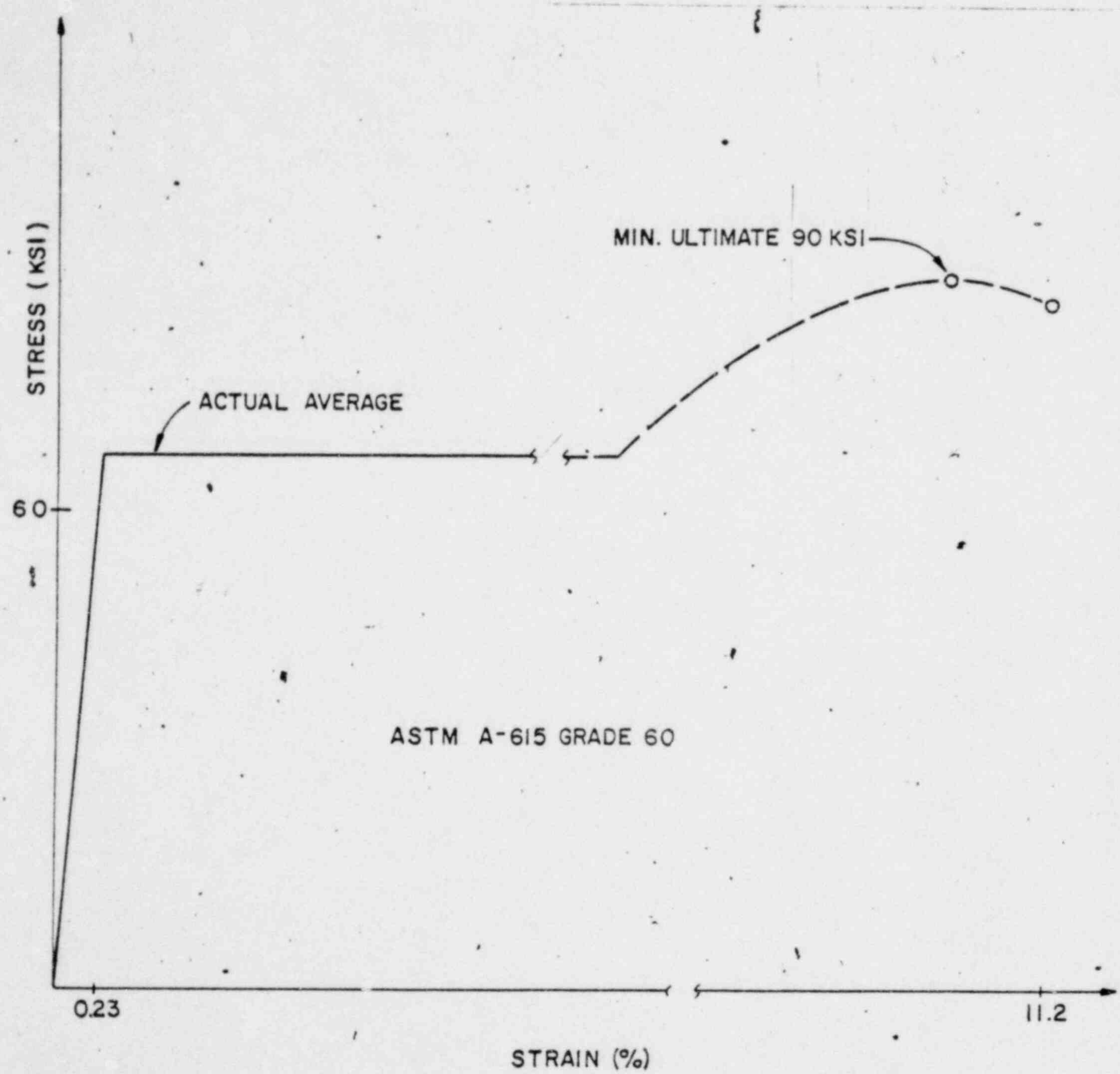
NUMBER OF NODES 78
NUMBER OF ELEMENTS 141
NUMBER OF MATERIALS 48

AXI - SYMMETRIC FINITE ELEMENT MODEL

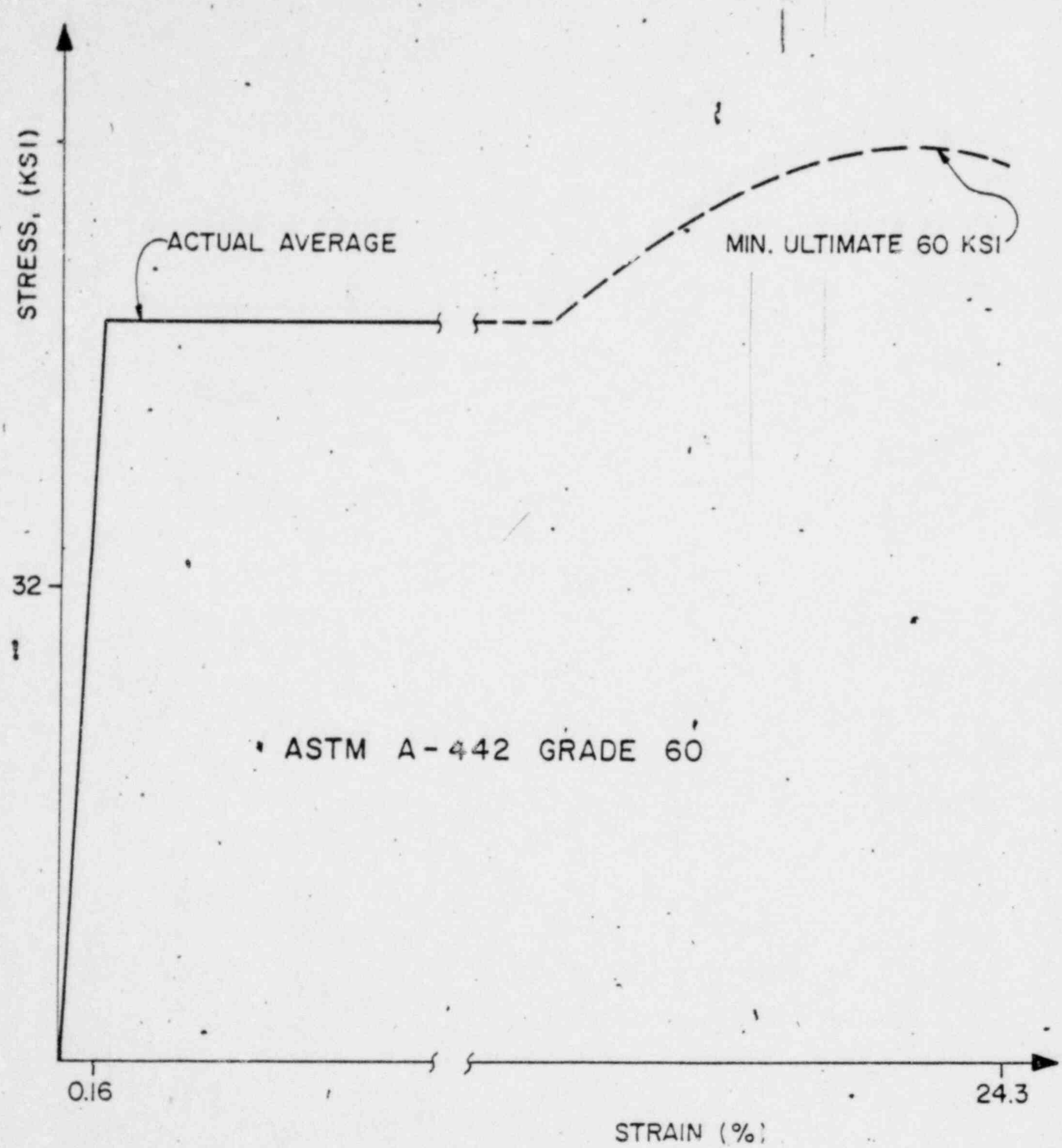
SARGENT & LUNDY ENGINEERS



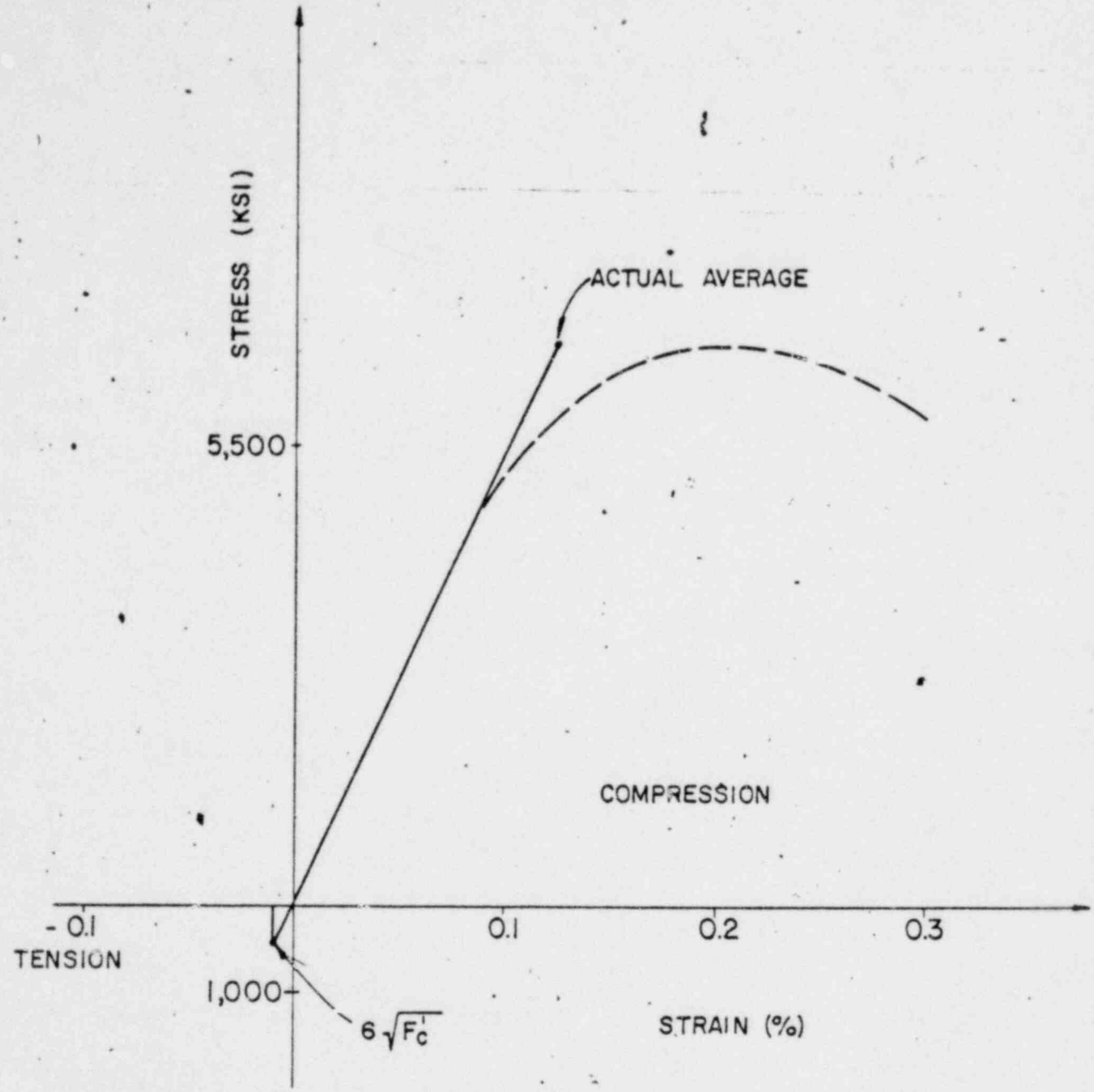
TENDON IDEALIZED STRESS - STRAIN CURVE



REINFORCEMENT IDEALIZED STRESS-STRAIN CURVE



LINER IDEALIZED STRESS - STRAIN CURVE

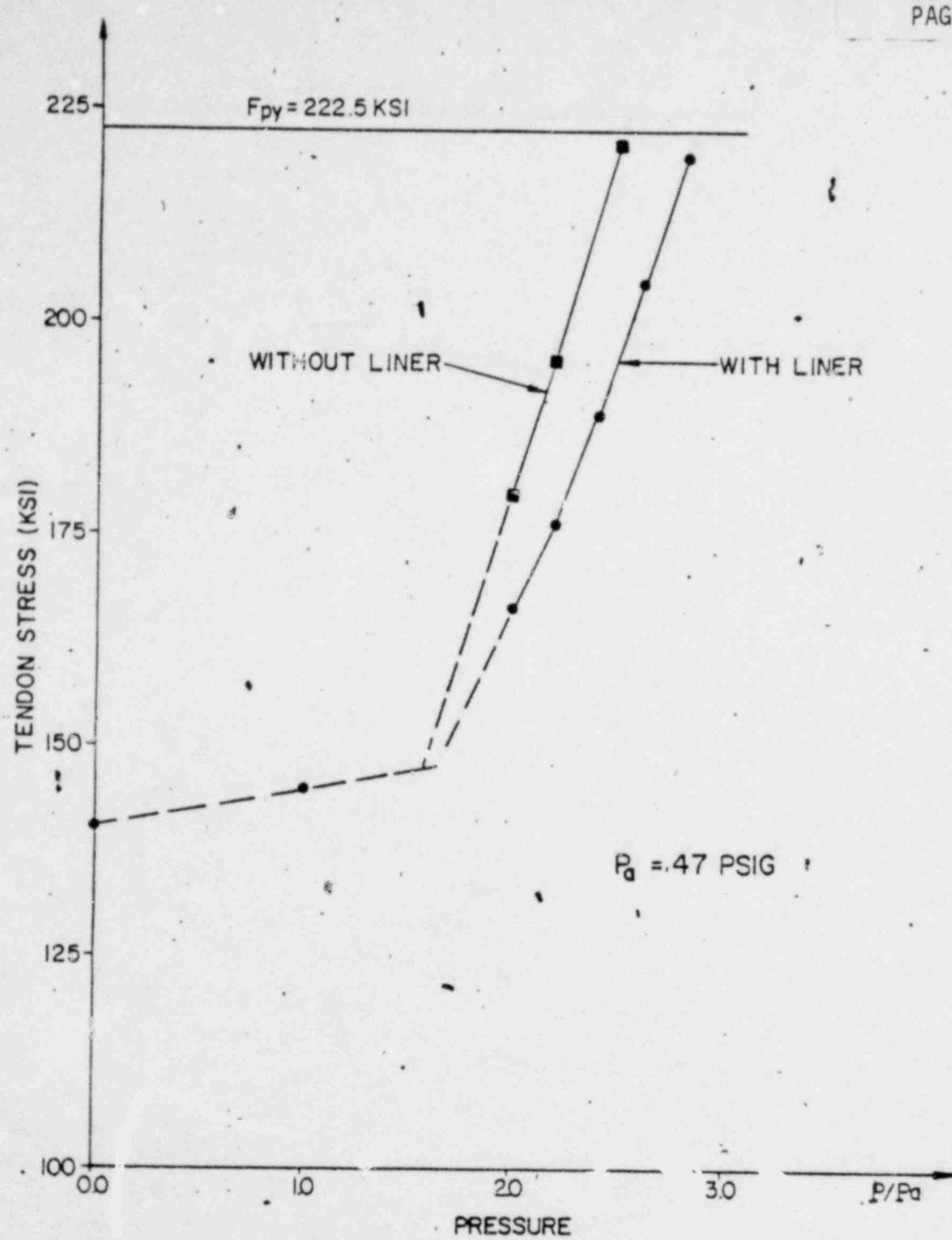


CONTAINMENT CONCRETE IDEALIZED STRESS - STRAIN CURVE

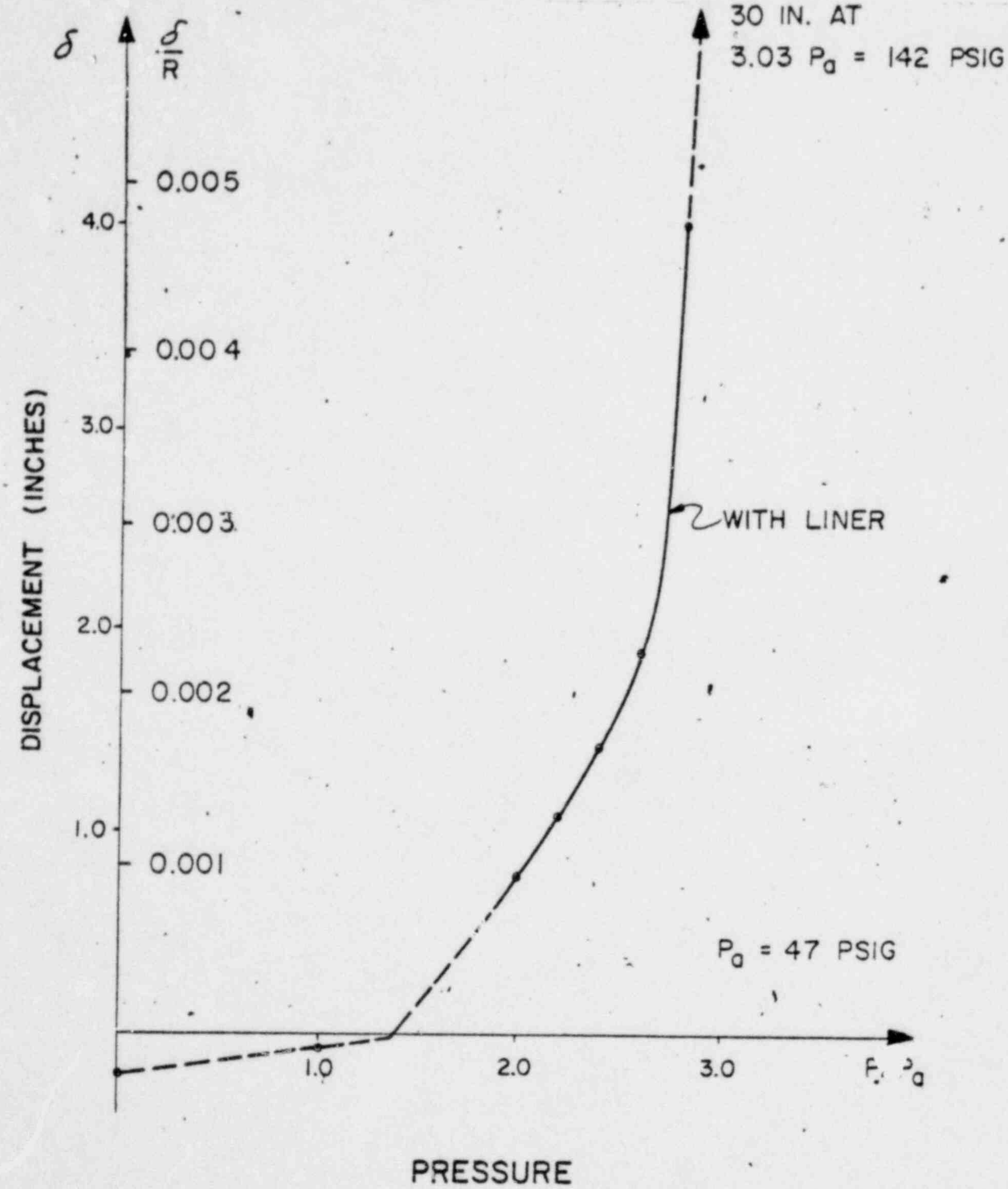
DIRECTION	LINER	CONC. CRACK	REBAR YIELD	TENDON YIELD	TENDON FAIL
HOOP	WITHOUT	75.4 (1.605)	116.2 (2.47)	120.0 (2.55)	128.0 (2.72)
HOOP	WITH	75.4 (1.605)	130.6 (2.778)	134.4 (2.86)	142.4 (3.03)
MERIDIONAL	WITHOUT	83.8 (1.78)	121.9 (2.59)	130.0 (2.765)	137.5 (2.925)
MERIDIONAL	WITH	83.8 (1.78)	150.7 (3.21)	158.8 (3.38)	166.3 (3.54)
DOME	WITHOUT	81.6 (1.73)	119.9 (2.55)	131.5 (2.80)	140.6 (2.99)
DOME	WITH	83.24 (1.77)	141.24 (3.00)	152.84 (3.252)	161.97 (3.446)

(   ) ≡ P/P_a P_a = 47 PSIG

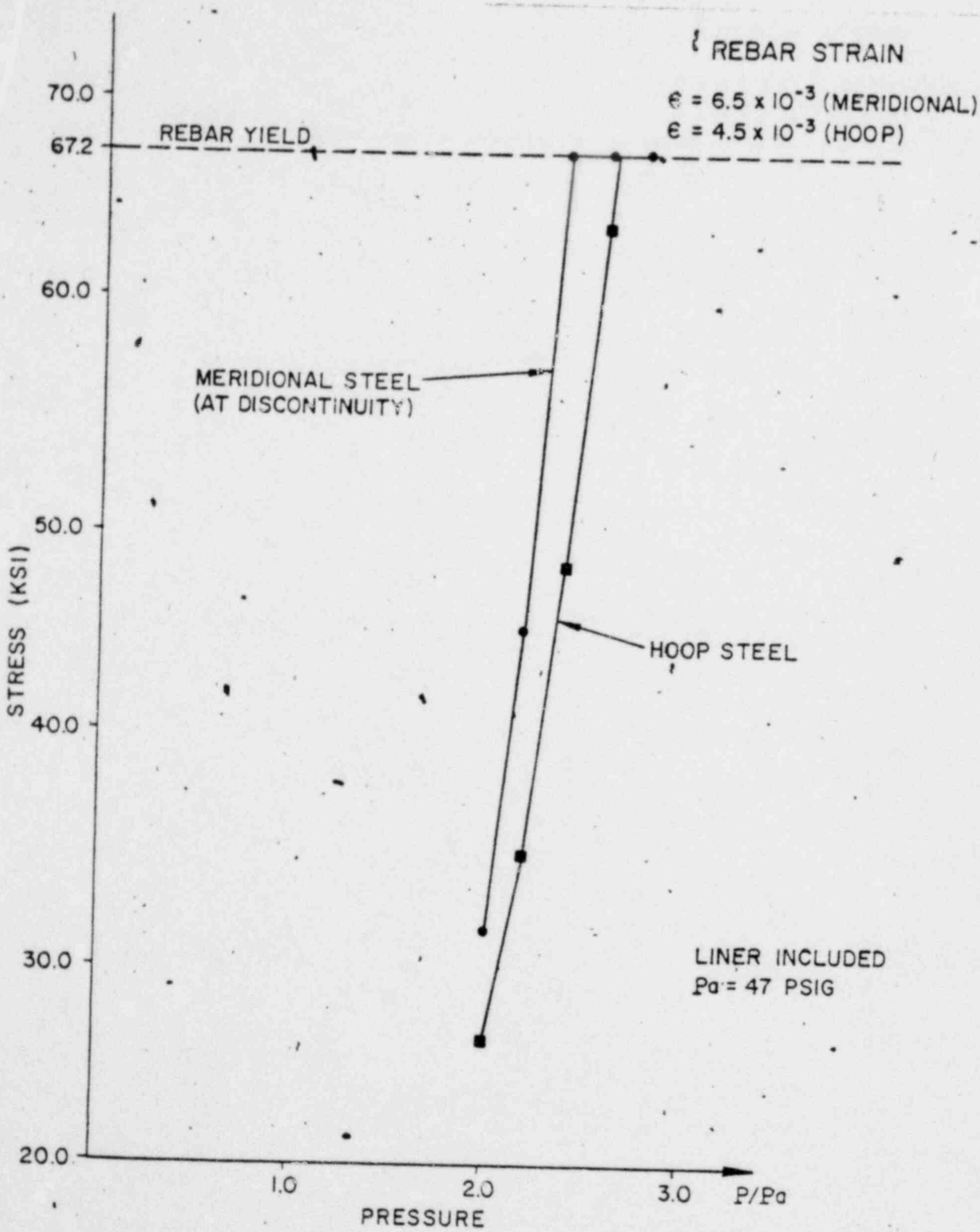
PRESSURE AT VARIOUS RESPONSE STAGES (PSIG)
HAND CALCULATIONS: EQUILIBRIUM REQUIREMENTS



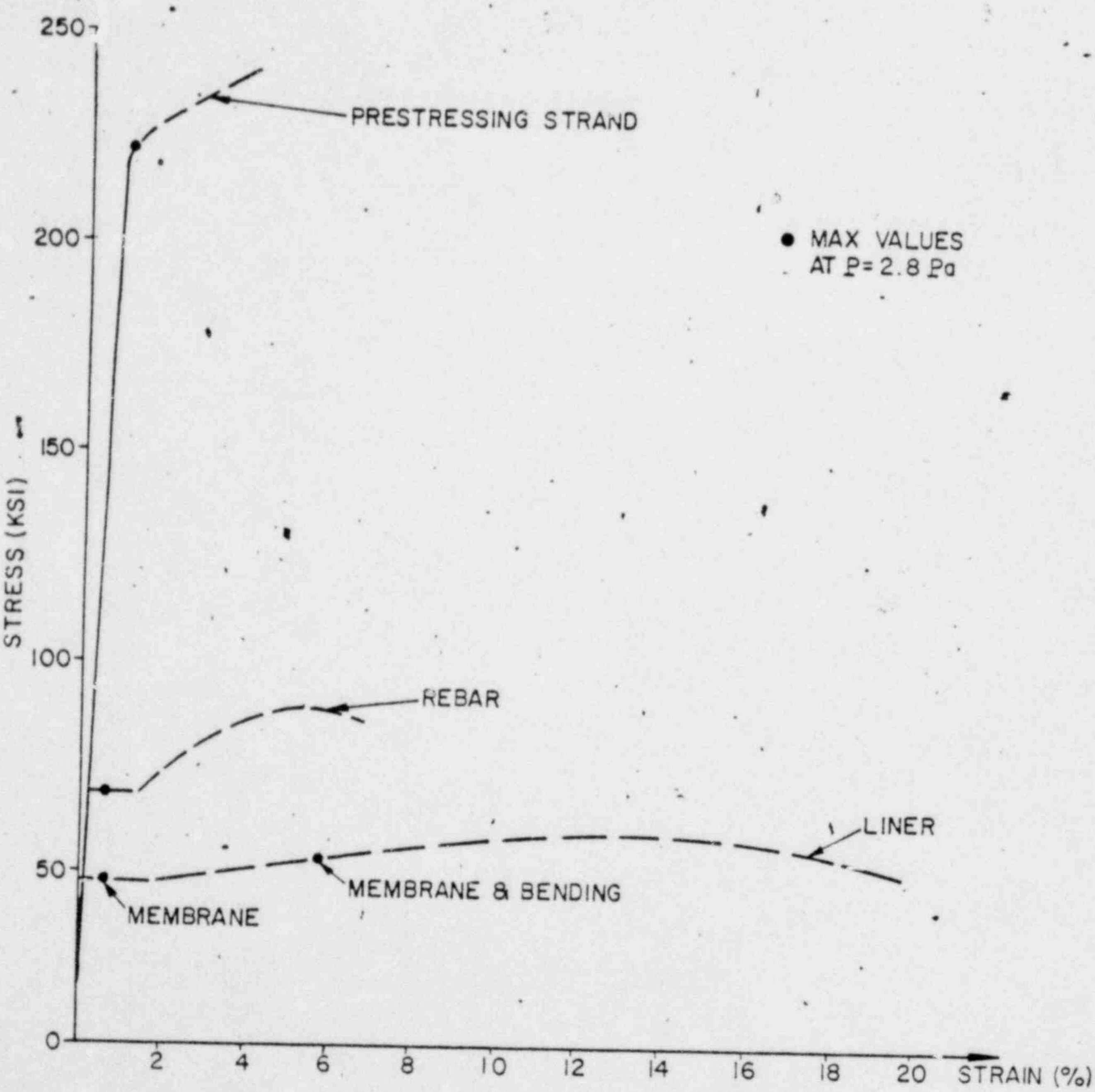
HOOP TENDON STRESSES vs. PRESSURE



RADIAL DISPLACEMENT OF CYLINDER VS PRESSURE



MAXIMUM REBAR STRESSES VS PRESSURE



MAXIMUM COMPONENT STRAINS AT P= 2.8 Pa

AT $P = 2.55\,P_a$ = 120 PSIG = 135 PSIA

PARAMETER	MARGIN FACTOR
SHEAR: BASEMAT	1.27
SHEAR: CONTAINMENT	1.28
SHEAR: EQUIPMENT HATCH	1.29
CONCRETE COMPRESSION: BASEMAT	3.85
CONCRETE COMPRESSION: CONTAINMENT	5.83
REINFORCEMENT:	1.32
EQUIPMENT HATCH (CBI)	1.0 (CONSERVATIVE)
SOIL PRESSURE	1.95
LINER FIBER STRAIN	2.10
REBAR STRAIN	4.74

MARGIN FACTORS FOR OTHER NONCRITICAL PARAMETERS

CONCLUSIONS :

1- ULTIMATE PRESSURE CAPACITY :

120 PSIG = 135 PSIA (WITHOUT LINER)

134 PSIG = 149 PSIA (WITH LINER)

2- FAILURE MODE : HOOP TENDON YIELDING

3- TEMPERATURE EFFECTS : PROBABLY NOT SIGNIFICANT

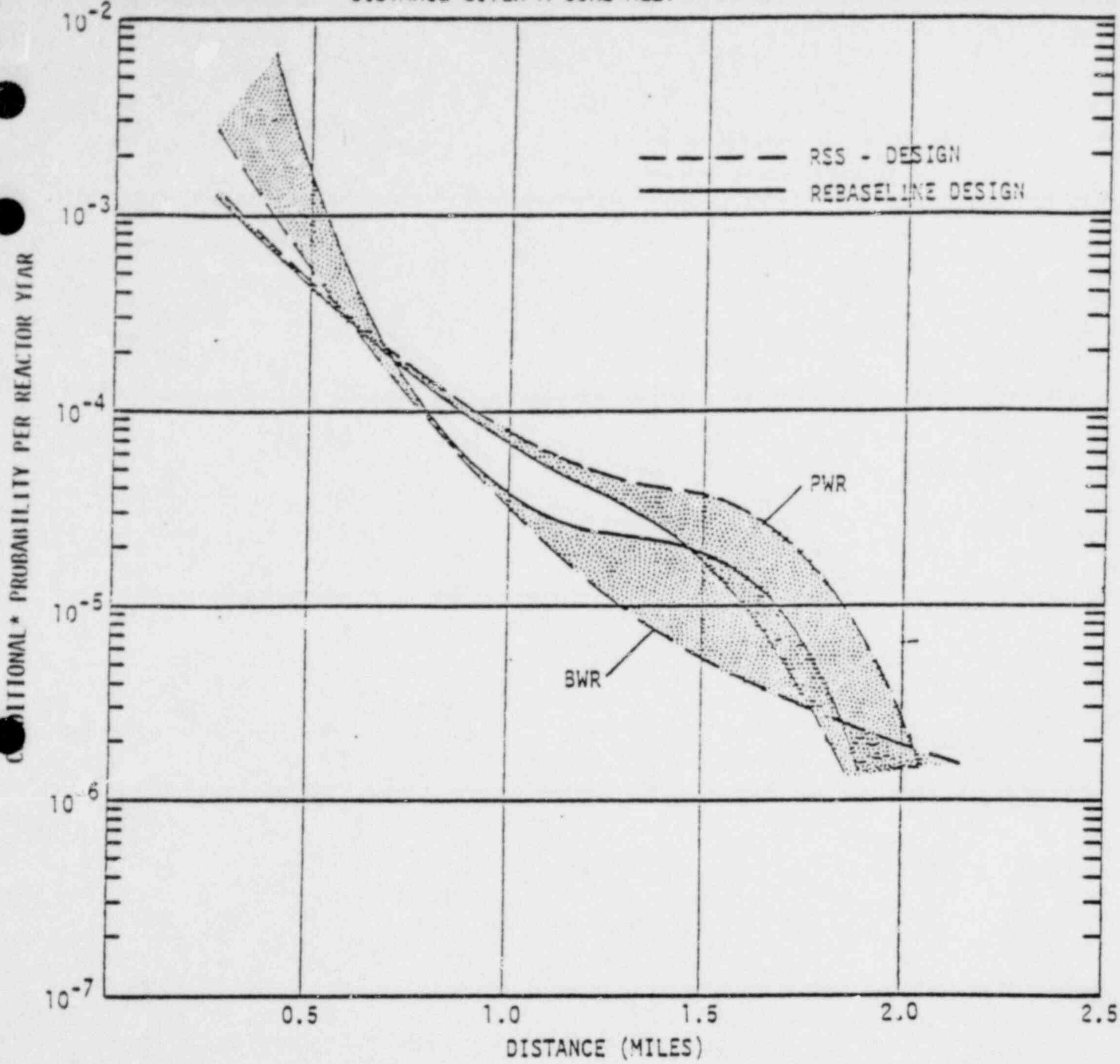
4- POSSIBLE REMEDIES : NONE RECOMMENDED

5- EFFECT OF RATE OF PRESSURE / TEMPERATURE RISE : PROBABLY NOT SIGNIFICANT

PARAMETRIC VARIATION FOR ACCIDENT RISK

- o SITE
- o PUBLIC PROTECTION
- o DESIGN & OPERATION

FIGURE B1 - RISK OF EARLY FATALITY TO AN INDIVIDUAL VERSUS DISTANCE GIVEN A CORE MELT*



ASSUMPTIONS: *CORE MELT PROBABILITY ASSUMED TO BE 10^{-4}/REACTOR YEAR

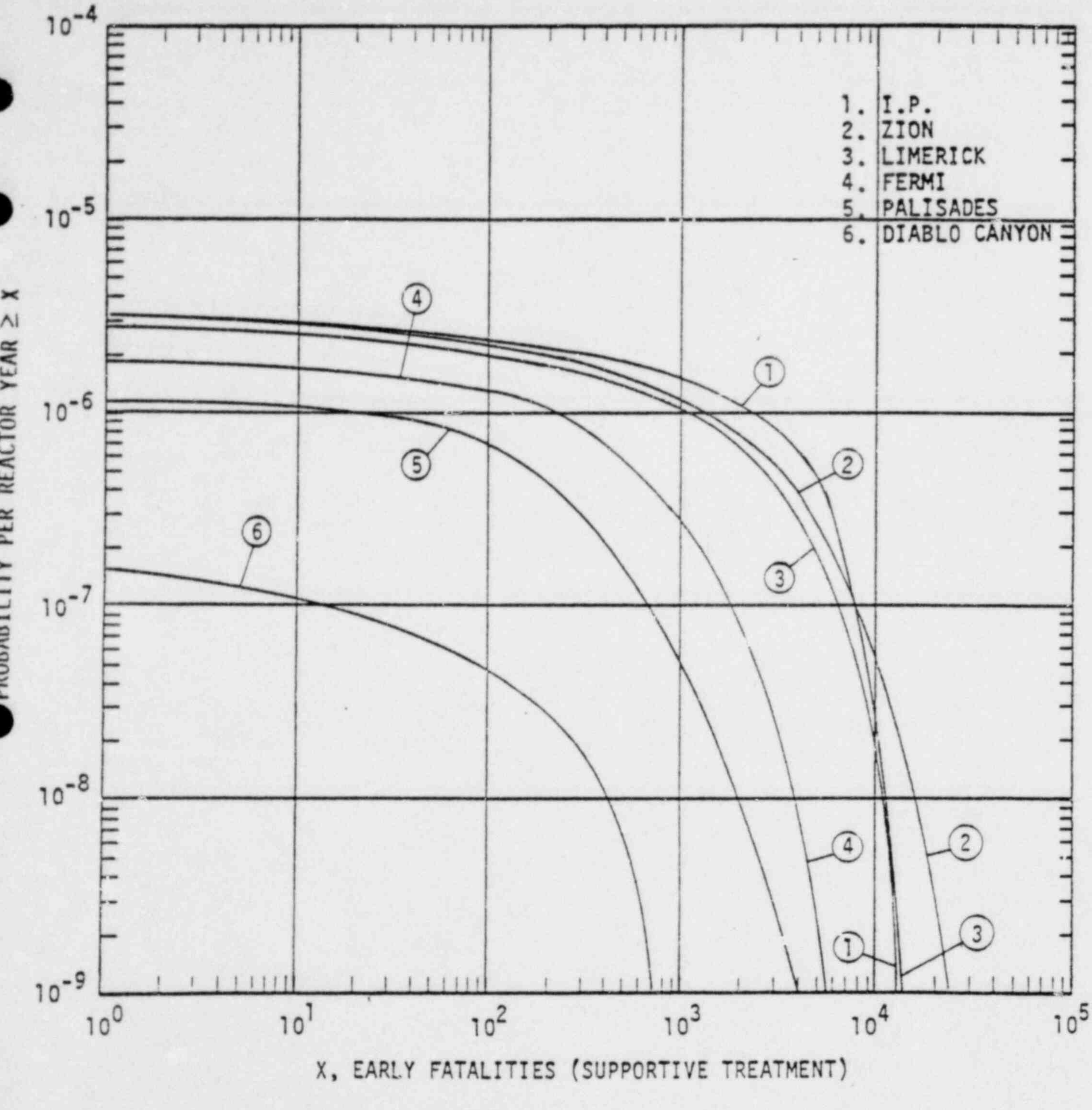
RSS-DESIGN
1. ALL RSS CORE MELT ACCIDENT RELEASE CATEGORIES
2. ALL RSS ASSUMPTIONS (E.G., SMOOTHING)

REBASELINE DESIGN
1. SMOOTHING ELIMINATED
2. EXPLICIT ACCIDENT SEQUENCES
3. NEGLIGIBLE PROBABILITY OF VESSEL STEAM EXPLOSION

EXPECTED CONSEQUENCES FROM 91 WEATHER SEQUENCES WITH 3200 MWT POWER LEVEL

ENTIRE CLOUD EXPOSURE + 24 HOUR GROUND EXPOSURE SHIELDING BASED ON NORMAL ACTIVITY

FIGURE 1 - EARLY FATALITY RISK FOR DIFFERENT SITES



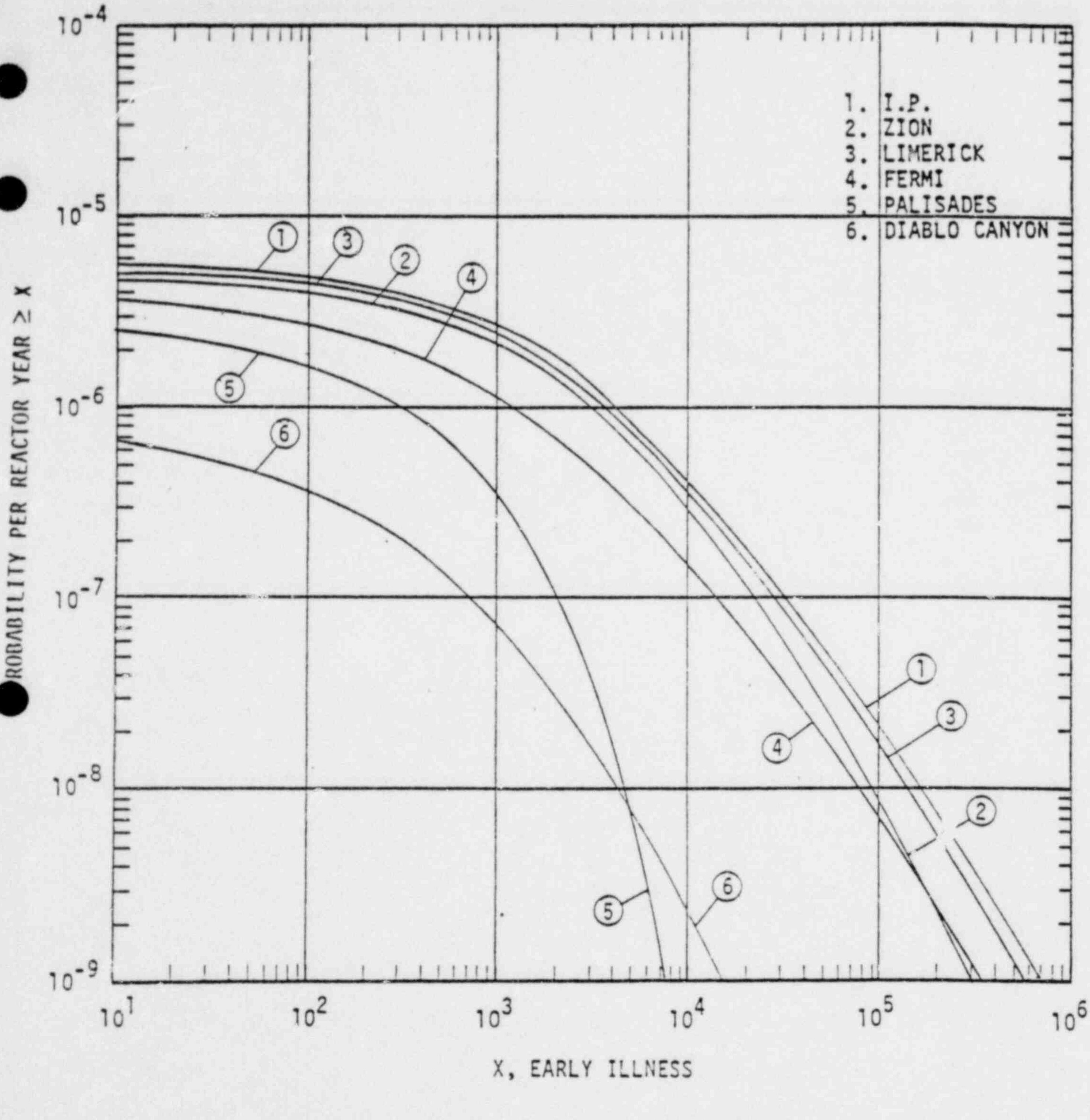
NOTE: THERE ARE LARGE UNCERTAINTIES WITH THE ABSOLUTE VALUES PRESENTED IN THIS FIGURE

ASSUMPTIONS:
1) SURRY DESIGN.
2) I.P. UNIT 3 POWER LEVEL (3025 MWT).
3) WITHIN 10 MILES - ENTIRE CLOUD EXPOSURE + 4 HOURS GROUND EXPOSURE NO SHIELDING
   BEYOND 10 MILES - ENTIRE CLOUD EXPOSURE + 7 DAY GROUND EXPOSURE SHIELDING BASED ON NORMAL ACTIVITY.
4) WIND ROSE WEIGHTED 1970 CENSUS POPULATION DISTRIBUTION.
5) IDENTICAL 91 WEATHER SEQUENCES FOR ALL SITES.

