UNITED STATES OF AMERICA
NUCLEAR REGULATORY COMMISSION

Title: BRIEFING ON NUREG-1150 PEER REVIEW GROUP STATUS

Location: ROCKVILLE, MARYLAND

Date: JUNE 20, 1990

Pages: 65 PAGES

SECRETARIAT RECORD COPY

NEAL R. GROSS AND CO., INC.

COURT REPORTERS AND TRANSCRIBERS
1323 Rhode Island Avenue, Northwest
Washington, D.C. 20005
(202) 234-4433

DISCLAIMER

This is an unofficial transcript of a meeting of the United States Nuclear Regulatory Commission held on June 20, 1990, in the Commission's office at One White Flint North, Rockville, Maryland. The meeting was open to public attendance and observation. This transcript has not been reviewed, corrected or edited, and it may contain inaccuracies.

The transcript is intended solely for general informational purposes. As provided by 10 CFR 9.103, it is not part of the formal or informal record of decision of the matters discussed. Expressions of opinion in this transcript do not necessarily reflect final determination or beliefs. No pleading or other paper may be filed with the Commission in any proceeding as the result of, or addressed to, any statement or argument contained herein, except as the Commission may authorize.

UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

- - - -

BRIEFING ON NUREG-1150 PEER REVIEW
GROUP STATUS

- - - -

PUBLIC MEETING

Nuclear Regulatory Commission
One White Flint North
Rockville, Maryland

Wednesday, June 20, 1990

The Commission met in open session, pursuant to notice, at 2:00 p.m., Kenneth C. Rogers, presiding.

COMMISSIONERS PRESENT:

KENNETH C. ROGERS, Commissioner
JAMES R. CURTISS, Commissioner
FORREST J. REMICK, Commissioner

STAFF AND PRESENTERS SEATED AT THE COMMISSION TABLE:

SAMUEL J. CHILK, Secretary

WILLIAM C. PARLER, General Counsel

DR. HERBERT J.C. KOUTS

DR. GEORGE APOSTOLAKIS

DR. LEO LeSAGE

DR. JOHN TAYLOR

P-R-O-C-E-E-D-I-N-G-S

2:00 p.m.

COMMISSIONER ROGERS: Good afternoon, ladies and gentlemen.

Chairman Carr will not be with us today.

In February of 1987, the NRC published and distributed for public comment the first draft of NUREG-1150. In response, 55 sets of comments were received, totalling approximately 800 pages. In addition, comments were received from three organized peer review committees, two sponsored by the NRC and one by the American Nuclear Society. And in April of 1989, a second draft entitled, "Severe Accident Risks: An Assessment for Five U.S. Nuclear Power Plants," was prepared for peer review.

At the request of the Commissioners, the Office of Nuclear Regulatory Research took the necessary steps to establish a peer review committee of internationally recognized professionals to review the second draft. The Commissioners felt that such a further review by objective experts of the highest competence was important because the large number of changes in the second draft resulted in essentially a new study, because of the importance and interest in the study both in the United States and abroad and

because of the ground breaking new methods used by the NRC staff in developing the study. Together, these dictated a critically appraised and thoroughly vetted product.

It's well to recognize what NUREG-1150 is and what it is not. It is a snapshot in time of severe accident risks in five specific commercial nuclear power plants. It is an estimate of the actual risks of the five studied plants. It is not the sole basis for making plant-specific or generic regulatory decisions, and it is not an estimate of the risks of all commercial nuclear power plants in the United States or abroad.

The Commission has been well pleased by the successful assembly by Mr. Eric Beckjord, Director of the Office of Research, of a distinguished review group which includes individuals with international reputations in a broad range of disciplines, drawn from a diverse group of institutions worldwide.

Today's meeting marks the fulfillment of the Commission's commitment to the public to subject the NUREG-1150 process and results to a rigorous peer review. The Commission welcomes Doctor Herbert Kouts, Chairman of the Special Committee to review the report and his colleagues, to brief us on their findings and

conclusions.

Doctor Kouts, you may begin.

DOCTOR KOUTS: Thank you, Commissioner Rogers.

Let me apologize at the outset for not having a more polished performance today. We held our last meeting of this Committee last week, last Wednesday and Thursday, and did not agree until the last minute on Thursday on all aspects of the report. That report in all its last aspects did not come out of the typewriter until 10:00 o'clock this morning. It still has to undergo sending back to the Committee for a final review to make sure that all their latest injunctions were put into it.

So, I'm not able to give you the final version of this report in its ultimate form today. I have turned over a copy to Mr. Bartlett, who was the designated federal official for this activity. I doubt very much that there will be any significant change from what it is in it, but it is not the final report of this Committee.

I'd like to begin by introducing the Committee to you. I'm sure you know by name and by reputation most of the members of the Committee, but let me -- if we can start looking at my first slide.

(Slide) As you know, the name of this Committee is the Special Committee to Review the Severe Accident Risks Report. Several of the Committee are here with me today, Doctor Apostolakis, Doctor Taylor, Doctor LeSage.

Doctor Apostolakis is from Argonne National Laboratory, an expert -- sorry, from the University of California Los Angeles, is an expert on many phases of safety and has interacted in many of these with the Commission in the past. He was especially valuable during our review in questions dealing with human reliability matters and management aspects of the study.

Doctor Birkhofer, who is next on the slide, is Chairman of the Geselschaft fur Reaktorsicherheit in Federal Republic of Germany which may be renamed any day now, and is also head of the German Reactor Safety Advisory Committee. He's spending most of his time these days reviewing the East German reactors for the union of the East and West Germany.

Doctor Lars Hoegberg is head of the regulatory activity in Sweden.

Doctor William Kastenberg, who could not be here today, issued the primary study of the first

draft of NUREG-1150 and is at the University of California Los Angeles. We'll be commenting on Mr. Kastenberg's review in just a minute.

Then comes me and then comes Doctor LeSage who is at Argonne National Laboratory, for sure, and has headed the American Nuclear Society review of NUREG-1150.

Doctor Norman Rasmussen is a name you all know from WASH-1400.

Doctor John Taylor is head of the nuclear aspects at Electric Power Research Institute and Doctor Harry Teague is from the regulatory system in the United Kingdom.