SITE VARIATION

- o BENCHMARK REACTOR
  - 3025 MWT SURRY
- o VARY ONLY THE SITE
  - 4 POPULOUS SITES
  - 1 TYPICAL SITE
  - 1 REMOTE SITE
- o SAME PUBLIC PROTECTION MEASURES
- o 4 MEASURES OF RISK

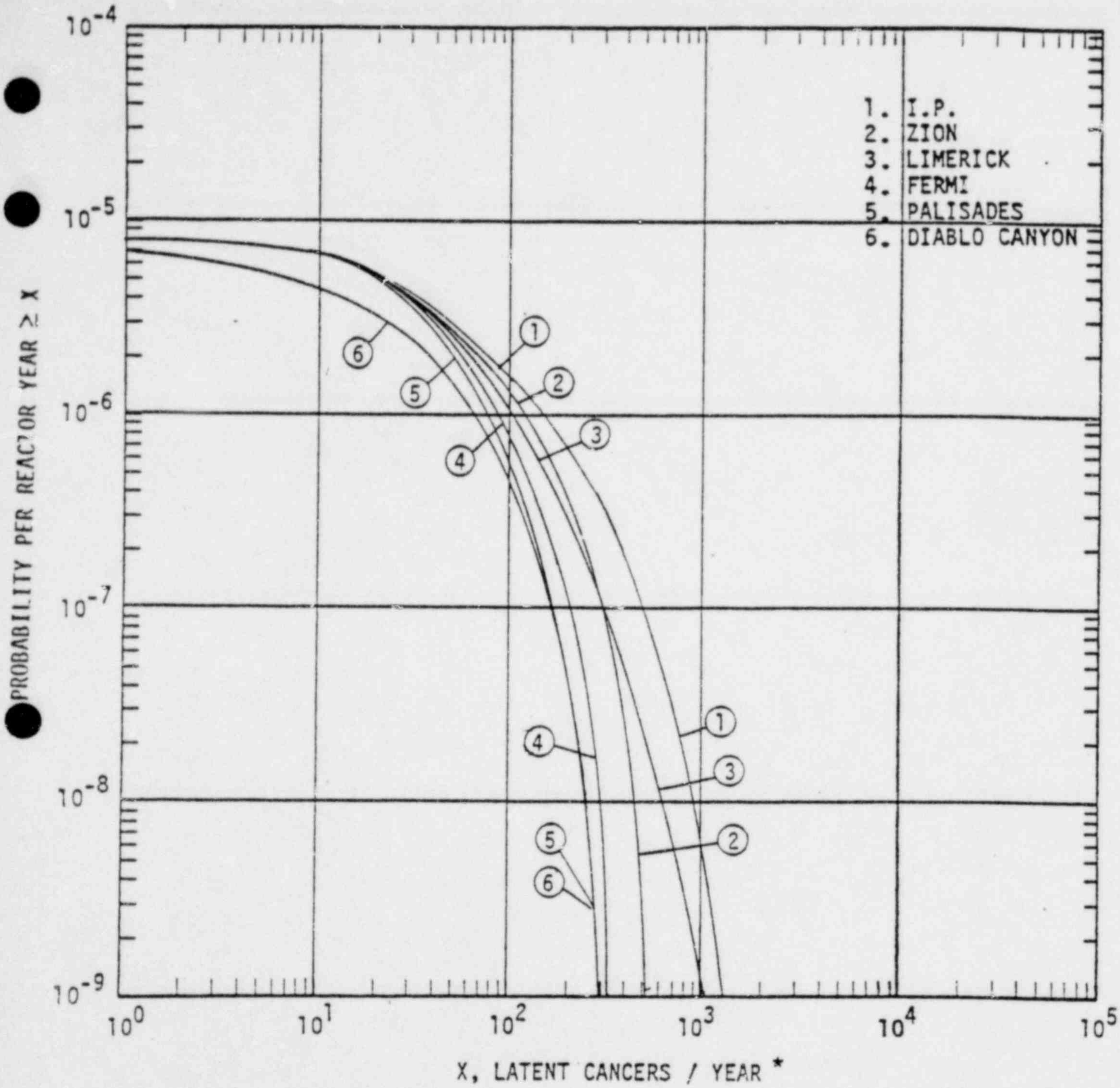
FIGURE 2 - EARLY ILLNESS RISK FOR DIFFERENT SITES



NOTE: THERE ARE LARGE UNCERTAINTIES WITH THE ABSOLUTE VALUES PRESENTED IN THIS FIGURE

ASSUMPTIONS: 1) SURRY DESIGN.
2) I.P. UNIT 3 POWER LEVEL (3025 MWT).
3) WITHIN 10 MILES - ENTIRE CLOUD EXPOSURE + 4 HOURS GROUND EXPOSURE NO SHIELDING
BEYOND 10 MILES - ENTIRE CLOUD EXPOSURE + 7 DAY GROUND EXPOSURE SHIELDING BASED ON NORMAL ACTIVITY.
4) WIND ROSE WEIGHTED 1970 CENSUS POPULATION DISTRIBUTION.
5) IDENTICAL 91 WEATHER SEQUENCES FOR ALL SITES.

FIGURE 3 - LATENT CANCER RISK (ANNUAL) FOR DIFFERENT SITES

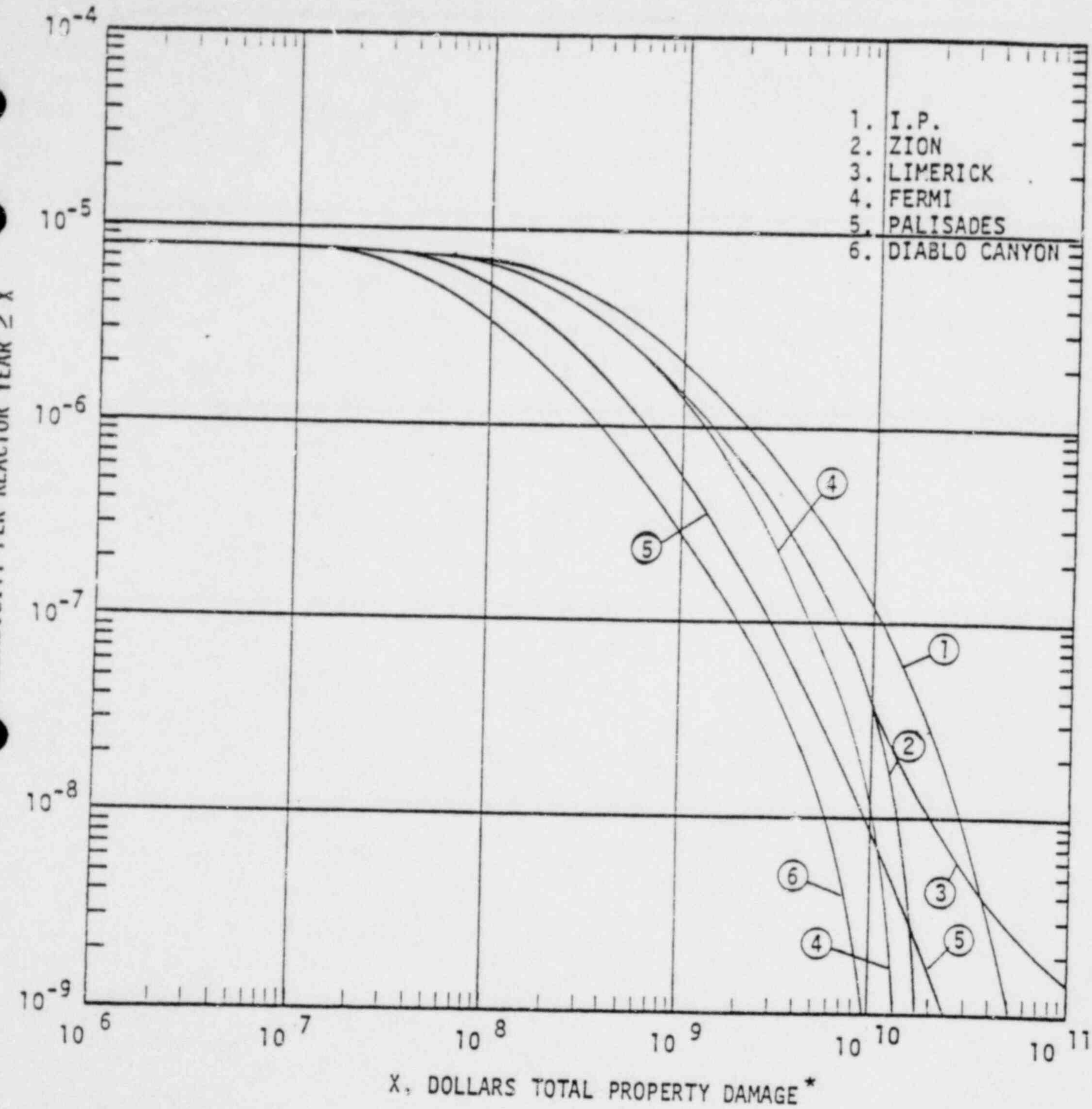


*TOTAL LATENT CANCERS WOULD BE 30 TIMES HIGHER

NOTE: THERE ARE LARGE UNCERTAINTIES WITH THE ABSOLUTE VALUES PRESENTED IN THIS FIGURE

ASSUMPTIONS: 1) SURRY DESIGN.
2) I.P. UNIT 3 POWER LEVEL (3025 MWT).
3) WITHIN 10 MILES - ENTIRE CLOUD EXPOSURE + 4 HOURS GROUND EXPOSURE NO SHIELDING
BEYOND 10 MILES - ENTIRE CLOUD EXPOSURE + 7 DAY GROUND EXPOSURE SHIELDING BASED ON NORMAL ACTIVITY.
4) WIND ROSE WEIGHTED 1970 CENSUS POPULATION DISTRIBUTION.
5) IDENTICAL 91 WEATHER SEQUENCES FOR ALL SITES.

FIGURE 4 - PROPERTY DAMAGE RISK FOR DIFFERENT SITES



* BASED ON 1974 DOLLARS

NOTE: THERE ARE LARGE UNCERTAINTIES WITH THE ABSOLUTE VALUES PRESENTED IN THIS FIGURE

ASSUMPTIONS: 1) SURRY DESIGN
2) I.P. UNIT 3 POWER LEVEL (3025 MWT)
3) WIND ROSE WEIGHTED 1970 CENSUS POPULATION DISTRIBUTION
4) IDENTICAL 91 WEATHER SEQUENCES FOR ALL SITES.

DO ZION AND INDIAN POINT DOMINATE SOCIETAL RISK?

- o SURRY AT INDIAN POINT OR ZION IS 10 TIMES WORSE THAN SURRY AT TYPICAL SITE
- o EQUATION

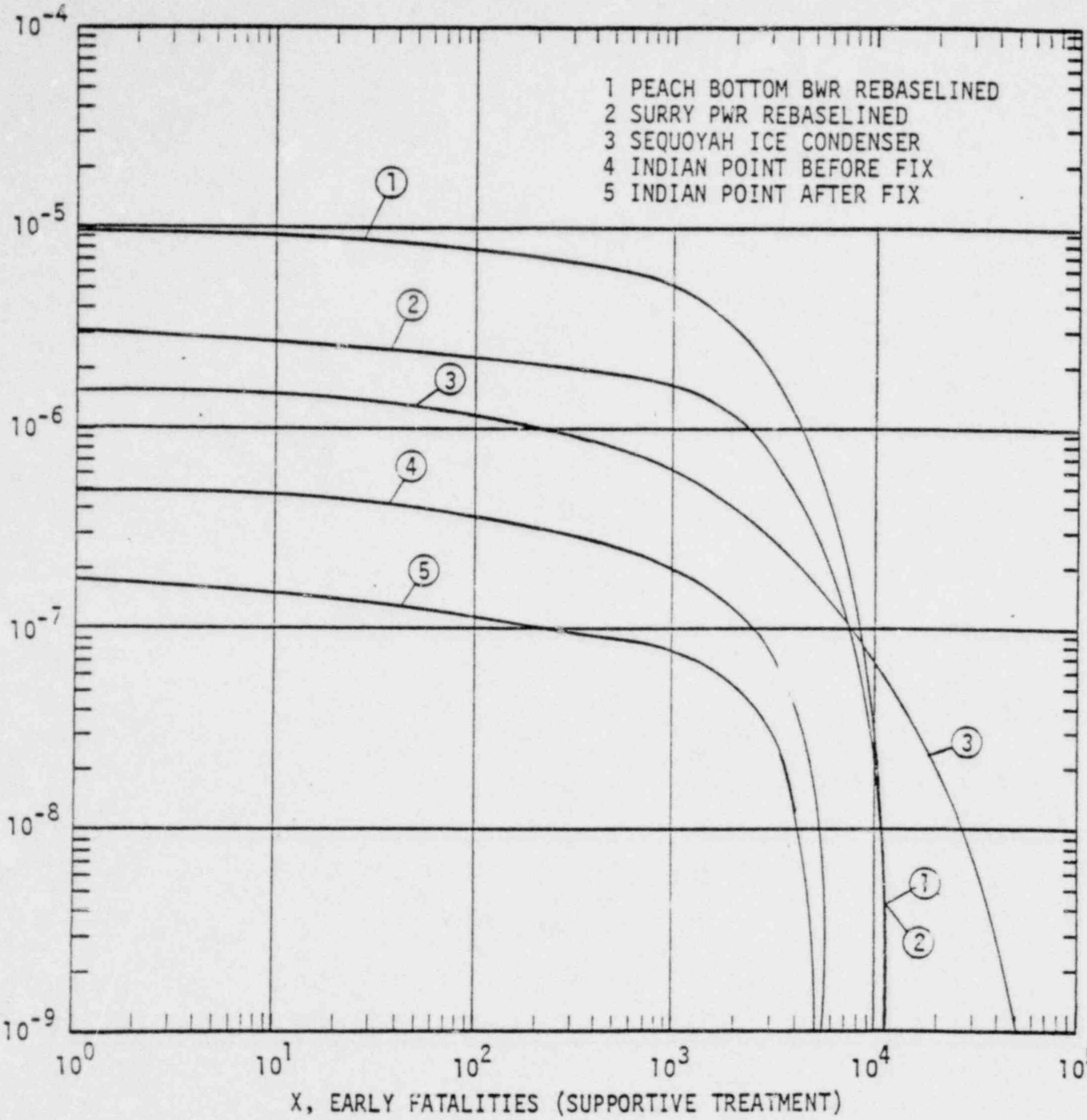
SITE	NUMBER OF REACTORS	UNITS OF RISK
IP	2	20
ZION	2	20
ALL OTHERS	68	68
		108

∴ ZION AND INDIAN POINT ≅ 40% OF RISK IF SURRY IS TYPICAL OF ALL DESIGNS

VARIATION OF DESIGN AND OPERATION

- o INDIAN POINT SITE
- o SAME PUBLIC PROTECTION MEASURES
- o DIFFERENT REACTORS AT 3025 MWT
  - SURRY
  - PEACH BOTTOM
  - SEQUOYAH
  - INDIAN POINT BEFORE
  - INDIAN POINT AFTER

FIGURE 7 - EARLY FATALITY RISK FOR DIFFERENT DESIGNS



NOTE: THERE ARE LARGE UNCERTAINTIES WITH THE ABSOLUTE VALUES PRESENTED IN THIS FIGURE

ASSUMPTIONS: 1) INDIAN POINT SITE
METEOROLOGY - 91 WEATHER SEQUENCES
WIND ROSE WEIGHTED 1970 CENSUS POPULATION DISTRIBUTION
UNIT 3 POWER LEVEL (3025 MWT)
2) WITHIN 10 MILES - ENTIRE CLOUD EXPOSURE + 4 HOURS GROUND EXPOSURE
NO SHIELDING
BEYOND 10 MILES - ENTIRE CLOUD EXPOSURE + 7 DAY GROUND EXPOSURE
SHIELDING BASED ON NORMAL ACTIVITY

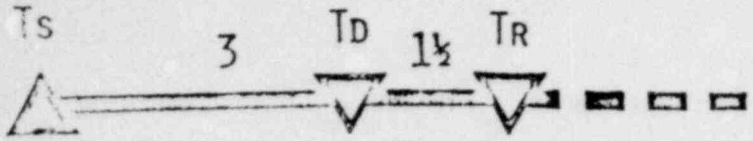
VARIATION OF PUBLIC PROTECTION

- o BENCHMARK REACTOR

- o INDIAN POINT SITE

- o VARIED PUBLIC PROTECTION
  - IN REPORT
    - SHELTERING
    - DIFFERENT RADII OF EVACUATION
    - DON'T BEAT THE CLOUD
  - IN PRESENTATION
    - DIFFERENT EARLY WARNING
    - DIFFERENT EVACUATION RATES
    - SOME BEAT THE CLOUD

SEQUENCE 1 (TMLB')



SEQUENCE 2 (AG)

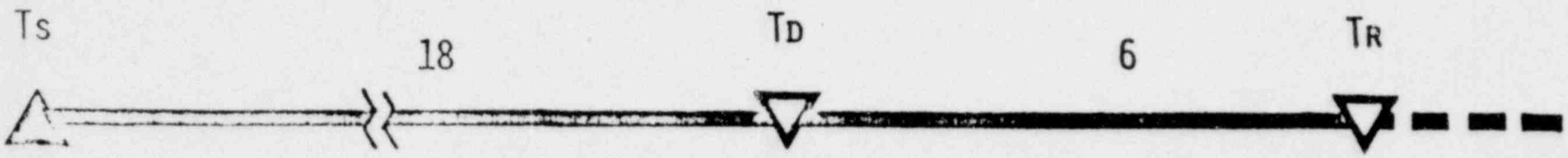
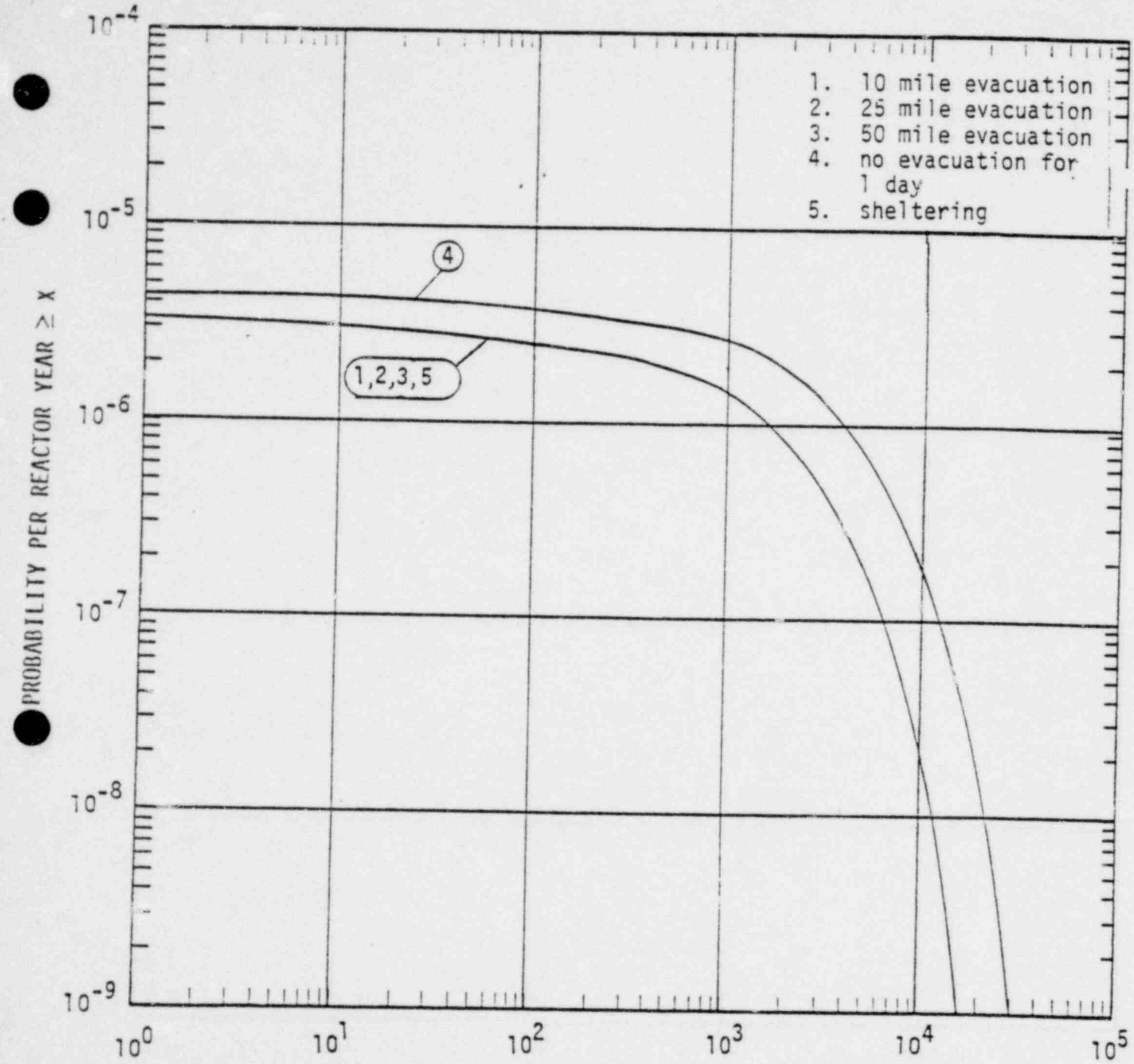


FIGURE 5 - EARLY FATALITY RISK AT INDIAN POINT FOR VARIOUS PUBLIC PROTECTION MEASURES



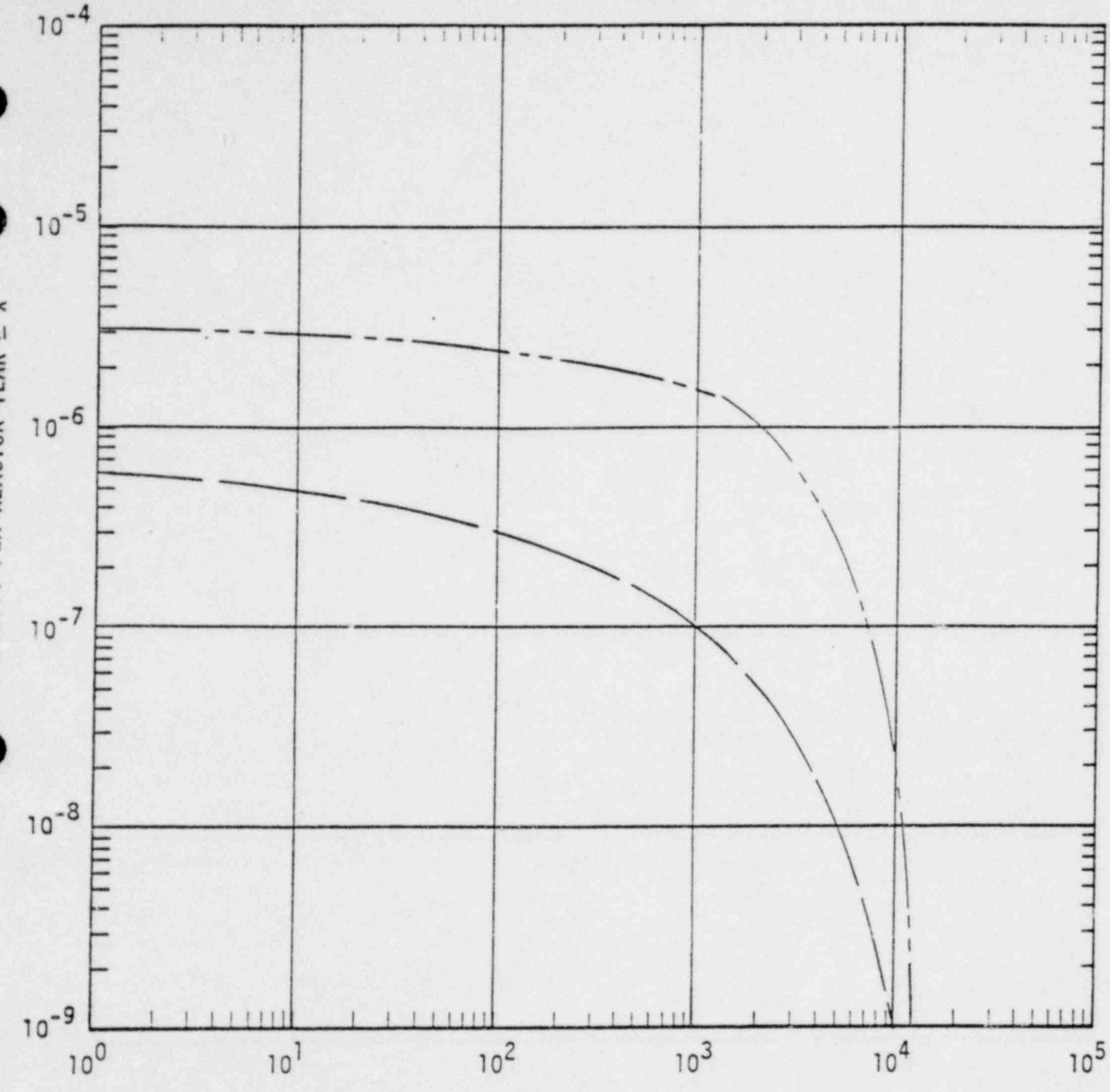
NOTE: THERE ARE LARGE UNCERTAINTIES WITH THE ABSOLUTE VALUES PRESENTED IN THIS FIGURE

ASSUMPTIONS: 1) SURRY DESIGN.
2) I.P. UNIT 3 POWER LEVEL (3025 MWT).
3) WIND ROSE WEIGHTED 1970 CENSUS POPULATION DISTRIBUTION
4) INDIAN POINT SITE (POPULATION AND METEOROLOGY)

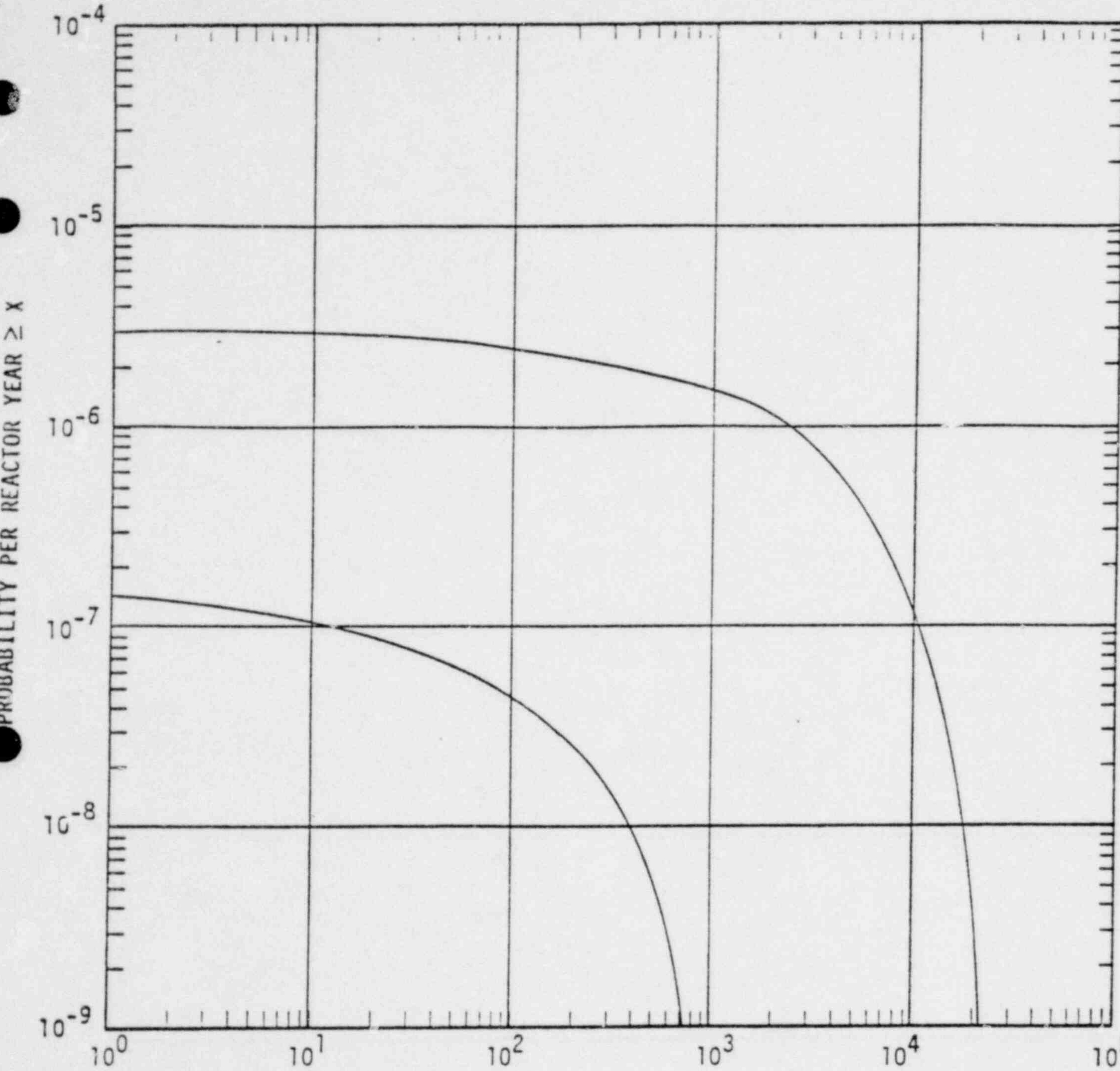
EVACUATION SCENARIOS - ENTIRE CLOUD EXPOSURE + EITHER 4 HOURS GROUND EXPOSURE, NO SHIELDING WITHIN GIVEN DISTANCE; OR 7 DAYS GROUND EXPOSURE, NORMAL SHIELDING BEYOND GIVEN DISTANCE

NO EVACUATION - ENTIRE CLOUD EXPOSURE + 1 DAY GROUND EXPOSURE, NORMAL SHIELDING

SHELTERING - ENTIRE CLOUD EXPOSURE + 1 DAY GROUND EXPOSURE, SHIELDING ASSUMES BRICK HOUSE WITH NO BASEMENT.

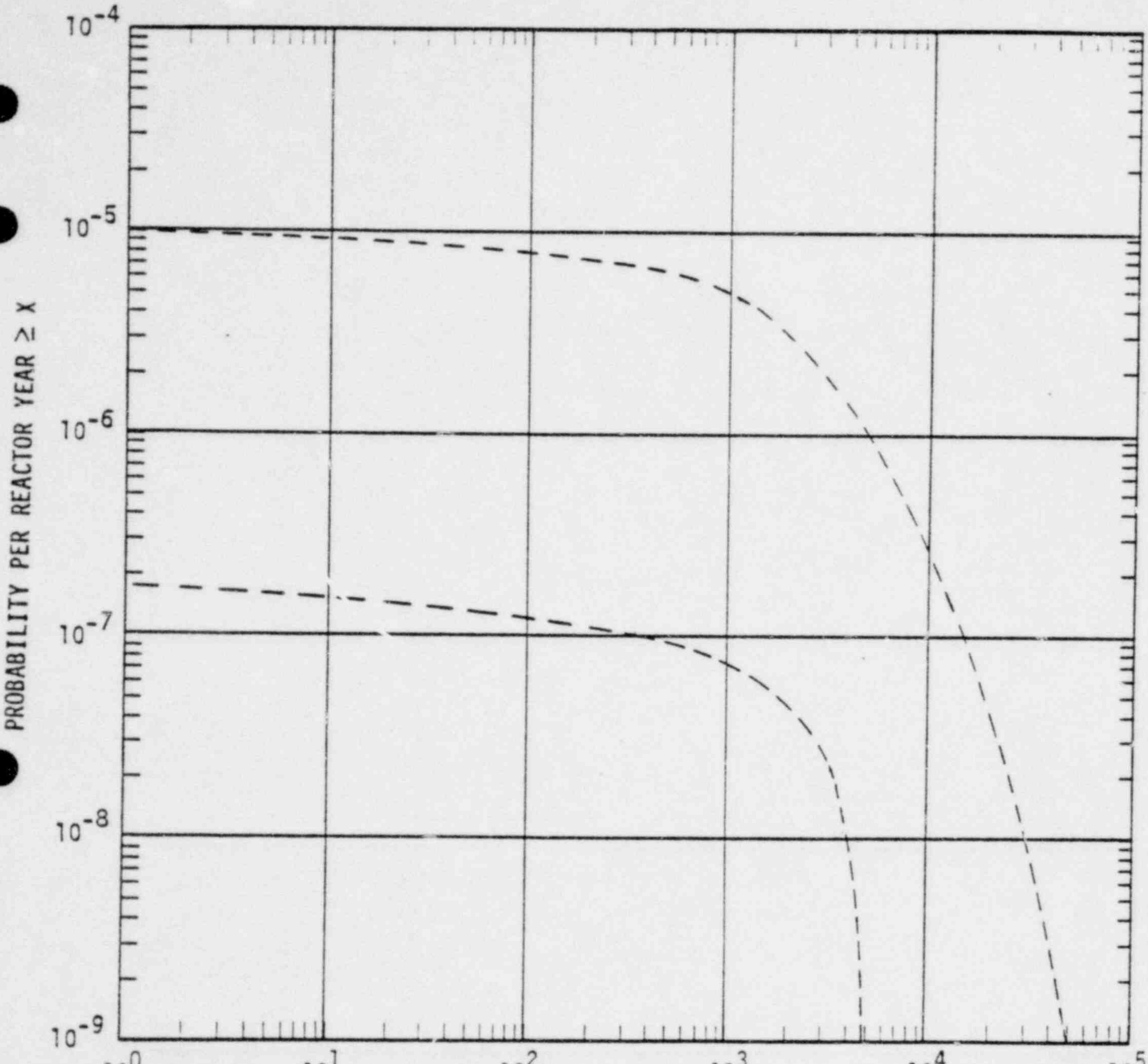


Emergency Response - 1-5 hour delay ——— ———; ——— - - ———

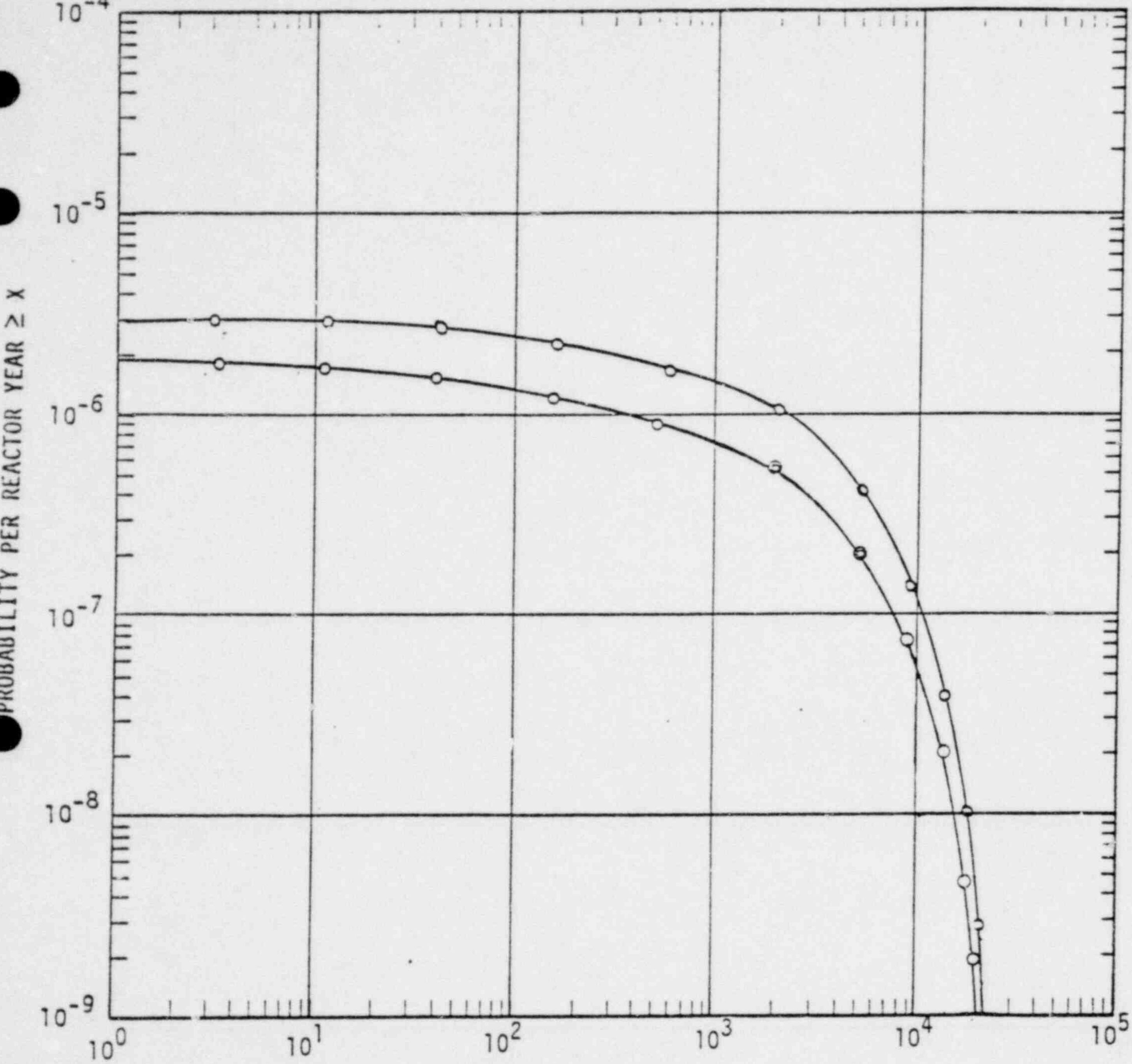


PROBABILITY PER REACTOR YEAR ≥ X
10^{-4}
10^{-5}
10^{-6}
10^{-7}
10^{-8}
10^{-9}
10^0
10^1
10^2
10^3
10^4
10^5

Site Spread ————



Design Spread - - - - - - - -



Power Reduction Spread

INSIGHTS

- o INDIAN POINT SITE WORSE THAN TYPICAL
- o INDIAN POINT REACTOR BETTER THAN TYPICAL
- o INDIAN POINT IS NOT DOMINANT SOCIETAL RISK
- o DESIGN/OPERATION IS LEAST CERTAIN AND MOST SIGNIFICANT VARIABLE

PEOPLES

PURPOSE OF UTILITY PRESENTATION TO ACRS SUBCOMMITTEE

JULY 2, 1980

1. CONVEY SERIOUSNESS AND DEPTH OF UTILITY WORK

2. REVIEW SHORT TERM "MINI WASH-1400" STUDY

3. REVIEW LONGER TERM PROBABILISTIC RISK ASSESSMENT

4. REVIEW PHENOMENOLOGY OF:

   DEGRADED CORE BEHAVIOR

   HYDROGEN BURN

   STEAM GENERATION

   CORE COOLABILITY

   CONTAINMENT STRUCTURAL RESPONSE

5. DEFINE DIRECTION AND SCOPE OF ONGOING WORK

6. INDICATE ZION/INDIAN POINT STUDY FIT TO DEGRADED CORE RULEMAKING

REQUEST FOR ACRS SUPPORT

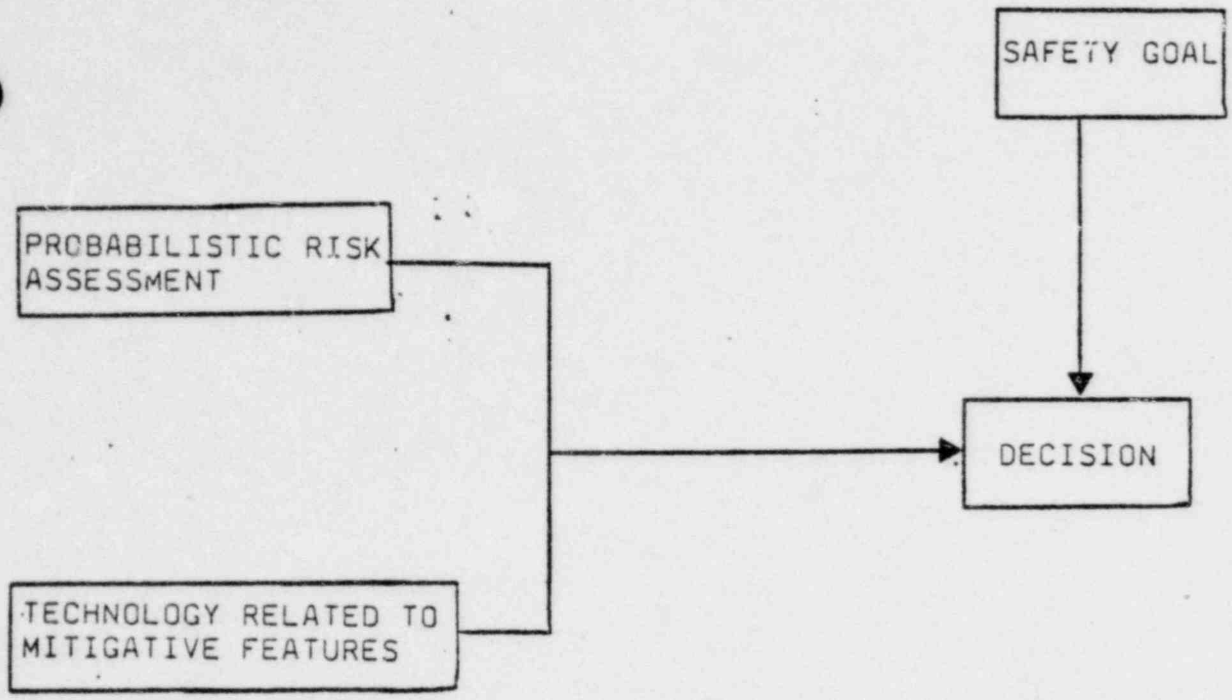
1. SAFETY GOAL

2. PROBABILISTIC RISK ASSESSMENT

3. USE OF REALISTIC BEST ESTIMATES IN ANALYSIS OF CLASS 9 ACCIDENTS

CURRENT STATUS

- CONFIRMATORY ORDERS
- ACCELERATION OF LICENSING ACTIONS
- TECHNOLOGY EXCHANGE
- NRC RESEARCH
  - PHENOMENOLOGY
  - MITIGATING FEATURES
  - PROBABILISTIC RISK ASSESSMENT
- DEGRADED CORE RULEMAKING


SAFETY GOAL
PROBABILISTIC RISK ASSESSMENT
DECISION
TECHNOLOGY RELATED TO MITIGATIVE FEATURES

MAGNITUDE OF UTILITY EFFORT

MAN-MONTHS WORK

	UTILITIES		CONSULTANTS	
12/79 TO 6/80	53		167	
7/80 TO 12/80	59		84	
TOTAL	112	MAN-MONTHS	251	MAN-MONTHS

COMPUTER EXPENSES

PICKARD, LOWE & GARRICK	$ 180,000
WESTINGHOUSE	350,000
TOTAL	$ 530,000

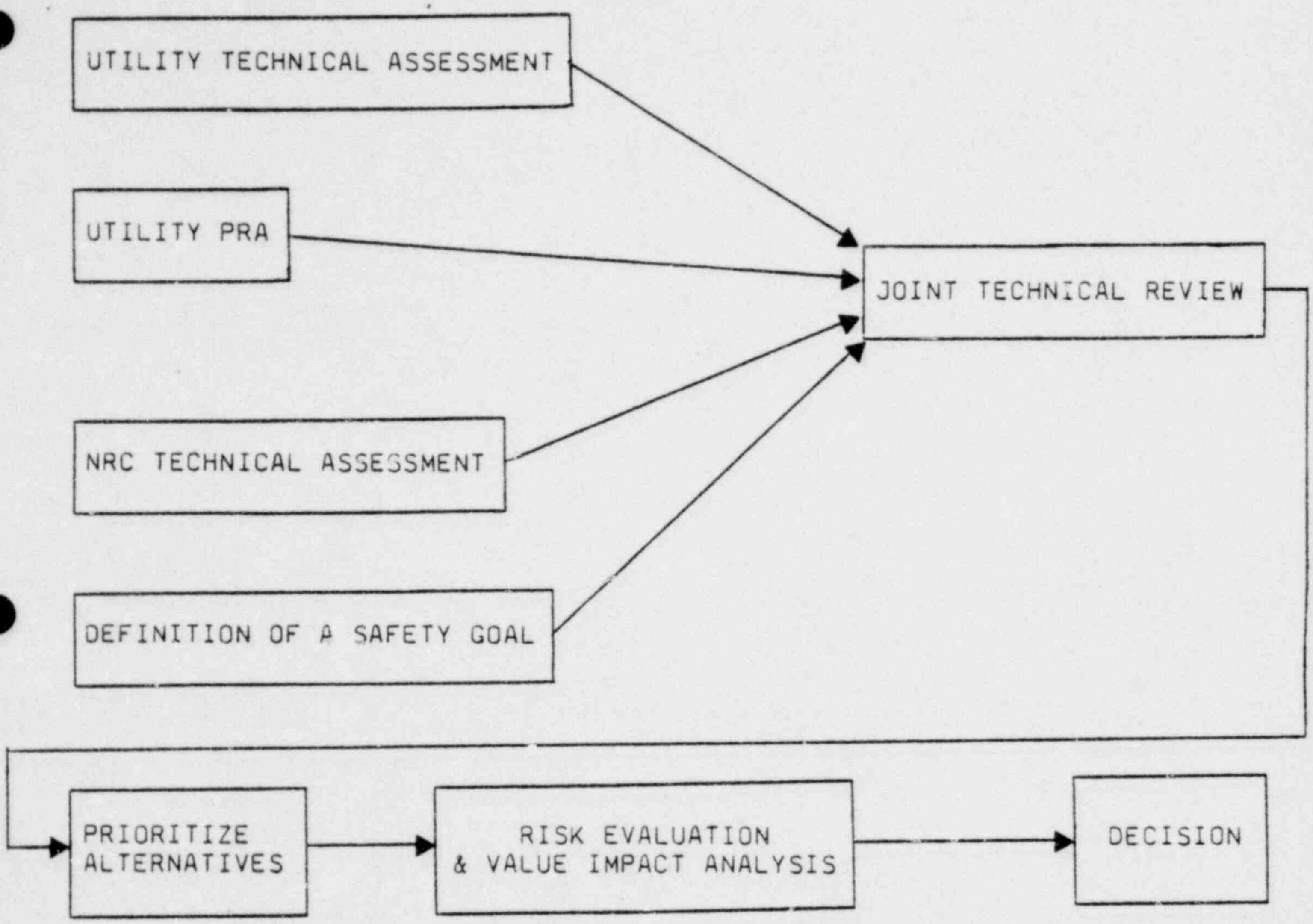
UTILITY PROGRAM PLAN

OBJECTIVES

- COMPLETE TECHNICAL ASSESSMENT
  - PROBABILISTIC RISK ASSESSMENT
  - HYDROGEN BEHAVIOR
  - SENSITIVITY STUDIES
  - CONCEPTUAL DESIGNS OF MITIGATING FEATURES
- COMMON TECHNICAL BASE
- PREPARATION FOR DEGRADED CORE RULEMAKING

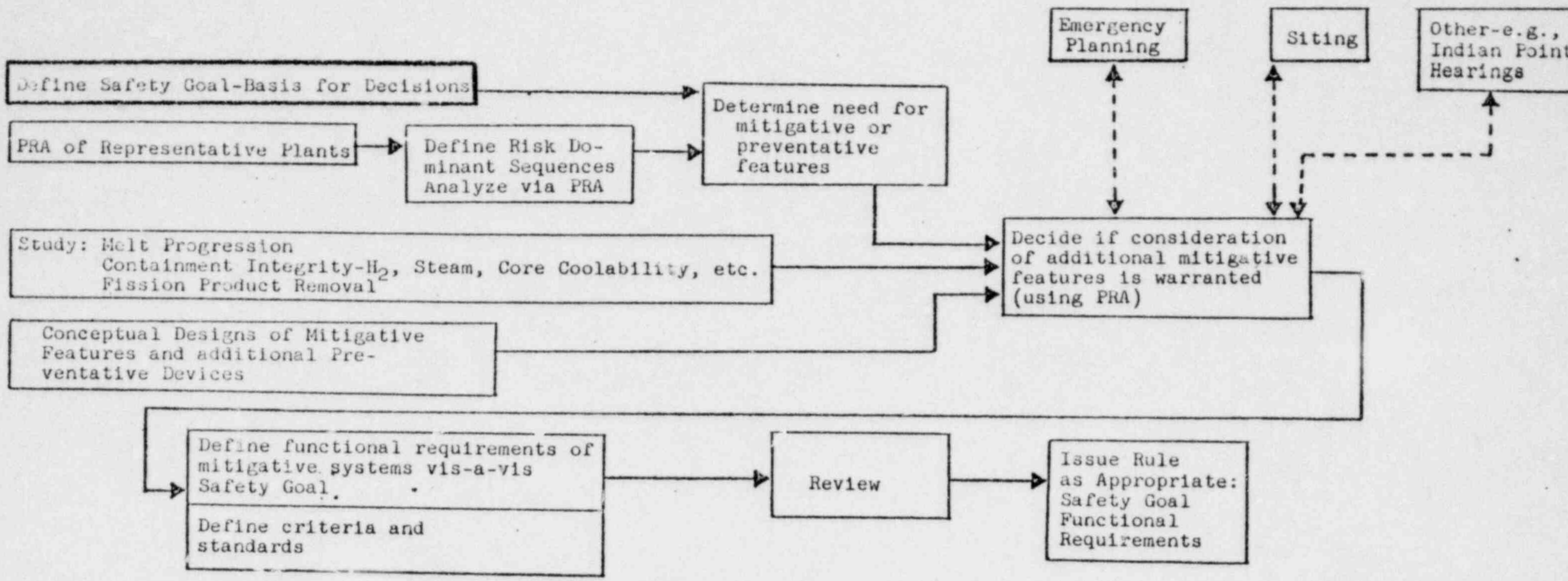
TASKS TO ACCOMPLISH

ZION/INDIAN POINT PROGRAM



Degraded Core Rulemaking

Functional Diagram



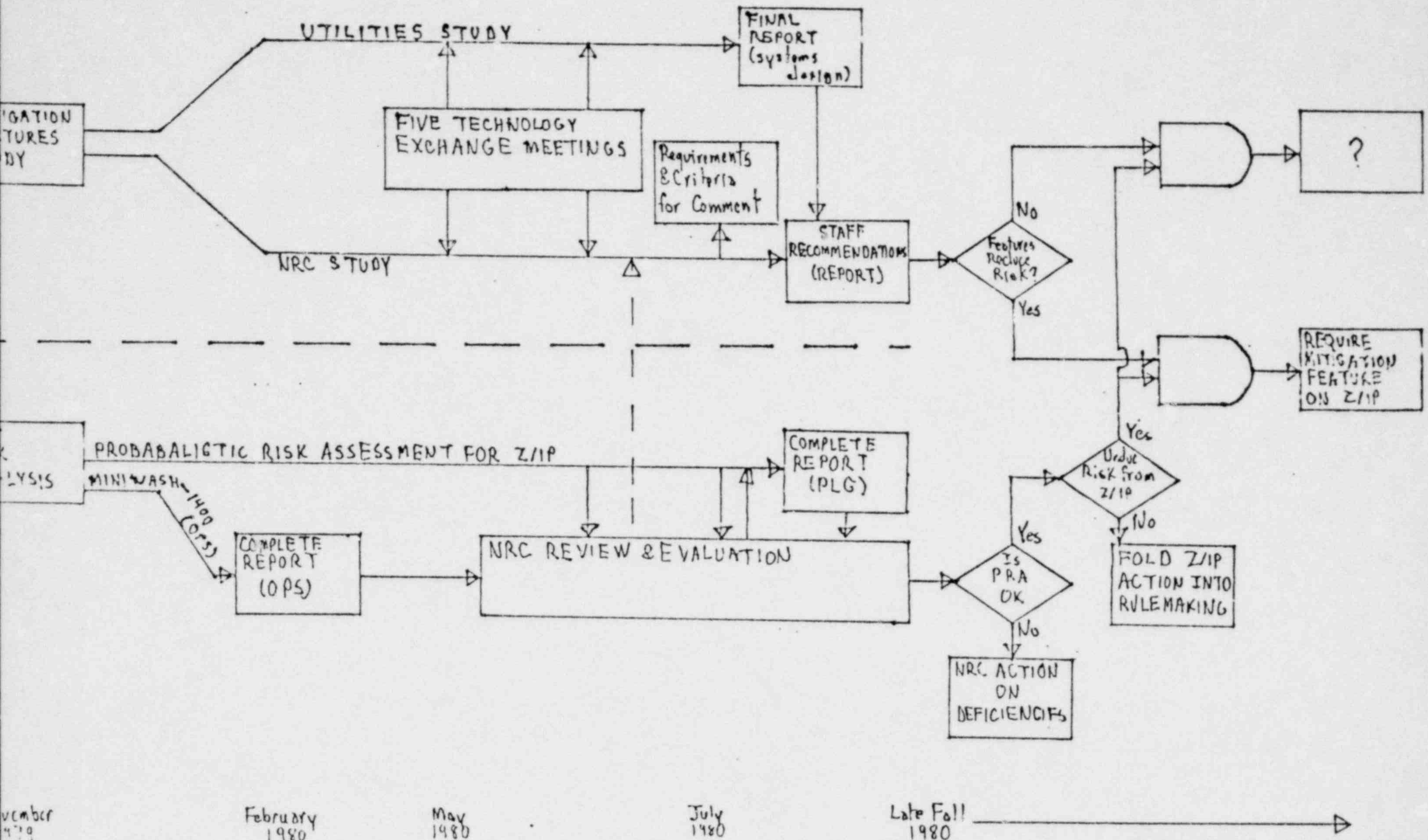
Slide 8

MEYER

NRC/NRR PRESENTATION AT
The ACRS SUBCOMMITTEE MEETING on
CLASS 9 ACCIDENTS

James F. Meyer
July 2, 1980

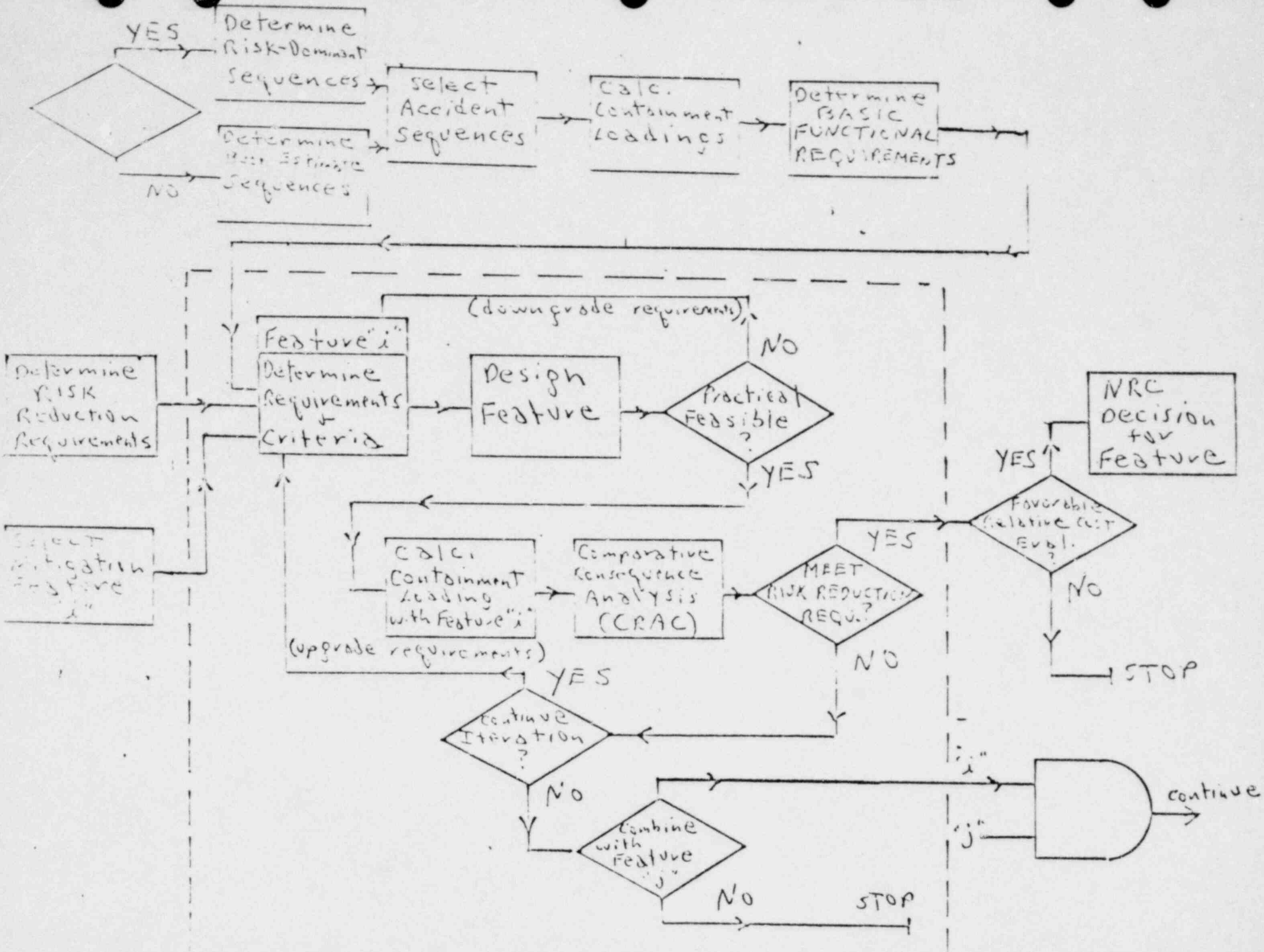
UPDATE: APPROACH FOR ZION/INDIAN POINT ACTION



BASES FOR DEVELOPING Z/IP MITIGATION FEATURES: REQUIREMENTS AND CRITERIA

1. REDUCE RISK.
2. PREVENT CONTAINMENT FAILURE (BY OVERPRESSURE OR BASEMAT MELT THROUGH).
3. DEFINE FUNCTIONAL REQUIREMENTS FOR SYSTEM WHICH WILL PREVENT CONTAINMENT FAILURE.
4. DEFINE/DESIGN SYSTEM THAT MEETS REQUIREMENTS.
5. ASSESS CONSEQUENCE MITIGATION CAPABILITIES OF SYSTEM.
6. ASSESS "RELIABILITY" OF SYSTEM.

BASES FOR DEVELOPING Z/IP MITIGATION FEATURES: REQUIREMENTS & CRITERIA



SAMPLE CRITERIA AND REQUIREMENTS FOR FVCS

- PRESSURE FOR VENTING INITIATION: 100 PSIA
- FLOW RATE (EXITING CONTAINMENT): 150,000 CFM
- DECONTAMINATION FACTORS:

  100 PARTICULATES
  100 IODINE
- SYSTEM - PASSIVE (BUT WITH RECIRCULATION CAPABILITY FOR LONG ΔT)
- SYSTEM - PRESSURE RELIEF TYPE
- SEISMIC CAT. 1
- SYSTEM - SUPPRESSION POOL WITH SUBMERGED GRAVEL

PRELIMINARY GENERAL DESIGN CRITERIA
FOR CORE RETENTION SYSTEM

- DESIGN FOR FULL CORE MELTDOWN, ONE (1) HOUR AFTER REACTOR SHUTDOWN
- DESIGN FOR PERMANENT RETENTION OF CORE DEBRIS
- PROTECT CONCRETE WITH A REFRACTORY MATERIAL THAT DOES NOT GENERATE GASES WHEN IT INTERACTS WITH MOLTEN CORE DEBRIS
- PROVIDE A COOLING SYSTEM TO DISSIPATE HEAT TRANSFERRED TO REFRACTORY MATERIAL (NATURAL & FORCED CONVECTION WILL BE CONSIDERED)
- PREVENT CORE DEBRIS ATTACK OF CONCRETE
- VENT GAS AND VAPOR GENERATION FROM CONCRETE (I.E., ELIMINATE SPARGING)
- INCORPORATION OF CORE RETENTION SYSTEM SHALL NOT COMPROMISE DESIGN BASIS SAFETY REQUIREMENTS

UPDATE ON Z/IP MITIGATION FEATURES STUDY

1. TECHNOLOGY EXCHANGE MEETINGS CONCLUDED.
2. MITIGATION FEATURE REQUIREMENTS AND CRITERIA -- ISSUED BY NRC FOR COMMENT (JULY 1980).
3. STAFF REPORT DUE LATE FALL.
4. LICENSEE REPORT DUE LATE SUMMER.
5. RES AND NRR PROGRAMS PROCEEDING THROUGH SUMMER ON KEY ISSUES.

RES RESEARCH TO MEET Z/IP NEEDS

NOTE: IN ORDER TO MEET NEEDS, CERTAIN WORK MUST BE COMPLETED IN A 4-MONTH TIME FRAME (NEAR-TERM) AND OTHER WORK IN A 15 MONTH TIME FRAME (LONG-TERM)

- STEAM SPIKE PHENOMENA: EXPERIMENTAL AT FITS FACILITY (NEAR-TERM)
- DEBRIS BED FRAGMENTATION CHARACTERIZATION:
  SURVEY IN COOPERATION WITH STEEL INDUSTRY (NEAR-TERM)
- CORE-MELT/CONCRETE & CORE-MELT/REFRACTORY MAT'L INTERACTION
  PHENOMENOLOGY: EXPERIMENTAL AT SANDIA TEST FACILITY (LONG-TERM)
- CORE-MELT/CONCRETE/WATER INTERACTION PHENOMENOLOGY: EXPERIMENTAL
  AT SANDIA TEST FACILITY (LONG-TERM)
- FVCS ANALYSIS IN AREAS OF COST BENEFIT, FAILURE MODES AND EFFECTS
  AND RELIABILITY (NEAR-TERM)
- BACKFIT C.R.D. DESIGNS FOR Z/IP: ANALYTICAL AT SANDIA (NEAR-TERM)
- HYDROGEN CONTROL SYSTEMS ASSESSMENT (NEAR-TERM)
- CONTAINMENT FAILURE MODES: ANALYSIS AT LASL AND SANDIA (NEAR-TERM)

LONG TERM NEEDS

NOTE: IN ORDER TO MEET "RULEMAKING" NEEDS WORK MUST BE COMPLETED IN A 2-3 YEAR TIME FRAME

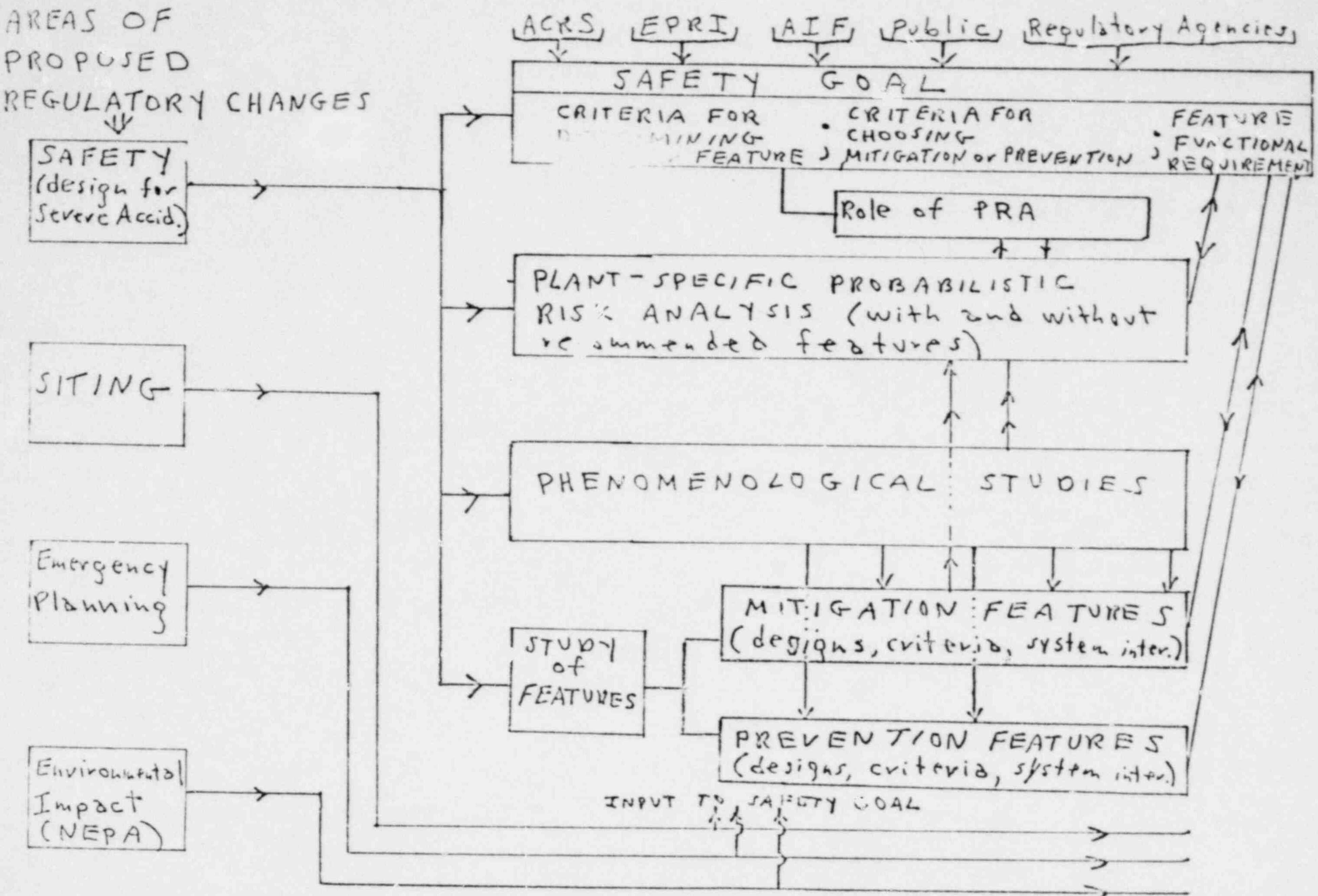
- CONTINUATION AND EXPANSION (TO OTHER REACTOR TYPES) OF PROGRAMS LISTED UNDER Z/IP NEEDS

AND IN ADDITION

- HYDROGEN MITIGATION AND BURNING: EXPERIMENTAL AND ANALYTICAL
- CORE-MELT ACCIDENT PROGRESSION: EXPERIMENTAL AND ANALYTICAL AT IDAHO & SANDIA
- RADIOLOGICAL SOURCE TERM

PROGRAM PLAN:

SEVERE ACCIDE (CLASS - 9)



HENRY

STEAM EXPLOSION

AND

CORE COOLABILITY

R. E. Henry
Fauske & Associates, Inc.

STEAM EXPLOSIONS

IN-VESSEL STEAM EXPLOSIONS

I. ELEVATED SYSTEM PRESSURES (> 150 PSIA)

- STEAM EXPLOSIONS WILL NOT OCCUR.

II. LOW SYSTEM PRESSURES (<150 PSIA)

- PROBABILITY OF A STEAM EXPLOSION SHOULD BE ASSUMED TO BE UNITY.
- A CONTINUOUS OVERLYING LIQUID LAYER CANNOT BE FORMED FOR ANY REASONABLE LEVELS OF FRAGMENTATION.
- A STEAM EXPLOSION IN THIS ENVIRONMENT WOULD RESEMBLE A SHALLOW UNDERWATER EXPLOSION, I.E., EXPANDING STEAM BUBBLE WOULD QUICKLY BREAK THROUGH A LIQUID LAYER.
- PROBABILITY OF VESSEL FAILURE BY A STEAM EXPLOSION IS INSIGNIFICANT - CONSEQUENTLY, THE PROBABILITY OF CONTAINMENT FAILURE IS ALSO INSIGNIFICANT.

EX-VESSEL STEAM EXPLOSIONS

- Low Pressure - The Probability Of A Steam Explosion Should Be Assumed To Be Unity.
- Shallow Underwater Explosion Analogy Is Particularly Relevant Here.
- Short Length For A Slug Acceleration.
- In-Core Instrument Shaft Is A Vent For The Explosion - i.e., No Long Term Acceleration (missile) Potential.
- Shock Waves From A Coherent Explosion Within The Reactor Cavity Would Not Be Sufficient To Fail The Containment Wall.

STEAM EXPLOSIONS

- CONCLUSIONS -

I. In-Vessel

- Elevated System Pressure-Steam Explosions Will Not Occur.
- Low System Pressures - Steam Explosions Can Occur, But Would Not Fail The Reactor Vessel- No Continuous Overlying Liquid Layer Can Be Formed.

II. Ex-Vessel

- Steam Explosions Can Occur, But The Shock Waves Generated Would Be Much Less Than The Containment Design Pressure.

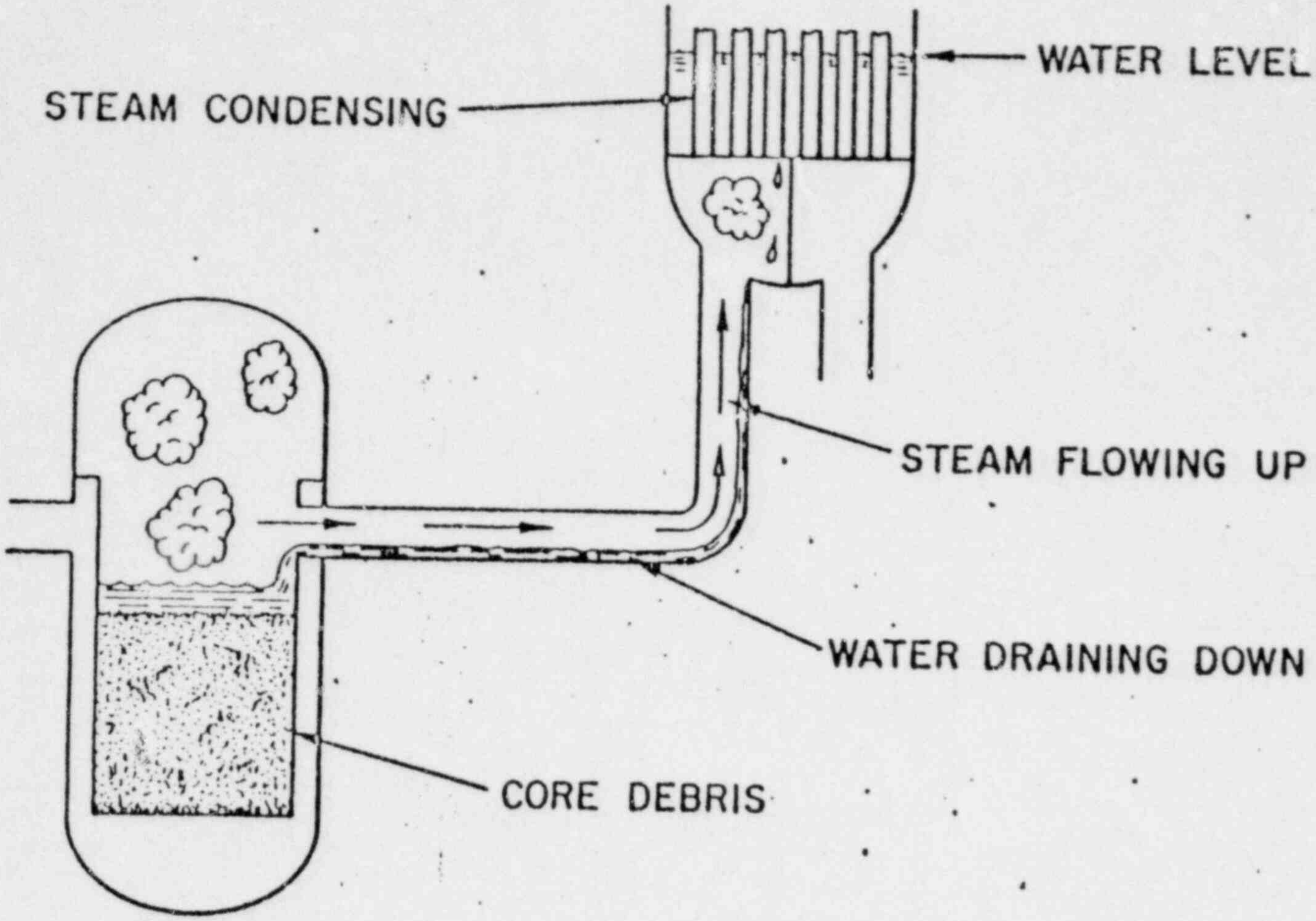
STEAM EXPLOSIONS

CONCLUSIONS

WHILE THE METHOD OF CALCULATION IS SOMEWHAT DIFFERENT, THE VARIOUS ORGANIZATIONS ALL CONCLUDE THAT FAILURE OF THE CONTAINMENT STRUCTURE AS A RESULT OF EITHER AN IN-VESSEL OR EX-VESSEL STEAM EXPLOSION IS HIGHLY UNLIKELY.

CORE COOLABILITY

IN-VESSEL CORE COOLABILITY


WATER LEVEL
STEAM CONDENSING
STEAM FLOWING UP
WATER DRAINING DOWN
CORE DEBRIS

REFLUX PATH

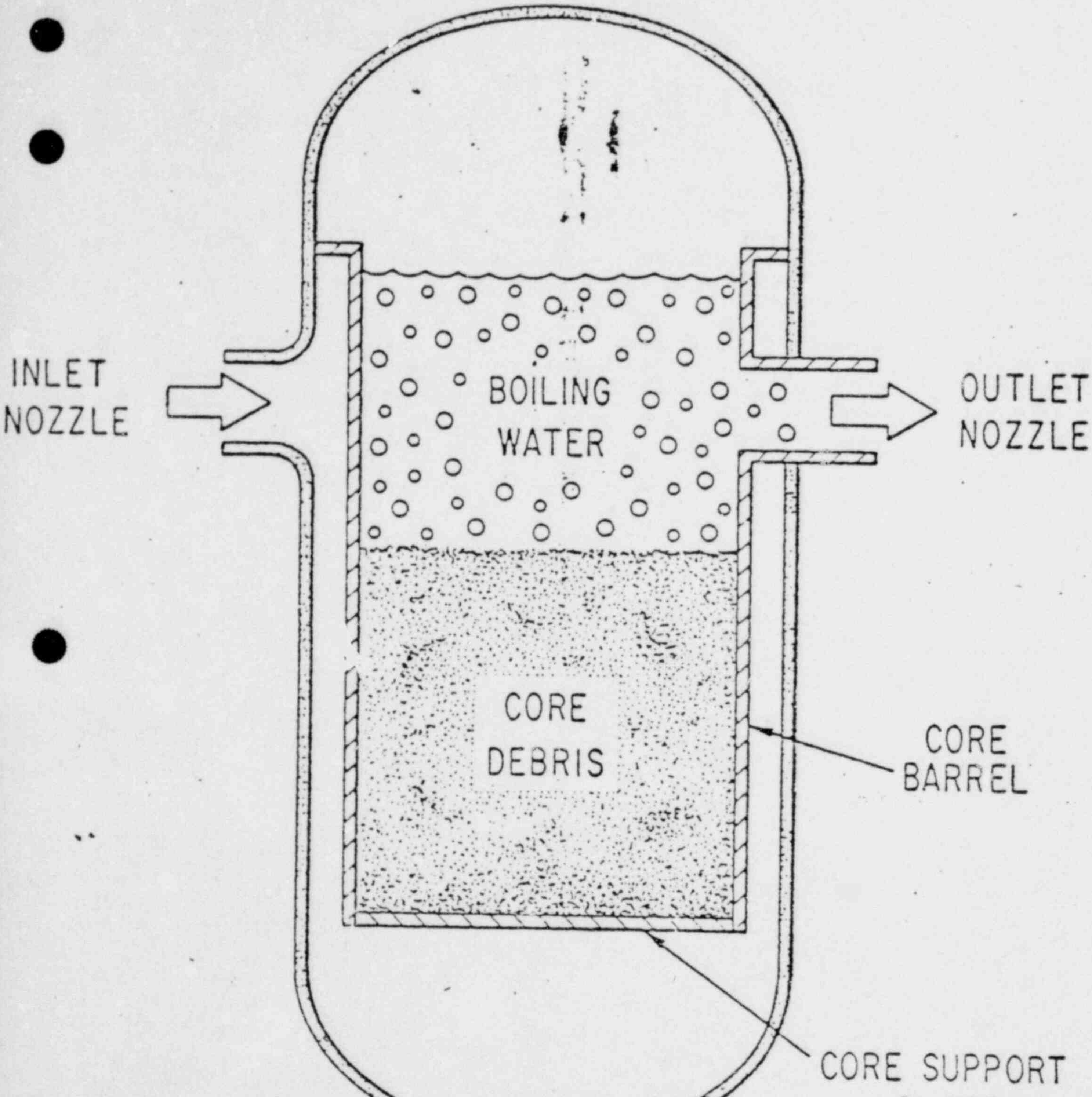
SPECIFIC CHARACTERISTICS OF ZION-INDIAN POINT SYSTEMS WHICH ARE ADVANTAGEOUS FOR IN-VESSEL HEAT REMOVAL

1. BOTTOM ENTRY ELEVATED STEAM GENERATORS MAXIMIZES THE WATER WHICH MUST BE LOST FROM THE PRIMARY SYSTEM BEFORE CORE UNCOVERY BEGINS.

2. INJECTION CAPABILITY IN THE HOT LEGS FOR PRESSURES LESS THAN ABOUT 1500 PSIA.

3. BOTTOM ENTRY ELEVATED STEAM GENERATOR MEANS NONCONDENSIBLE GASES CANNOT BLOCK THE ENERGY TRANSPORT PATH FROM THE CORE TO THE HEAT SINK.

4. BOTTOM ENTRY ELEVATED STEAM GENERATORS CAN ESTABLISH A REFLUX HEAT REMOVAL PATH THROUGH THE VESSEL OUTLET PIPING.


INLET
NOZZLE
BOILING
WATER
OUTLET
NOZZLE
CORE
DEBRIS
CORE
BARREL
CORE SUPPORT
PLATE

CORE COOLABILITY

I. CHF CRITERION - VAPOR REMOVAL RATE UPWARD MUST NOT PRECLUDE LIQUID RETURNING TO THE SURFACE

$$Q/A = 0.14\, H_{fg}\sqrt{\rho_g}\left[\, g\sigma\left(\rho_f - \rho_g\right)\right]^{1/4}$$

II. DEBRIS BED LIMITATION - PRESSURE GRADIENT INDUCED BY VAPOR FLOW THROUGH THE BED MUST NOT EXCEED THE STATIC HEAD OF THE LIQUID. THIS LIMITATION APPLIES AT THE TOP OF THE BED SINCE THIS IS THE LOCALE OF MAXIMUM VAPOR VELOCITY.

$$-\frac{dP}{dZ} = 2\, C_f\, \frac{\rho_g J_g^{2}}{D}\; \frac{1-\epsilon}{\epsilon^3} < \rho_f g$$

$$C_f = \frac{75}{R_E} + 0.875$$

$$R_E = \frac{\rho_g J_g D}{(1-\epsilon)\mu_g} \qquad J_g = \frac{Q}{\rho_g A\, H_{fg}}$$

CRITICAL HEAT FLUX

$$Q/A = 0.14\, H_{FG} \sqrt{\rho_G} \left[g \sigma (\rho_f - \rho_g) \right]^{1/4}$$

P = 15 MPA $\quad \sigma = 0.005$ N/M $\quad \rho_g = 100$ KG/M^3

$H_{FG} = 1000$ KJ/KG $\quad \rho_f = 602$ KG/M^3

$Q/A = 3118$ KW/M^2 $\quad A = 12$ M^2 $\quad Q = 37.4$ MW

P = 7 MPA $\quad \sigma = 0.018$ N/M $\quad \rho_g = 36.5$ KG/M^3

$H_{FG} = 1505$ KJ/KG $\quad \rho_f = 741$ KG/M^3

$Q/A = 4220$ KW/M^2 $\quad A = 12$ M^2 $\quad Q = 50.6$ MW

P = 1 MPA $\quad \sigma = 0.045$ N/M $\quad \rho_g = 5.2$ KG/M^3

$H_{FG} = 2015$ KJ/KG $\quad \rho_f = 887$ KG/M^3

$Q/A = 2857$ KW/M^2 $\quad A = 12$ M^2 $\quad Q = 34.3$ MW

- NOT LIMITED ON THE UPPER SURFACE -

PARTICLE BED DRYOUT MODELS

1. 60 DAY STUDY

$$Q/A = H_{FG}\left[\frac{g\rho_{FG}\rho_G D\epsilon^3}{2C_F(1-\epsilon)}\right]^{\frac{1}{2}} \qquad C_F = \frac{75}{N_{Re}} + 0.875 \qquad N_{Re} = \frac{\rho_G J_G D}{\mu G}$$

SMALL PARTICLE APPROXIMATION

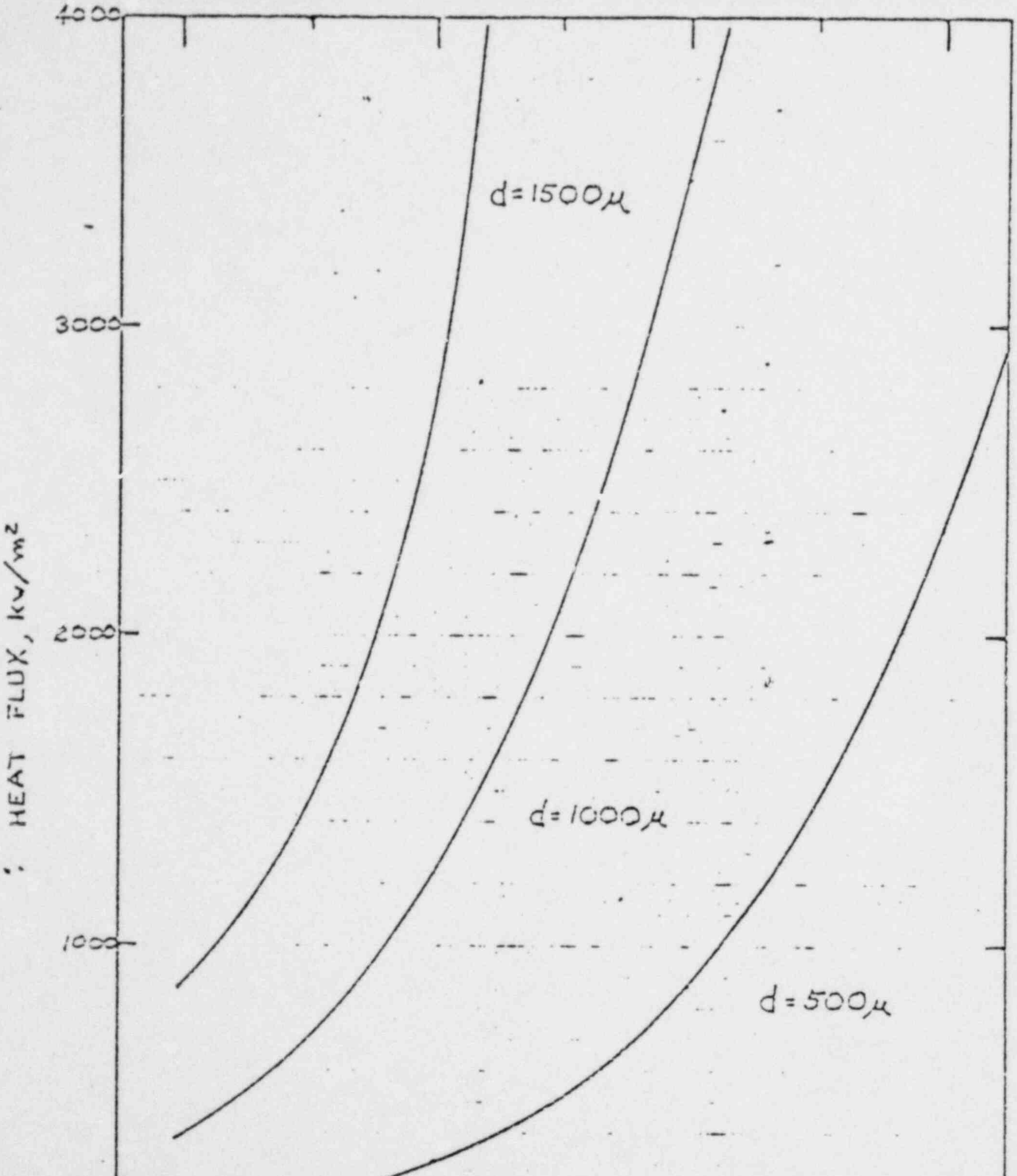
$$Q/A = \frac{g\rho_F H_{FG}}{\nu_G} \cdot \frac{D^2\epsilon^3}{150(1-\epsilon)^2}$$

2. HARDEE & NILSON

$$Q/A = \frac{g\rho_F H_{FG}}{\left[\sqrt{\nu_G} + \sqrt{\nu_F}\right]^2} \cdot \frac{D^2\epsilon^3}{180(1-\epsilon)^2}$$

3. DHIR & CATTON

$$Q/A = 0.0177\frac{g\rho_F H_{FG}}{\nu_F}(1 - \rho_G/\rho_F) \cdot \frac{D^2\epsilon^3}{180(1-\epsilon)^2}$$



4000
3000
2000
1000
0
HEAT FLUX, kW/m²
d=1500μ
d=1000μ
d=500μ
0.30
0.40
0.50
0.60
POROSITY

MODEL FROM 60 DAY STUDY

IN-VESSEL DEBRIS BEDS*

MINIMUM COOLABLE PARTICLE SIZE

PRESSURE	POROSITY	
MPa	$\epsilon = 0.4$	$\epsilon = 0.5$
1	550 μm	330 μm
2	400 μm	240 μm
4	350 μm	210 μm
7	280 μm	160 μm

*Q = 20 MW   A = 10.5 m^2

EX-VESSEL CORE COOLABILITY

CONDITIONS EVALUATED

TIME INTO THE ACCIDENT - 4 HRS.