We were really very fortunate in being able to have the cooperation of individuals from these foreign countries in the review that took place. They enriched our review and brought us new points of view. They were able to attend all of our meetings, although we had to hold one meeting in London just so we could get Doctor Birkhofer. No one objected very strongly.

Let me begin by answering questions which the Commission put to us in the charter of our Committee. These questions -- there were five such questions and they singled out, I assume, the areas of principal concern that the Commission has, principal

interest that the Commission has, and I will address these in turn. I will then make some additional remarks, a few additional remarks on the report and then my colleagues, each of my colleagues has something he wishes to say about specific aspects. Doctor LeSage will talk about the expert elicitation process which was central to the NUREG-1150 draft. Then Doctor Taylor -- no, I'm sorry, then Doctor Apostolakis will talk about the human reliability aspects in management, and Doctor Taylor finally will talk about the implications of some of the results as compared with WASH-1400, which are extremely interesting.

So, let me turn to the questions which the Commission asked.

(Slide) The first of these was does NUREG-1150 adequately reflect the comments made-- it's indicated on my next slide -- adequately reflect the comments made by the Kastenberg review group given the uncertainties in data and models? The answer that we come to was yes, it certainly did. It has addressed the comments made by the Kastenberg review group quite thoroughly. It also has managed to answer questions that were raised in the other reviews that you mentioned, Commissioner Rogers. That is the one

of which I headed for the question of uncertainties in the first draft of NUREG-1150 and the other a review by, again, the American Nuclear Society, chaired by Doctor LeSage.

Of course there was a great deal of change introduced in the reports in the process of answering these comments. There is a substantial difference between the first draft of NUREG-1150 and the final draft, a substantial difference in methodology used, in the applications of the methodology and in the conclusions which were reached. There were substantial effects that on reexamination changed character and changed emphasis in between the two reports.

But by and large, NUREG-1150, in answering the comments made by the Kastenberg review group and the other review groups, was substantially improved in our view.

(Slide) The second question was, have the uncertainties associated with both front and back end analyses been adequately described in NUREG-1150? Is the use of expert opinion elicitation appropriate in developing these uncertainties? The answer to this is by and large yes. There's some qualification to this, as you will see. The uncertainties that were not

included are mainly uncertainties associated with some aspects of the human reliability methods that were used. Uncertainties associated with calculations at the back end because the back end uncertainties were not included in the total uncertainties of the study. That is the human effects studies had no uncertainties propagated through the study.

Expert opinion elicitation was the basis for a large part of the uncertainty analysis. This is a novel, but acceptable way to get at many of these uncertainties. Of course there is a question which arises in a situation where you try to develop expert opinions through an elicitation process and the principal question there is who is expert. Sometimes, and very often, you settle the matter of what the answer is by selecting the person whom you give the question to. There were several instances in the applications in the report in which that stood out very strongly.

(Slide) The third question, to what extent did probabilistic risk assessment focus on the low probability tails of the accident frequency distributions? Is there an appropriate cutoff in terms of reported accident frequencies? Problems come with respect to low frequency tails in distributions

of this kind when there is an overlap of low frequencies. That is when you have one distribution function feeding another distribution function and you are in tails of both distribution functions and this manages to propagate effects into the final results which have large uncertainties attached to them.

It is impossible, of course, to ignore situations like this. You must take them into account. But there is an appropriate cutoff to be attached to these as well as to other matters where low frequency tails come in and we recommend in our report cutoff values for distributions of this kind. We recommend, in fact, cutoff values for both probabilities that events take place and for consequences of these events.

Well, just to state what they are, for the consequences we say that consequences in the range of 10^{-7} to 10^{-8} of an event would be inconsequential in our view. This is a cutoff point. Probabilities that have a 10^{-7} value begin to be questionable because we're now approaching the age of the earth, certainly going past the age of the human race and numbers like this do not have any significance in our view.

(Slide) The fourth question was, and this is again a slide, do the methods, models and data used

in NUREG-1150 suggest they could be used as standardized methods for preparing probabilistic risk assessments? Much of the methodology that was used in the report is standard PRA methodology and of course this will continue to be used.

There were a number of new techniques applied. The principal one of these was the elicitation process. The elicitation process was very lengthy, very difficult and very costly and it's unlikely, in our view, that this elicitation process in all its glory will be done again very soon. But the elicitation process that was used would certainly have value for settling individual matters that come up, specific questions that might arise and we recommend it to that end.

(Slide) Finally, among your questions, does the Committee have recommendations to make on the need for further improvement in probabilistic risk assessment methods? The answer is yes. Not throughout the structure but with respect to a number of specific areas, it appears that benefit could be gained from further research. One of these is very definitely human reliability methods. We've dealt with human reliability analysis in great detail in the report and Doctor Apostolakis will say some words

about it.

The seismic area needs cleaning up, because especially there is disagreement in scientific circles between two seismic risk distributions in the literature and work needs to be done to settle this matter. We have gone into great detail in this in the report.

We recommend that work be done in the area of errors of commission as apart from errors of conduct -- errors of omission, I'm sorry. By this we mean errors in -- mistakes that start which are initiating events in sequences. We recommend that further work in PRA include this area which has been neglected largely up to now of accidents that start from low power or shutdown conditions.

Just as an aside, this area was taken up quite recently in a PRA by the French with respect to their larger new plants and they find that something like 25 percent of the risk is attributable to accidents which would take place under these circumstances.

COMMISSIONER REMICK: Herb, excuse me. Would you clarify if you're recommending further research on errors of commission or omission?

DOCTOR KOUTS: Commission.

COMMISSIONER REMICK: Commission, yes, okay.

DOCTOR KOUTS: Errors which begin, which initiate events.

COMMISSIONER REMICK: Okay.

DOCTOR KOUTS: Now, let me get to a bottom line.

COMMISSIONER ROGERS: Did you come to any particular categories or areas to focus on?

DOCTOR KOUTS: Yes, I just named four.

COMMISSIONER ROGERS: No, I mean in the-- of commission, errors of commission.

DOCTOR KOUTS: Oh, errors of commission?

COMMISSIONER ROGERS: Yes.

DOCTOR KOUTS: No, but we point out several instances in the literature in which errors of commission have been initiating events. Perhaps Doctor Apostolakis will address this matter further.