	ZION	IP-2	IP-3
TOTAL DECAY POWER (MW)	28.6	24.3	26.7
DECAY POWER IN MELT (MW)	19.1	16.2	17.8
SURFACE AREA (M^2)	54.0	39.1	39.1
REQUIRED HEAT FLUX (KW/M^2)	354	414	455
COOLABLE PARTICLE SIZE, μM (ϵ = 0.5)			
0.1 MPA	320	374	411
0.2 MPA	254	297	327
0.5 MPA	183	214	235

DEBRIS BED COOLABILITY

AGREEMENT

1. AGREEMENT ON MANNER IN WHICH BED DRYOUT HEAT FLUXES ARE CALCULATED.

2. AGREEMENT ON THE STRONG SENSITIVITY OF BED DRYOUT TO BOTH PARTICLE SIZE AND BED AVERAGE POROSITY.

3. AGREEMENT THAT THE APPLICABLE DATA IN THE LITERATURE IS VERY LIMITED AND NOT PARTICULARLY WELL CHARACTERIZED.

FURTHER DISCUSSION

1. THE SENSITIVITY OF DEBRIS DISTRIBUTION TO THE ACCIDENT SCENARIO.

STEAM SPIKE

MAXIMUM QUENCHING RATE

TUNNEL AREA = 7.2 M^2

$$K = \frac{U_G \sqrt{\rho_G}}{\sqrt[4]{G\sigma(\rho_F - \rho_G)}} = 3$$

ASSUME P = 0.3 MPa $\rho_F = 932$ KG/M^3 $\rho_G = 1.65$ KG/M^3

$U_G = 10.8$ M/SEC

$\dot{M}_G = \rho_G A U_G = 128$ KG/SEC

$\dot{Q} = \dot{M}_G H_{FG} = 2.8 \times 10^5$ KW

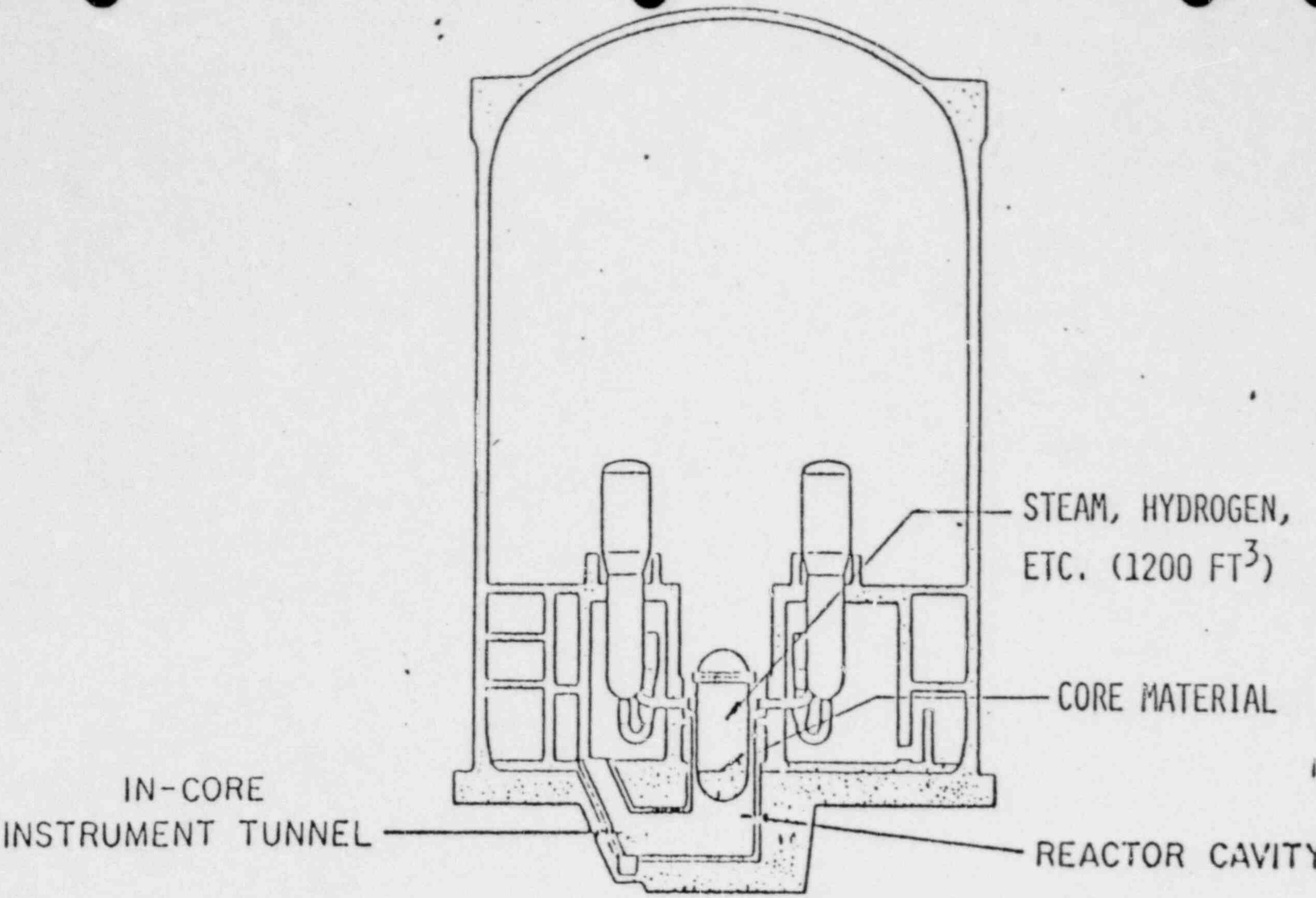
$Q = MC_P \Delta T = 1.3 \times 10^8$ KJ

$\Delta\Theta = Q/\dot{Q} = 464$ SEC $= 7.7$ MIN

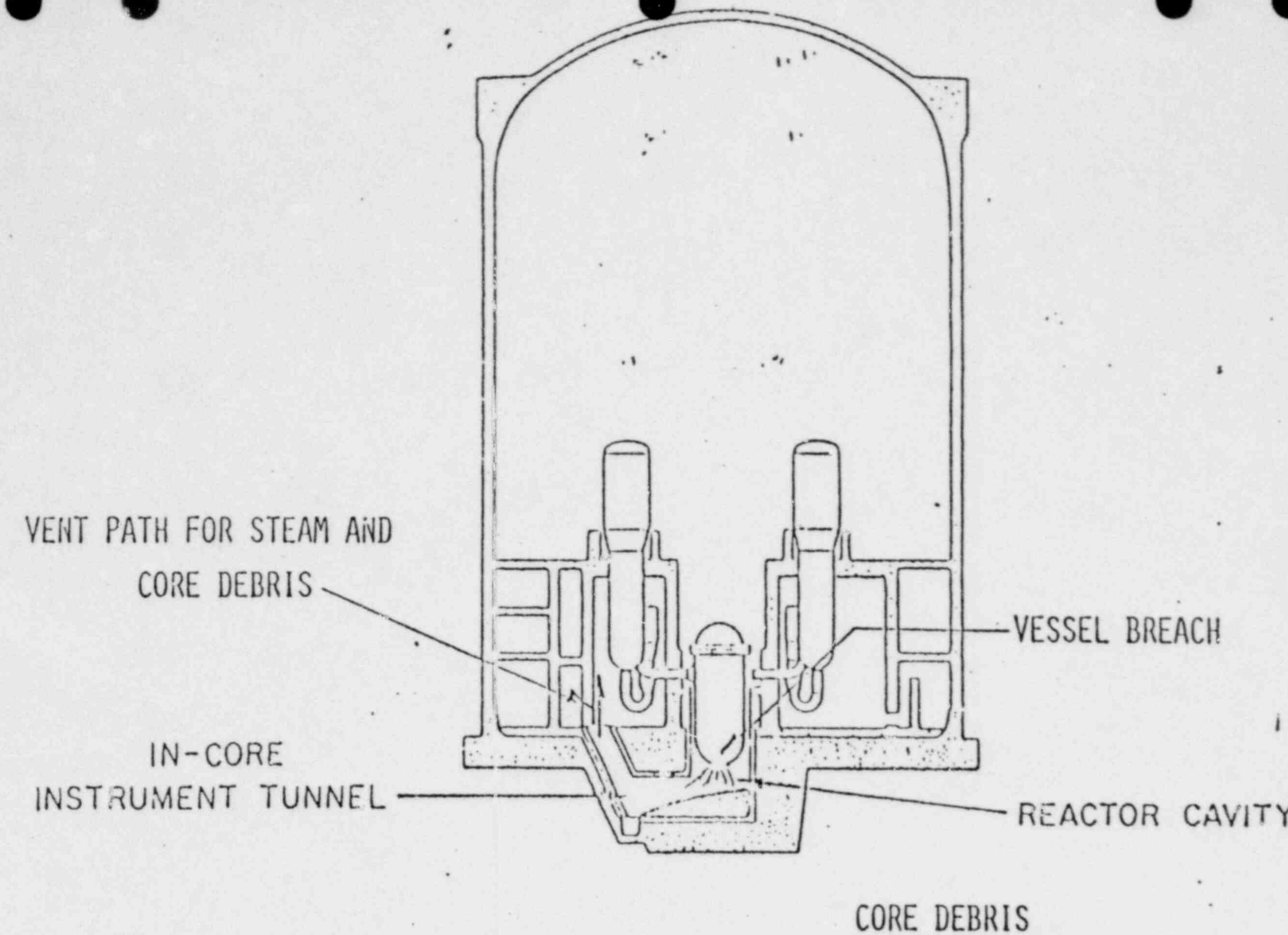
MECHANICAL DISPERSION

OF

CORE MATERIAL

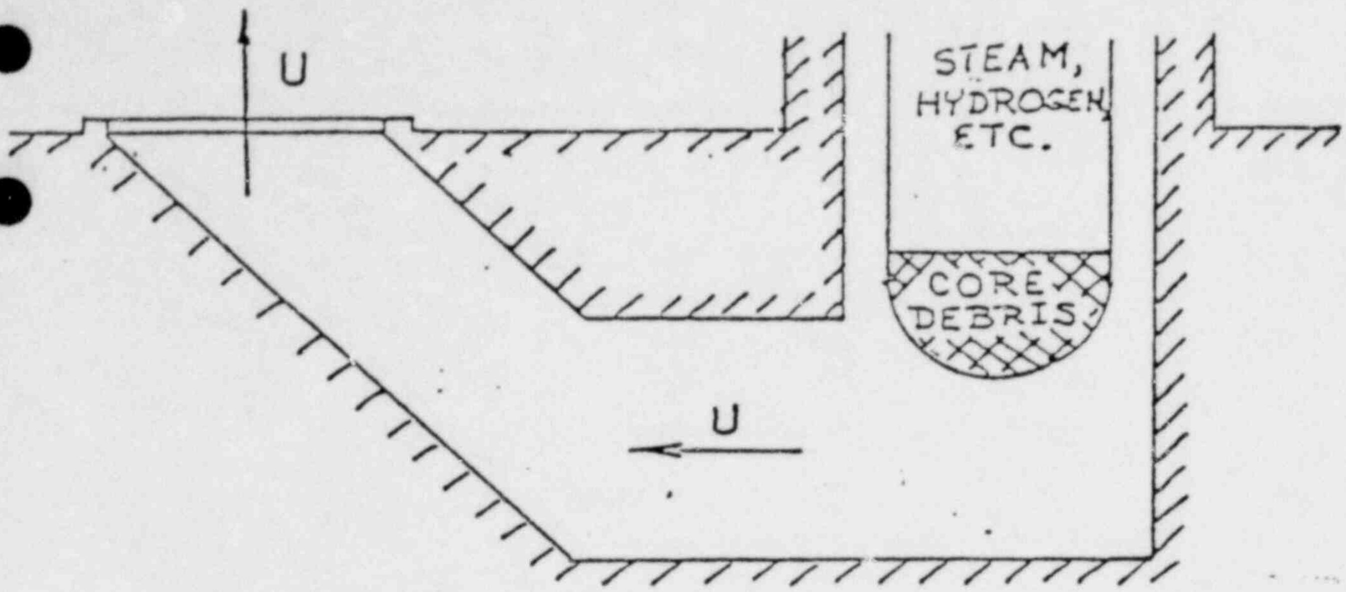


REACTOR BUILDING



REACTOR BUILDING

DISPERSAL CALCULATIONS

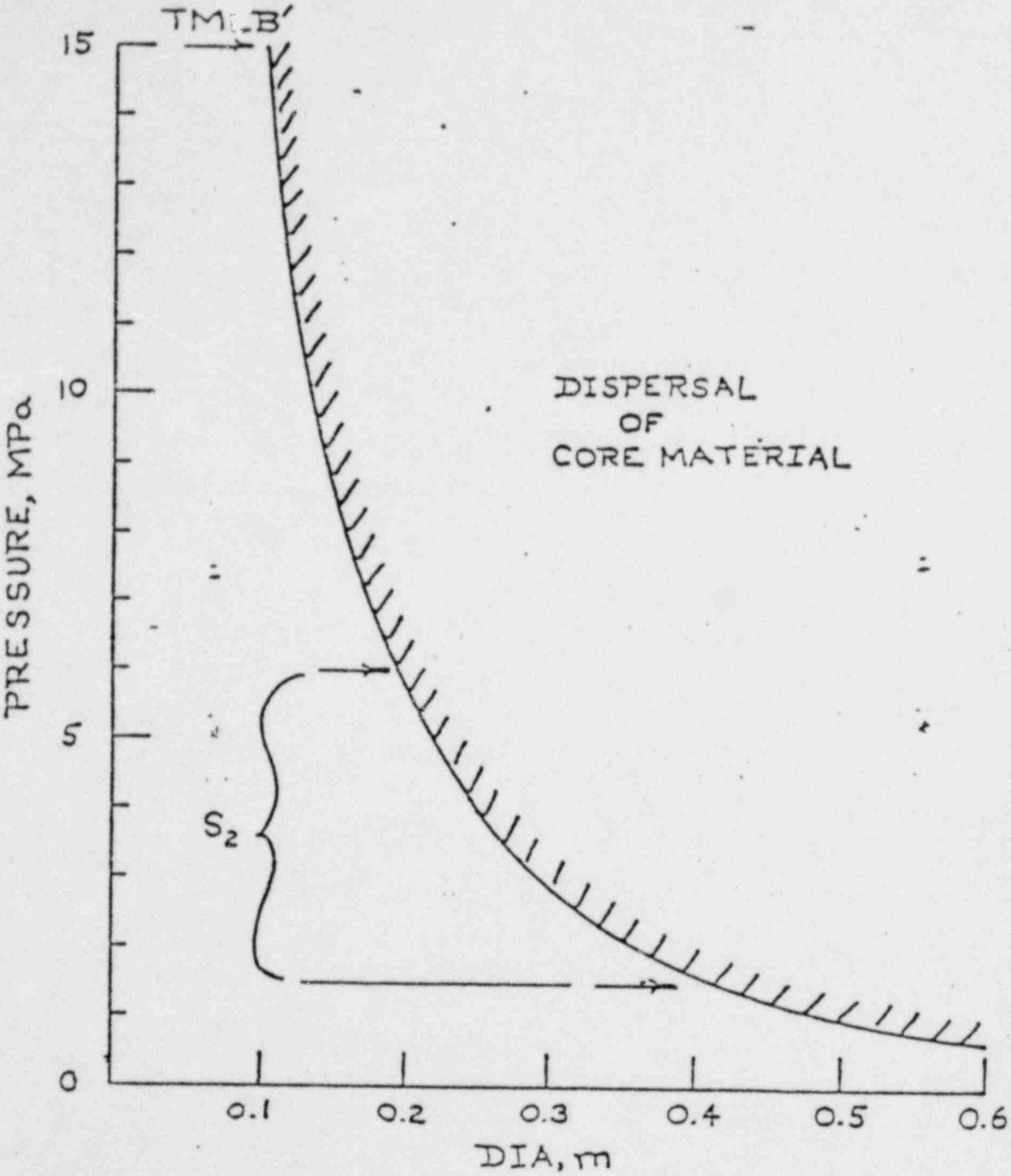


$$U = \frac{3\sqrt[4]{g\sigma(\rho_F - \rho_G)}}{\rho_G} \qquad W = \rho_G A_T U$$

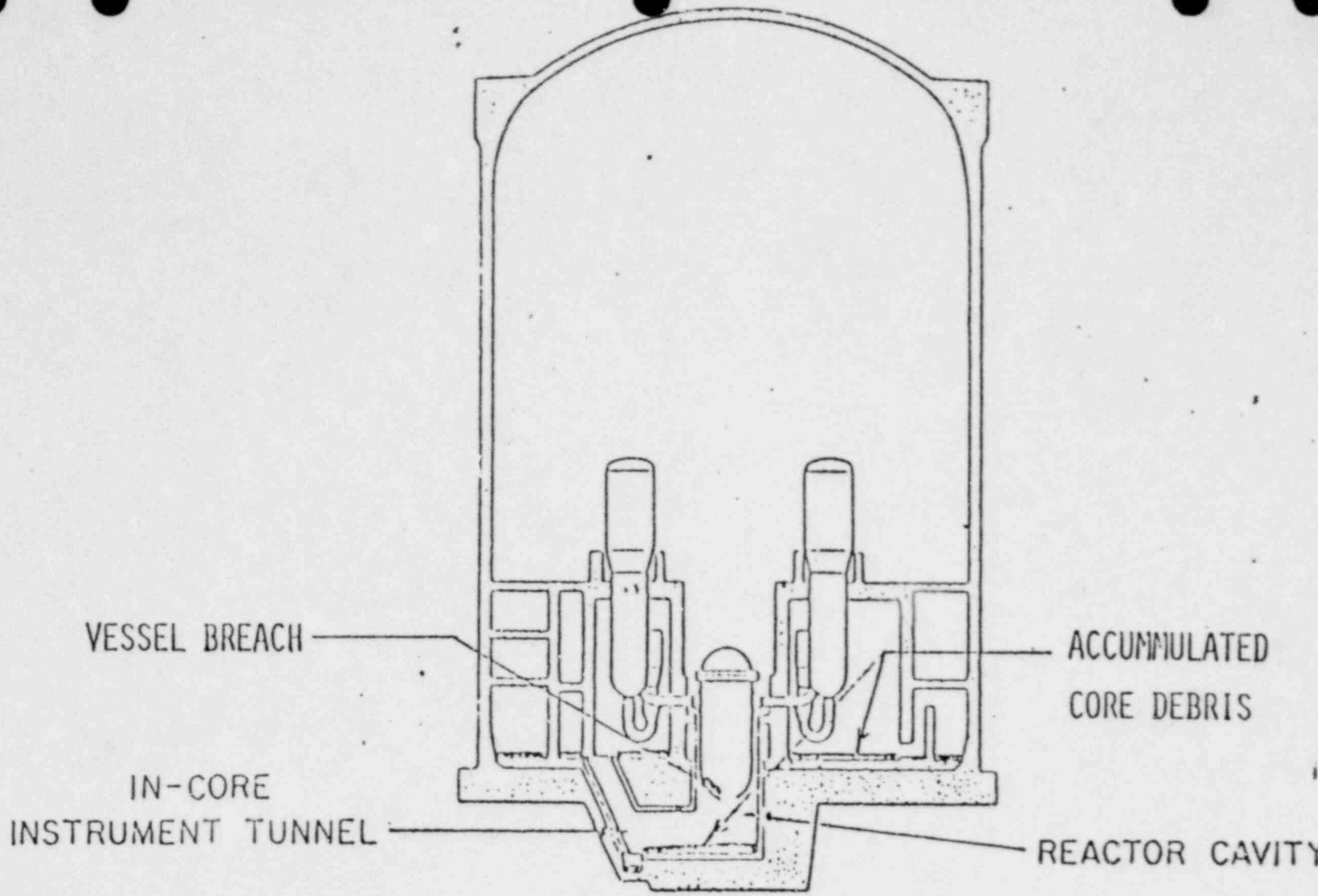
$$G = \eta \, Po/\sqrt{RT} \qquad A_V = W/G$$

$$D_F = \frac{3}{8}\,\frac{\rho_G U^2}{\rho_F g}$$

$$v_0 = \frac{RT}{P_0} \qquad M_0 = \frac{V_V}{v_0} \qquad \Delta\theta = \frac{M_0}{W}$$



DISPERSAL POTENTIAL VERSE VESSEL BREACH DIAMETER



REACTOR BUILDING

SUMMARY

DISPERSIVE CHARACTERISTICS OF WATER AND CORE MATERIAL

1. MOST ACCIDENT SCENARIOS HAVE SIGNIFICANT DISPERSIVE POTENTIAL FOR CORE MATERIAL.

2. IF THE CORE MATERIAL REMAINS IN THE REACTOR CAVITY AND INSTRUMENT TUNNEL (NO STEAM EXPLOSION) THE QUENCHING RATE (STEAM SPIKE) IS FIRST LIMITED BY THE RATE AT WHICH WATER CAN ENTER THE TUNNEL AND SECONDLY BY CRITICAL HEAT FLUX ON THE SURFACE OF THE DEBRIS. IN THIS CASE, THE DEBRIS IS QUITE LARGE (SEVERAL MM IN DIAMETER).

3. IF A STEAM EXPLOSION OCCURS, THE DISPERSIVE FORCES WILL BE LARGE AND THE SMALL DEBRIS IN PARTICULAR WILL BE DISPERSED THROUGHOUT THE CONTAINMENT.

DISPERSED CONDITIONS

- Available surface area $\sim 800\ m^2$
- Amount of cor' debris $\sim$100,000 kg
- Bed porosity $\epsilon \sim 0.5$
- Bed depth $\sim$3 cm

CONCLUSIONS

- MECHANICAL DISPERSIVE POTENTIAL IS LARGE AND PROBABLY DICTATES THE FINAL CORE DEPOSITION.

- WATER IS AVAILABLE ON A CONTINUOUS BASIS ON ALL SURFACES WHERE SIGNIFICANT FUEL ACCUMULATION CAN OCCUR.

- DISPERSED CORE IS COOLABLE AND NO SIGNIFICANT ATTACK OF THE CONCRETE OCCURS.

CONSEQUENCES OF A MELTDOWN

I. STEAM EXPLOSION

A. IN VESSEL

1. ELEVATED PRESSURE - NO STEAM EXPLOSION, NO CONTAINMENT FAILURE.

2. LOW PRESSURE - NO CONTINUOUS OVERLYING LIQUID SLUG, NO VESSEL FAILURE, NO CONTAINMENT FAILURE, PERHAPS SOME FRAGMENTATION - BUT NOT THE ENTIRE CORE, INCOHERENCE OF MELTDOWN PLAYS A MAJOR ROLE.

B. EX-VESSEL - SHOCK WAVE FROM A VERY ENERGETIC STEAM EXPLOSION IS EASILY ACCOMMODATED BY THE CONTAINMENT WALL - NO CONTAINMENT FAILURE.

II. CORE COOLABILITY

A. IN-VESSEL - NO FINE FRAGMENTATION, CORE IS COOLABLE AS SOON AS WATER IS AVAILABLE, HEAT SINK CANNOT BE BLOCKED BY NONCONDENSIBLE GASES.

B. EX-VESSEL

1. SIGNIFICANT DISPERSIVE POTENTIAL FOR ACCIDENTS IN GENERAL.

2. STEAM EXPLOSION WOULD ALSO DISPERSE THE CORE MATERIAL.

3. DISPERSED DEBRIS BEDS ARE COOLABLE.

4. WATER IS AVAILABLE IN THE REACTOR CAVITY, ON ALL HORIZONTAL SURFACES, AND ON THE WALLS - MINIMIZES OR ELIMINATES CONCRETE ATTACK.

TMI HYDROGEN BURN ANALYSIS

TMI DATA

- ΔP = 28 PSI

- HYDROGEN PERCENT AFTER BURN = 1.7%

- BASED ON BUREAU OF MINES DATA IN ORDER TO HAVE 1.7% HYDROGEN UNBURNED AN INITIAL CONCENTRATION OF APPROXIMATELY 9.2% IS NEEDED.

- OXYGEN BALANCE

  - MEASURED TWO OXYGEN SAMPLES AFTER BURN
    PERCENTS OXYGEN WERE 15.7% AND 16.5%

  - ASSUME AIR WAS 21% OXYGEN
    THEN OXYGEN REACTED CAN BE CALCULATED
    OXYGEN REACTED IS 5.3% AND 4.5%

  - THIS INDICATES THAT HYDROGEN REACTED WAS BETWEEN 10.6% AND 9.0%. THIS IS CONSISTENT WITH UNBURNED HYDROGEN PERCENT.

TMI HYDROGEN FLAME TEMPERATURE CALCULATION

- INITIAL CONDITIONS
  - P_{TOTAL} = 16.2 PSIA
  - T = 115^{o}F
  - P_{STEAM} = 1.575 PSIA (SATURATED)

HYDROGEN PERCENT (%)	FLAME TEMPERATURE (^{o}C)	ΔP_{MAX} (ADIABATIC 100% REACTION) (PSI)	FRACTION BURNED (%)	ΔP_{ACTUAL} (PSI)
9.2	767	44.9	77	34.6
9.0	753	44.0	75	33.0
8.5	719	41.8	57	23.8
8.0	677	39.7	16.3	6.5

- AT 9.2% HYDROGEN THE FLAME TEMPERATURE OF 767^{o}C IS WELL ABOVE OUR TEMPERATURE CRITERIA

- DIFFERENCE BETWEEN MEASURED ΔP AND CALCULATED ΔP PROBABLY DUE TO HEAT LOSSES AND INSTRUMENT RESPONSE

UNITED STATES
NUCLEAR REGULATORY COMMISSION
WASHINGTON, D. C. 20555

June 12, 1980

INFORMATION REPORT

SECY-80-283

FOR: The Commissioners

FROM: Edward J. Hanrahan, Director
Office of Policy Evaluation

Leonard Bickwit, Jr.
General Counsel

SUBJECT: REPORT OF THE TASK FORCE ON INTERIM OPERATION OF INDIAN POINT (DOCKET NOS. 50-247 AND 50-286)

CONTACT:
Robert Bernero, RES, 492-8528 (Section 1)
George Eysymontt, OPE, 634-3302 (Section 2)
George Sege, OPE, 634-3295, (Section 3 and general)

SECY NOTE: Copies of this paper were advanced to each Commissioner on June 12, 1980. One copy of NUREG-0340, which is referenced in the report has been provid to each Commissioner for information.

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ACRS
Secretariat

CONTENTS

INTRODUCTION

This report is submitted in response to Section D. The Task Force on Interim Operation, of the Commission's Order of May 30, 1980, in the Matter of Consolidated Edison Company of New York, Inc. (Indian Point, Unit No. 2) and Power Authority of the State of New York (Indian Point, Unit No. 3). (Docket Nos. 50-247 and 50-286.)

The May 30 Order established an approach, including adjudication, for resolving the issues raised by a petition by the Union of Concerned Scientists (UCS) that called, among other things, for shutdown of Indian Point Units 2 and 3. The Director of the Office of Nuclear Reactor Regulation had issued a decision regarding that petition on February 11, 1980.

Section D of the May 30 Order directed the General Counsel and the Director, Office of Policy Evaluation, to establish a task force to prepare a report to the Commission on information available at this time that bears on the question of whether to permit, prohibit, or curtail operation of Indian Point Units 2 and 3 during pendency of the adjudication. The task force report was to include information on at least certain specified topics listed in the Order. The topics fall into two categories: accident risk considerations (items 1 to 4 of Section D, at pages 6-7 of the Order) and social and economic impact considerations (item 5, at page 7 of the Order).

The accident risk considerations are addressed in Section 1 of this report. Those considerations include comparative site demography; accident risk comparisons; effects of emergency response; and effects of differences between Units 2 and 3, of changes ordered by the Director of NRR, and of power-level reduction. Effects of uncertainties are discussed. Some explanatory details are appended. (Appendices A and B)

Social and economic impact considerations are addressed in Section 2. The principal considerations addressed include effects of shutdown or power reduction on (a) reliability of the electric power supply for the region, including New York City, and (b) sources and cost of electrical energy. Supporting information from the Department of Energy is appended. (Appendix C)

Public comments relevant to interim operation or shutdown, received in response to the Commission's February 15 solicitation of comments, are summarized in Section 3.

The principal contributors to this work were Robert M. Bernero, Roger M. Blond, W. Clark Pritchard and Merrill A. Taylor, of the Office of Nuclear Regulatory Research; and George Eysymontt and George Sege, of the Office of Policy Evaluation.

SECTION I. ACCIDENT RISK CONSIDERATIONS

This section presents estimates of the accident risk posed by operation of the plants in their present condition; a comparison of the risk from other sites and designs; the sensitivity of that risk to emergency protective measures, and the sensitivity of risk to a reduction in power level during operation.

THE POPULATION DISTRIBUTION

The Indian Point Power Station, with New York City less than 50 miles to the south, has the largest population in its immediate surroundings of any nuclear power station in the United States. "Demographic Statistics Pertaining to Nuclear Power Reactor Sites," NUREG-0348, tabulates all U. S. nuclear power stations according to the total population within a circle of given radius from the reactor. Tables 1, 2, and 3 show the populations at distances of 10, 30 and 50 miles based upon the 1970 census. The region around the Indian Point station is the most densely populated as shown by these data.

When considering reactor accident risk, the population in a given direction, (i.e., in one 22½ degree sector), is often more significant than population density averaged over all directions. Reactors have been ranked by their sector population in Table 4. Here too, Indian Point ranks among the highest. However, a number of other U. S. reactor sites, for example, Zion and Limerick, also have relatively high populations in their vicinity.

TABLE 1

Population Statistics Between 0 and 10 Miles

POPULATION STATISTICS- 1979 REVISION 5/79
BASED ON THE YEAR 1970
POPULATION STATISTICS WITHIN 0-10 MILES
TOTAL NUMBER OF SITES- 111
MINIMUM POPULATION- 0 MAXIMUM POPULATION- 218398
MEAN POPULATION- 36931 MEDIAN POPULATION- 24269
90% PERCENTILE POPULATION- 83557
STANDARD DEVIATION- 39164.6 COEF. OF VARIATION- 1.060

NO.	SITE NAME	POPULATION	NO.	SITE NAME	POPULATION	NO.	SITE NAME	POPULATION
1	SUNDESERT	0	38	DAVIS BESSE	15390	75	OYSTER CREEK	36797
2	WPPSS 2	455	39	SKAGIT	16038	76	FORKED RIVER	36797
3	PEBBLE SPRINGS	878	40	CALVERT CLIFFS	16827	77	STERLING	37705
4	PALO VERDE	1892	41	WOOD	16889	78	OCONEE	37831
5	WPPSS 1&4	2648	42	FORT CALHOUN	17401	79	MCGUIRE	39374
6	SOUTH TEXAS	3254	43	PHIPPS BEND	17665	80	ERIE	40206
7	VOGTLE	3500	44	RIVER BEND	19347	81	NEW ENGLAND	41882
8	HATCH	4803	45	PRAIRIE ISLAND	19401	82	HUMBOLDT BAY	45403
9	WOLF CREEK	5260	46	BYRON	20377	83	GREENE COUNTY	45786
10	COMANCHE PEAK	5353	47	MARBLE HILL	20959	84	SAINT LUCIE	46066
11	SUMMER	5656	48	POINT BEACH	21073	85	GINNA	46325
12	RANCHO SECO	6061	49	YANKEE ROWE	21763	86	SUSQUEHANNA	50436
13	LACROSSE	6209	50	BRAIDWOOD	21942	87	PILGRIM	51203
14	DIABLO CANYON	6302	51	BELLEFONTE	22709	88	COOK	53006
15	COOPER	6363	52	ZIMMER	23023	89	SHOREHAM	54251
16	GRAND GULF	7245	53	VERMONT YANKEE	23030	90	HADDAM NECK	60374
17	BIG ROCK POINT	7551	54	ELK RIVER	23890	91	TROJAN	61855
18	WATTS BAR	7674	55	ARKANSAS	24141	92	MIDLAND	62000
19	NORTH ANNA	7713	56	NEW HAVEN	24397	93	CATAWBA	65901
20	HALLAM	8365	57	SAN ONOFRE	25725	94	SURRY	66630
21	FORT ST. VRAIN	8366	58	PEACH BOTTOM	25984	95	HAVEN	67981
22	TYRONE	8632	59	MAINE YANKEE	26000	96	PIQUA	72560
23	YELLOW CREEK	8828	60	ROBINSON	26016	97	PERRY	73600
24	CALLAWAY	8914	61	QUAD-CITIES	26739	98	DUANE ARNOLD	79310
25	FARLEY	9528	62	BROWNS FERRY	27215	99	SEABROOK	79478
26	WPPSS 3&5	9767	63	SALEM	28562	100	BAILLY	83608
27	BRUNSWICK	10000	64	HOPE CREEK	28562	101	PATHFINDER	84117
28	BLACK FOX	10404	65	PALISADES	29528	102	TURKEY POINT	88000
29	HARTSVILLE	11340	66	DRESDEN	31126	103	BONUS	89000
30	CRYSTAL RIVER	11599	67	CHEROKEE	31877	104	BEAVER VALLEY	105000
31	CLINTON	11899	68	DOUGLAS POINT	32020	105	MILLSTONE	105619
32	CVTR	12029	69	SEQUOYAH	32145	106	FERMI	134206
33	SHEARON HARRIS	12132	70	JAMESPORT	33200	107	THREE MILE ISLAND	136400
34	MONTICELLO	12344	71	PERKINS	34369	108	SHIPPINGPORT	143371
35	KEWAUNEE	12759	72	WATERFORD	35678	109	LIMERICK	152644
36	LASALLE	13343	73	NINE MILE POINT	36000	110	ZION	190314
37	CARROLL COUNTY	13999	74	FITZPATRICK	36000	111	INDIAN POINT	218398

TABLE 2

Population Statistics Between 0 and 30 Miles

POPULATION STATISTICS- 1979 REVISION 5/79
BASED ON THE YEAR 1970
POPULATION STATISTICS WITHIN 0-30 MILES
TOTAL NUMBER OF SITES- 111
MINIMUM POPULATION- 87 MAXIMUM POPULATION- 3984844
MEAN POPULATION- 531127 MEDIAN POPULATION- 321643
90% PERCENTILE POPULATION- 998939
STANDARD DEVIATION- 645852.0 COEF. OF VARIATION- 1.216

NO.	SITE NAME	POPULATION	NO.	SITE NAME	POPULATION	NO.	SITE NAME	POPULATION
1	SUNDESERT	87	38	MAINE YANKEE	197000	75	SHEARON HARRIS	495900
2	PEBBLE SPRINGS	4752	39	TROJAN	197480	76	MILLSTONE	496143
3	PALO VERDE	20039	40	HAVEN	208201	77	COOK	522300
4	CRYSTAL RIVER	32055	41	VERMONT YANKEE	211630	78	SURRY	524100
5	SOUTH TEXAS	40950	42	LASALLE	215680	79	NEW ENGLAND	563343
6	BIG ROCK POINT	46538	43	PALISADES	216535	80	DRESDEN	568123
7	COOPER	58916	44	HALLAM	218551	81	FORT CALHOUN	589809
8	WOLF CREEK	61905	45	DUANE ARNOLD	232995	82	PERKINS	622997
9	COMANCHE PEAK	65049	46	KEWAUNEE	245806	83	SUSQUEHANNA	631467
10	ARKANSAS	76582	47	PRAIRIE ISLAND	264432	84	JAMESPORT	655123
11	HATCH	81252	48	BROWNS FERRY	265532	85	DAVIS BESSE	672000
12	GRAND GULF	90049	49	MONTICELLO	271182	86	PERRY	703553
13	HUMBOLDT BAY	93330	50	STERLING	275717	87	CATAWBA	707512
14	WPPSS 2	92185	51	VOGTLE	280137	88	MCGUIRE	805535
15	WPPSS 1&4	98886	52	GREENE COUNTY	288026	89	PEACH BOTTOM	830276
16	YELLOW CREEK	104404	53	YANKEE ROWE	303271	90	GINNA	870591
17	BRUNSWICK	108479	54	PHIPPS BEND	308144	91	PILGRIM	883583
18	DIABLO CANYON	114014	55	NEW HAVEN	309178	92	SALEM	893626
19	BELLEFONTE	114998	56	CLINTON	334115	93	HOPE CREEK	893626
20	FARLEY	119394	57	CVTR	360589	94	PIQUA	895367
21	SAINT LUCIE	120843	58	BRAIDWOOD	360694	95	DOUGLAS POINT	900652
22	CALLAWAY	122389	59	OCONEE	363543	96	RANCHO SECO	907789
23	WPPSS 3&5	124551	60	RIVER BEND	371036	97	TURKEY POINT	909916
24	PATHFINDER	135451	61	SUMMER	378538	98	WATERFORD	957223
25	HARTSVILLE	135984	62	SAN ONOFRE	408362	99	THREE MILE ISLAND	995200
26	HOOD	138451	63	QUAD-CITIES	415500	100	SEABROOK	1003843
27	LACROSSE	143321	64	SEQUOYAH	432375	101	ZIMMER	1052883
28	SKAGIT	151774	65	FORT ST. VRAIN	434802	102	ELK RIVER	1202027
29	NORTH ANNA	152432	66	OYSTER CREEK	451606	103	ZION	1262593
30	TYRONE	153801	67	FORKED RIVER	451606	104	SHIPPINGPORT	1677889
31	WATTS BAR	161537	68	BONUS	453000	105	BEAVER VALLEY	1700000
32	POINT BEACH	187086	69	BYRON	455409	106	SHOREHAM	1760382
33	CALVERT CLIFFS	188755	70	MARBLE HILL	457928	107	HADDAM NECK	1763975
34	ROBINSON	192140	71	BLACK FOX	459832	108	BAILLY	2200000
35	NINE MILE POINT	195143	72	MIDLAND	470000	109	FERMI	2371808
36	FITZPATRICK	195143	73	CHEROKEE	475129	110	LIMERICK	3836244
37	CARROLL COUNTY	196357	74	ERIE	483519	111	INDIAN POINT	3984844

TABLE 3

Population Statistics Between 0 and 50 Miles

POPULATION STATISTICS- 1979 REVISION 5/79
BASED ON THE YEAR 1970
POPULATION STATISTICS WITHIN 0-50 MILES
TOTAL NUMBER OF SITES- 111
MINIMUM POPULATION- 7784 MAXIMUM POPULATION-17471479
MEAN POPULATION- 1705750 MEDIAN POPULATION- 948747
90% PERCENTILE POPULATION- 4085400
STANDARD DEVIATION-2196315.2 COEF. OF VARIATION- 1.287

NO.	SITE NAME	POPULATION	NO.	SITE NAME	POPULATION	NO.	SITE NAME	POPULATION
1	SUNDESERT	7784	38	SEQUOYAH	659015	75	SUSQUEHANNA	1537373
2	PEBBLE SPRINGS	74814	39	CVTR	661462	76	Y[illegible]KEE ROWE	1538765
3	HUMBOLDT BAY	100728	40	PHIPPS BEND	691304	77	S[illegible]Y	1550000
4	BIG ROCK POINT	128631	41	FORT CALHOUN	711117	78	PIQUA	1654093
5	ARKANSAS	150464	42	SUMMER	724009	79	TURKEY POINT	1660498
6	WOLF CREEK	165677	43	OCONEE	730291	80	ZIMMER	1786790
7	CRYSTAL RIVER	169908	44	CARROLL COUNTY	733928	81	NEW ENGLAND	1862933
8	COOPER	171895	45	CLINTON	768171	82	THREE MILE ISLAND	1868000
9	BRUNSWICK	174066	46	COMANCHE PEAK	783124	83	MONTICELLO	1956232
10	WPPSS 1&4	181928	47	NORTH ANNA	827109	84	DAVIS BESSE	2052000
11	WPPSS 2	184296	48	NINE MILE POINT	843725	85	PRAIRIE ISLAND	2057725
12	SOUTH TEXAS	196206	49	FITZPATRICK	843725	86	ELK RIVER	2101115
13	DIABLO CANYON	209444	50	BELLEFONTE	845838	87	CALVERT CLIFFS	2305635
14	PATHFINDER	242751	51	HARTSVILLE	869776	88	ERIE	2411[illegible]7
15	HATCH	251612	52	BYRON	881721	89	PERRY	2583218
16	GRAND GULF	269314	53	LASALLE	918803	90	MILLSTONE	2591658
17	CALLAWAY	299254	54	NEW HAVEN	921367	91	DOUGLAS POINT	3167529
18	HALLAM	307945	55	HAVEN	922246	92	JAMESPORT	3173531
19	SAINT LUCIE	318784	56	WOOD	970248	93	HADDAM NECK	3267732
20	FARLEY	320667	57	PALISADES	984252	94	OYSTER CREEK	3290000
21	LACROSSE	321073	58	BONUS	999000	95	FORKED RIVER	3290000
22	PALO VERDE	328088	59	MIDLAND	1000000	96	SAN ONOFRE	3572478
23	YELLOW CREEK	344716	60	SHEARON HARRIS	1062200	97	SEABROOK	3605493
24	WPPSS 3&5	345935	61	COOK	1120000	98	SHIPPINGPORT	3735300
25	SKAGIT	366247	62	TROJAN	1146188	99	BEAVER VALLEY	3900000
26	TYRONE	372980	63	VERMONT YANKEE	1149200	100	BRAIDWOOD	4088663
27	VOGTLE	456631	64	STERLING	1154607	101	PEACH BOTTOM	4121297
28	MAINE YANKEE	486000	65	GINNA	1215870	102	PILGRIM	4234545
29	ROBINSON	530817	66	MARBLE HILL	1245001	103	SALEM	4773288
30	DUANE ARNOLD	552745	67	CATAWBA	1245504	104	HOPE CREEK	4773288
31	POINT BEACH	564251	68	CHEROKEE	1308327	105	SHOREHAM	4940868
32	KEWAUNEE	574631	69	MCGUIRE	1380228	106	FERMI	5446957
33	QUAD-CITIES	601843	70	RANCHO SECO	1381581	107	DRESDEN	6305057
34	BROWNS FERRY	625608	71	GREENE COUNTY	1383978	108	BAILLY	6747815
35	RIVER BEND	627983	72	FORT ST. VRAIN	1396284	109	LIMERICK	7036199
36	BLACK FOX	641797	73	WATERFORD	1479345	110	ZION	7083759
37	WATTS BAR	657836	74	PERKINS	1506152	111	INDIAN POINT	17471479

TABLE 4

SITES WITH HIGHEST SECTOR POPULATIONS

	Population in Highest 22 1/2° Sector(s)
A. Based on 1970 census data at 10 miles	
1. Zion	65,000; 43,000; 41,000
2. Millstone	39,000
3. Duane Arnold	38,000
4. Three Mile Island	35,000
5. Indian Point	32,000
6. Trojan	32,000
7. Beaver Valley	31,000; 31,000
8. Indian Point	30,000; 30,000
B. Based on 1970 census data at 30 miles	
1. Indian Point	1,500,000; 820,000
2. Limerick	1,300,000; 950,000
3. Bailly	900,000
4. Fermi	800,000; 770,000
5. Waterford	700,000
C. Based on 1970 census data at 50 miles	
1. Indian Point	8,000,000; 2,900,000; 2,300,000
2. Dresden	3,300,000
3. Bailly	3,200,000
4. Zion	3,200,000
5. Salem	2,700,000
6. Shoreham	2,100,000
7. Fermi	2,100,000

REACTOR ACCIDENT RISK PARAMETERS

The accident risk to the public posed by a reactor at a particular site can be analyzed by carefully considering the design and operating characteristics of the reactor plant, the local meteorology, the population distribution around the plant, and the various measures such as sheltering or evacuation which could be taken to reduce the effect of a reactor accident on the public. Ideally, this analysis should be plant and site specific. Experience has already shown that plant design and operating characteristics are not so standardized that it is sufficient to analyze any one reactor, or any one type of reactor, or even any one reactor plant designed by a single supplier. The estimated probabilities and scenarios of reactor accidents are so sensitive to differences in details of component reliability design and procedures, including human errors, that apparently similar plants can be substantially different.

The same need for plant specific analysis holds true for the siting aspects of plants, i.e., the meteorology and especially the demography. Since there exists no exhaustive risk analysis of the Indian Point plants, the following analyses will deal separately with the siting and then the design aspects of the Indian Point plants comparing what we do know of them to similar risk analyses of other U. S. plants. Understanding the overall accident risk of a nuclear power plant or comparison of the risk posed by it to that posed by any other plant requires consideration of the siting as well as the design and operating characteristics of the plant.

SITE ASPECTS

The Reactor Safety Study (WASH-1400), subject, to be sure, to large uncertainties, provides a basic accident risk model which can be used to assess the potential accident risk of a plant, at least in comparison to other plants. The model was developed in the detailed review of only two plants, the Surry pressurized water reactor (PWR) and the Peach Bottom boiling water reactor (BWR). The Indian Point Unit 2 and 3 reactors are PWRs, furnished by the same nuclear steam system supplier (Westinghouse), but of a larger size and later vintage. To compare reactor sites to one another, the Surry PWR is used as a benchmark and, through the facility of calculation, is moved from site to site calculating the overall risk for four principal risk measures: early fatalities; early (radiation) illnesses; latent cancer fatalities; and public property damage costs. If the power of the benchmark reactor is held constant, then this set of calculations provides a good comparative measure of one site to another.

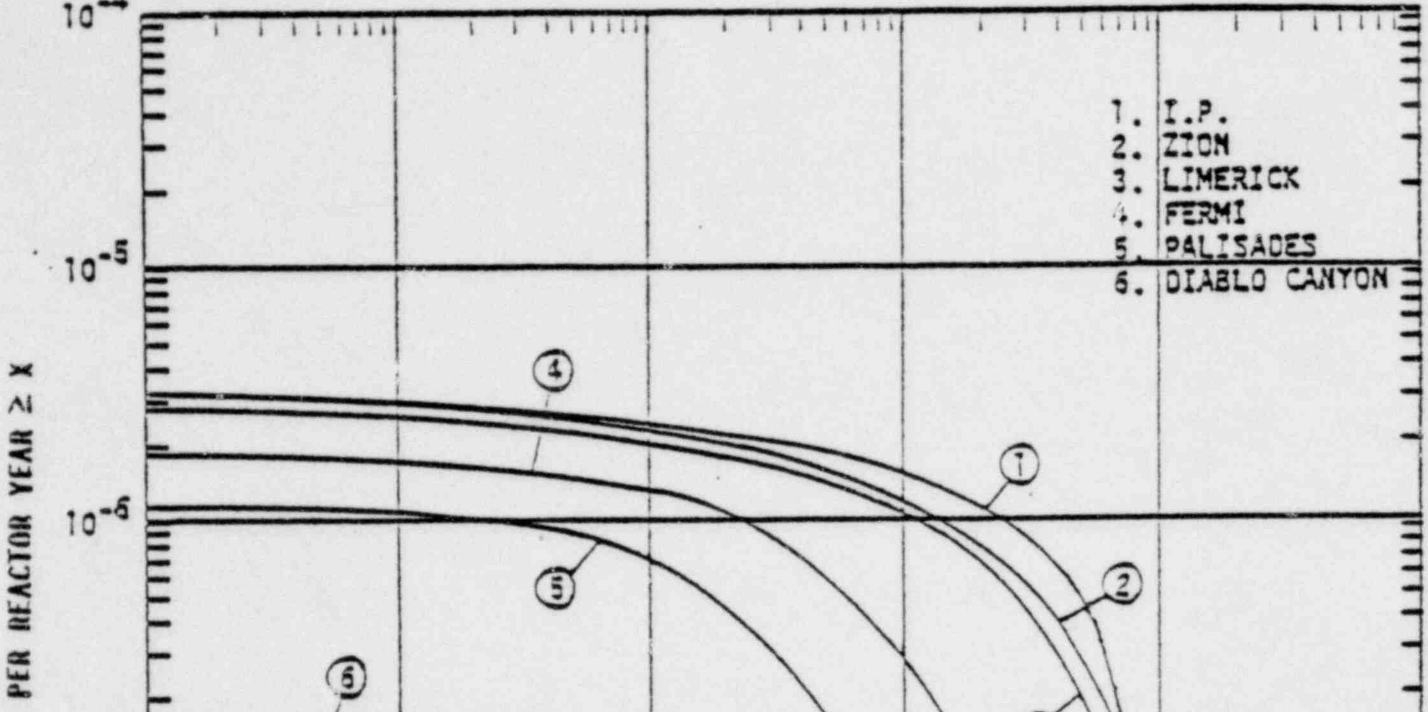
The staff has performed a set of these benchmark calculations using the Surry benchmark reactor with its power increased to 3025 MWT, the rating of Indian Point 3. In general, the risk a reactor poses is proportional to its power level. Six sites were analyzed for this comparison. Four, Indian Point, Zion, Limerick and Fermi, represent sites of relatively high population. One, Palisades, represents what the staff believes is a typical or average population distribution. The last, Diablo Canyon, represents a remote site, thatis, one with relatively low population density. The results of the analyses of the enlarged Surry plant at these six sites are shown in Figures 1 through 4 for the four measures

The results shown in these figures are the complementary cumulative distribution functions (CCDF)* which are the variation of the consequences of a reactor accident per year with their associated probability of occurrence. The estimated risk of accidents for a given reactor, the product of probabilities and consequences, is the area under the curve. On Figures 1, 2, and 3 are listed the key assumptions about public protective action, namely that people within a 10 mile radius of the plant suffer the entire cloud exposure and then four hours of ground exposure before they are evacuated; people outside the 10 mile radius receive the entire cloud exposure and a subsequent seven day ground exposure assuming normal indoor and outdoor activity.

Before studying the curves consider for a moment the range of consequences that can be caused by a nuclear plant accident. For severe consequences, substantial amounts of radioactive material must be spread out over the surrounding area. The forces ejecting the material and the local meteorology will control how much gets out and how far it will reach. The areas closest to the reactor will stand to receive the highest doses and those farther away, less. The Reactor Safety Study analysis showed that for severe accident releases, only those people within about 10 miles are exposed to fatal doses, beginning at about 300 Rem. Thus, the population within 10 miles of a site will be significant to the early fatality risk for that site; the population beyond 10 miles will not. This was a p...

*The CCDF shows the probability that a consequence will be equalled or exceeded. Appendix A discusses how a CCDF is constructed. For further discussion of the consequence model used in these calculations, please refer to Overview of the Reactor Safety Study Consequence Model (NUREG-0340) and Appendix VI of the Reactor Safety Study (WASH-1400).

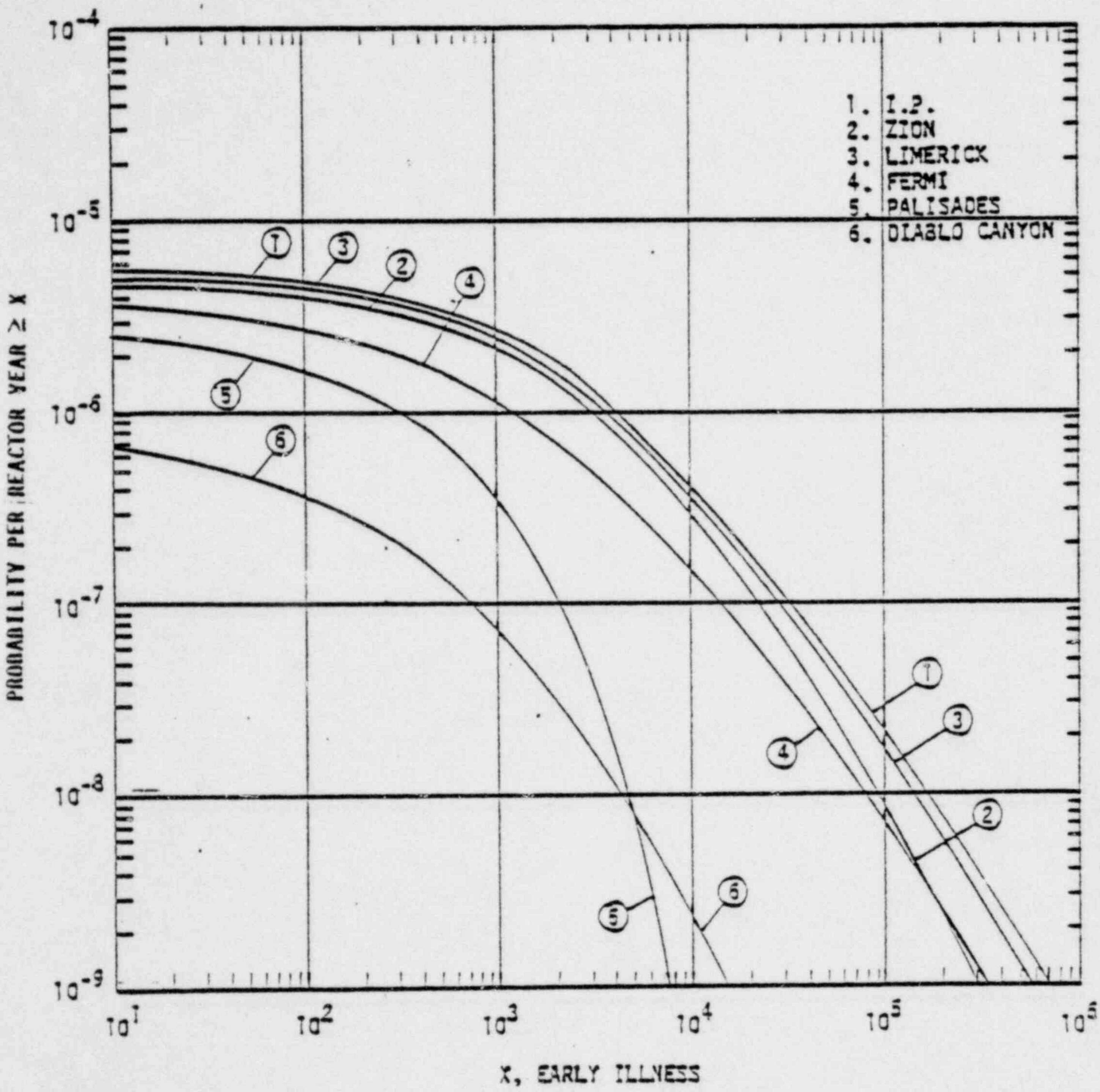
FIGURE 1 - EARLY FATALITY RISK FOR DIFFERENT SITES



NOTE: THERE ARE LARGE UNCERTAINTIES WITH THE ABSOLUTE VALUES PRESENTED IN THIS FIGURE

ASSUMPTIONS: 1) SURRY DESIGN.
2) I.P. UNIT 3 POWER LEVEL (3025 MWT).
3) WITHIN 10 MILES - ENTIRE CLOUD EXPOSURE + 4 HOURS GROUND EXPOSURE NO SHIELDING
BEYOND 10 MILES - ENTIRE CLOUD EXPOSURE + 7 DAY GROUND EXPOSURE SHIELDING BASED ON NORMAL ACTIVITY.

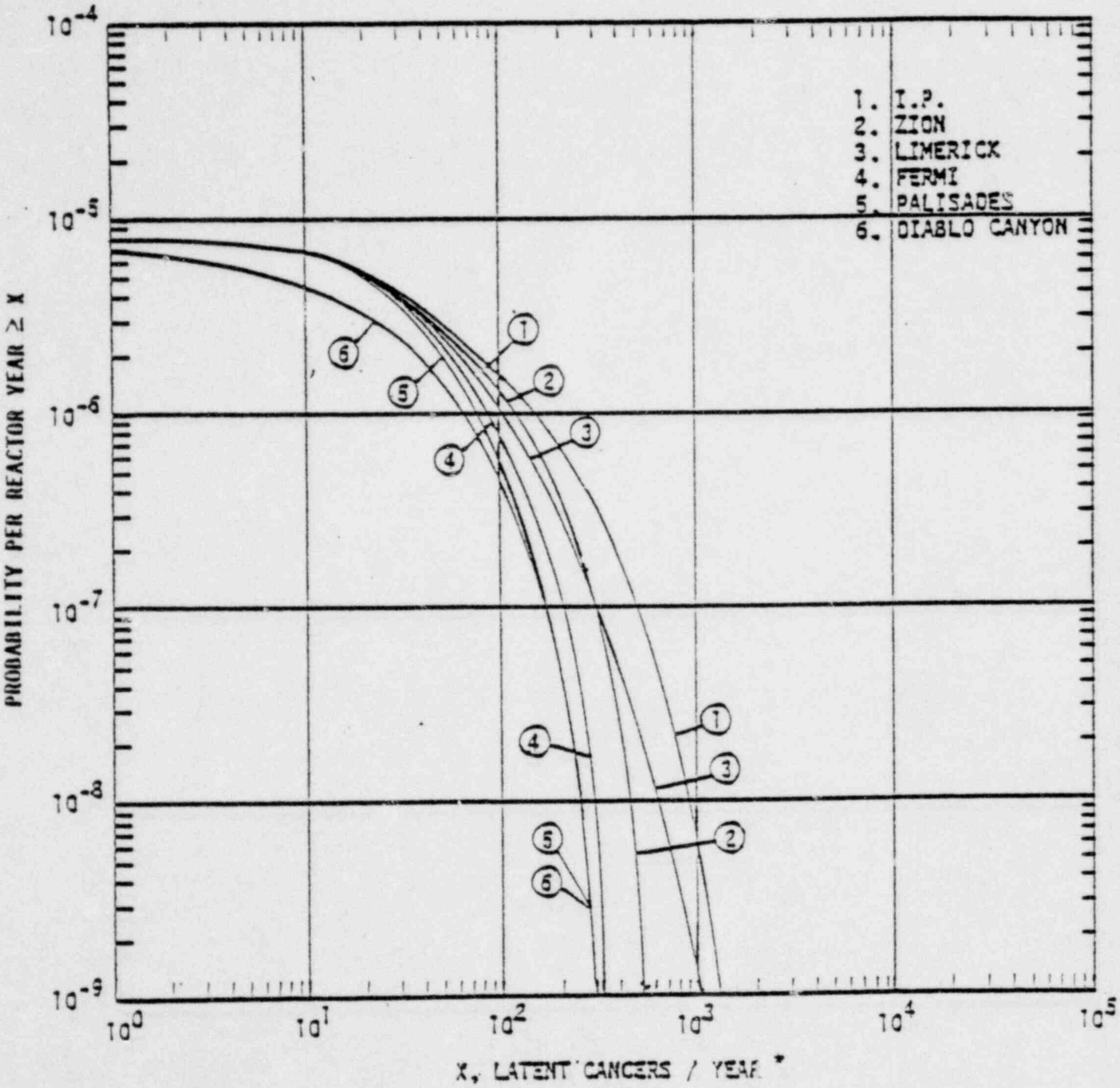
FIGURE 2 - EARLY ILLNESS RISK FOR DIFFERENT SITES



NOTE: THERE ARE LARGE UNCERTAINTIES WITH THE ABSOLUTE VALUES PRESENTED IN THIS FIGURE

ASSUMPTIONS: 1) SURRY DESIGN.
2) I.P. UNIT 3 POWER LEVEL (3025 MWT).
3) WITHIN 10 MILES - ENTIRE CLOUD EXPOSURE + 4 HOURS GROUND EXPOSURE NO SHIELDING
BEYOND 10 MILES - ENTIRE CLOUD EXPOSURE + 7 DAY GROUND EXPOSURE SHIELDING BASED ON NORMAL ACTIVITY.

FIGURE 3 - LATENT CANCER RISK (ANNUAL) FOR DIFFERENT SITES

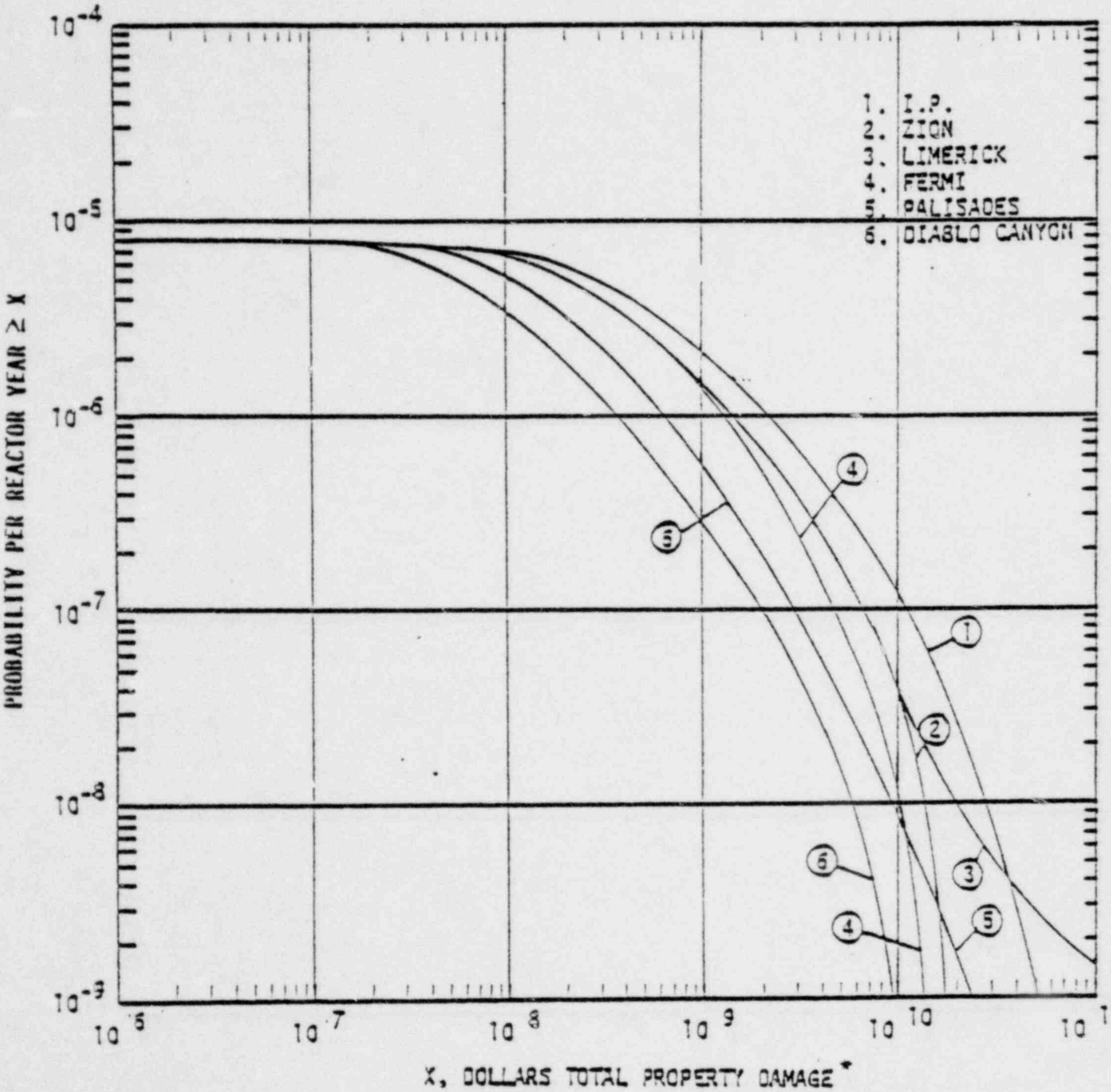


*TOTAL LATENT CANCERS WOULD BE 30 TIMES HIGHER

NOTE: THERE ARE LARGE UNCERTAINTIES WITH THE ABSOLUTE VALUES PRESENTED IN THIS FIGURE

ASSUMPTIONS: 1) SURRY DESIGN.
2) I.P. UNIT 3 POWER LEVEL (3025 MWT).
3) WITHIN 10 MILES - ENTIRE CLOUD EXPOSURE + 4 HOURS GROUND EXPOSURE NO SHIELDING
BEYOND 10 MILES - ENTIRE CLOUD EXPOSURE + 7 DAY GROUND EXPOSURE SHIELDING BASED ON NORMAL ACTIVITY.

FIGURE 4 - PROPERTY DAMAGE RISK FOR DIFFERENT SITES



* BASED ON 1974 DOLLARS

NOTE: THERE ARE LARGE UNCERTAINTIES WITH THE ABSOLUTE VALUES PRESENTED IN THIS FIGURE

ASSUMPTIONS: 1) SURRY DESIGN
2) I.P. UNIT 3 POWER LEVEL (3025 MWT)
3) WIND ROSE WEIGHTED 1970 CENSUS POPULATION DISTRIBUTION
4) IDENTICAL 91 WEATHER SEQUENCES FOR ALL SITES.

reason for selecting 10 miles as the radius for emergency planning zones (see NUREG-0396, Planning Basis for the Development of State and Local Government Radiological Emergency Response Plans in Support of Light Water Nuclear Power Plants).

Radiation injuries, caused by doses of 50 Rem or more, can reach farther out in the event of a severe reactor accident, to the population as far as 50 miles away. Therefore, the population up to that distance away is significant in estimating the early illness risk; the population beyond 50 miles is not. The estimation of latent cancer fatalities includes even low exposures so populations as far away as 200 miles will significantly influence the latent cancer risk estimate. Thus, for the latent cancer risk, the differences between sites are relatively small since the populations of such large regions are frequently similar.

Figure 1 shows that the three sites with the highest local population density, Indian Point, Zion and Limerick, have essentially the same risk profile for early fatalities. The other sites show progressively lower risks. As was discussed, early fatality risk is dominated by the population within 10 miles of the plant, so the large population of New York City is not a factor here. The absolute values of these risk estimates are subject to large uncertainties but the range should be noted. For low probability--high consequence events, thousands to tens of thousands of early deaths are estimated for most sites.

Early illnesses are defined as radiation exposures in excess of 50 Rem, whole body for an individual. These illnesses or injuries, shown in

Figure 2, are dominated by the size of the population within a 50 mile radius. Thus, New York City is important to the risk of early illness for Indian Point. Zion, Limerick and Fermi also have enough population in the 50 mile range to be comparable to Indian Point as shown by Figure 2. Also for this aspect of risk, the typical Palisades site and the Diablo Canyon site are not very different from each other but are substantially lower than the others. For the sites with higher population density, thousands to hundreds of thousands of early illnesses are projected for the lower probability events.

The latent cancer risk, as shown in Figure 3, is dominated by the population within about a 200 mile radius of the plant. Because of this, the individual site risk curves for latent cancers reflect the character of the region. Remember that Indian Point is outside New York City, Zion outside Chicago on the north shore, Limerick to the northwest of Philadelphia, and Fermi near Detroit. Palisades is on the western side of the Michigan lower peninsula and Diablo Canyon is on the California coast well above Santa Barbara. The latent cancer risk for these sites, and probably all other sites is approximately the same. The number of latent cancer deaths projected is on the order of hundreds per year or thousands per accident for the lower probability events (on the order of 10^{-9}/yr).

Please note that the latent cancer risk is presented throughout this discussion as latent cancers per year, that is, the average number of cancer deaths that would be expected to occur per year in the population which was exposed to the accident. The total number of latent cancer deaths associated with an accident would be 30 times higher, reflecting the calculated rate of cancer death continuing for a generation. For further discussion of latent cancer risk see NUREG-0340 at page 30.

The curves for property damage are presented in Figure 4. The model still calculates in 1974 dollars; the correction for inflation is probably about a factor of 1.5. The flatness of the curve at the upper left indicates that any accident with substantial releases will cause damage of many millions of dollars. The projected damage for low probability events reaches up into the range of tens of billions of dollars. However, the property damage here does not include damage to the plant. The Three Mile Island accident, which did no offsite property damage, caused several hundred million dollars worth of damage to the plant and replacement power costs, analogous to interdiction costs, on the order of a billion dollars. The property damage risk estimate is directly proportional to population density. With the present property damage model (see NUREG-0340 at page 22) the population out to about 30 miles is significant. However, the use of more strict interdiction and cleanup criteria, as may well be warranted, would make populations beyond that distance important.

The estimated overall probability of core melts for the benchmark reactor (Surry) rebaselined* from WASH-1400 is about one chance out of twenty

*The Reactor Safety Study plants were "rebaselined" for all the analyses presented in this report in order to take into account peer group comments (e.g., the Lewis Committee) and to use better data and analytical techniques which are now available such as the MARCH and CORRAL codes. Further discussion of this rebaselining is presented in Appendix B.

thousand ($5x10^{-5}$) per reactor year. The CCDF curves have been constructed to display the probability vs. consequence relationship for those cases of core melt accidents where offsite harm is done. Note that the majority of core melts are not estimated to do harm offsite. For example, in Figure 1 the benchmark Surry reactor at the Indian Point site is predicted to cause one or more acute fatalities at a frequency of $3.2x10^{-6}$/yr. This means that only $3.2x10^{-6} \div 5x10^{-5}$ = .064 or less than 10 percent of the core melt accidents are predicted to give lethal doses offsite. Conversely about 90 percent of the core melt accidents are not expected to produce lethal doses for that plant. For other plants a larger or smaller fraction of core melt accidents may be expected to cause lethal doses offsite. Our ability to predict how often core melt accidents occur is very limited. However, we are quite reasonably confident from the work so far that most core melt accidents will not give lethal doses offsite. Only certain accident scenarios in the plant, those entailing core meltdown and gross containment failure, coincident with particularly adverse weather conditions, will result in lethal doses or severe offsite ground contamination (i.e., property damage). However, those few core melt accidents that do give lethal doses are likely to do so over a significant area (out to a few miles downwind). If even one person receives a lethal dose offsite, it is quite likely that one thousand will receive a lethal dose. However, in no case are more than a few tens of thousands predicted to receive lethal doses. No combination of weather conditions, ineffectual emergency response and severe accident can be found at any probability that is realistically expected to give lethal doses to as many as one hundred thousand. There are, of course, higher numbers of latent casualties predicted for such accidents, as can be seen in Figure 3.

Consider the differences among the curves; the curves have been constructed on logarithmic scale, which tends to minimize small differences. There are a few perspectives which the CCDFs should clearly provide. For illustrative purposes consider Figure 1; Early Fatality Risk for Different Sites. The probability axis shows the chance of equalling or exceeding a number of early fatalities per reactor year. At 10 fatalities, the range of probabilities for the sites represents the variation between sites of the likelihood of having at least 10 people receive lethal doses. At this level, there is about a factor of 30 difference in probability between the Indian Point and Diablo Canyon sites. Thus, the CCDFs show the variation in probability for given levels of consequences. The CCDFs also give the range of consequences for a given probability level. At the one in one hundred million (10^{-8}) probability level, one would expect the Diablo Canyon area population to suffer at least 400 fatalities whereas the number of fatalities estimated at Indian Point would be about 10,000 or more.

In addition to the probability and consequence perspective, the curves give a sense of the importance of the consequences and probabilities. When the curves have a clear knee in them, that is they have an approximately horizontal slope out to some level of consequences and then fall off sharply (see the Indian Point curve in Figure 1, the knee is at about the 4,000 fatalities level) the most important part of the curve is the horizontal portion where one would expect to have about an equal chance of suffering consequences up to about that "knee" level. When the curve drops off, the uncertainties become very large and the importance of perceived differences should be minimized. When the curves do not have a clear knee, as in the case of Indian Point on Figure 2, the probabilities are dropping at about the same rate as the consequences are increasing. This result leaves a question as to the limit of how many consequences could be expected. That is, the low probability-high consequence range (bottom right of curve) is clearly contributing to the overall risk.

The risk curves in Figures 1-4 can be reduced to probability weighted values, or expected consequences and these can be termed the likelihood of the consequence occurring in a year. Table 5 presents these expected consequences. The principal differences between the risks at these sites is seen to be in early fatalities and injuries. The Indian Point site poses about 20 times more risk of early fatality than a typical site such as Palisades. With respect to early injuries, the Indian Point site is about 10 times more risky than Palisades. The differences in other aspects of risk are not so great.

The risks of early fatalities and early illnesses for the Indian Point site alone where only public protective measures are changed are shown in Figures 5 and 6, respectively. For the Indian Point site alone, the sensitivity of early fatalities and early illness to no evacuation at all until a day after the accident, to differences in evacuation radius,

TABLE 5

EXPECTED ANNUAL CONSEQUENCES (RISK) FROM 6 SITES
WITH THE SURRY REBASELINED PWR DESIGN

Probability of Consequence Occurrance per yr / Site	Early Fatalities	Early Injuries	Latent Cancer/Yr*	Property Damage $**
Diablo Canyon	$1.6x10^{-5}$	$2.5x10^{-4}$	$1.8x10^{-4}$	1290
Palisades	$2.9x10^{-4}$	$1.2x10^{-3}$	$2.7x10^{-4}$	2670
Fermi	$9.2x10^{-4}$	$6.3x10^{-3}$	$3.6x10^{-4}$	4780
Limerick	$3.5x10^{-3}$	$1.1x10^{-2}$	$4.7x10^{-4}$	6980
Zion	$4.7x10^{-3}$	$1.2x10^{-2}$	$4.3x10^{-4}$	6030
Indian Point	$6.1x10^{-3}$	$1.5x10^{-2}$	$5.4x10^{-4}$	9550

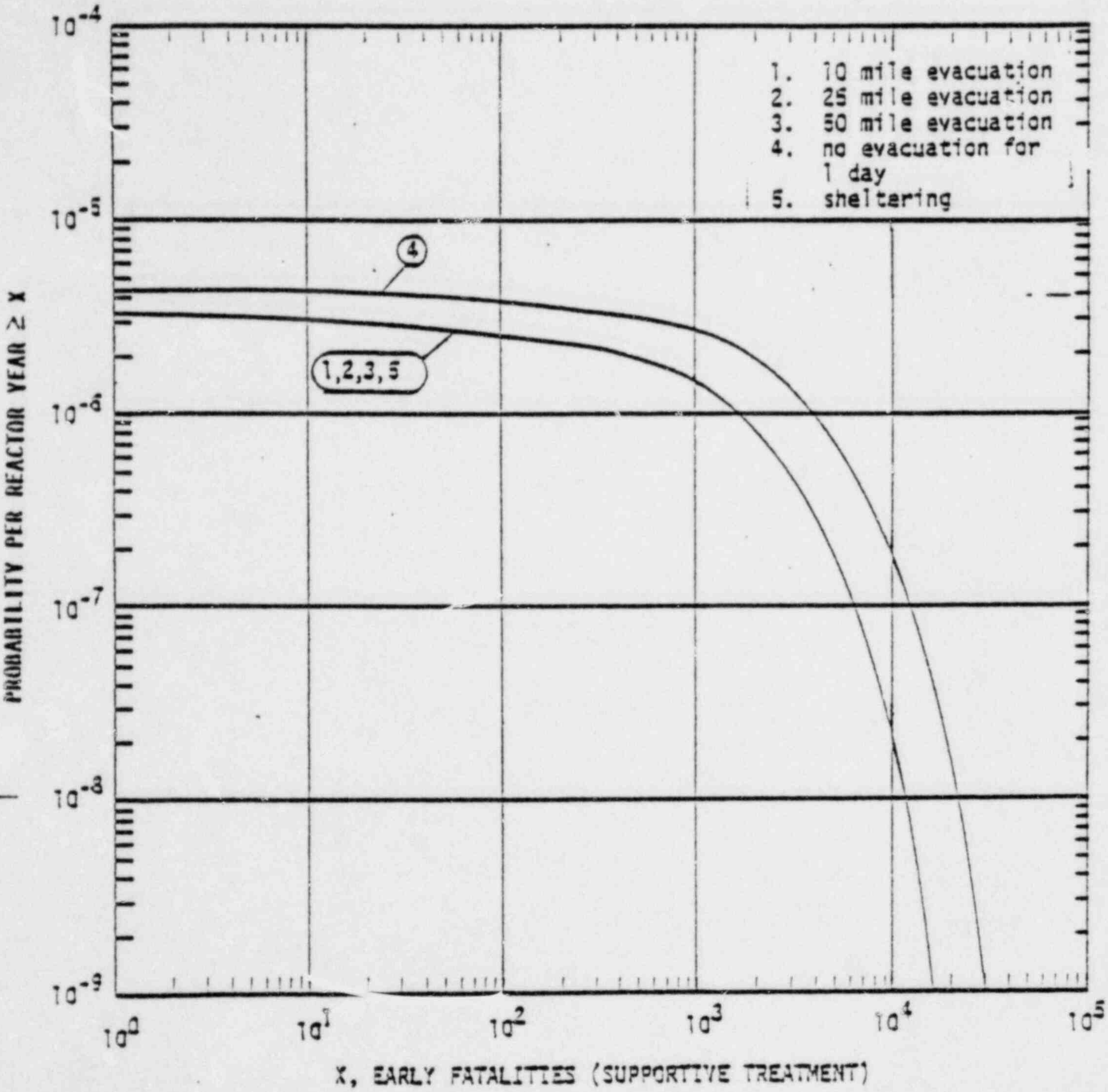
*Total Late.. Cancers Would Be 30 Times Higher

**Based on 1974 Dollars

NOTE: THERE ARE LARGE UNCERTAINTIES WITH THE ABSOLUTE VALUES PRESENTED IN THIS TABLE.

ASSUMPTIONS:
1. SURRY DESIGN.
2. I.P. UNIT 3 POWER LEVEL (3025 MWT).
3. WITHIN 10 MILES - ENTIRE CLOUD EXPOSURE + 4 HOURS GROUND EXPOSURE NO SHIELDING
   BEYOND 10 MILES - ENTIRE CLOUD EXPOSURE + 7 DAY GROUND EXPOSURE SHIELDING BASED ON NORMAL ACTIVITY.
4. WIND ROSE WEIGHTED 1970 CENSUS POPULATION DISTRIBUTION.
5. IDENTICAL 91 WEATHER SEQUENCES FOR ALL SITES.

FIGURE 5 - E?RLY FATALITY RISK AT INDIAN POINT FOR VARIOUS PUBLIC PROTECTION MEASURES

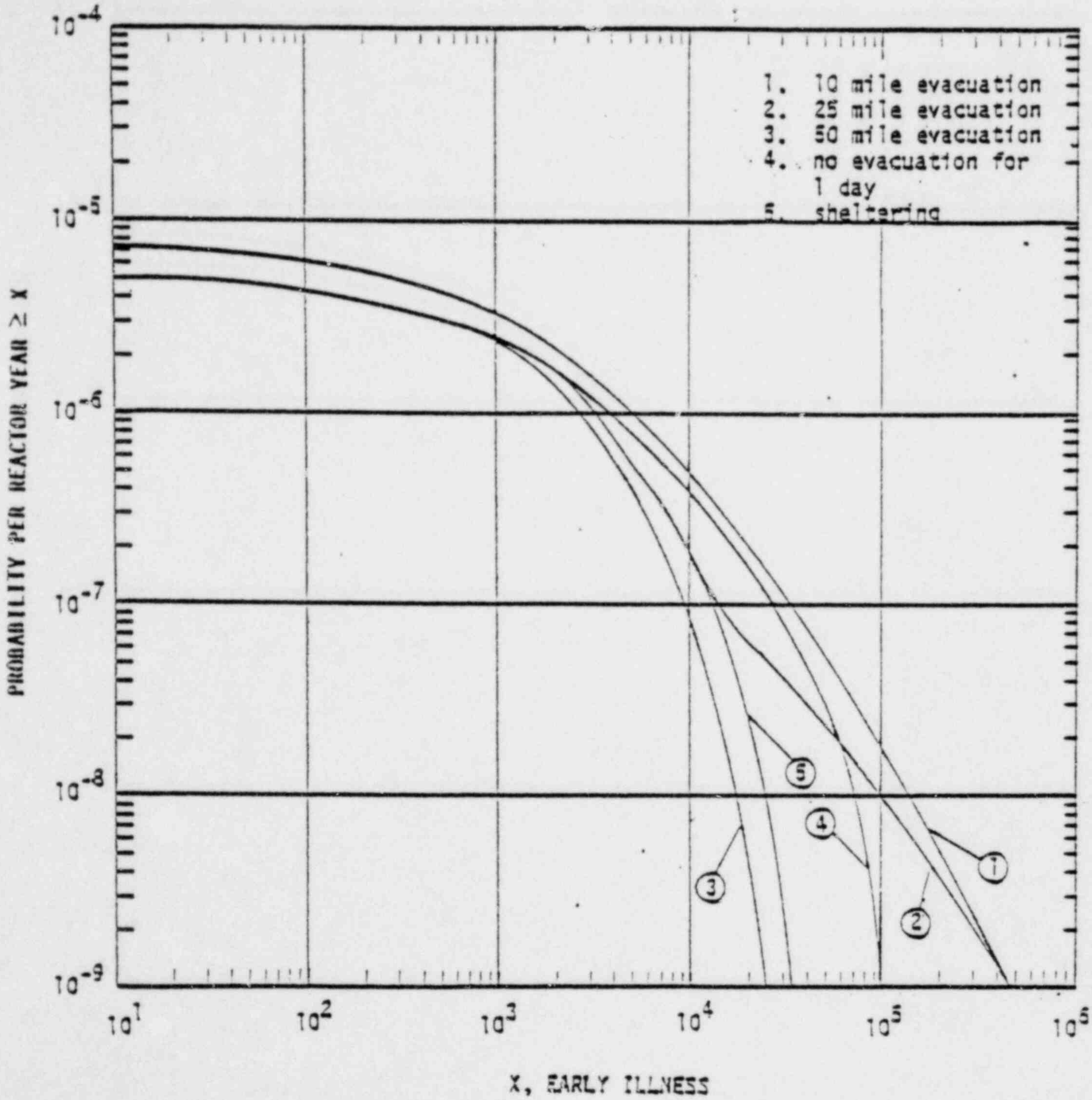


NOTE: THERE ARE LARGE UNCERTAINTIES WITH THE ABSOLUTE VALUES PRESENTED IN THIS FIGURE

ASSUMPTIONS: 1) SURRY DESIGN.
2) I.P. UNIT 3 POWER LEVEL (3025 MWT).
3) WIND ROSE WEIGHTED 1970 CENSUS POPULATION DISTRIBUTION
4) INDIAN POINT SITE (POPULATION AND METEOROLOGY)

EVACUATION SCENARIOS - ENTIRE CLOUD EXPOSURE + EITHER 4 HOURS GROUND EXPOSURE, NO SHIELDING WITHIN GIVEN DISTANCE; OR 7 DAYS GROUND EXPOSURE, NORMAL SHIELDING BEYOND GIVEN DISTANCE

FIGURE 6 - EARLY ILLNESS RISK AT INDIAN POINT FOR VARIOUS PUBLIC PROTECTION MEASURES



NOTE: THERE ARE LARGE UNCERTAINTIES WITH THE ABSOLUTE VALUES PRESENTED IN THIS FIGURE

ASSUMPTIONS: 1) SURRY DESIGN
2) I.P. UNIT 3 POWER LEVEL (3025 MWT)
3) WIND ROSE WEIGHTED 1970 CENSUS POPULATION DISTRIBUTION
4) INDIAN POINT SITE (POPULATION AND METEOROLOGY)

EVACUATION SCENARIOS - ENTIRE CLOUD EXPOSURE + EITHER 4 HOURS GROUND EXPOSURE, NO SHIELDING WITHIN GIVEN DISTANCE; OR 7 DAYS GROUND EXPOSURE, NORMAL SHIELDING BEYOND GIVEN DISTANCE namely, 10, 25 and 50 miles and sheltering were analyzed. For Indian Point, this last would include New York City itself. In Figure 5 for early fatalities, only two curves are shown, one for no evacuation for one day and a second curve representing a range of the public protection options since their differences are too small to distinguish. All evacuations are assumed to include direct exposure of the people to the cloud and then four hours of ground exposure while evacuating. Obviously, if one assumed that the evacuees could leave before suffering less or even any cloud and ground exposure, the risk profile would be drastically lowered. Since early fatalities are dominated by the population within the first 10 miles, evacuating beyond that range produces little reduction in early fatalities.

The early illnesses that could be suffered around the Indian Point site with varying public protection strategies is shown in Figure 6. The lowest risk is with a 50 mile evacuation. The alternative of sheltering for a period of 24 hours and then evacuating selectively appears to provide nearly the same risk reduction for the Indian Point environs. The other alternatives depicted do not appear to offer as much benefit for the low probability-high consequence events.

THE EFFECT OF DESIGN ON RISK AT INDIAN POINT

The extensive use of quantitative risk assessment for U. S. power reactors began with the Reactor Safety Study (RSS), WASH-1400, which studied a 3-loop Westinghouse PWR, Surry, and a General Electric BWR, Peach Bottom. Since the Reactor Safety Study, other reactor risk assessments of somewhat lesser depth have been made. For example, the NRC staff has been pursuing the Reactor Safety Study Methodology Application Program. This program is considering four reactors: Sequoyah, a Westinghouse 4-loop PWR with ice condenser containment; Oconee, a Babcock-Wilcox 2-loop PWR with dry containment; Calvert Cliffs, a Combustion Engineering 2-loop PWR with dry containment; and Grand Gulf, a General Electric BWR with Mark III containment. These designs are being reviewed with application of the Reactor Safety Study event and fault tree techniques. The reports on these studies will not be complete until later this year but some of the preliminary results are available to the staff.

The staff recently began a new program, the Interim Reliability Evaluation Program. The first plant covered in this program is Crystal River 3, a Babcock and Wilcox 2-loop PWR with dry containment. The initial report on this study is now in peer review, and its preliminary results are available to the staff. Also available for comparison are the results of the German reactor risk study of the Biblis B reactor.

The staff used the information gained from these studies to guide a short term risk evaluation of the Indian Point 2 and 3 plants. This evaluation relies heavily on the judgement of the reviewer with respect to the accident sequences being considered and to the parts of the plants involved. The approach was to consider the key accident sequences which involve core meltdown* or containment failure modes that would be expected to dominate risk. The Indian Point plants were briefly reviewed against these scenarios and their designs were surveyed for single point vulnerabilities such as single manual valves or human errors which can trigger or control a significant accident sequence. Particular attention was given to common interactions which could cut across more than one system or be caused by a single initiating event. Rough estimates were made of the likelihood and consequences of various sequences using the data and release characteristics of previous studies, particularly the Reactor Safety Study and its follow-on work, the Methodology Application Program. Prior risk studies showed that a handful of accident scenarios would most likely define and dominate a reasonably complete spectrum of core melt accident scenarios for the PWR design. Table 6 lists the accident scenarios which were so considered and which were among those quantitatively estimated for the Indian Point 2 and 3 study. We found no risk significant differences between the Indian Point 2 and 3 designs.