(Slide) Let me give a bottom line for our review. NUREG-1150 is a good report. We've been very impressed by the report. It, of course, was a report which is the result of a great deal of intense work by a number of talented individuals and it certainly is a step ahead in the field of PRA. We recommend that this report be published as quickly as possible. We

have some suggestions made for -- some places where we think improvements could be made, but we do not believe issuance of NUREG-1150 should be held up for any improvements of that kind. I'll come back to this in a moment when I talk about recommendations that we make.

The report responded well to the earlier concerns, as I said, and to those expressed in the earlier reviews. In most respects, the work was state of the art. We have areas in which we find state of the art is deficient in our view and work should be done there. The report did break new ground, especially with respect to the expert elicitation process, and I think particularly in this respect it illuminated the role that expert opinion has had in the past in PRAs in places where it was not so well recognized.

(Slide) The report has a couple of very strong vulnerabilities, places where people will probe it in the future to establish to what degree new lines can be taken for improvement. One of these is the treatment of human reliability, which I've mentioned several times, and the other is, of course, the expert elicitation process itself, which was -- in many aspects was new in this report.

(Slide) A few other problems. External events were not fully treated in this report, and as we know, external events are extremely important in risk for nuclear plants. That is seismic and fire particularly. There were only two of the five PRAs in which external events were treated. We believe that external events should be made a part of every PRA and the report would have profited if this could have been true.

We have to recognize that the decision, the realization of the importance of expert opinion-- of external events was not forthcoming until the report was -- the activities were fairly well underway and including the external events for these two plants was a backfit matter.

(Slide) There were some other missing matters. Pressure vessel failure was not explicitly included in the review. The initiating events did not include main steam line failures, that is in the second circuits of pressurized water reactors. As I said earlier, errors of commission were not included in the -- as initiating events, and the report did not include initiation from start-up and low power operations.

(Slide) Among our recommendations, the

early publication I referred to earlier. We believe that this report is going to be important for the IPEs as they come along. And for that reason, we believe it should get out into the literature, become available for application in this way as soon as possible.

We believe that the Commission should give additional guidance on seismic matters in light of this disagreement in the seismic risk curves. We have some discussion of some of the background of this that you may find interesting. We believe, in fact, that further seismic research may be necessary to clear up this question of a disagreement and we believe that human factors research is a fitting topic for additional research.

Well, that's as far as I would like to carry it at the moment. You'll find at each of your seats a list of items which the Committee has specifically asked -- that be brought to your attention at this time. They are in greater detail, however, than I think we could profitably expend at this meeting. They may provide a basis for questions you may want to raise after we finish our presentation.

So, I wonder if we could now turn to the

other members of the Committee who have things to say.

Leo?

DOCTOR LeSAGE: Okay. Well, I plan to add a few comments to what Herb has already said about the expert elicitation process. I think you've already touched on some of the points, but I'll go on through since my presentation is fairly brief anyway.

(Slide) Clearly, this was a rather distinctive and really one of the most controversial features of NUREG-1150. Second, the expert elicitation process in the first draft was severely criticized by many, many parties.

(Slide) Going on to my next viewgraph, another comment.

Expert judgment of some kind, either informal or formal, is necessary in all PSAs -- we've used the term "PSA" instead of "PRA." They're interchangeable mostly -- because knowledge of phenomena and scenarios is not adequate to provide reliable analytical and experimental values for many parameters required in a PSA. I guess that's what Herb mentioned. Some form of expert judgment, if it's not formal, is informal in all PRAs.

(Slide) Just a little background. I have one viewgraph here which gives kind of the steps in

the process, since this is a formal, structured process, the way it was handled.

First, there was the selection of the panels. There was an attempt to get diversity of technical expertise and a diversity of organizational backgrounds on the panels to get a broad scope of input. We have some -- as Herb mentioned, we have some questions about the makeup of the panels, but that was the intent at least at this point.

There were experts -- professionals in the expert elicitation process were then brought in to train each of the individual panels in the formal way in which this is done in other fields. They used examples to show people how sometimes they have too much confidence in their judgment and they should not be so confident and things like this. But it was a standard training process.

Then the panels were given technical presentations by experts in sub-areas of these various things, real experts, so that the panels were sort of all brought up to the same level of background and knowledge. They were given all the relevant references in the field and so they could take them home. Then the panels had discussions and sometimes they had two or three meetings. I don't remember

exactly, but the panels got together, they had more presentations in some cases and they discussed the issues. I sat in on some of them. Some of us have sat in on some of these panels. They were open to the public.

Then they had the elicitation process, which was conducted in private, however, by professionals. So, after they had their discussions, there was some -- well, the reason it was conducted in private, I guess, was primarily to prevent sort of a group psychology taking over.

Then finally they were combined into a final result which usually was a distribution function that was input into the PSA. So that's how the process worked. It took, I think, most of six months for this. This is a repeat of the process.

COMMISSIONER ROGERS: You observed this training process to some extent.

DOCTOR LeSAGE: To some extent, yes.

COMMISSIONER ROGERS: What was your impression of how the experts reacted to that? That sounds to me like the kind of thing that experts might not like very much, to get trained.

DOCTOR LeSAGE: I saw a limited amount. Some other people that were on the American Nuclear

Society Committee sat in on more of it. I think that there was a -- I don't recall any mention of any antagonism toward or anything. It was about a day's worth of training on the process, before they went into discussing the technical issues. No, I don't think that was a problem, let's put it that way.

(Slide) My next viewgraph, I think, summarizes our major conclusions regarding the expert elicitation process. First, the structured use of expert opinion in NUREG-1150 represents an important advance over previous applications of expert opinion. I think we all agreed on that. It really was a much more structured process than had been used in the past.

Second, there was a major improvement between Draft-1 and Draft-2. The professionals were brought in and they cleaned up the areas that had been criticized.

Third, the Committee still has some concerns, mainly in the area of the selection of the experts. There were many who felt that others than on the panel should have been selected. This gets to be very judgmental, but there was some really reasonable people who were not sure that the right people had been selected. Second, there was a concern on the mix

of the expertise. There primarily the concern was that there were more people on the panels who were users of the specialty information than the people who actually generated it. More people were -- there were more PRA experts, but not so many researchers. We felt there was some unbalance, what it seemed to be.