An estimate of the overall probability of severe core damage or core melt was made for Indian Point 2 and 3 as of December 1979. Then the estimate was revised to reflect those changes that were made or committed to in early 1980. This very preliminary estimate for Indian Point indicates an initial probability of severe core damage of about $3x10^{-5}$

*Here, as in WASH-1400, the terms core meltdown and severe core damage are used interchangeably. The analysis presumes procession to core melting once severe damage is suffered.

TABLE 6

DOMINANT ACCIDENT SEQUENCES

Accident Scenario	Sequence Code From WASH-1400	Offsite Consequences Expected
LOCA and failure of ECCS in injection mode	AD S_1D S_2D	Low to modest
LOCA and failure of ECCS in recirculation mode	AH S_1H S_2H	
Transient and loss of feedwater or serious failure and no feed and bleed on primary side (X)	TMLX TMKX	
LOCA and loss of containment heat removal with subsequent interactions with ECCS	AG S_1G S_2G	Intermediate
LOCA and failure of ECCS and containment ESFs in recirculation phase due to common cause	AHF S_1HF S_2HF	High
LOCA and coupled damage to ECCS and potential bypass of containment	Event V	
Transient involving loss of all AC power (or possibly DC) and failure of auxiliary feedwater	TMLB'	

per year. The improvements made or committed to this year are estimated to reduce that probability by a factor of three to about 1×10^{5} per year. For comparison, Table 7 presents the estimated probability of severe core damage for the Indian Point reactors along with similar estimates from the Reactor Safety Study and other studies mentioned previously. The overall effect of the Indian Point improvements is estimated to be a three-fold reduction in the probability of severe core damage if these improvements are successfully implemented. As it turns out, it is not important to this overall analysis to determine whether each of the committed changes has been made and when. The changes committed to are clearly beneficial in reducing risk but it is questionable whether the factor of improvement, three, is statistically significant. The probabilities of severe core damage listed in Table 7 are subject to at least a factor of 5 uncertainty in either direction due to uncertainties in the data upon which all this analysis is based. Therefore, one should be very careful about attaching significance to differences in these estimates which are less than about one order of magnitude.

The effect on risk at the Indian Point site is best seen by comparison of the CCDF's. Figure 7 shows the early fatality risk curves for five different reactor designs, all at the Indian Point site, including the early fatality risk curves estimated for the Indian Point 2 reactor before the 1980 changes and after the 1980 changes.

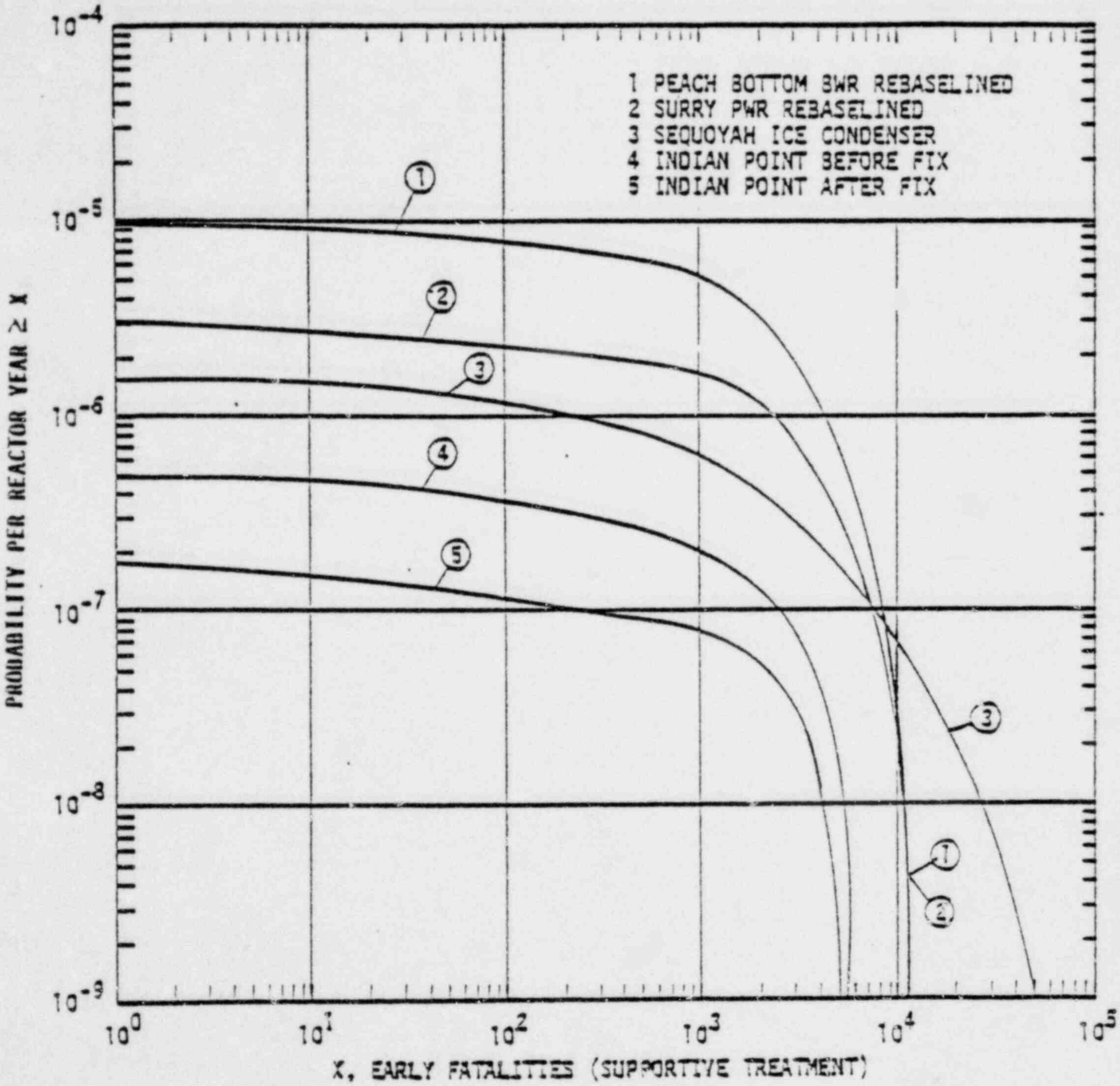
Figures 8, 9 and 10 display the same comparisons for the other risk indicators, early injuries, latent fatalities and property damage.

TABLE 7 - ESTIMATED PROBABILITY OF SEVERE CORE DAMAGE

REACTOR NAME	TYPE	PROBABILITY* OF SEVERE CORE DAMAGE PER REACTOR-YEAR
SURRY	3-loop PWR	$6x10^{-5}$
PEACH BOTTOM	BWR (Mark I)	$3x10^{-5}$
SEQUOYAH	4-loop PWR (Ice Condenser)	$4x10^{-5}$
OCONEE	2-loop PWR	$2x10^{-4}$
CALVERT CLIFFS	2-loop PWR	$2x10^{-4}$
CRYSTAL RIVER-3	2-loop PWR	$3x10^{-4}$
BIBLIS	4-loop PWR	$4x10^{-5}$
INDIAN POINT	4-loop PWR	$1x10^{-5}$

* Reflects median values

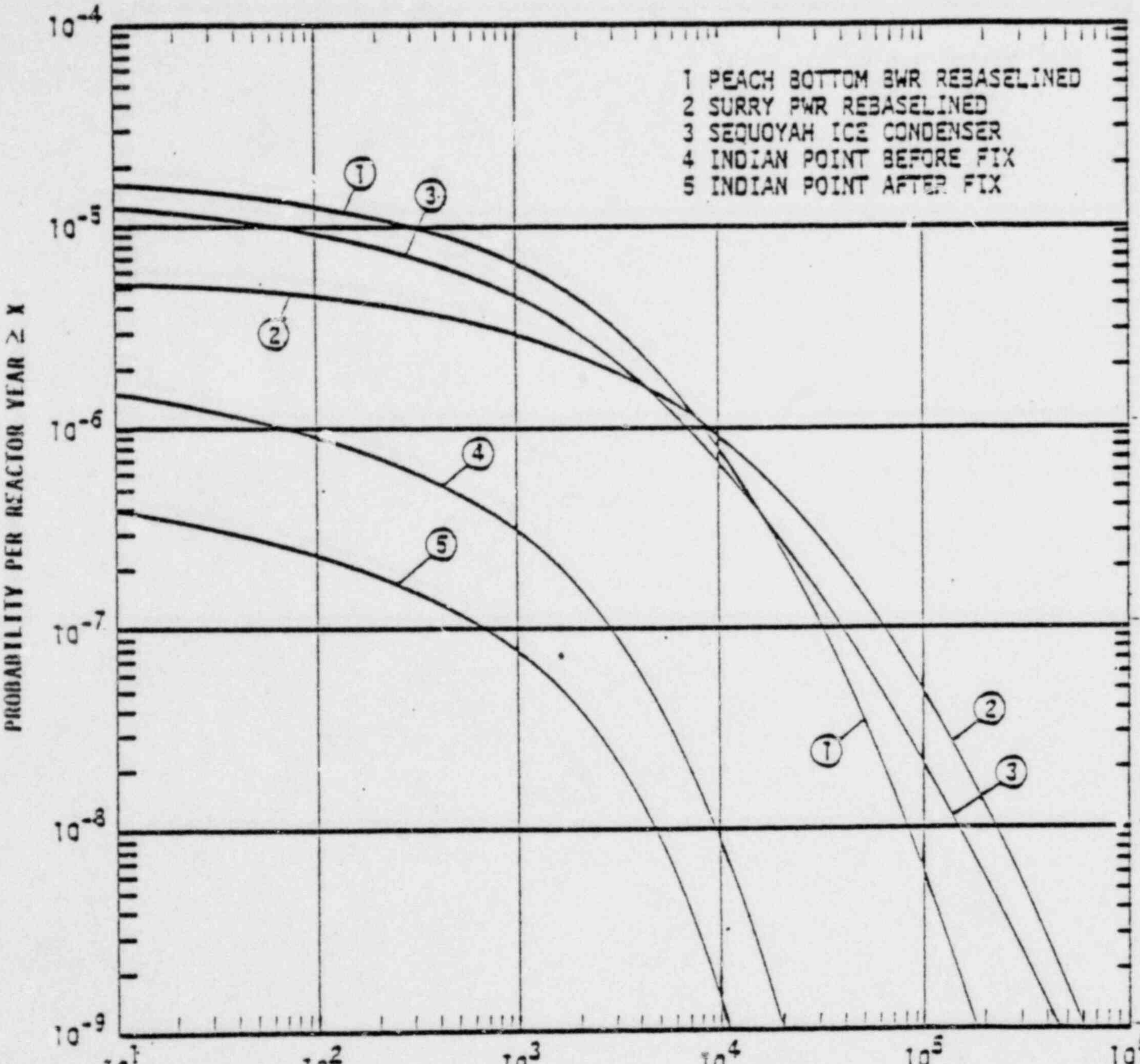
FIGURE 7 - EARLY FATALITY RISK FOR DIFFERENT DESIGNS



NOTE: THERE ARE LARGE UNCERTAINTIES WITH THE ABSOLUTE VALUES PRESENTED IN THIS FIGURE

ASSUMPTIONS: 1) INDIAN POINT SITE
METEOROLOGY - 91 WEATHER SEQUENCES
WIND ROSE WEIGHTED 1970 CENSUS POPULATION DISTRIBUTION
UNIT 3 POWER LEVEL (3025 MWT)
2) WITHIN 10 MILES - ENTIRE CLOUD EXPOSURE + [illegible] HOURS GROUND EXPOSURE
NO SHIELDING
BEYOND 10 MILES - ENTIRE CLOUD EXPOSURE + 7 DAY GROUND EXPOSURE
SHIELDING BASED ON NORMAL ACTIVITY

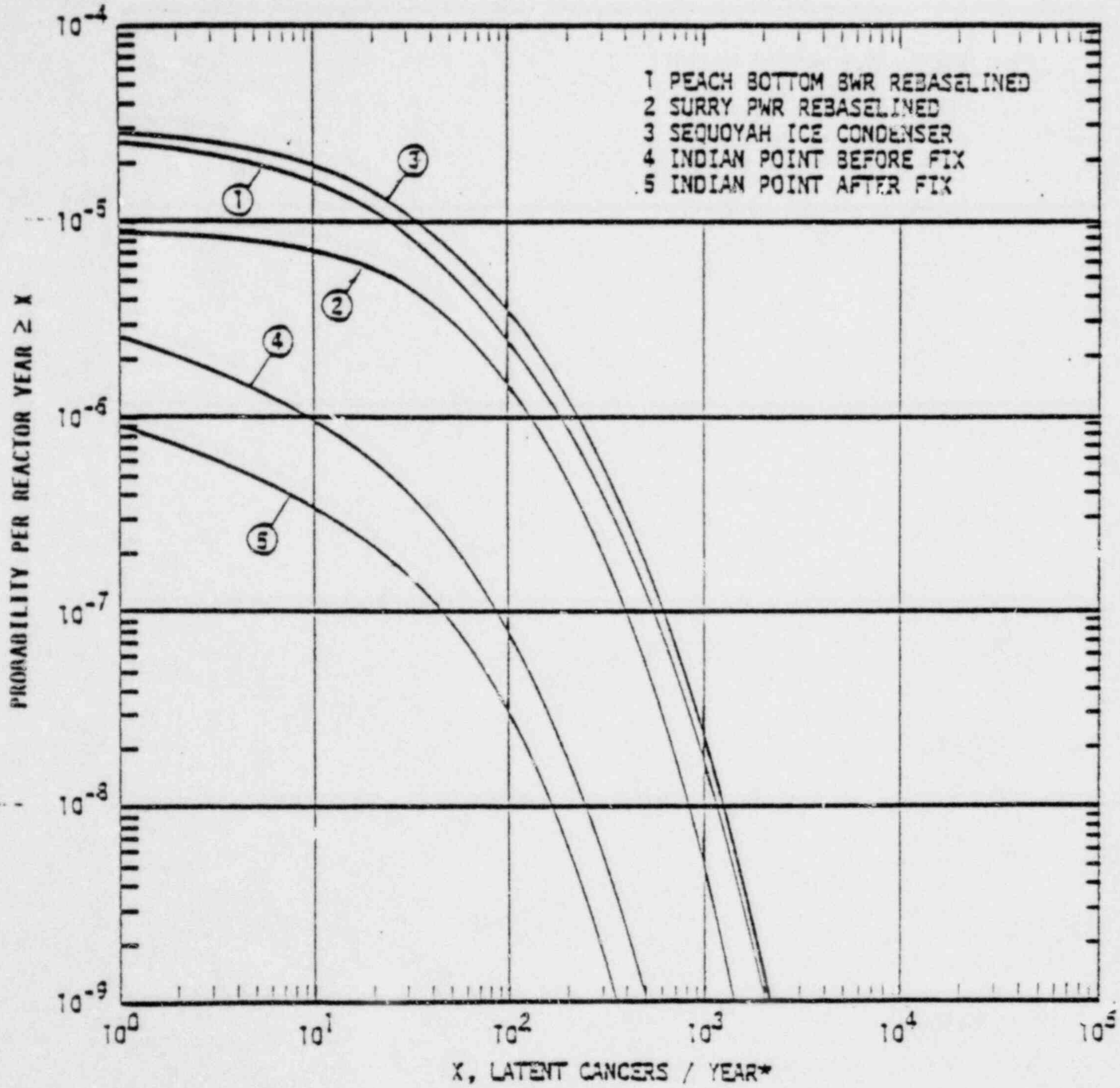
FIGURE 8 - EARLY ILLNESS RISK FOR DIFFERENT DESIGNS



NOTE: THERE ARE LARGE UNCERTAINTIES WITH THE ABSOLUTE VALUES PRESENTED IN THIS FIGURE

ASSUMPTIONS: 1) INDIAN POINT SITE
METEOROLOGY - 91 WEATHER SEQUENCES
WIND ROSE WEIGHTED 1970 CENSUS POPULATION DISTRIBUTION
UNIT 3 POWER LEVEL (3025 MWT)
2) WITHIN 10 MILES - ENTIRE CLOUD EXPOSURE + 4 HOURS GROUND EXPOSURE
NO SHIELDING
BEYOND 10 MILES - ENTIRE CLOUD EXPOSURE + 7 DAY GROUND EXPOSURE
SHIELDING BASED ON NORMAL ACTIVITY

FIGURE 9 - LATENT CANCER RISK FOR DIFFERENT DESIGNS

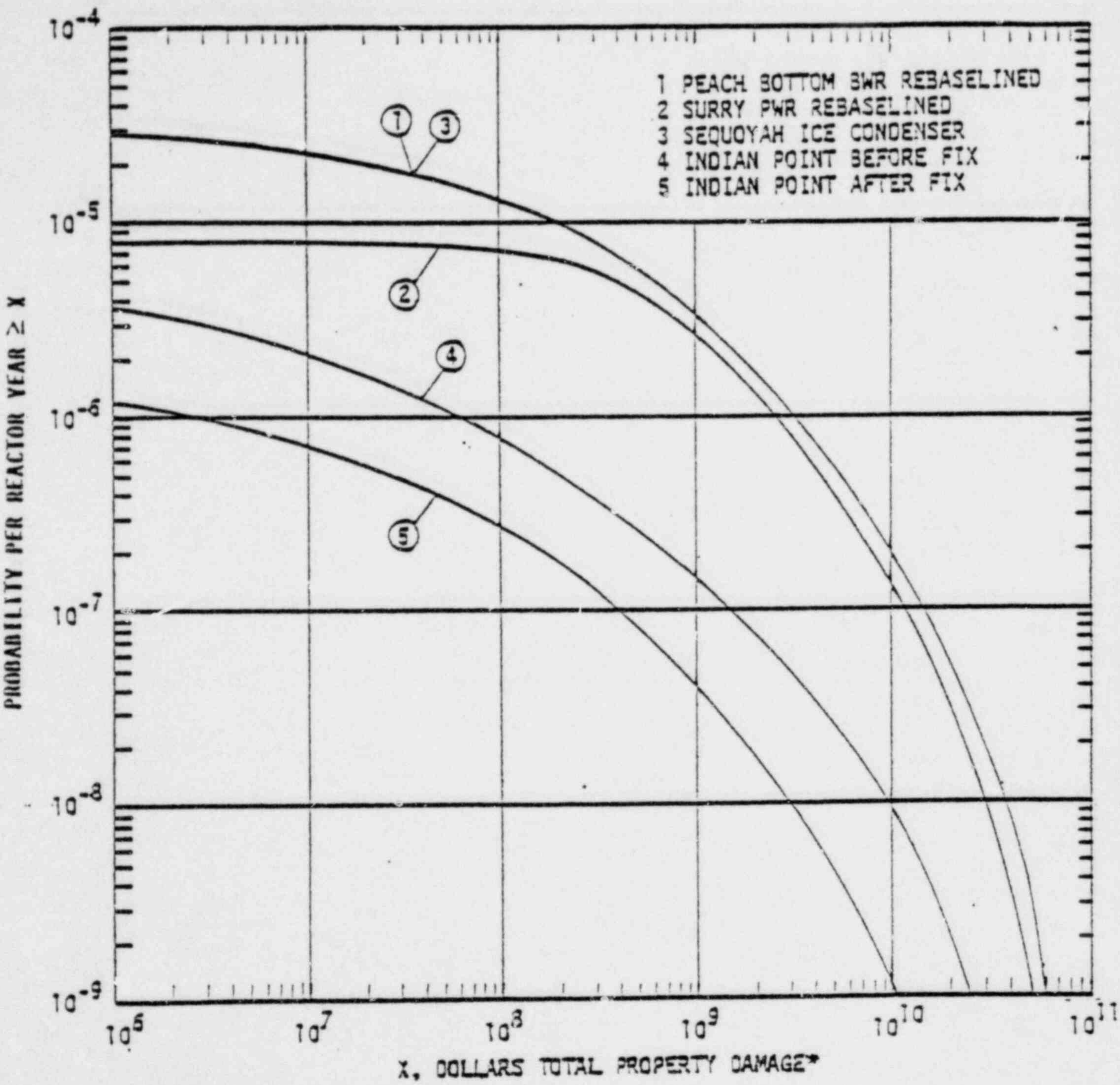


*TOTAL LATENT CANCERS WOULD BE 30 TIMES HIGHER

NOTE: THERE ARE LARGE UNCERTAINTIES WITH THE ABSOLUTE VALUES PRESENTED IN THIS FIGURE.

ASSUMPTIONS: 1) INDIAN POINT SITE
METEOROLOGY - 91 WEATHER SEQUENCES
WIND ROSE WEIGHTED 1970 CENSUS POPULATION DISTRIBUTION
UNIT 3 POWER LEVEL (3025 MWT)
2) WITHIN 10 MILES - ENTIRE CLOUD EXPOSURE + 4 HOURS GROUND EXPOSURE
NO SHIELDING
BEYOND 10 MILES - ENTIRE CLOUD EXPOSURE + 7 DAY GROUND EXPOSURE
SHIELDING BASED ON NORMAL ACTIVITY

FIGURE 10 - PROPERTY DAMAGE RISK FOR DIFFERENT DESIGNS



*BASED ON 1974 DOLLARS

NOTE: THERE ARE LARGE UNCERTAINTIES WITH THE ABSOLUTE VALUES PRESENTED IN THIS FIGURE

ASSUMPTIONS: 1) INDIAN POINT SITE
METEOROLOGY - 91 WEATHER SEQUENCES
WIND ROSE WEIGHTED 1970 CENSUS POPULATION DISTRIBUTION
UNIT 3 POWER LEVEL (3025 MWT)

The reactor designs whose risk profiles are considered here include the two reactors considered in the Reactor Safety Study, Surry and Peach Bottom; the Sequoyah plant with its ice condenser and the two versions of the Indian Point design. The risk profiles are presented only for these reactors and not the others listed in Table 7 because there was not time to do the others.

When considering the CCDFs presented in Figures 7, 8, 9 and 10, it is important to keep the uncertainties in mind. WASH-1400 assigned an uncertainty of plus or minus a factor of five to analysis such as this. The Lewis Committee questioned that small an uncertainty. We believe it is prudent to consider that these curves have an uncertainty, plus or minus, of about a factor of 10 at the higher probabilities and perhaps as much as a factor of 100 at the lower probabilities. Thus, one can attach significance to the range shown but not to modest differences between curves.

As indicated by the curves, the risk of the Indian Point reactors appears to be even lower compared to the other reactors than the ratio of their core damage probabilities would suggest. Table 8 presents the expected annual consequences or the risk from these five different designs at the Indian Point site. If one postulates that the Surry design is a typical reactor, then "Indian Point After Fix" poses about 30 times less risk of early fatalities, about 50 times less risk of early injuries, about 30 times less risk of latent cancers, and about 50 times less risk of property damage. At this time, not enough is known about the overall risk profile of all the individual plants in the U.S. to say what is typical or even what the range is. The variation of the design and operation parameter done in this analysis was based on information available, not on identifiable bounds.

TABLE 8

EXPECTED ANNUAL CONSEQUENCES (RISK) FROM 5 LWR DESIGNS
AT THE INDIAN POINT SITE

Design \ Prob. of Consequence Occurrence per yr	Early Fatalities	Early Injuries	Latent Cancer/Yr*	Property Damage $**
IP After Fix	$2.2x10^{-4}$	$2.7x10^{-4}$	$1.6x10^{-5}$	199
IP Before Fix	$6.3x10^{-4}$	$9.5x10^{-4}$	$4.4x10^{-5}$	700
Surry Rebaselined	$6.1x10^{-3}$	$1.5x10^{-2}$	$5.4x10^{-4}$	9550
Sequoyah Ice Condenser	$2.7x10^{-3}$	$2.2x10^{-2}$	$1.2x10^{-3}$	14800
Peach Bottom BWR Rebaselined	$1.7x10^{-2}$	$3.1x10^{-2}$	$1.1x10^{-3}$	13500

*Total Latent Cancers Would Be 30 Times Higher

**Based on 1974 Dollars

NOTE: THERE ARE LARGE UNCERTAINTIES WITH THE ABSOLUTE VALUES PRESENTED IN THIS TABLE.

ASSUMPTIONS:
1. INDIAN POINT SITE
   METEOROLOGY - 91 WEATHER SEQUENCES
   WIND ROSE WEIGHTED 1970 CENSUS POPULATION DISTRIBUTION
   UNIT 3 POWER LEVEL (3025 MWT)
2. WITHIN 10 MILES - ENTIRE CLOUD EXPOSURE + 4 HOURS GROUND EXPOSURE
   NO SHIELDING
   BEYOND 10 MILES - ENTIRE CLOUD EXPOSURE + 7 DAY GROUND EXPOSURE
   SHIELDING BASED ON NORMAL ACTIVITY

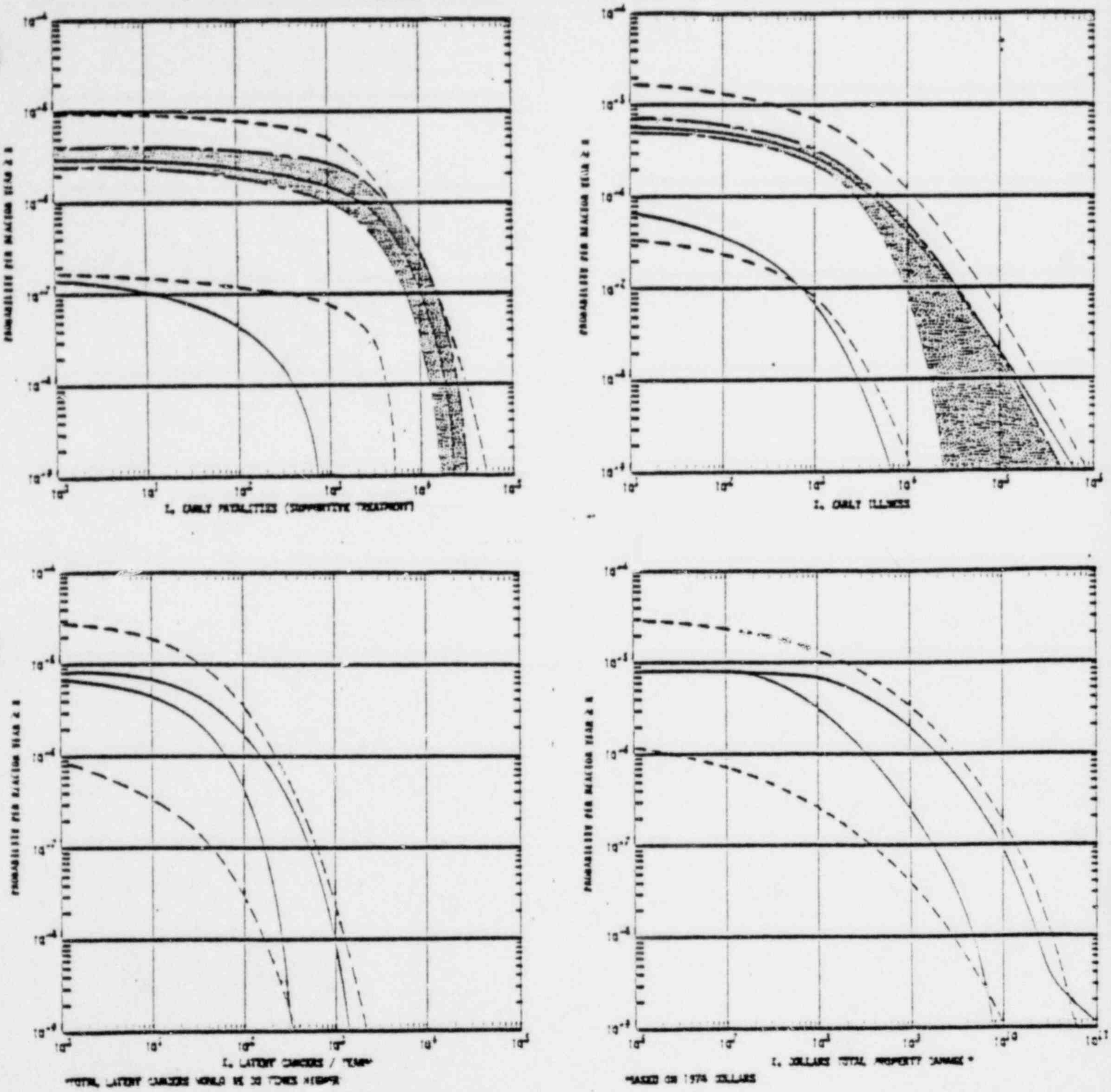
THE SENSITIVITY OF RISK TO VARIATIONS IN SITE, PUBLIC PROTECTION, AND DESIGN/OPERATING CHARACTERISTICS

In the preceeding sections the risk was considered for variation of three basic parameters, the reactor site, the public protection measures taken, and the different reactor plant design and operating characteristics. For the first, a single reactor design, Surry, was placed at six different sites. The degree of uncertainty in this site comparison is not as great as for the design comparison because, although there are substantial uncertainties in the model, the sites differ only by two relatively well understood parameters, demography and meteorology. The demography differences dominate the comparison. The same degree of uncertainty exists for the public protection measure variation, since no evacuation logistics analysis is made here. The model used for these analyses works just on the demography.

For the design variation there is much greater uncertainty. The comparison of one plant to another involves different levels of study, different dominant accident scenarios, and the use of a great deal more judgment by the analyst. Previous work by the staff in evaluating the reliability of auxiliary feedwater systems in many PWRs was done on a more consistant basis, where each plant received approximately the same depth and scope of analysis. The results of that analysis showed reliability variations for that one important system from plant to plant ranging over two orders of magnitude, about as much as was shown here for site variation.

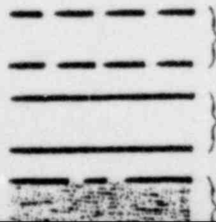
Figure 11 was drawn to display the range of variation for the three parameters of this analysis. On each of the four graphs shown in Figure 11, the solid lines show the bounds of variation when the same reactor was moved from site to site. The long-short-long lines with shading in the first two graphs show the bounds for variation of public protective action options, all with the pessimistic (or realistic) exposure assumptions described previously. The dashed lines on all four graphs show the range of variation of a few reactor designs that were analyzed. We expect the full range of variation of risk due to design factors from the best to worst plant in the country to be broader than the small sample shown here. Figure 11 suggests that the most significant parameter affecting risk is the design and operation of the plant. The site is a significant variable more for early effects and the public protection options as shown here are the least significant.

FIGURE 11 - RANGES OF RISK VARIAT...N



NOTE 1. THE RANGES REPRESENT BEST ESTIMATES ON A COMPARATIVE BASIS. THERE ARE LARGE UNCERTAINTIES WITH THE ABSOLUTE VALUES PRESENTED IN THIS FIGURE.

2. PUBLIC PROTECTIVE MEASURES HAD NO SIGNIFICANT IMPACT ON TOTAL LATENT CANCER OR ON TOTAL PROPERTY DAMAGE.



ESTIMATED RANGE OF CONSEQUENCES FOR VARIOUS DESIGNS CONSIDERED AT INDIAN POINT SITE.

ESTIMATED RANGE OF CONSEQUENCES FOR 6 SITES CONSIDERED WITH SURRY DESIGN.

ESTIMATED RANGE OF CONSEQUENCES FOR VARIOUS PUBLIC PROTECTIVE MEASURES CONSIDERED AT INDIAN POINT SITE.

THE RISK OF AN INDIAN POINT REACTOR COMPARED TO OTHER REACTORS

The preceeding sections examined the risk of the Indian Point site and the Indian Point reactor designs separately. From those examinations it appears that the site is about an order of magnitude more risky than a typical site and the design about as much less risky than a typical design. There is much more certainty in our comparison of the relative site risks than there is in the comparison of the design risks. It is reasonable to conclude that the two about cancel, that is, the overall risk of the Indian Point reactor is about the same as a typical reactor on a typical site. We recognize that such a comparison makes no explicit compensation for the Indian Point risk entailing notably higher consequences even if at lower probability than is typical. It is not unusual in risk aversion to demand lower risk as the potential consequences increase - as the stakes get higher. Accordingly, one might argue that the probability should be more than a magnitude lowe if the consequences can be a magnitude higher.

REDUCTION OF OPERATING POWER LEVEL

Obviously, reactor accident risk can be essentially eliminated by shutting down the reactor. Reducing the operating power level can reduce risk in two ways, by reducing the potential consequences of an accident and by reducing the probability of an accident occurring or running its course. Reducing the operating power level of a reactor does not reduce the potential consequences proportionately until long after the power level reduction is enforced. A typical PWR core is divided into three sets of fuel assemblies. One set is replaced at each refueling, so that each fuel assembly experiences three operating cycles in its period of use. The accident risk posed by a reactor arises from the inventory of fission products which builds up in these fuel assemblies. Based on the WASH-1400 analysis, about half that risk comes from iodine isotopes with half-lives of no more than eight days. For these iodine isotopes, the equilibrium inventory level is proportional to power level, and is reached in about a month at that power. After about a month, then the iodine contribution to risk is going to be directly proportional to steady state power level.

The other half of the estimated accident risk is dominated by isotopes of elements such as tallurium, casium and strontium, having fairly long half-lives, e.g., of years. Some of these isotopes never reach an equilibrium level in the fuel as do the short-lived ones but continue to build up in proportion to both power level and the time spent at that level, in essence, in proportion to the number of fissions. Therefore, an operating power level reduction will not proportionately reduce the risk from these isotopes unless there is also a reduction in the fuel burnup allowed.

The reduction of operating power level can also have an effect on accident risk by reducing the fuel operating temperature levels and by reducing the amount of decay heat which must be removed after shutdown. At lower power levels the heat output of the fuel is lower. Since the coolant temperature remains essentially the same as at full power, the result is lower temperature of the fuel and much of the metal surrounding it. The advantage of reduced fuel temperature in an accident is the fact that the fuel has that much more capability of absorbing heat before it reaches severe damage temperature or melts. Thus, the core can tolerate longer periods without proper cooling before damage is done.

Continued operation at reduced power level will also reduce the amount of decay heat generated after shutdown, in proportion to the degree of power reduction. This, as well as lower fuel temperatures, increases the length of time the core can run without proper cooling before damage occurs. With increased tolerance of poor core cooling, there is more time for corrective action by the operators in the event of an accident.

No quantitative analyses were performed to estimate the degree of risk reduction that can be achieved by reduction of the operating power level but, from the factors involved, it appears reasonable to say that risk would be reduced in proportion to the reduction in power level.

SECTION 2. SOCIAL AND ECONOMIC IMPACT CONSIDERATIONS

EFFECTS OF AN INDIAN POINT STATION SHUTDOWN ON ELECTRICAL POWER RELIABILITY IN THE NEW YORK POWER POOL

The New York Power Pool (NYPP) coordinates the generation and delivery of electric power for the State of New York. Its members operate according to certain standards, including the requirements that NYPP members maintain an installed generating capacity reserve equal to 18 percent of maximum one hour net load. There are seven investor-owned and one state owned utility in the NYPP with a total capacity as of Summer 1979, of nearly 30,000 Mw.

Consolidated Edison represents about 31 percent (9400 Mw) and the Power Authority of the State of New York (PASNY) about 22 percent (6700 Mw) of the total capacity of the NYPP. The electric service area of CON ED consists of the five boroughs of New York City and a major part of Westchester County, an area of 600 square miles with over eight million customers. PASNY does not have any geographically defined "service territory" but serves particular classes of customers in all parts of the State of New York.

Southeastern New York State is a summer peaking region. CON ED's summer peak load, in particular, is about 40 percent higher than its winter peak load mainly due to the widespread use of electric air conditioning. The remainder of New York State is a winter peaking region. The total NYPP System peaks in the summer.

According to a recent DOE analysis,[1] attached here as Appendix C, the forecast of the combined 1980 summer peak for CON ED and PASNY is 9403 Mw as shown in Table 1. Total PASNY and CON ED capacity is approximately 16,000 Mw. If Indian Point Units 2 and 3 are removed from the system and an 18 percent reserve margin is added to the forecast summer 1980 CON ED-PASNY peak, there is still an apparent excess capacity of about 3000 Mw.

However, much of PASNY's capacity is not in the Southeastern New York area, but elsewhere in the State. Major PASNY facilities in Southeastern New York include Indian Point #3 and Astoria #6 with a combined megawatt rating of approximately 1740 Mw.

If one assumes that one-half of the projected summer peak demand for the PASNY system originates in the New York City area[2], and if the location of PASNY's generating capacity is taken into account, then the reserve picture changes considerably as shown in Table 2. It should be noted that of the total capacity of some 9300 Mw nearly 2000 Mw are combustion turbines which are generally not planned or designed for prolonged operation. Given the projected summer load for the Southeastern New York area, the shutdown of Indian Point #2 and #3 would result in insufficient capacity (by some 250 Mw) to maintain an 18 percent reserve. All of the reserve capacity disappears and energy would have to be imported from other parts of the NYPP if scheduled outages, summer capacity reductions and historically experienced forced outages of some 1500 Mw are accounted for. In addition, if the largest unit (Ravenswood #3 - 928 Mw) is lost, the DOE analysis concludes that the utilities would be forced to use all available capacity and interties to the maximum reasonable extent.

1/Letter to Edward J. Hanrahan from Richard Weiner, Director, Division of Power Supply and Reliability, Economic Regulatory Administration, DOE, May 15, 1980.

2/Letter to Hanrahan, op. cit., p.2, DOE states that PASNY's projected summer 1980 peak load is 2503 Mw "of which less than half is in New York City and Westchester County areas".

Table 1

Reserve Situation for the CON ED and PASNY Systems
(Summer, 1980)
(Mw)

	CON ED	PASNY	TOTAL
(1) Summer Peak, 1980	6900	2503	9,403
(2) Summer Peak, 1980 + 18% reserve margin	8142	2953	11,095
(3) Capacity with Indian Point 2, 3	9441	6740	16,181
(4) Capacity without Indian Point 2, 3	8592	5775	14,367
(4) - (2) Apparent Excess Capacity	450	2822	3,272

Table 2

Revised Reserve Situation for CON ED and
PASNY Systems (Summer, 1980)
(MW)

	CON ED	PASNY	TOTAL
(1) Summer Peak, 1980	6900	1251	8,151
(2) Summer Peak, 1980 + 18% reserve	8142	1475	9,617
(3) Capacity without Indian Point 2, 3	8592	775	9,367
(3) - (2) Excess Capacity	450	-700	- 250

The bulk power transmission tie line capability above scheduled transfers is limited as shown in Table 3. All but LILCO is expected to have sufficient excess capacity in summer 1980 to transfer to the limit of the intertie. LILCO is expected to be able to supply an average of only 100 Mw. There also may be some contingency support through the submarine cable from Connecticut, but this would be limited to only 145 Mw.3/

Table 3

Bulk Power Transmission Capability
Above Scheduled Transfers (Mw)

FROM	SUMMER 1980	WINTER 80-81
Upper State New York	500	2200
Pennsylvania-New Jersey-Maryland Interconnection (PJM)	150	50
Long Island Lighting Co. (LILCO)	475	550
TOTAL	1125	2800

OTHER EFFECTS OF INDIAN POINT SHUTDOWN

Aside from reliability consideration, the costs to the service area of the CON ED and PASNY Systems of a shutdown of the Indian Point Station include expected increases in cost of service. Indian Point provides electrical energy to the system at a cost in between hydroelectric and oil-fired generation. These types of facilities along with the Fitzpatrick nuclear plant provide

3/ Letter to Hanrahan, op. cit., p. 2.

almost all of the power for the CON ED and PASNY system. The least expensive method of replacing power lost as a result of the shutdown of Indian Point station appears to be PASNY's hydro facilities as well as the purchase of hydro-generated power from the NYPP and Hydro Quebec if available. These facilities, however, are not in the Southeastern New York area, and the transmission facilities into that area are limited according to the DOE analysis.

Assuming that oil-generated power replaces the energy lost by shutting down Indian Point station, it is possible to calculate an upper bound to the economic costs of such an action. If Indian Point station operated at its historic capacity factor of 60 percent, it would produce about 800 million kilowatt-hours per month. Approximately 1.4 million barrels of oil per month would be needed to produce the equivalent amount of oil-fired electricity. At $31 per barrel this would amount to approximately $42 million per month in fuel costs without adjustment for differences in non-fuel operating costs and uranium fuel costs saved. The major impact would be the bill for oil, much of which would likely be imported. This, of course, assumes that none of the energy shortfall could be made available from non-oil generated power.

SECTION 3. SUMMARY OF PUBLIC COMMENTS

This section summarizes public comments that bear on interim operation. Numbers in parentheses accompanying the comment summaries refer to the comment numbers assigned in SECY-80-168, which contains a full compilation of public comments on the Director of NRR's Indian Point decision received in response to the Commission solicitation of comments. Considerations in the Director's decision that bear on interim operation are also summarized.

SAFETY ARGUMENTS

Director's Decision

The Director relies on two considerations in not ordering interim shutdown for the one to two-year period required to determine and install required additional design safety features:

First, several compensating features for the high population density already exist in the design of Indian Point 2 and 3. These include:

1. Containment weld channels and weld channel pressurization system.
2. Containment penetration pressurization system.
3. Isolation valve seal water system.
4. Extra containment fan cooler capacity.
5. Post-LOCA hydrogen control capability by both recombiner and purge.
6. Third auxiliary feedwater pump, providing added assurance over a twice 100 percent capacity system.
7. HEPA and charcoal filters for containment atmosphere cleanup.
8. Confirmatory actuation signals to power operated valves which are not required to change position.

9. Extra margin in service water and component cooling water supply.

10. Redundant electrical heat tracing on borated systems.

Second, a number of extraordinary interim measures are to be implemented by the licensees -- some immediately and others within various deadlines (30, 60, 90, and 120 days, and 6 months). These measures are specified in Appendix A of the Director's Order. Included among them are matters dealing with modes of operation, shift manning levels, enhanced training of operators, and special containment tests. Some of the numerous specific requirements are:

A. Effective immediately:

   1. Limit power level to keep peak fuel clad temperature at or below 2000° F under large LOCA conditions.

   2. Operate in base load mode only, without load following.

   3. Have at least two senior operators in the control room during operation or hot shutdown.

B. Within 30 days:

   1. Have vendor representative on site for engineering consultation.

   2. Assure control room habitability under accident conditions.

   3. Enhanced training and retraining provisions.

   4. Special diesel generator tests.

Comments Favoring Interim Shutdown

Commenters' safety arguments for interim shutdown relate to emergency plans, timing of long-term fixes, interim measures, short-term risks, dense population, and psychological impact.

1. Emergency plans:

UCS (#85) argues that no plans exist today to evacuate the public within even 10 miles of the site. (#85 at 8 and 13.) Both UCS and Mid-Hudson Nuclear Opponents cite testimony by the County Executive of Westchester County that existing plans are not workable. (#85 at 13 and #86 at 2.) UCS argues that there has never been an assessment of the consequences of a major accident at Indian Point, implying that a basis for emergency planning is lacking, despite NRC's post-TMI commitment to improve emergency planning. (#85 at 8.) They refer to great difficulty of making effective emergency plans for the area due to physical and demographic characteristics. (#85 at 8 and 13.) They further comment that the staff has not clearly found that the licensees' emergency plans comply with the applicable Regulatory Guide (1.101) and that, moreover, Regulatory Guide 1.101 does not require evacuation plans out to 10 miles -- a requirement that will not become operative till 1981. (#85 at 20-21.) They conclude that today, in the absence of effective protection, the risk is too great to permit the plants to operate. (#85 at 34.)