So, those are our concerns. I don't know that it -- how much -- we have no idea of how much it actually could have biased the results or not.

I put a little -- this last bullet is kind of a caution and I read it. "Further expert opinion, even when properly structured, should be applied with caution and the results treated with some skepticism, since we are dealing with poorly understood and complicated phenomena." That's the reason they're using experts, because nobody can really do a good analysis. It's probably about as well as could be done with these reservations, but it's still data that one has to be cautious with.

(Slide) I have a few -- next two or three give a few additional comments. I want to emphasize, those on the previous page were our major conclusions regarding expert opinion. These are additional comments which I think have some relevance.

The first one is that the structured

expert elicitation process is certainly preferable to the informal one where it's not documented. Second, the training we feel of the experts was a useful thing to bring into it. Third, and I think this third one is fairly important, the process provided a structured method for introducing additional analysis and experiments. That was part of the process. They were given all the experimental results, say, that were relevant to this. It was impossible in any sort of analytical way to factor these in, but they could consider these in their judgments. So, this was a way and a formal way of introducing additional experiments.

In fact, one of the criticisms of Draft-1 was that it wasn't clear what experiments had been considered. But these experiments were presented to the experts. So, in a sense, they were considered in Draft-2.

(Slide) On the next page, the next bullet. The process was well documented and the documentation will be useful. I think that the people doing the IPEs have stated to me, several of them, they think the documentation will be very useful in bringing their thoughts together.

The next bullet, there were a couple of

instances where we felt that expert opinion was used when experimental data could have been used directly. I think one area was in pipe break frequencies. These were not -- there weren't many of these, but there were a couple we identified.

The next bullet, the number of issues addressed by the experts was limited. That was because the panels were very cumbersome. Even so, the work load on the experts was sometimes excessive and I'll state that. Even though they only addressed a limited number of issues, they looked at many permutations, many different pressures. So, they may have been addressing five or six issues at most, or even less, but they may have generated 5,000 numbers. So, it's a little bit -- I'm saying on one hand they addressed a limited number of issues and on the other hand it was still a lot.

(Slide) Then the final viewgraph that's addressing expert opinion. Again, even though the experts were overloaded, there were a number of issues which turned out in the long run to be important which were not addressed by the panels. The way it was set up, the important issues were addressed by the panels. There were other issues where you needed some sort of expert opinion where the project staff addressed

these. The intention was to give the really important ones to the expert panels. As it turned out in a couple of cases, issues that were not given to the panels turned out to be important when the final numbers came in and these were addressed -- and so there was a little bit less an optimum situation. One reason is that the project staff did not document their results nearly as well as the expert panels.

Another slightly negative comment was that the weighting -- there was equal weighting given to all experts. There are other possibilities. They were really not investigated, our feeling was. They might have ended up giving equal weight in the long run, but there wasn't -- it wasn't clear to us whether that was the case or not.

And then finally, something that Herb has mentioned before, and that is that this process is very time consuming and expensive. It isn't going to be a standard procedure for utilities to use this, obviously. It's just too time consuming. So, it will not be repeated again probably very soon.

That concludes what I have to say on expert opinion.

DOCTOR KOUTS: Okay. George?

DOCTOR APOSTOLAKIS: Well, I will comment

on methodology from the point of view of how we are doing things different now from WASH-1400 and then safety culture and human reliability analysis. Doctor Taylor will give you more details on the actual results and their comparison with the actual safety study. So, I will limit my comments to methodology.

(Slide) I think NUREG-1150 and other PSAs that are being published by various groups these days show that the reactor safety study methodology is still the main framework that is used to do PSAs, which is of course to the credit of the people who did the study. Of course there have been some changes, but that's natural. We're talking about a study that was done more than 15 years ago.

We have better data now. Mainly because we look more into the LERs and other records. We have programs like the precursor studies that are telling us what is happening at the plants and so on. So, we certainly have a better database.

We certainly have better models for external events, for earthquakes and fires which were dismissed by the reactor safety study as unimportant. We are doing more on human errors, although as Doctor Kouts mentioned, this is an area where we still need more work.

NUREG-1150 introduced this formal use of expert opinions which, of course, were used in the reactor safety study, but not we have a more formal and structured way of doing it. Now, I think, we have reached a level of maturity where we started worrying about various intangibles like safety culture and so on.

(Slide) So, in my next viewgraph, I address this fairly new issue. Major incidents of the last several years, Chernobyl being one, have indicated that the so-called safety culture that prevails at the plant and the overall management quality are important. And as I say in my first bullet, plant policies may set priorities of operator actions long before emergencies occur. An example of that is the Davis-Besse incident where financial loss was certainly a factor in the decision of the operations to initiate bleed-and-feed cooling.

It's difficult to define what a safety culture is. I think the best definition was given by the International Nuclear Safety Advisory Group of the IAEA, where they state that the fundamental management responsibility is the establishment of a safety culture that governs the actions and interactions of all individuals and organizations engaged in

activities related to nuclear power, and then they proceed to describe what that safety culture would be. It encourages people to be responsible and so on and so on.

But unfortunately, the last bullet tells us that the current state of the art does not allow us to include this very important element in PRAs. So, we certainly don't want to blame NUREG-1150 for not including it, but we felt as a committee that this was an important enough matter to bring to your attention, that it is not included in NUREG-1150.

(Slide) Now we come to the human reliability analysis. For inactions or pre-accident actions, I don't think anybody can disagree with what NUREG-1150 has done. They used mainly the handbook that was published a number of years ago from Sandia and it's still state of the art. Most people use that. However, when we go to post-accident actions, we have some problems with what was done. They used mainly the ASEP procedures, which are really a variation of THERP that was developed at Sandia by Alan Swain mainly.

However, there are other ways of -- other models that are available in the open literature and these were not included. They were not -- there was

not even an attempt to see what the impact of using alternate models would be. In other words, model uncertainty was ignored and NUREG-1150 is, of course, a study that was the first PSA to actually do something about model uncertainty in the so-called back end. But in this particular area, they did not do anything.

Then we have the importance of model uncertainties shown in the results of a fairly recent human factors reliability benchmark exercise, that's RBE, that was conducted by the Commission of European Communities Joint Research Center of Ispra, Italy.

(Slide) And in the next slide, you will see why model uncertainty is so important in the human reliability analysis. This is one key figure from a paper that was presented last year at the American Nuclear Society meeting in Pittsburgh, and it shows the probability estimates for not responding correctly to an assumed scenario which involved loss of off-site power and other things.

You see they had 15 teams from the European -- from member countries of the European Communities as well as American teams. You see that there was considerable scatter in the results. They used methods like THERP, the human cognitive

reliability model, which was developed under EPRI sponsorship, SLIM mod, which was developed in England and also at Brookhaven, and we see a considerable scatter in the results of the various teams. But also if you pick any team, like team B for instance, using different methods, they get different results.

So, the message from this exercise is that model uncertainty is very poor. Now, this is probably the only major issue in the front end of PSA where model uncertainty is so important. All this was neglected in NUREG-1150.

(Slide) In the next slide, we see another example from NUREG-1150 that also shows how important model uncertainty is. As I said earlier, ASEP procedures were used, except in the important case of manually initiating the standby liquid control system during an ATWS sequence at the Peach Bottom plant. The Grand Gulf analysis was done using the THERP or ASEP methodology by a team from Sandia headed by Alan swain, and their probability estimate for failure to initiate the SOC system was 10^{-4}. Now, there's a question whether this is a median or a mean. It could be a little bit higher if it's the mean, but this gives you the idea of the order of magnitude.

Now, the same event for the same sequence

but for Peach Bottom was analyzed by a team from Brookhaven using a different approach and different time reliability curves. In other words, they didn't use what Swain has developed for ASEP. They came up with a number that's 0.02. So, we are talking about a difference here of roughly a factor of 200, I believe. This is part of NUREG-1150 and it had not been recognized by the team, by the way.

Then, we look at the two plants and we were not able to find any significant differences that would justify such a tremendous difference in the results. So, indeed, we have a problem here, a serious problem of model uncertainty, we believe.

COMMISSIONER REMICK: Let me just ask a question. On that specific example, would it have made much difference in the core damage frequency, either one?

DOCTOR APOSTOLAKIS: I cannot answer that now.

DOCTOR KOUTS: There's only one sequences.

DOCTOR APOSTOLAKIS: There was one sequence, but I don't know how high it was. I really cannot answer that.

(Slide) Now, the first bullet on the next slide is something that Herb mentioned. We recognize

that errors of commission due to misdiagnosis were not investigated and this was a little bit disappointing because other PSAs, namely those for the Oconee and Seabrook plants, which were published seven years or so ago, did do something about it. I don't think anybody can claim that what these studies did really solved the issue or they modeled satisfactorily the errors of commission, but at least it was a good first step. NUREG-1150 did absolutely nothing in this area.

So, while most of NUREG-1150 is state of the art or in some areas they actually advanced the state of the art. Here, I think they were behind.

I think this is my last slide. In fact, it is.

DOCTOR TAYLOR: (Slide) Well, major progress has been made in severe accident technology and risk assessment methodology since the publication of the pioneering reactor safety study WASH-1400. NUREG-1150, in my judgment, is a comprehensive statement of the results obtained with these new capabilities and their use in updating the risk assessments of nuclear power plants.

It's of interest, therefore, to examine the changes which have occurred and the results of those risk assessments. The comparison between NUREG-

1150 and WASH-1400 must, of course, be limited to the Surry and Peach Bottom plants, which are the only two plants evaluated by WASH-1400, and in addition limited to median results and internal events since WASH-1400 didn't compute mean results nor explicitly treat external events.

(Slide) The changes have resulted, if I may have the -- I see I have the first slide -- from two broad categories of progress, the first primarily bearing on preventing a severe accident. Here, a major increase has occurred in data on equipment reliability and in the analytical methods for the transient behavior of systems which give greater insight as to accident initiators. These resulted from continuation of preWASH-1400 R&D and from increased attention to understanding, avoiding, and mitigating the possible small break loss of coolant accidents whose importance was so clearly identified in WASH-1400.

The second category there is primarily on mitigation of the severe accident, and here we see a radical infusion of experimental data and an extensive development of analytical methods which have been effected to improve the analysis of engineering questions pertinent to severe accident progression,

containment performance and the severe accident source term. R&D in these areas was relatively sparse before issuance of WASH-1400, but was accelerated greatly after the TMI accident.

(Slide) I'd like to in a very summary way, as shown in the next slide, make these comparisons in this limited form in the categories you see: core damage frequency, accident progression, containment performance, severe accident source terms, and a comment on off-site consequences which was not fully practical to compare. And note, again, that these comparisons are only done for two plants.

(Slide) The next slide covers the core damage frequency issue. The median core damage frequency for Surry is reduced from 6×10^{-5} reactor years in WASH-1400 to 2.3×10^{-5} in NUREG-1150, a factor of 2.6; and for Peach Bottom, from 2.9×10^{-5} to 1.9×10^{-6}, a factor of 15, a tribute in a broad sense to WASH-1400 for this long time frame, having come as close to the best evaluations we can make today.

Now the modifications of the Surry plant since WASH-1400 have been substantial and have provided cross connection of high pressure safety injection systems, auxiliary feedwater system improvements, refueling water storage tanks for the

two units, all of which have substantially reduced the probability of core damage from loss of coolant accidents.

However, NUREG-1150 added reactor coolant pump seal failures as a new initiator to the small break LOCA sequence and thereby increased the probability of a small break loss of coolant by a factor of 10. So the plant modifications which I've enumerated above have offset this increase leading to the overall decrease we see in Surry.

In the case of Peach Bottom, those interaction of changes have not had the same impact and as a result of the changes in the venting for Peach Bottom we see a substantial reduction in the estimated core damage frequency.

Another comparison is based on estimating the contribution of individual scenarios to core damage frequency for the two plants. It's not been possible to do this in a direct sense, because again only median data is given in WASH-1400 and only mean data in NUREG-1150 for the individual accident scenarios. However, the percentage contributions of the individual scenarios to the total mean CDF have been compared by the Committee with the results as follows. And I'd like to mention here that we're

indebted to Ed Worman of Stone and Webster for developing in detail comparisons of which these are selected ones. Ed has presented those results already to the ACRS, so they're available in very full detail.