Mid-Hudson Nuclear Opponents (#86) urge interim shutdown in view of the large population density and absence of adequate evacuation plans for a reasonable distance (15 to 25 miles) (#86 at 4).

New York Public Interest Research Group asserts that it would take an estimated two weeks to evacuate The Bronx, whereas only 1-1/2 days would be available in case of a disaster at Indian Point (4-1/2 days with a "core catcher"). (#67 at 4.)

2. Timing of long-term fixes:

UCS contends that there is no licensee commitment and no requirement established by the Director's order for licensee implementation of the protective-action time-buying provisions (filtered vented containment and core ladle): only a review of possible modifications is required. (#85 at 10-11.) They see evidence of a dispute between the staff and the licensee concerning possible imposition of Class 9 accident related requirements. (#85 at 11-12.) UCS argues that the mere possibility of future protection offers no protection today. (#85 at 11.)

Mid-Hudson Nuclear Opponents refer to post-accident monitoring, aging, and asymmetric LOCA loads as serious unresolved safety issues. They consider it insufficient for control of present risks to merely say that these issues are being examined -- with an unspecified schedule. (#86 at 3.)

3. Interim measures:

UCS comments to the effect that (a) the special safety measures already present at Indian Point 2 and 3 are of little real value and (b) that the special interim measures yet to be implemented (which, in any case, they regard as inadequate for the long term) should not be counted now, because implementation is largely deferred. (#85 at 15-21, 27-34, and passim.) With respect to the special safety features identified in the Director's Decision as already present, UCS comments specifically on each. (#85 at 15-20.) They impugn each, usually on one or both of two grounds: (a) that they do little or no good -- or are even counterproductive -- and (b) that they merely reflect implementation of present requirements or correction of inadequacies that could not be tolerated anywhere. Thus, for weld channel and penetration pressurization and the isolation-valve seal-water system, they argue that these measures merely compensate for bad welds or leaky valves. (#85 at 16.) For containment atmosphere cleanup, they contend that NRC regulations (Design Criterion 41) require such provision for all plants. (#85 at 18.) Purging for hydrogen control is criticized as counterproductive. ("[T]he staff proposes to seal the containment normally but to vent it during an accident with no capability to filter ....") (#85 at 17.) For further interim measures, they argue that they are neither extraordinary nor sufficient, and not yet in place. (#85 at 33 and passim.) The interim measures leave the safety issues raised by UCS unresolved. (#85 at 33.) They stress fire protection, post-accident monitoring, equipment aging, and asymmetric LOCA loads. (#85 at 26-31.)

4. Short-term risks:

UCS asserts: "Little by little, the short-term grows into the long-term." (#85 at 32.)

Dean Corren, of Greater New York Council on Energy, expresses the view that distinction between short-term and long-term risks is "an improper and misleading use of the notion of statistical risk assessment." (#80 at 1.) He contends that any safety improvements that are deemed necessary at all are necessary forthwith. Brooklyn SHAD offers a similar argument. (#63)

Westchester People's Action Coalition views the risks pending completion of fixes as excessive even "for one more day." (#19 at 3.)

5. High population density:

UCS stresses the high population density as an obstacle to effective emergency action. They cite Robert Ryan (NRC's Director of State Programs) as characterizing Indian Point as an "insane" site, "a nightmare from the point of view of emergency preparedness," with difficulties exacerbated by severe traffic problems. (#85 at 8-9.)

Westchester People's Action Coalition argues that dense population inevitably makes Indian Point 10 times more dangerous than the average plant, since plant safety improvements practical at Indian Point should be made nationwide. (#19 at 5.)

Mid-Hudson Nuclear Opponents ask for suspension of the licenses pending the Commission's decision, in view of the large population density and inadequate emergency plans. (#86 at 4.)

6. Psychological impact:

Westchester People's Coalition calls for consideration of human responses to minor mishaps, rumors of accidents, or threat of accident. They write of human costs in anxiety and potential panic. (#19 at 3.)

Comments Opposed to Interim Shutdown

Arguments against interim shutdown relate to risk estimates, evacuation, and population density.

1. Risk estimates:

Power Authority of the State of New York (PASNY) (#66) maintains that the staff's risk estimates for Indian Point overstate the risk. (#66 at 17.) They argue that special plant features already existing (identified in the Director's Decision) distinguish Indian Point from average PWR's and lower the Indian Point risks substantially below those derived from WASH-1400. (#66 passim.) They present plots of Indian Point risks with and without adjustments for plant-specific features. (#66 at Appendix 2.) The plant-specific adjustments include elimination of some WASH-1400 sequences that PASNY contends are not significant contributors to core melt probability. These include loss of auxiliary feedwater after shutdown and reactor transient followed by failure of reactor trip. (#66 at 16.)

PASNY also asserts that in-vessel steam explosions now appear less likely than estimated in WASH-1400, so that containment failure due to missiles from such an explosion is also less likely. (#66 at 17.)

2. Evacuation:

Scientists and Engineers for Secure Energy (SE 2) (#62) describes the emergency evacuation of Mississauga, Canada, a city of 240,000, in November 1979, in connection with derailment of a train that included 11 propane tanks. SE 2 cites that experience as showing that massive evacuations are feasible. (#62 at 3.)

Corren (#80) encloses a statement of PASNY before the Committee on Environmental Protection of the New York City Council, dated December

14, 1979, in which PASNY argues evacuability to 10 miles and also argues that a likelihood of evacuation being required for New York City residents under any circumstances is not realistically foreseeable. (Page 6 of PASNY enclosure to #80.)

3. Population density:

   SE 2 argues that population density around Indian Point is not unusually high by world reactor siting standards. They cite Canadian, French, British, and Japanese practices of siting reactors in densely populated areas. (#62 at 2-3.)

Differences Between Units 2 and 3

UCS contentions that Indian Point Unit 2 lacks some important safety features of Unit 3 suggest that their arguments for interim shutdown would apply to Unit 2 *a fortiori*. (#85 at 21-23.)

IMPACT ARGUMENTS

The Director's Decision does not reflect consideration of social or economic impacts of interim shutdown.

Comments on this general subject deal with need for power, cost of power, and effect on oil imports.

1. Need for power:

   Westchester People's Action Coalition (#19) contends that Indian Point's power is not needed. They assert that there is 50 percent excess capacity in New York; 30 percent without nuclear facilities. They further assert that there have been no capacity-related blackouts, even though Indian Point Unit 2 has been off-line for four months since last June, and Unit 3 for five. (#19 at 6.) They enclose a New York Times article from which they draw their assertions.

Dean Corren, of Greater New York Council on Energy (#80) contends that there is no need for the Indian Point capacity. (#80 at 2.) He presents capacity figures that assertedly show that there is a 3,026-MW unutilized excess capacity (on top of an 18 percent reserve over peak demand), as compared with a total Indian Point capacity of 1,838 MW. (Page 3 of first enclosure to #80.) Corren states that Con Ed still claims a 1.8 percent annual peak demand growth, although that growth has slowed to 0.1 percent. He also states that 69.3 percent of the system was idle in 1978, on the average. (Page 4 of first enclosure to #80.) He concludes that ability to meet demand would not be compromised by closing Indian Point 2 and 3. (Page 5 of first enclosure to #80.)

Corren (#80) also encloses statements by UCS and PASNY. The UCS statement (at 1) argues that the Indian Point plants are often out of service, yet New York City does not go dark. The PASNY statement (at 7 and passim) argues need for power on economic (not absolute or reliability) grounds.

2. Cost of power:

Stanley Fink, Speaker of the New York State Assembly, comments that shutdown would cause economic hardship in the Metropolitan New York area. He considers it the responsibility of NRC to work with FERC and others to secure replacement non-oil power at comparable cost, if NRC orders Indian Point temporarily shut down. (#1)

The New York State Building and Construction Trades Council sees a threat to "local economic livelihood" in any Indian Point shutdown. (#7)

PASNY contends that shutdown would be an economic calamity for New York City, costing PASNY's and Con Ed's ratepayers about $700 million in 1980 alone. Increases would escalate with imported oil price increases. (#66 at 20-21.) According to PASNY, 45 percent of the power cost increase would fall on public customers -- New York City and its Metropolitan Transportation Authority (MTA). These entities are already financially hard pressed. MTA's projected $200 million deficit for 1980 would increase by $100 million for increased cost of electricity for subway and commuter rail lines. (#66 at 21.)

Corren estimates that shutdown of Indian Point would cost the average residential ratepayer between $2 and $4 per month. (Pages 11-12 and passim, first enclosure to #80; calculations at Appendix A to that enclosure.) Corren also encloses a concurring analysis by UCS. In addition, he encloses a PASNY statement (with which he takes issue). That PASNY statement is generally consistent with PASNY's comment on the Director's decision. (#66)

3. Oil imports:

Fink states that shutdown of Indian Point would exacerbate the region's dependency on imported oil and calls on NRC to work with FERC and others to secure non-oil replacement power in event of Indian Point shutdown. (#1)

PASNY comments that the region depends on oil and nuclear sources for electric power generation. (#66 at 19.) Indian Point shutdown would require 20 million barrels of imported oil per year for replacement power. (#66 at 20.)

Corren presents a "worst-case" replacement-power-cost estimate of $5.21 per month for an average residential customer, based on oil at $30 per barrel. However, he maintains that replacement fuel is likely to be a more economical mixture of oil, gas, and coal. (Pages 7 and 8 of first enclosure to #80 and Appendix A to that enclosure.)

Corren (#80) encloses a statement by UCS, which contains an estimate that replacement fuel would cause a 0.7 percent increase in total U.S. imported oil consumption. Corren's (#80) last enclosure includes a remark by Commissioner Bradford that nuclear electric generation frees up "residual oil, of which there is something of a surplus anyway."

APPENDIX A

SAMPLE GENERATION OF A COMPLEMENTARY CUMULATIVE DISTRIBUTION FUNCTION - CCDF

The CCDF is used to present the risk of reactor accidents in the form of a plot of probability vs consequences. The average reader is unaccustomed to studying risk in this form of presentation. To facilitate understanding of the CCDF, consider generating a CCDF for the risk of death from air crash from high altitude using the attached figure.

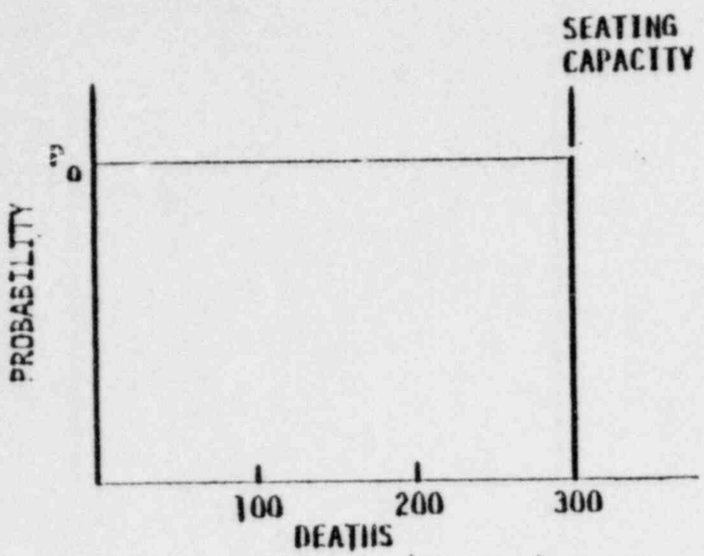
If an airplane crashes from a high altitude, it is virtually certain that all on board will perish. Thus, Figure A-1 is a reasonable first approximation of a CCDF for such a crash; it shows a probability, P_o, that 300 deaths, the seating capacity of the aircraft, will occur. P_o is the probability that the plane will crash; 300 is the limit of those on board who will die in a crash. For this simple CCDF curve the expected risk is P_o, say 0.33 crashes per year, times 300 deaths per crash or 100 deaths per year.

The CCDF can be corrected first to show that the falling aircraft might strike and kill people on the ground. Figure A-2 shows a tail on the CCDF curve reflecting that if the plane crashes, it will most likely not kill many people on the ground. At lower and lower probability, there is the chance of killing crowds in buildings or gatherings so the curve tails off toward some higher number of deaths. Presumably there is a limit to the ground deaths that can be caused by the crash of a 300 passenger aircraft, perhaps 10,000 or 20,000 if it crashed into a crowded sports stadium. At that limit, the curve would no longer tail off to the right but become a vertical line showing a physical limit analogous to the seating capacity limit.

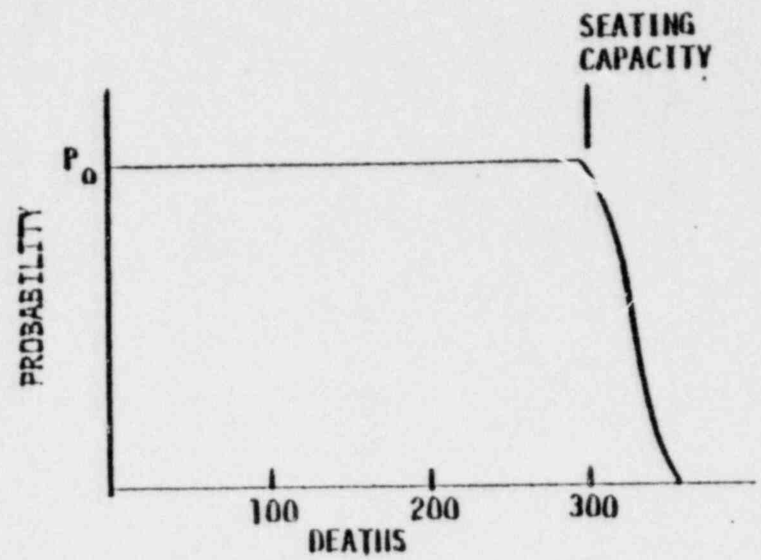
A second stage of refinement in this CCDF can be obtained if the airline gives us figures on the actual passenger loads the aircraft usually carries. If the data are limited, they might simply be reduced to the approximation that on 1/3 of the trips the plane is 1/3 full, on another 1/3 of the trips it is 2/3 full, and on another 1/3 of the trips it is completely full. The CCDF can now be refined as shown in Figure A-3. One hundred deaths occur at probability P_0, the probability of crash, because the plane is always at least 1/3 full. At 0.67 P_0 the curve shows 200 deaths because the plane is at least 2/3 full 2/3 of the time. And the curve shows 300 deaths at 0.33 P_0 because on one third of its flights all seats are filled. We can reflect the probability of ground deaths by putting soft tails on the sharp steps of the curve.

As more accurate flight data are accumulated, the steps in Figure A-3 can be refined into a more accurate curve as shown in Figure A-4. This last curve would represent the most accurate distribution of the likelihood of death from high altitude air crash.

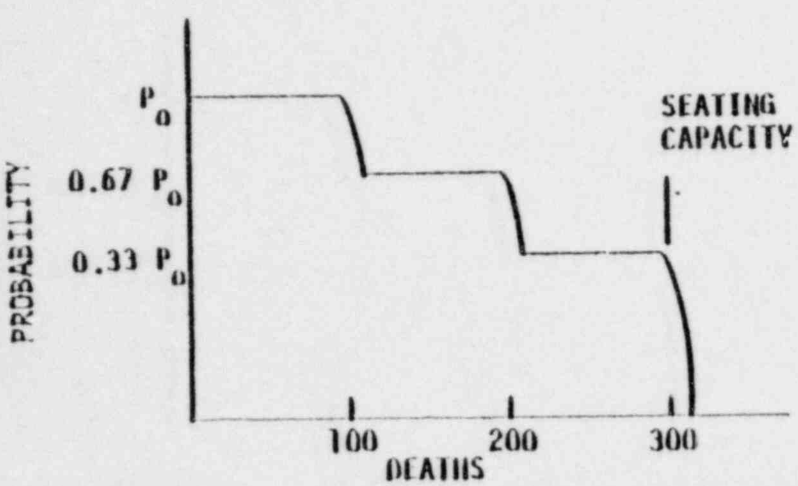
CCDF FOR AIR CRASH FROM HIGH ALTITUDE



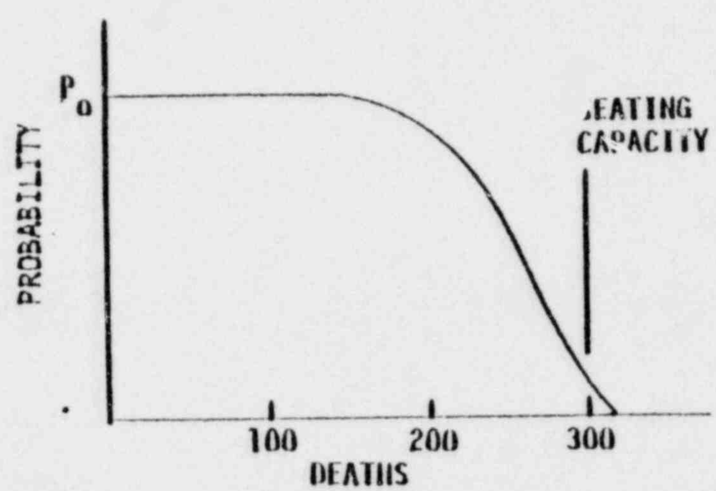
A-1



A-2



A-3



A-4

APPENDIX B

REBASELINING OF THE RSS RESULTS

The results of the Reactor Safety Study (RSS) were updated for purposes of this comparative study. The update was done largely to incorporate results of research and development conducted after the October 1975 publication of the RSS and to provide a baseline against which the risk associated with various LWRs could be consistently compared.

Primarily, the rebaselined RSS results reflect use of advanced modeling of the processes involved in meltdown accidents, i.e., the MARCH computer code modeling for transient and LOCA initiated sequences and the CORRAL code used for calculating magnitudes of release accompanying various accident sequences. These codes[1] have led to a capability to predict the transient and small LOCA initiated sequences that is considerably advanced beyond what existed at the time the Reactor Safety Study was completed. The advanced accident process models (MARCH and CORRAL) produced some changes in our estimates of the release magnitudes from various accident sequences in WASH-1400. These changes primarily involved release magnitudes for the iodine, cesium and tellurium families of isotopes. In general, a decrease in the iodines was predicted for many of the dominant accident sequences while some increases in the release magnitudes for the cesium and tellurium isotopes were predicted.

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[1]It should be noted that the MARCH Code was used on a number of scenarios in connection with the TMI-2 recovery efforts and for Post-TMI-2 investigations, e.g., Rogovin) to explore possible alternative scenarios that TMI-2 could have experienced.

Figures B1 and B2 show a comparison of the original RSS and the rebaselined PWR and BWR designs for the individual risk versus distance of early fatalities and latent cancer fatalities, respectively. These figures show the expected values conditioned upon a core melt accident of about one chance in ten thousand reactor years ($1x10^{-4}$). This particular conditioned value reflects an average of the core melt probabilities estimated from a number of LWR designs.

Entailed in this rebaselining effort was the evaluation of individual dominant accident sequences as we understand them to evolve rather than the technique of grouping large numbers of accident sequences into encompassing, but synthetic, release categories as was done in WASH-1400. The rebaselining of the RSS also eliminated the "smoothing technique" that was criticized in the report by the Risk Assessment Review Group (sometimes known as the Lewis Report; NUREG/CR-0400).

For rebaselining of the RSS BWR design, the sequence TCγ' was explicitly included into the rebaselining results. The accident processes associated with the TC sequence had been erroneously calculated in WASH-1400. For rebaselining of the RSS PWR design, the release magnitudes for the Event V and TMLB' sequences were explicitly calculated and used in the consequence modeling rather than being lumped together into Release Category #2 as was done in WASH-1400.

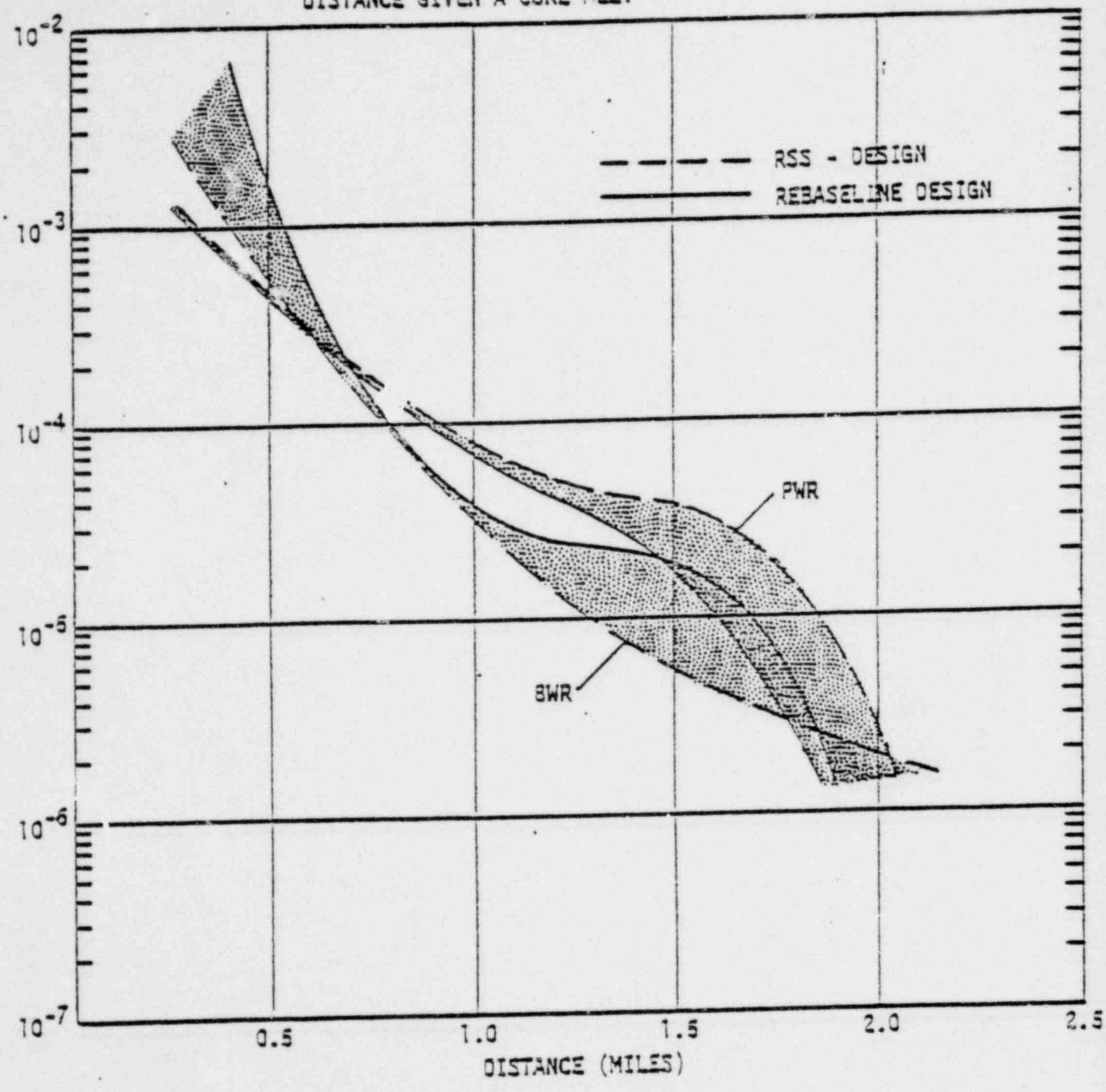
In both of the RSS designs (PWR and BWR) the likelihood of an accident sequence leading to the occurrence of a steam explosion (α) in the reactor vessel was decreased. This was done to reflect both experimental and calculational indications that such explosions are unlikely to occur in those sequences involving small size LOCAs and transients because of the high pressures and temperatures expected to exist within the reactor coolant system during these scenarios. Furthermore, if such an explosion were to occur, there are indications that it would be unlikely to produce as much energy and the massive missile-caused breach of containment as was postulated in WASH-1400.

As can be seen from Figures B1 and B2, the net (or overall) change in consequences predicted from the rebaselined RSS results are quite small. In general, the rebaselined results led to slightly increased health impacts being predicted for the RSS BWR design. This is believed to be largely attributable to the inclusions of TCγ'.

The rebaselined RSS-PWR led to a small decrease in an individual risk of early fatalities and latent cancer fatalities below the original RSS PWR. This is believed to be largely attributable to the decreased likelihood of sequences involving vessel steam explosions (α).

In summary, the rebaselining of the RSS results led to small overall differences from the predictions in WASH-1400. It should be recognized that these small differences due to the rebaselining efforts are likely to be far out-weighed by the uncertainties associated with such analyses.

FIGURE B1 - RISK OF EARLY FATALITY TO AN INDIVIDUAL VERSUS DISTANCE GIVEN A CORE MELT*



ASSUMPTIONS: *CORE MELT PROBABILITY ASSUMED TO BE 10^{-4}/REACTOR YEAR

RSS-DESIGN

1. ALL RSS CORE MELT ACCIDENT RELEASE CATEGORIES
2. ALL RSS ASSUMPTIONS (E.G., SMOOTHING)

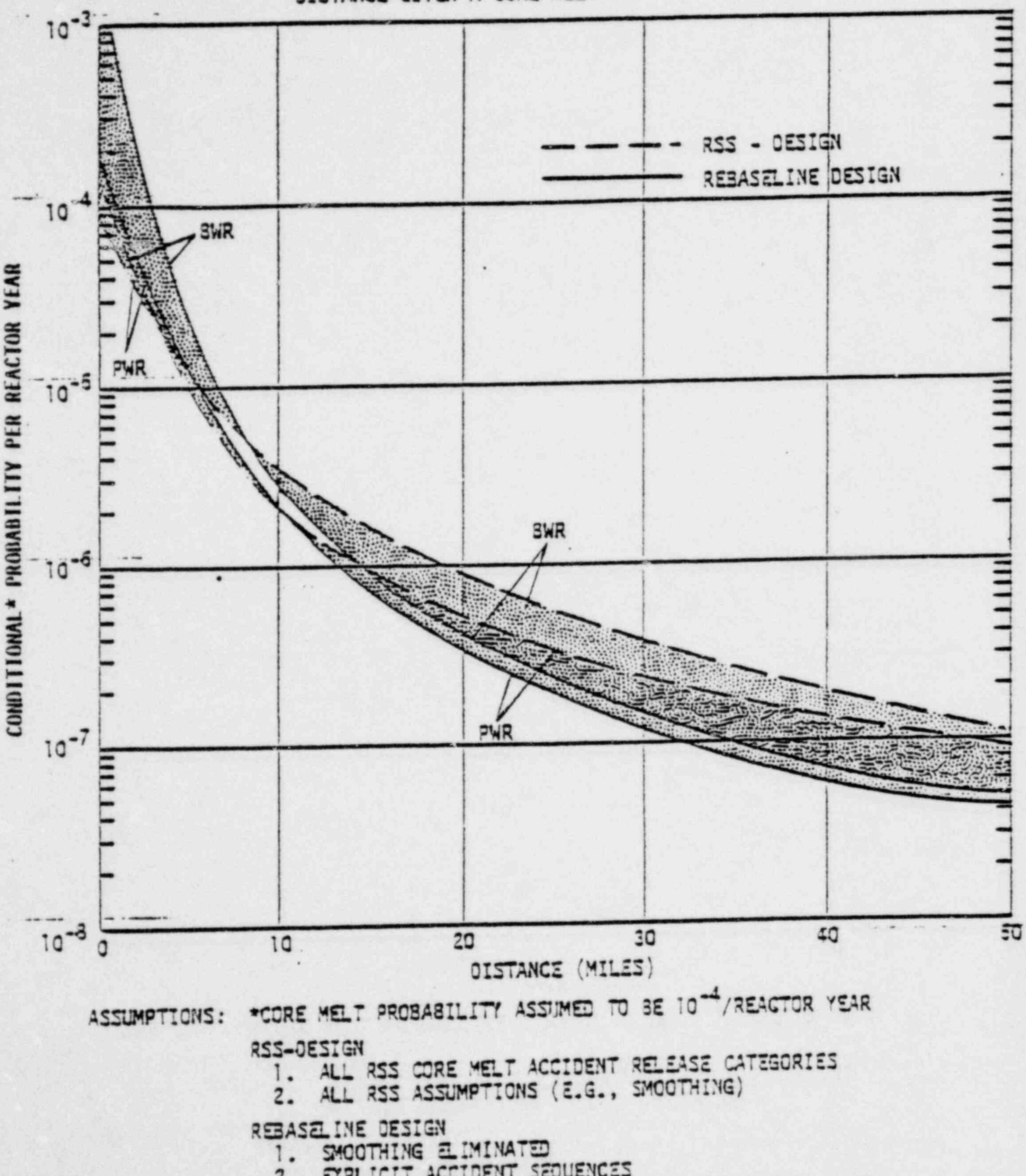
REBASELINE DESIGN

1. SMOOTHING ELIMINATED
2. EXPLICIT ACCIDENT SEQUENCES
3. NEGLIGIBLE PROBABILITY OF VESSEL STEAM EXPLOSION

EXPECTED CONSEQUENCES FROM 91 WEATHER SEQUENCES WITH 3200 MWT POWER LEVEL

ENTIRE CLOUD EXPOSURE + 24 HOUR GROUND EXPOSURE SHIELDING BASED ON NORMAL ACTIVITY

FIGURE B2 - RISK OF LATENT CANCER FATALITY TO AN INDIVIDUAL VERSUS DISTANCE GIVEN A CORE MELT*



ASSUMPTIONS: *CORE MELT PROBABILITY ASSUMED TO BE 10^{-4}/REACTOR YEAR

RSS-DESIGN
1. ALL RSS CORE MELT ACCIDENT RELEASE CATEGORIES
2. ALL RSS ASSUMPTIONS (E.G., SMOOTHING)

REBASELINE DESIGN
1. SMOOTHING ELIMINATED
2. EXPLICIT ACCIDENT SEQUENCES
3. NEGLIGIBLE PROBABILITY OF VESSEL STEAM EXPLOSION

EXPECTED CONSEQUENCES FROM 91 WEATHER SEQUENCES WITH 3200 MWT POWER LEVEL

ENTIRE CLOUD EXPOSURE + 24 HOUR GROUND EXPOSURE
SHIELDING BASED ON NORMAL ACTIVITY

APPENDIX C

Department of Energy
Washington, D.C. 20461

MAY 15 1980

Mr. Edward J. Hanrahan, Director
Office of Policy Evaluation
Nuclear Regulatory Commission
Washington, D.C. 20555

Dear Mr. Hanrahan:

This letter summarizes the views of the Economic Regulatory Administration's Division of Power Supply and Reliability (DPSR) regarding the electric system reliability impact of various modes of operation of nuclear power units Indian Point 2 and 3 as described in your April 28, 1980, letter.

Indian Point 2 is a 849 MW (summer rating) PWR unit owned and operated by the Consolidated Edison Company of New York (CON ED). Indian Point 3 is a 965 MW PWR unit owned and operated by the Power Authority of the State of New York Inc. (PASNY). The units are co-located in Westchester County, 25 miles north of New York City. Both units are included in their respective entity's planned resources available to meet customer demands in 1980.

A shutdown of Indian Point 2 and 3 would impact the reserve capacity in the New York subregion (New York Power Pool) of the Northeast Power Coordinating Council (NPCC). Without Indian Point 2 and 3 the available reserves in the New York Power Pool for the summer 1980 and winter 1980-81 seasons would decline from 46.6 and 58.2 percent to 38.0 and 49.0 percent respectively. This level of reserves is still considered adequate to provide reliable electric service when viewed on a state-wide basis. However, due to limited transmission capability into the New York City and Westchester County areas, the complete shutdown of these two Indian Point units during the 1980 summer peak period could adversely impact the system reliability of CON ED and PASNY.

The electric service area of CON ED consists of the five boroughs of New York City and a major part of Westchester County, an area of 600 square miles. CON ED supplies electricity to over eight million customers. CON ED's summer peak load is about 40 percent higher than its winter peak load due mainly to the widespread use of electric air conditioning. CON ED's projected 1980 summer peak load is 6900 MW.

PASNY does not have any geographically defined "service territory" but serves particular classes of customers in all parts of the State c New York. PASNY's projected summer 1980 peak load is 2503 MW of which less than half is in New York City and Westchester County areas.

The DPSR collected data concerning the latest electric system conditions, maintenance schedules, expected forced outages, and expec peak loads for CON ED and PASNY. The data was compared to historical data contained in various DOE documents, and revised where it was fel necessary. The conclusions drawn in this letter are based upon our analysis of this data.

The adverse impact on reliability due to the status of IP 2 and 3 results from the limited transmission system capability for importing power from other parts of the state, or from neighboring states, into the area in which these units are located. CON ED has bulk powe transmission tie lines with neighboring utilities having a megawatt transfer capability (above scheduled transfers) as shown below:

FROM	SUMMER 1980	WINTER 80-81
Upper State New York	500	2200
Pennsylvania-New Jersey-Maryland Interconnection (PJM)	150	50
Long Island Lighting Co. (LILCo.)	475	550
TOTAL	1125	2800

Energy transfers from areas outside of PJM or Upper State New York (USNY) would have to rely on the same transmission ties as transfers directly from Upper State New York and PJM. Therefore, the only time capacity available from these outside areas would need to be considered would be if the PJM and USNY areas did not have sufficient capacity to supply the transfer limit. Overall New York State generating capacity will be adequate in this time frame and all possible transfers from the north, up to the limit of the transmission system, could come from this area. PJM also projects sufficient available excess capacity during the 1980 summer to be able to provide the 150 MW of capacity for transfer. Long Island Lighting Company (LILCo) does not have sufficient available capacity this summer to provide the full 475 MW of the transfer capability and would only be able to supply an average of 100 MW. There are no other systems to the east of the area that could supply power over these Long Island transmission connections. There may be some contingency support through the submarine cable from Connecticut but this would be limited to only 145 MW.

The four possible modes of operation of IP 2 and 3 which DPSP was requested to analyze are detailed below indicating their impact on CON ED and PASNY:

- <u>Operate IP 2 and 3 at 50 percent capacity for a 3 month period beginning June 1, 1980</u>. The loss of 907 MW from the CON ED and PASNY systems during the summer peak load period along with the expected amounts of forced and scheduled outages, will enable the companies to supply their expected peak loads plus withstand the loss of the largest unit (Ravenswood #3 - 928 MW). This should be adequate for maintain reliable electric service since normal tie transfers were considered.

- <u>Shutdown IP 2 and 3 for a three month period beginning June 1, 1980</u>. The loss of 1814 MW from the CON ED and PASNY systems during the summer peak load period will force the New York City and Westchester County areas to depend very heavily upon the transmission interties with neighboring areas. Given the projected loads and expected forced and scheduled outages, the loss of the largest unit (Ravenswood #3 - 928 MW) would force the utilities to use all available capacity and interitie to the maximum reasonable extent. Further facility failure, or loads greater than forecast would force the utilities to institute voltage reductions, load curtailments, or other actio as required to prevent widespread loss of customer load. Sustained high loads during the summer period would force CON ED to operate its 1987 MW of combustion turbine generation capacity for longer periods than the units are planned and designed to operate. This mode of operation places the system in a very vulnerable position and is <u>not</u> considered consistent with providing reliable electric service.

- <u>Operate IP 2 and 3 at 50 percent capacity for a 12 month period beginning June 1, 1980</u>. The loss of 907 MW in the CON ED and PASNY systems for 12 months will have its greatest impact on system reliability during the summer months. This situation is discussed above. During the remainder of the year, CON ED and PASNY will have sufficient capacity to provide reliable electric service to their customers.

- <u>Shutdown IP 2 and 3 for a 12 month period beginning June 1, 1980</u>. The loss of 1814 MW from CON ED and PASNY will have its greatest impact on system reliability during the summer months as discussed above. During the other months of the year, CON ED and PASNY will have sufficient available capacity on their own systems and from transfers from other areas to provide reliable electric service.

This analysis deals only with electric system reliability and energy supply; it does not consider the need to reduce operating costs and conserve oil or natural gas. The outages of any large non-oil generating unit in Southeastern New York will result in increased costs to the consumers of electricity because of the resulting increased use of low sulfur oil-fired generation. I would appreciate being notified of the decision regarding Indian Point 2 and 3.

Sincerely,

Richard E. Weiner

Richard E. Weiner, Director
Division of Power Supply
and Reliability
Economic Regulatory Administration

BY ~~[illegible]~~ CYBULSKIS

MARCH
MELTDOWN ACCIDENT RESPONSE CHARACTERISTICS

Presentation by

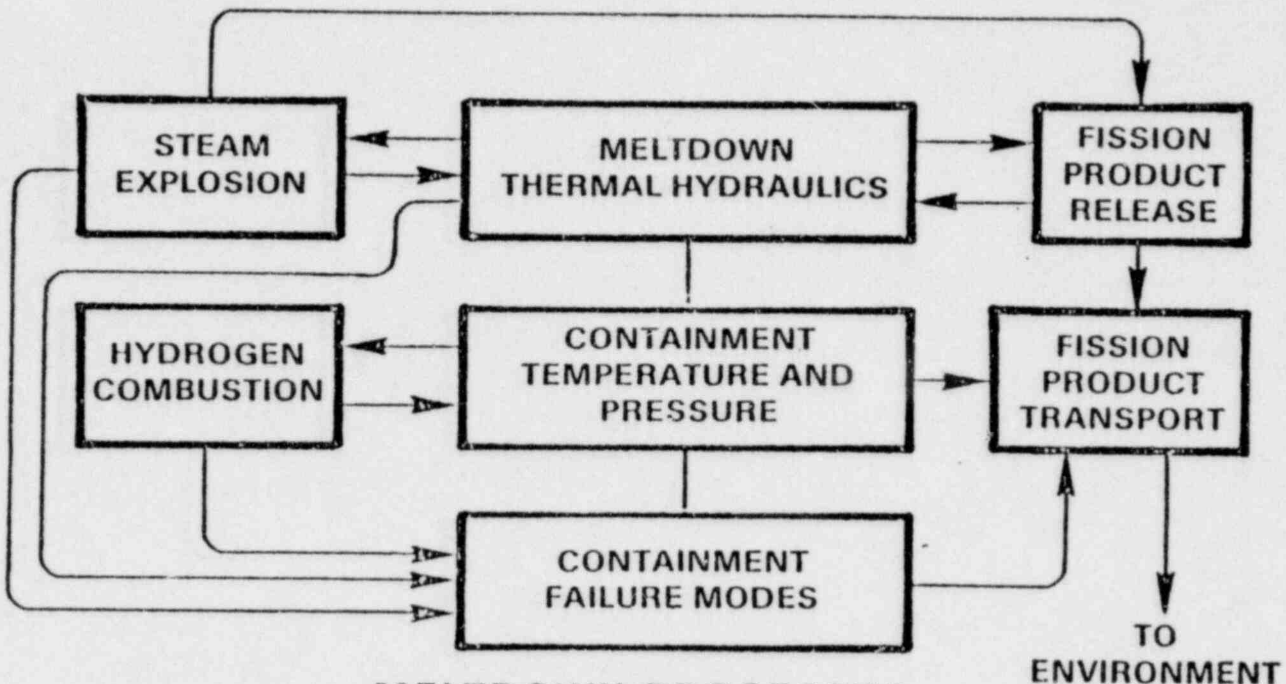
Peter Cybulskis

to

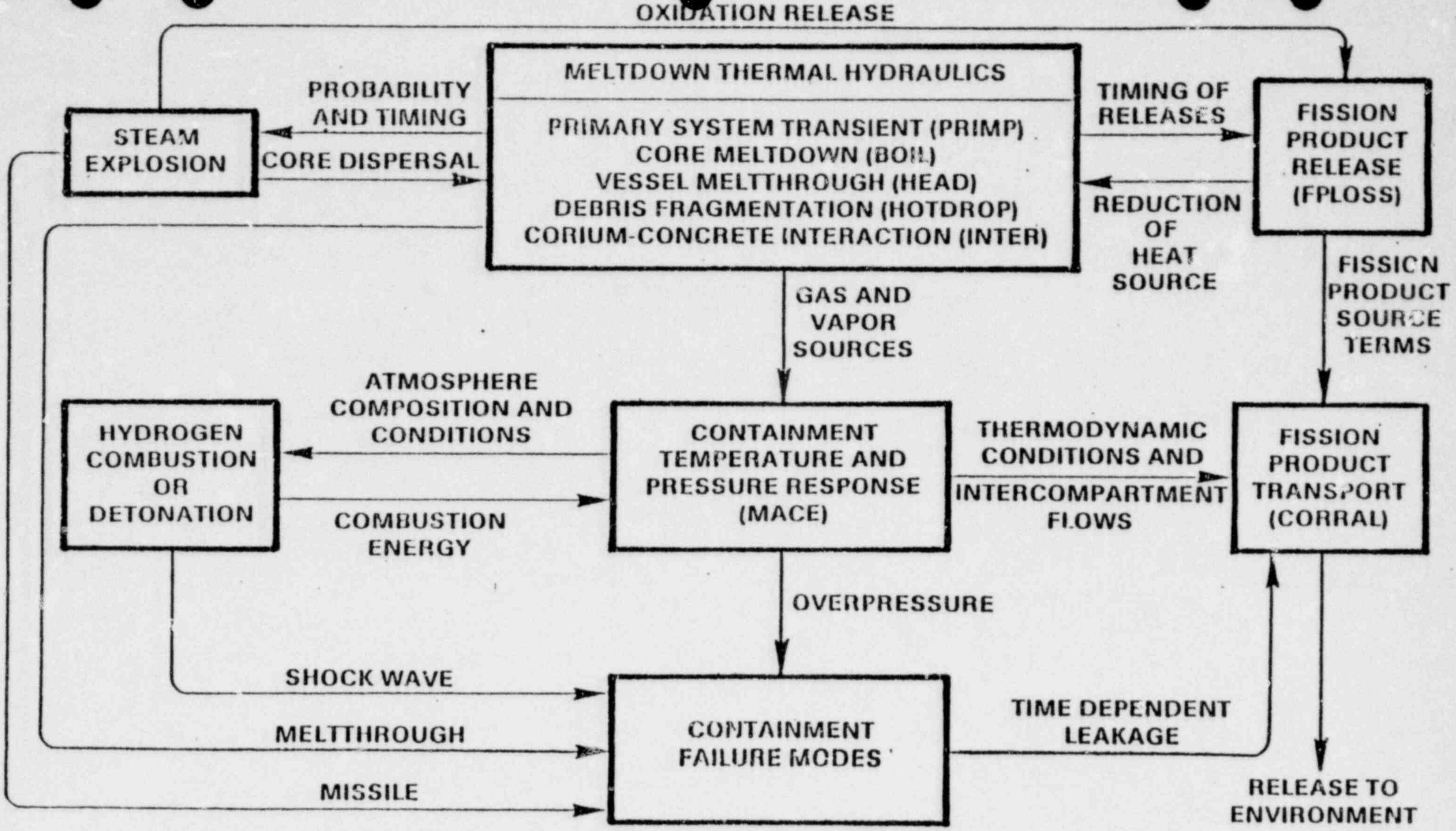
ACRS Class 9 Accident Subcommittee
Los Angeles, California