In the WASH-1400 Surry analysis which you see here, 64 percent of the core damage frequency was associated with LOCAs, 28 percent with transients, and 8 percent with containment bypass events. You can see the substantial change in the NUREG-1150 analysis: 15 percent of the CDF associated with LOCAs, 77 percent with transients and station blackouts, and 8 percent again containment bypass. So there's a very real shift in focus on the risk dominant areas of these scenarios. Although containment bypass events account for only 8 percent of the core damage frequency for Surry in both these studies, we have to recall that these sequences dominate off-site risk due to the large releases and that's an issue that has to be kept in mind in all these evaluations.

(Slide) The next slide shows that same comparison for Peach Bottom, and we see not a very graphic change. The same general picture is shown with the emphasis again on transients and station blackouts.

(Slide) I'd like now to turn to slide 6

and it gives a very summary simplistic picture of containment performance by presenting the percentage of severe accidents which result in containment failure between the two reports. As you know, the WASH-1400 made a fundamental assumption that every severe accident would result in containment failure, so we see the 100 percent failure and no containment intact. Now with the improved evaluation capability and experimental data, that assumption was not made in NUREG-1150, and we see that in the Surry plant only 19 percent of the severe accidents lead to failure and 81 percent the containment retains its integrity. Peach Bottom, a higher number of containment failure cases associated with the bypass issues there, 74 percent, and 26 percent of the severe accidents retaining containment integrity.

(Slide) I'd like now to turn to the source term, which is a more complex subject, and in this short time just give you a sample. We have in our report a selection of graphs such as you see on the slide, which compare the median data from WASH-1400 against the mean, median, and 95 percent percentile and 5 percent percentile of the NUREG-1150 data. And you can just scan that and you can see, looking at the median, the shift downward in the

source term work. This particular slide is for the frequency of exceeding iodine release fractions in the Surry analyses.

(Slide) The next slide is another sample, and it's for Peach Bottom and it shows the same comparisons for exceeding cesium release. Now I'm not going to ask you to try to dig out all the large amount of information in these graphs and some six additional ones you have in the handouts, but in the next slide give a very simple summary.

(Slide) The median radionuclide releases from Surry of 10 percent or more of the core inventory of iodine or cesium are ten times lower in NUREG-1150 than WASH-1400. If you look at the releases of strontium of one percent or more of core inventory, those estimates are 1,000 times lower in NUREG-1150. And if you look at the releases of lanthanum of .04 percent or more of the core inventory, you again find a number that's about 1,000 times lower in NUREG-1150 than WASH-1400.

(Slide) And the next graph is an attempt to compare the median probabilities. The radionuclide releases in this case from Surry would exceed specific fractions of core inventory given a severe accident. And we see that 10 percent or more of the iodine or

cesium would be released. That event is five times less probable in NUREG-1150 estimates than WASH-1400. And the probability that one percent or more of strontium would be released is 1,000 time less in NUREG-1150 than WASH-1400. And finally, that .04 percent or more of lanthanum would be released, the probability here runs about 500 times less in the new evaluation of WASH-1400.

So in summary, we're encouraged that on a comparison basis where the comparisons can be made with this large infusion of additional information we're seeing lower core damage frequencies and more effective mitigation and lower source terms in the event that releases occur. No reason suggesting for complacency, but nevertheless a reflection that the heavy work has been done by NRC, its research organization, by the industry in its plants and also its research work is paying off in improvements in our overall picture of the risk of these plants.

COMMISSIONER CURTISS: John, are these findings limited in large measure to the two plants you looked at or can they be extrapolated beyond those two?

DOCTOR TAYLOR: I think it would be extremely difficult, Commissioner, to extrapolate this

to other plants. The IPE work is necessary to assure that the differences of plants are taken into account.

COMMISSIONER CURTISS: Okay.

DOCTOR TAYLOR: I think when we're finished with the IPE work we'll be able to make that assessment.

DOCTOR KOUTS: I think we have to point out in this connection the importance that Three Mile Island accident had in this connection, because it was after the Three Mile Island accident and the digestion of the results that were observed that realization arose that the releases from that accident were lower than those that had been predicted by WASH-1400, and this report and all the things which have led into it have been the result of that realization. And these substantially lower numbers are largely a result of the realization of the importance of water in accidents of this kind. That would appear in all the PRAs.

DOCTOR TAYLOR: The phenomena treated here, when we look carefully, do have generic implications. I just would hesitate to suggest we assign risk factors per plant on that basis. But one would expect that this better understanding of the phenomenology which TMI did give us the first hint on

is going to reflect the lower source term as we go.

DOCTOR KOUTS: And that's largely our report. I'd like to end this formal part of it on another note, which is to thank the staff of the Atomic Energy Commission -- sorry, the staff of the Nuclear Regulatory Commission for --

COMMISSIONER ROGERS: You're getting a little old now.

DOCTOR KOUTS: Shows how long I've been around -- the staff of the NRC for all the assistance which it gave us. The questions as they arose were promptly answered and we benefited from every method which they could use to satisfy our curiosity with respect to a number of things.

I'd like to particularly single out Doctor Beckjord; Doctor Murphy of your staff; Mr. Bartlett, who is the designated federal official; and also Doctor John Weeks of Brookhaven National Laboratory, who has worked with us and made all our arrangements possible.

COMMISSIONER ROGERS: Good.

DOCTOR KOUTS: Now if you have questions, we'd be very happy to answer.

COMMISSIONER ROGERS: Commissioner Remick?

COMMISSIONER REMICK: Yes, I do.

First, I'd like to express my appreciation to the Special Committee for the fine job and your report today.

I do have few questions, Doctor Kouts. You indicated that external events were not fully treated and you gave two examples done for only two of the five plants, and also that there were differences in the seismic risk curves. Are there other areas in which the external events you feel were inadequately covered?

DOCTOR KOUTS: Well, we had a substantial discussion about the role of fire and how well fire was treated with respect to these two plants. We were not able to come to any strong conclusions about how well fire was treated, because most of the information is in auxiliary publications, some of which still have not been written and are still coming out. And I can't really address the question of how well fire was treated.

As far as we could see, the treatment of fire was pretty much state of the art, but fire and its treatment in any risk study is a very muddy subject. It's very difficult to pin down. Fire modeling is not a simple thing, and it probably is less well understood than people think it is. That's

a personal view. It probably is shared pretty well.

COMMISSIONER REMICK: So those were the two major reasons, then, on external events, you felt?

DOCTOR KOUTS: Yes, those were our principal comments on external events.

COMMISSIONER REMICK: Doctor LeSage, you indicated that there were several occasions when project staff provided opinions rather than seeking that, eliciting it from experts. Could you give me a couple examples where that occurred?

DOCTOR LeSAGE: Yes. Let me think. I think the chief one that -- chief from the standpoint of being important -- was that some of the bypass sequences on the PWRs were largely -- when they were quantifying the amount of releases such as steam generator tube rupture -- were largely handled by the project staff. As it turned out, these turned out to be a principal contributor to the risk for PWRs. And there were some fairly large approximations made in some of the areas. For example, if I remember, the hold-up in some of the buildings was completely ignored, things like this, some of the auxiliary buildings. So that would be one that comes to mind right away. I'm trying to think of another one. There were a number of others, but that's an example.

COMMISSIONER REMICK: All right.

You chaired the ANS study. Am I correct?

DOCTOR LeSAGE: That's right.

COMMISSIONER REMICK: Do you see, having served on both, any major differences in the findings or conclusions of the two groups, major differences?

DOCTOR LeSAGE: Well, there are no major differences in opinion. There was a difference in focus a little bit between the two committees, but I think the bottom line conclusions were pretty consistent.

The ANS committee did not have expertise in human reliability on our committee, so we didn't focus as much on that area. And I think we didn't quite appreciate its importance until later in our view and then we realized that this would be covered in this committee, so we didn't focus on that. We maybe focused a little more on the mechanics of how this whole thing was put together, but I don't think there were any -- I think the overall evaluation of the reports are about the same by the two committees.

COMMISSIONER REMICK: I don't want to put you on the spot, but in your personal opinion do you think it would be advantageous for the Commission to receive a review of the ANS study, or do you think

that there's enough similarity that that's not necessary?

DOCTOR LeSAGE: I think you'd probably find it interesting. Yes, I think probably it would be useful to have a presentation.

COMMISSIONER REMICK: Doctor Apostolakis, I don't know if you have an opinion or if you've had a chance to look at our Human Factors Research Program. But do you feel that -- if you have, do you have any kind of opinion on whether that'll enable us to better model human reliability, the things that are ongoing, perhaps.

DOCTOR APOSTOLAKIS: I really don't know--

COMMISSIONER REMICK: All right. Okay.

DOCTOR APOSTOLAKIS: -- the programs.

COMMISSIONER REMICK: Okay.

And Doctor Taylor, on your slide 3, where you were making a comparison between the core damage frequency in Surry and Peach Bottom and the results from WASH-1400 and NUREG-1150, you later pointed out that one was based on mean, the other was median. On that chart, are they on the same basis or are some of those -- are the WASH-1400 median values and 1150 mean?

DOCTOR TAYLOR: We used point values for

the NUREG-1150 case to compare against the median values in WASH-1400. To the best of our ability with the data available, they're apples and apples.

COMMISSIONER REMICK: Apples and apples, all right.

DOCTOR LeSAGE: Yes, I can say that too.

COMMISSIONER REMICK: Okay.

DOCTOR LeSAGE: We looked at the same figures in ANS, and I think they were median to median comparisons on those.

COMMISSIONER REMICK: Okay. All right.

And then, a general question to anybody who wants to answer it. Based on your knowledge now of NUREG-1150 and the IPE process, any special advice you might have to licensees on how they might use 1150 in the conduct of their IPE programs? Anything come to mind?

DOCTOR LeSAGE: I'm somewhat more familiar with 1150 than I am the IPE process, but I've heard it stated by the people who do this that there is a lot of useful information in 1150. I can't go too much further than that, especially in the documentation from the expert opinion process which is very voluminous and very detailed. That's about all I'm really prepared to say, I think.

DOCTOR KOUTS: I would think primarily in the quantification of event trees. The information in event tree quantification is extremely detailed in many topics. You'll find in this list of detailed comments that I furnished you some reference to the detail with which the containment -- the source term calculation, the level 2 calculation was carried out, and a view that probably it was much too detailed, at least from the standpoint of how much is understood about the processes which take place there.

But the trees which were -- the accident progression event trees, as they are called, are discussed in such great detail that there is a great deal of background information to be available in the numbers that are assigned, and I think that could be quite valuable. Our comment in this respect was that once you had gone through all the thought process and having arrived at your understanding, it was not necessary to use that detail because it exceeded your understanding. But the detail is there and can be very valuable.

DOCTOR TAYLOR: I think I concur with Herb on level 1, it really is a state of the art effort and utilizing it as a base for the IPE without having to repeat all the detail would be extremely

valuable.

External events was a more limited study in NUREG-1150, and I don't think the authors are claiming the same level of in-depth work. It is more limited in many ways. So the IPE process will have to be forging some of its own ground there, but can be certainly benefited by looking at the work in NUREG-1150.

DOCTOR KOUTS: Since you've brought up the external events, I'd like to emphasize again how important it's going to be for the purposes of IPE that the Commission give some better guidance with respect to seismic risk, because the licensees are confronted with the contradiction between the Lawrence Livermore risk curves and the EPRI sponsored risk curves and they need to know what the Commission is going to be requiring of them in this respect.

DOCTOR TAYLOR: We'd very much like you to go over the discussion in our report on that subject. We have not chosen to make a judgement ourselves. That's your job. But we've laid out the background, I think, in a summary way that would be valuable to you.

COMMISSIONER REMICK: I'm glad you're giving us such a simple task.

One final question. With the hind sight

which you now have, if we were to start off on a new 1150 type of study, is there anything that comes to mind that you would do differently in general, without getting into specific details?

DOCTOR KOUTS: I'm sure we have, I would guess.

Leo?

DOCTOR LeSAGE: I'd like to think about that for a second before I answer that.