July 2, 1980

BATTELLE
Columbus Laboratories
505 King Avenue
Columbus, Ohio 43201



MELTDOWN PROCESSES



Core Meltdown Phenomena

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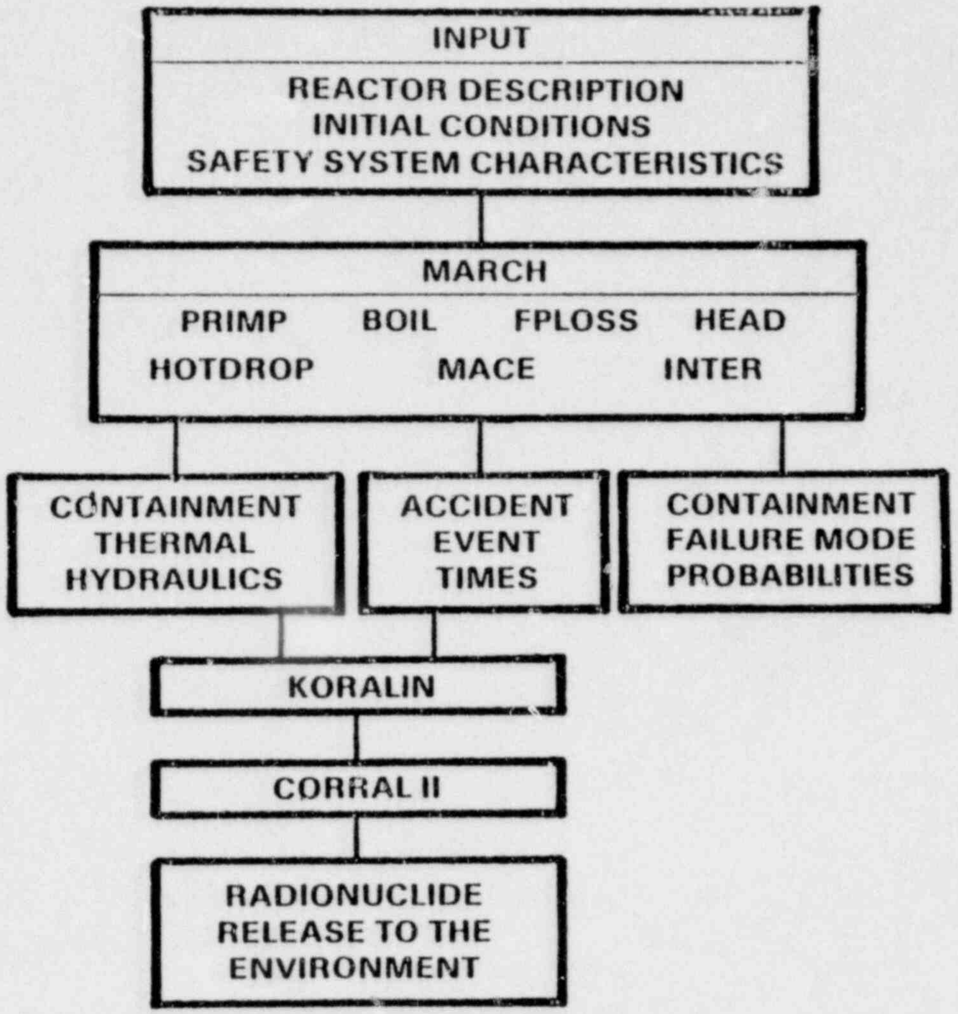

Flow Diagram for MARCH/CORRAL Analyses

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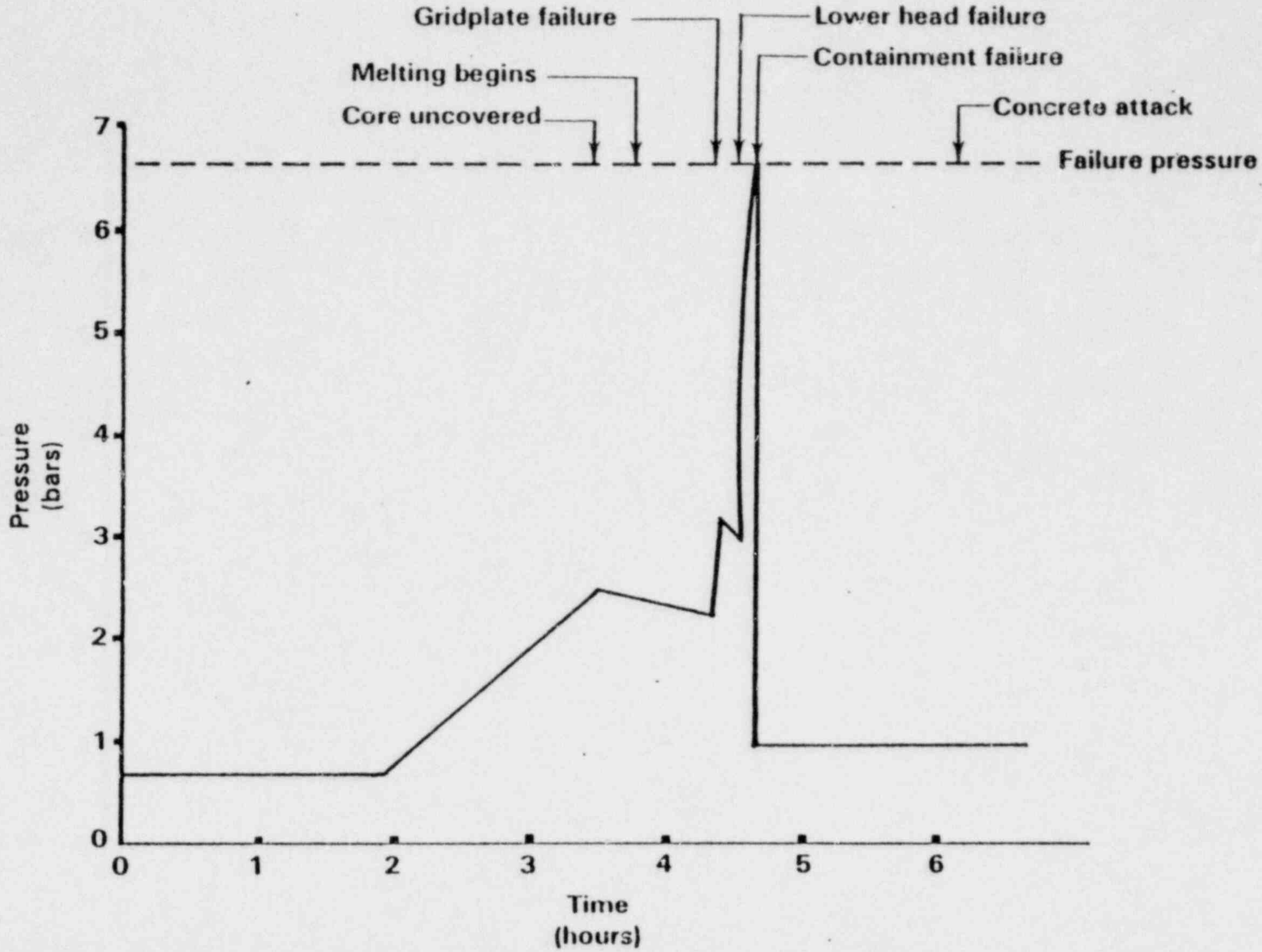
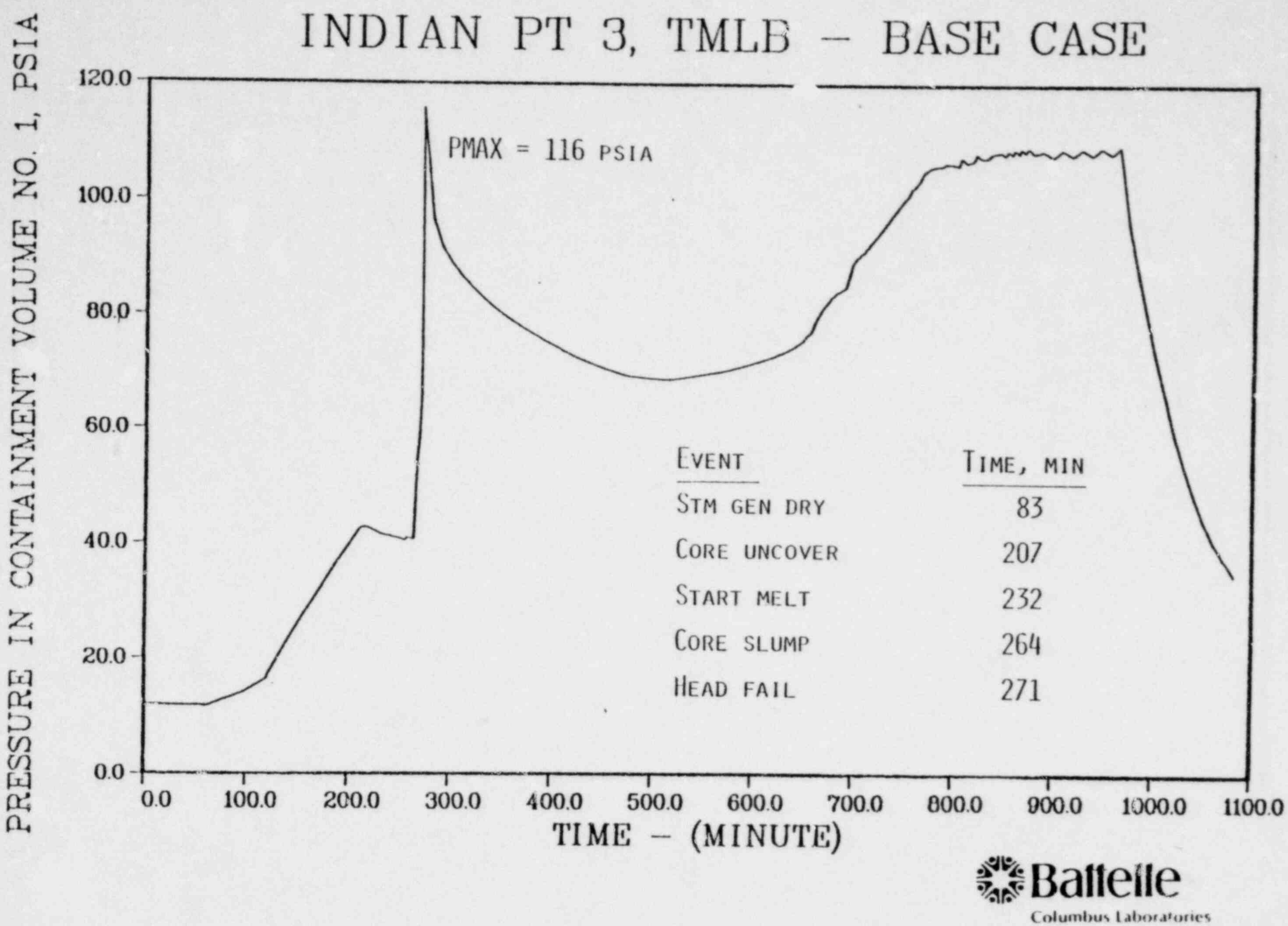
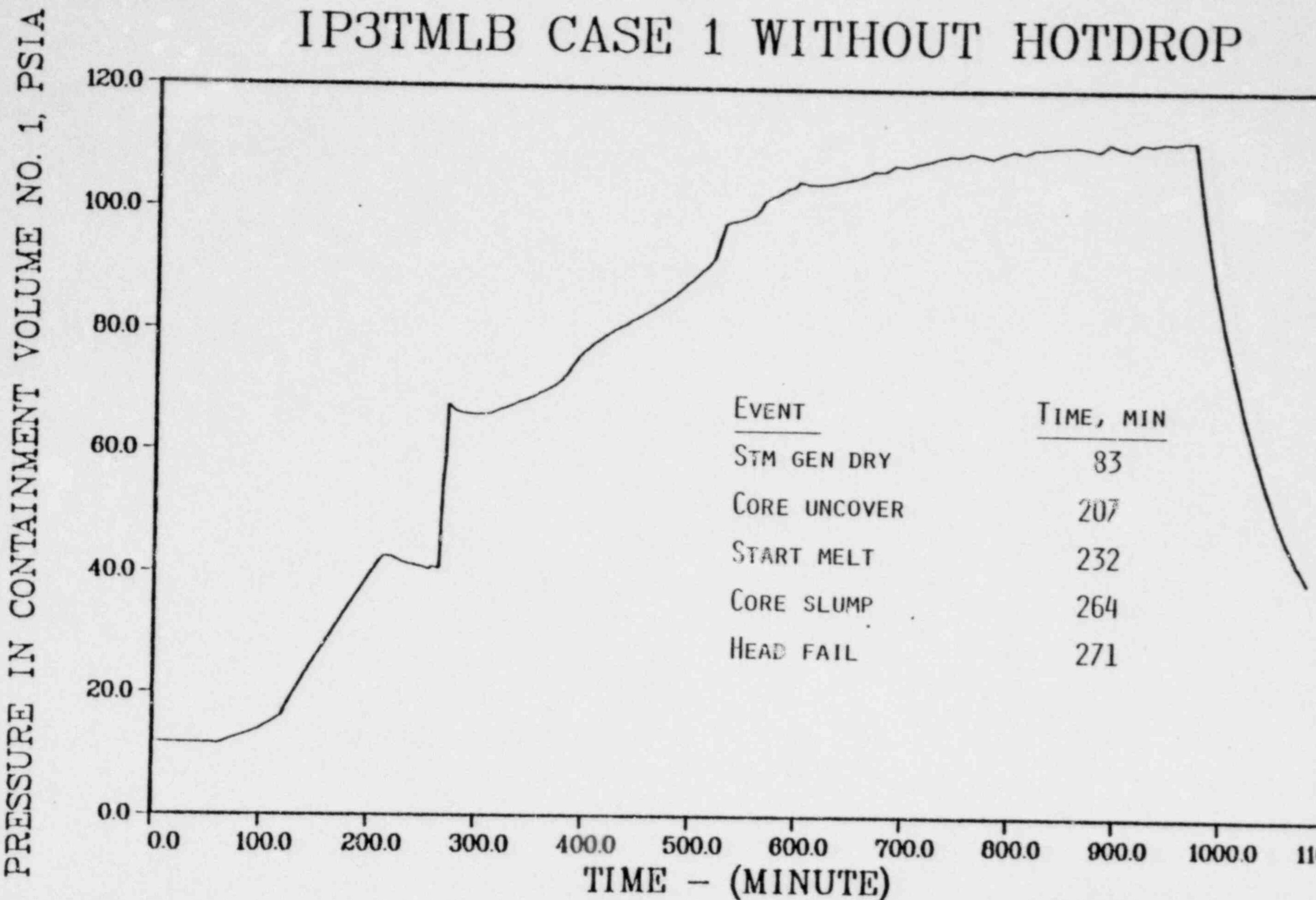
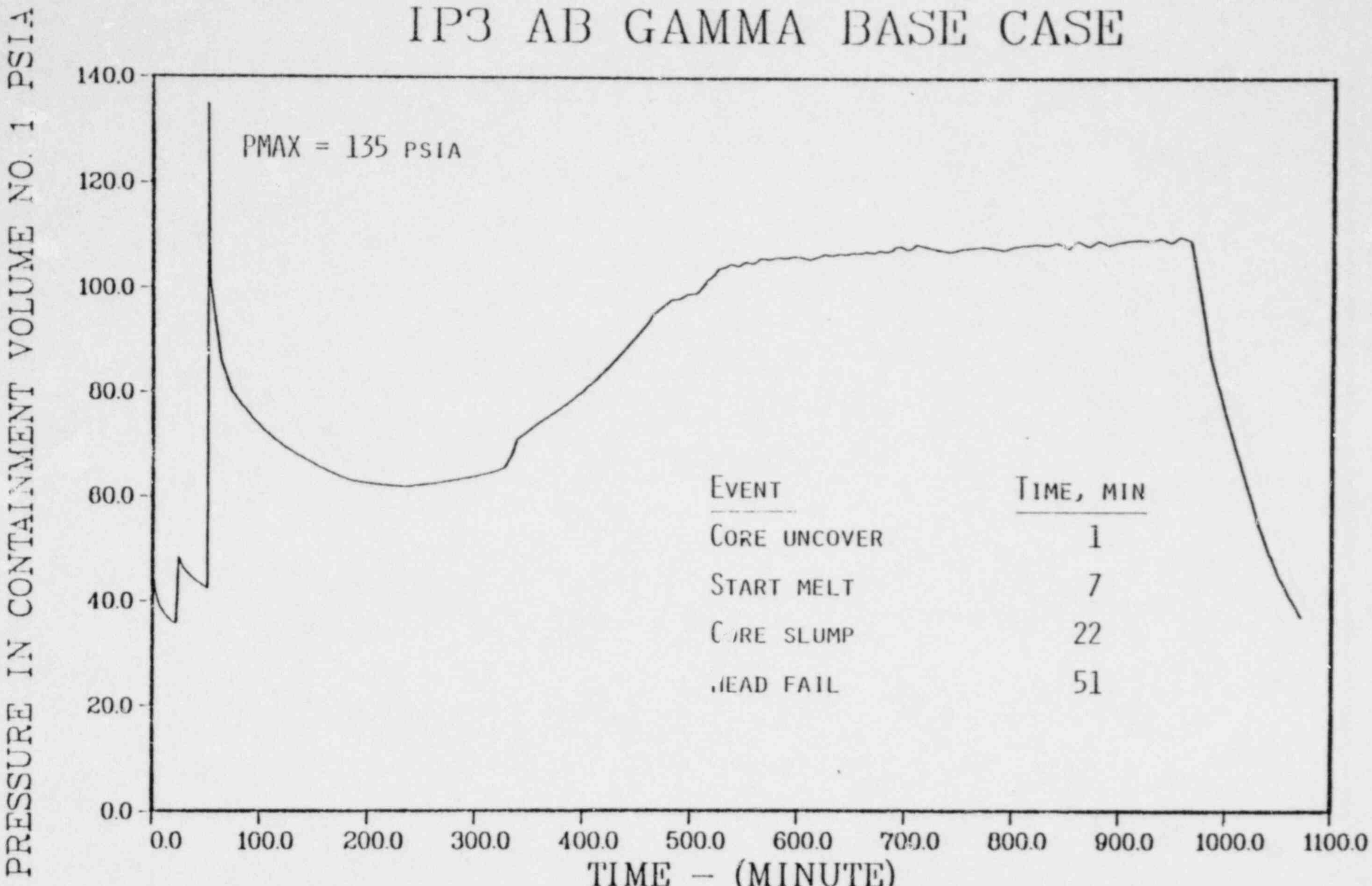


Pressure History for TMLB′ δ


INDIAN PT 3, TMLB - BASE CASE
PRESSURE IN CONTAINMENT VOLUME NO. 1, PSIA
TIME - (MINUTE)
PMAX = 116 PSIA
0.0
20.0
40.0
60.0
80.0
100.0
120.0
100.0
200.0
300.0
400.0
500.0
600.0
700.0
800.0
900.0
1000.0
1100.0
EVENT
TIME, MIN
STM GEN DRY 83
CORE UNCOVER 207
START MELT 232
CORE SLUMP 264
HEAD FAIL 271
Battelle
Columbus Laboratories



Battelle
Columbus Laboratories



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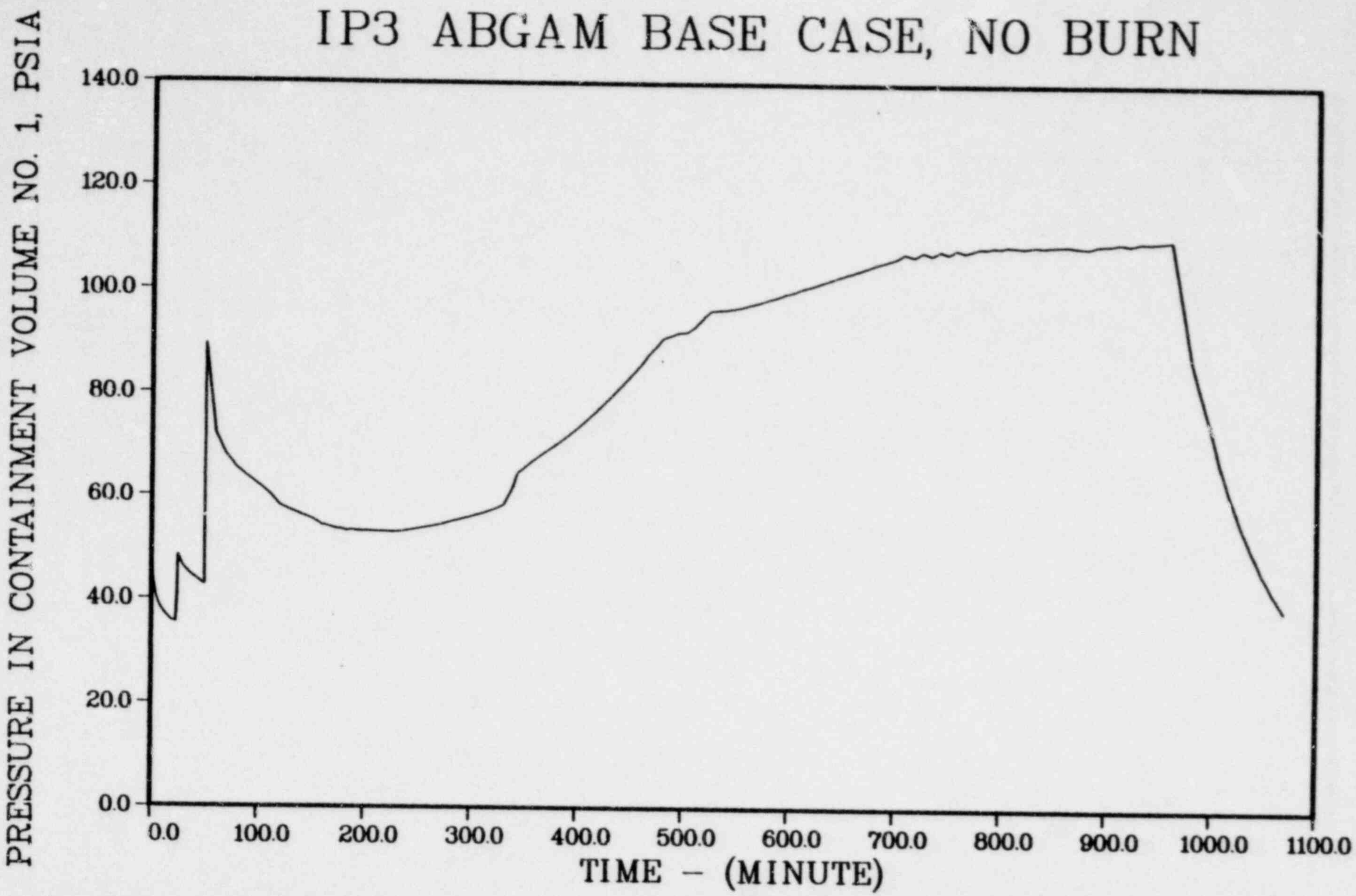
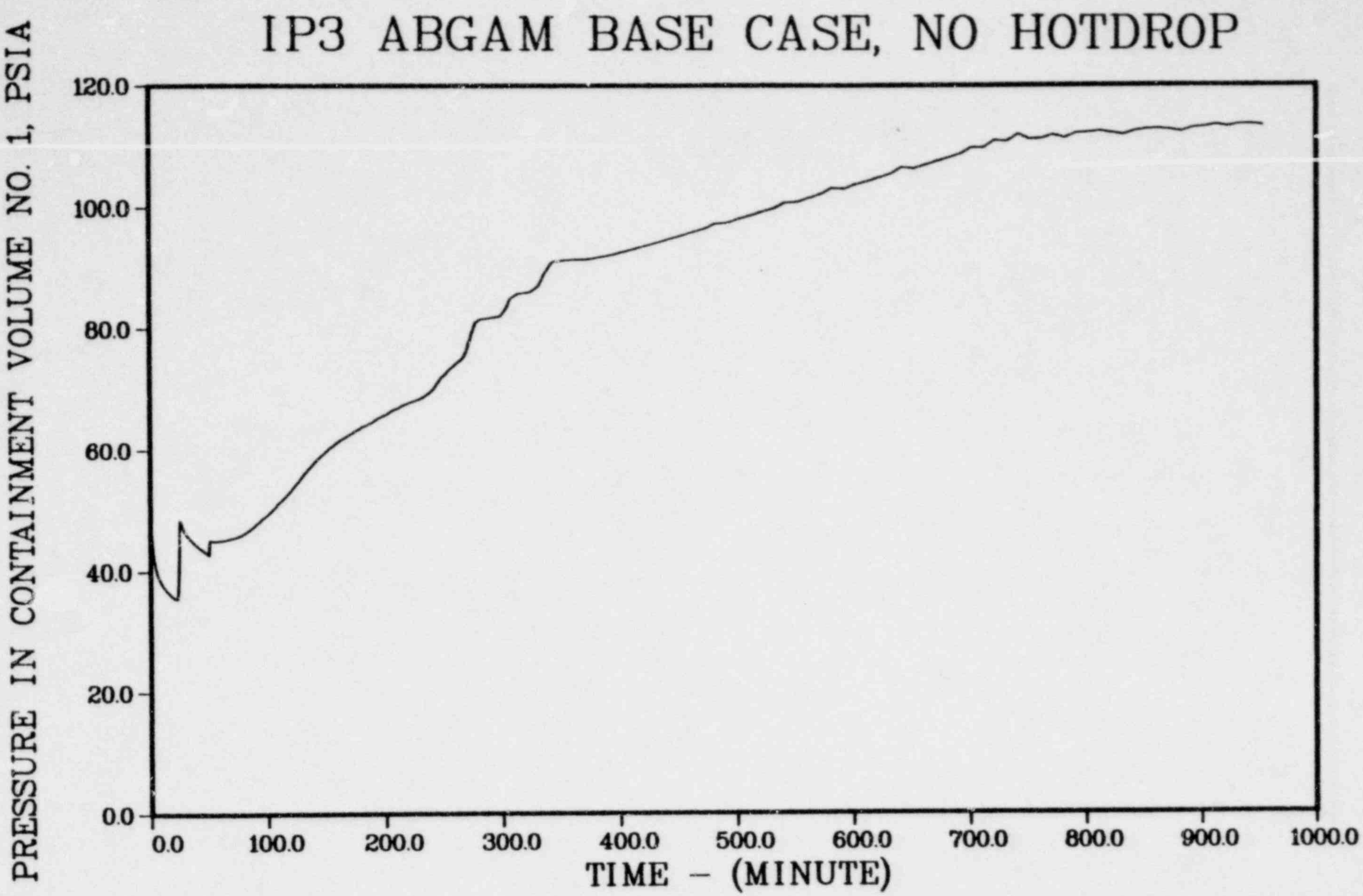
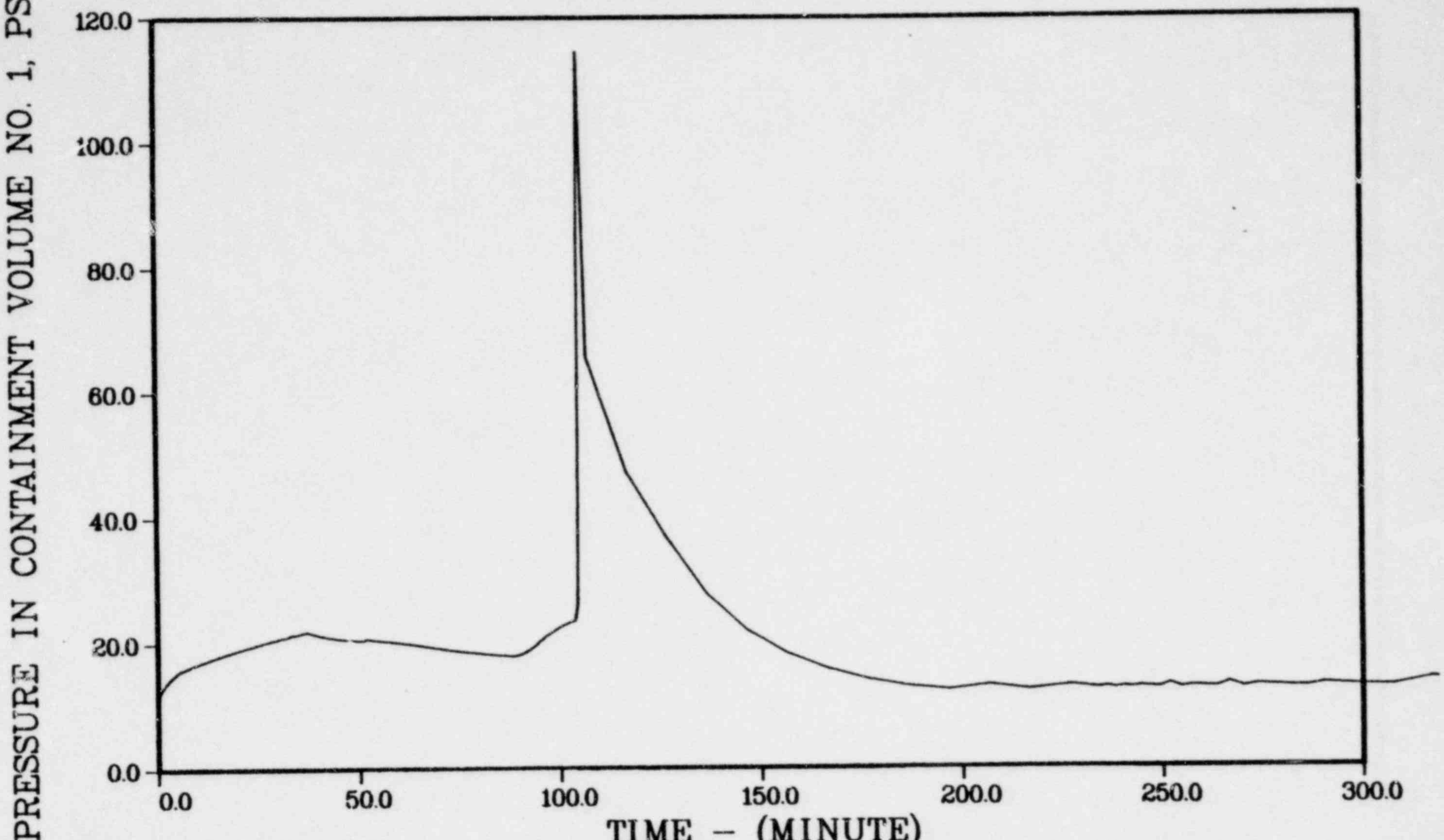
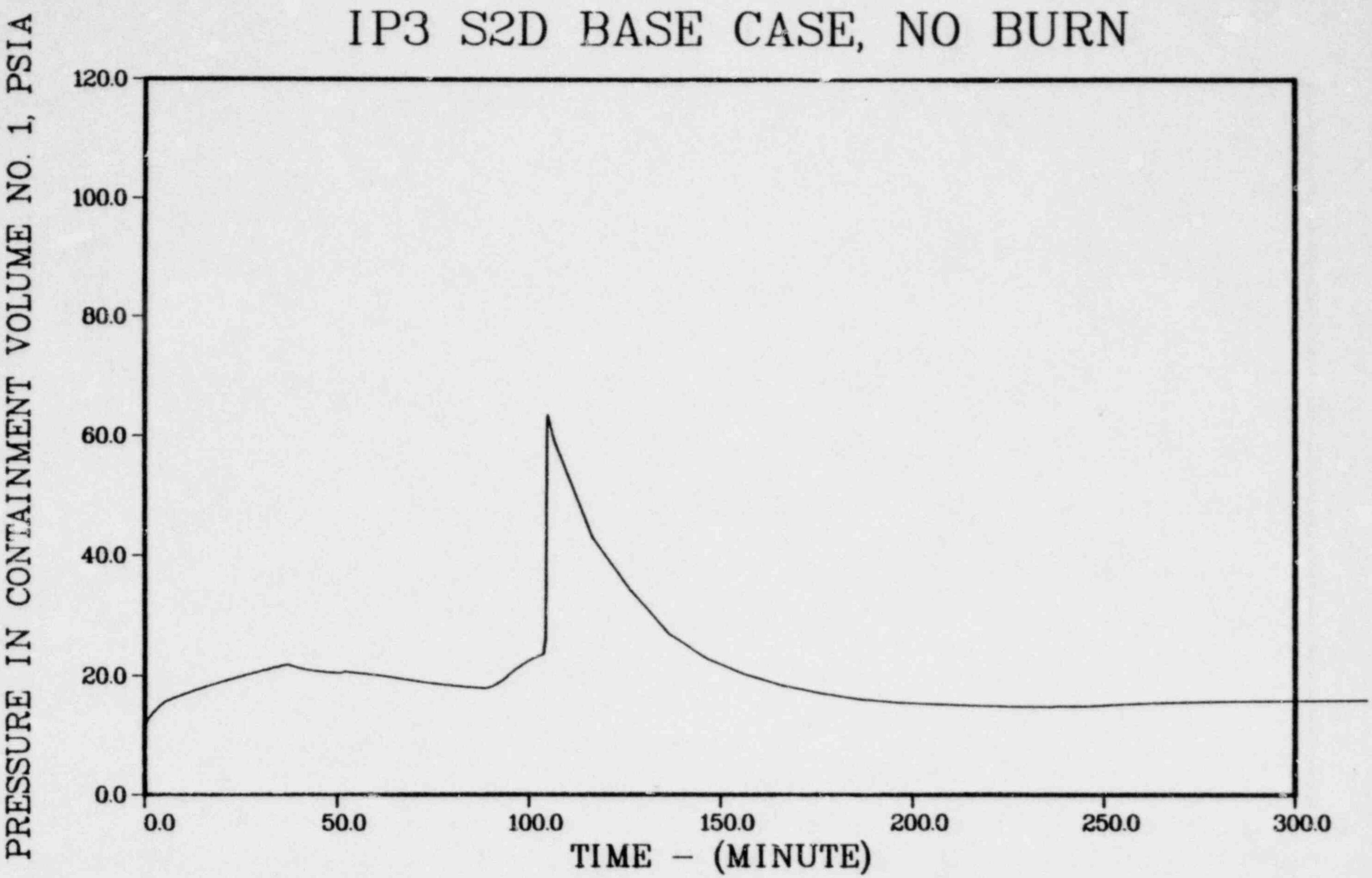
IP3 ABGAM BASE CASE, NO BURN



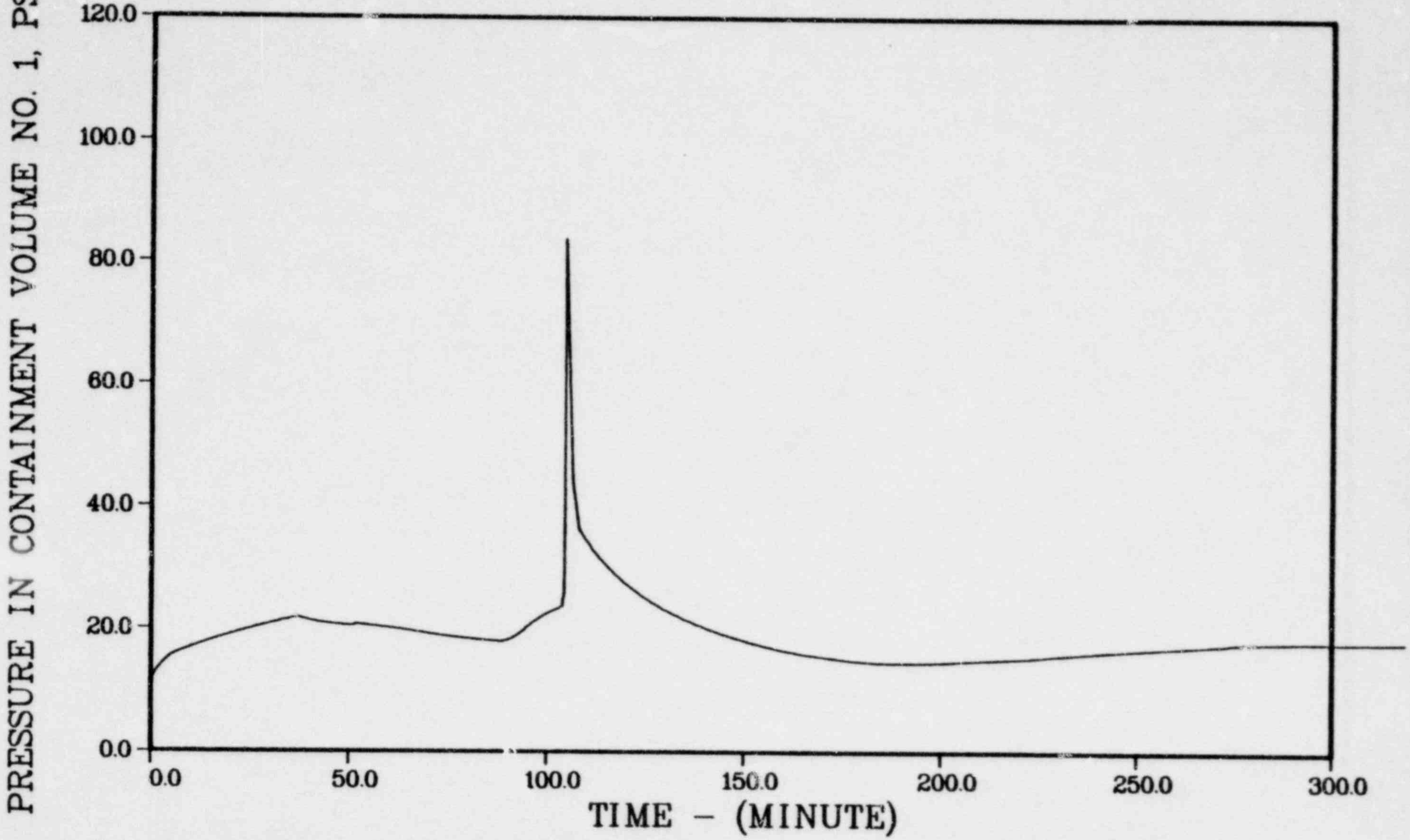

IP3 ABGAM BASE CASE, NO HOTDROP
PRESSURE IN CONTAINMENT VOLUME NO. 1, PSIA
0.0
20.0
40.0
60.0
80.0
100.0
120.0
0.0
100.0
200.0
300.0
400.0
500.0
600.0
700.0
800.0
900.0
1000.0
TIME - (MINUTE)

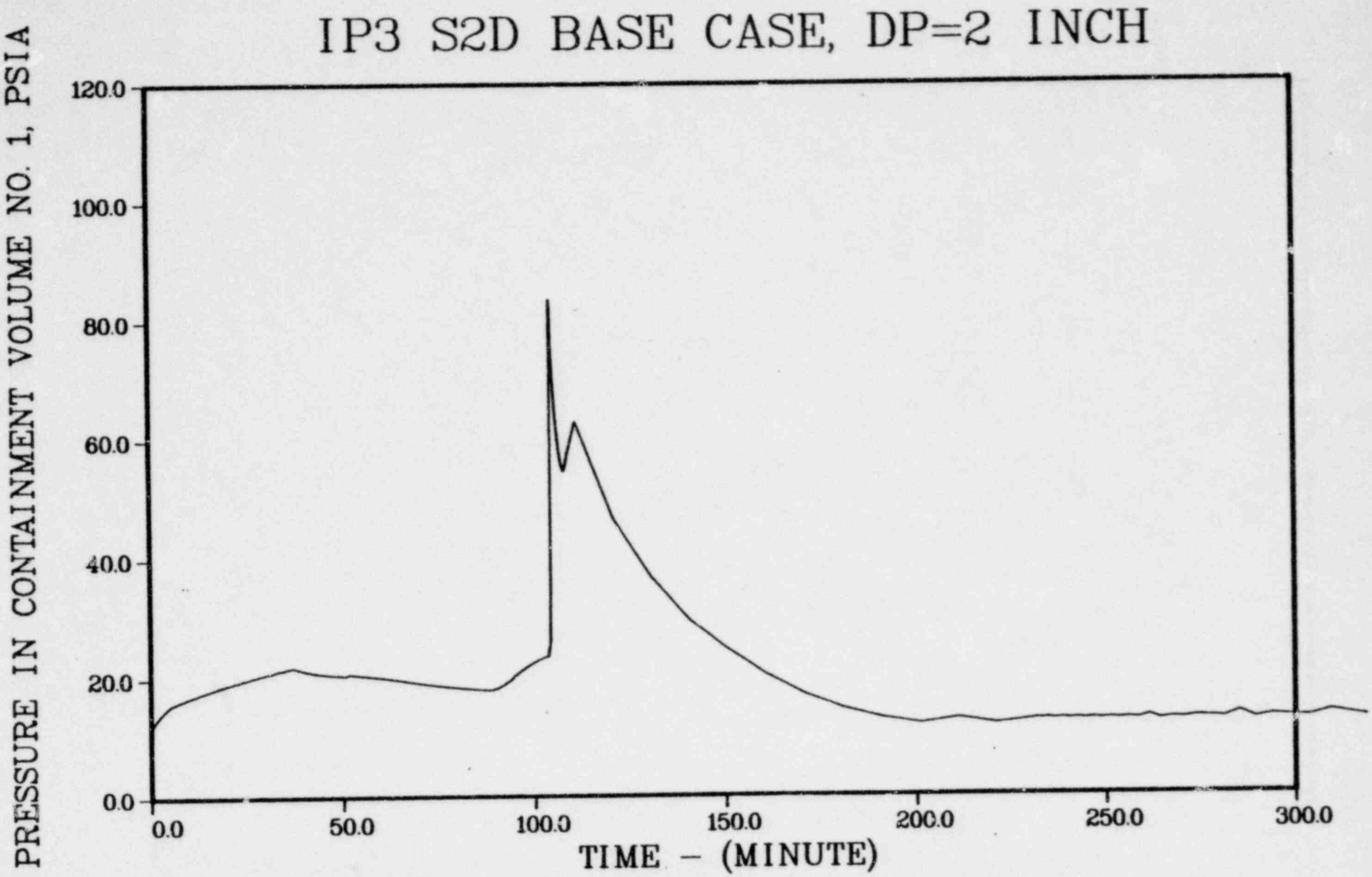
IP3 S2D BASE CASE




IP3 S2D BASE CASE, NO BURN
PRESSURE IN CONTAINMENT VOLUME NO. 1, PSIA
120.0
100.0
80.0
60.0
40.0
20.0
0.0
0.0
50.0
100.0
150.0
200.0
250.0
300.0
TIME – (MINUTE)

IP3 S2D BASE CASE, NO HOTDROP




IP3 S2D BASE CASE, DP=2 INCH
PRESSURE IN CONTAINMENT VOLUME NO. 1, PSIA
120.0
100.0
80.0
60.0
40.0
20.0
0.0
0.0
50.0
100.0
150.0
200.0
250.0
300.0
TIME – (MINUTE)

IP3 S2D BASE CASE, COOLER FAILS