DOCTOR TAYLOR: Well, you know, there's been a substantial identification of the difficulty of treating the human reliability element. I think myself the benchmark program was somewhat flawed in its-- I've never know a benchmark the first time around-- you look back and say, "My God, we didn't define the ground rules right and people didn't get marching off the same way." And so you get this big divergence which you say if you'd defined the benchmark effort effectively you'd get a much more meaningful result. So I would sure want to have a-- based on the experience that we've gained thus far-- a better benchmarking of human reliability.

And I know in our own program at EPRI, and this is therefore a personal remark, we are of the opinion we can learn an awful lot, although not the

whole story, but an awful lot from evaluation of operator response on the simulator as well as discussion with the operators in detail of what their problems are. And more of that is needed and I would hope we will see it in the subsequent work that is done.

COMMISSIONER REMICK: Any other comments on that?

DOCTOR KOUTS: Well, you've had some comments already. We'd certainly get the external events in.

COMMISSIONER REMICK: Sure.

DOCTOR KOUTS: I think we'd pay some attention to fire that we don't discern has been paid so far.

I would support strongly what John Taylor has just said about the human reliability aspects.

We think that there should be some -- the contributions to uncertainty in health effects should be propagated in with all the other uncertainties. That's a failing of the report. There are a number of detailed items of this kind.

COMMISSIONER REMICK: Okay.

Yes?

DOCTOR APOSTOLAKIS: One comment. Perhaps

a committee like this one should be formed earlier in the game, because after the bulk of the work has been done and you come in there and you present something like the results of the useful exercise and point out there are other models and so on, it's really very difficult to go back and redo the study in essence. So some independent body that would give some advice early in the game so that they would have time to implement these things, some of them.

Of course, you don't want that committee to be part of the study, because then it defeats the purpose, of course, already of your committee. This committee didn't have that much of a problem, but I was a member of the earlier committee, the Kastenberg Committee, and clearly we were very late. We were giving them advice that was really useless to them.

DOCTOR KOUTS: One other suggestion. Wait another 15 years.

DOCTOR LeSAGE: I'd like to add another comment too, another area where I think the ANS Committee agrees wholeheartedly with this committee. And that is that we don't see anything in the report, even though we've made a number of comments, that should delay publication. If they can be incorporated in very easily, fine. If it can't, it can be handled

in some other report later on. But this report will lose its value if it's delayed much longer.

COMMISSIONER REMICK: Once again, I want to thank the Special Committee very much.

COMMISSIONER ROGERS: Commissioner Curtiss?

COMMISSIONER CURTISS: I just have three or four areas that I'd like to follow-up on a couple that Commissioner Remick has raised.

On the issue of seismic events, I take it what you're recommending there is additional research on the seismic question that would permit us to more accurately and objectively make a decision between the risk -- the hazard curves that have been advanced. My question goes to the relationship of the research that you're proposing with the External Events Program. And specifically, is the kind of research that you're recommending here of such a nature that we need to take a look at the question in the context of the IPEEE where the staff has laid out a seismic PRA and a margins approach that, if I understand the briefing last week for the margins approach, calls for -- I'll over-simplify -- some averaging of the two?

DOCTOR KOUTS: No. I think when we contemplate the research which might be appropriate

here, it's a different kind. You do have two seismic risk models and the conflict between them is important and we discussed at some great length the way this difference arose in our report.

What we did not put in the report and what we discussed putting in the report at one time was reference to some research that was done by EPRI in which they tested models against one particular earthquake in Canada, the Saguenaw earthquake, and came to some conclusions themselves about the applicability of various models.

Now there are other earthquakes taking place around the world to which this particular kind of technique can be applied, and this is the crucial way by which resolution of this issue will be made. It won't be done by paper studies. It will be done by actual application of measurements, as was done in the EPRI study. So I recommend myself personally and very strongly that the research program include things of this kind.

COMMISSIONER CURTISS: And can that kind of research go forward in parallel with an IPE program that would --

DOCTOR KOUTS: Oh, yes.

COMMISSIONER CURTISS: -- ask the

licensees to go ahead and do the evaluations using both curves?

DOCTOR KOUTS: I think that you have no recourse at present but to ask them to use both curves, but it should go on in parallel. And at some point, you'll be able to distinguish which of these should be chosen.

COMMISSIONER CURTISS: Okay.

DOCTOR TAYLOR: Well, I think it goes without saying in view of my position that -- and this is not a committee position but my own and the EPRI position that the curves that we have produced are the curves that should be used in the IPEEE. But that's a personal position based on our best judgement of evaluating all the data.

COMMISSIONER CURTISS: Okay.

DOCTOR TAYLOR: And I think the committee is urging that a decision be made and this not be let linger for a long time, because there's a major difference between the two and the issue of closing the IPE process could be severely jeopardized if a decision isn't made.

COMMISSIONER CURTISS: I gather there's a consensus within the committee that the additional research of the kind that Doctor Kouts described is

the way to proceed at this point to reach a decision on which of the two --

DOCTOR KOUTS: I think there is no difference in that respect.

COMMISSIONER CURTISS: Doctor Apostolakis, I wonder if I could ask you maybe a variation on Commissioner Remick's question about human reliability research. Without perhaps knowing the details of the Commission's research program in that area, can you sharpen the focus on the kind of reliability research in the human factors area that you think needs to be done?

DOCTOR APOSTOLAKIS: Well, clearly we're talking about accident situations, not pre-accident/post-accident or post-initiating event.

One of the things that has to be done and has not happened yet is someone to look at the models that are available now, like THERP, the EPRI approach -- I don't think it's called HCR anymore, but that approach -- SLIM mod, and various others, and look at the different assumptions that these analysts have made and question the assumptions and then perhaps attempt to synthesize these models and come up with a universal model. Now that's easier said than done, of course, but I don't think that these models

have been out in the literature for a sufficient amount of time now for the community to use them and understand them and absorb them.

So really, NUREG-1150 is no different in that respect in the sense that they used the models that were available to them. The experts were certainly available to them. You've got another organization and they are using the models that are available to them without really questioning their validity. Sometimes you hear things like, "Well, we recognize that there is a lot of uncertainty. We put a factor of 10 there up and down and maybe we cover the uncertainties." But that's not really very satisfactory.

And for instance, the SLIM mod approach which identifies performance shaping factors and attempts to quantify the impact of these things on the performance of the crew, that's certainly a very noble effort. But only the practitioners of SLIM mod -- the developers of SLIM mod have used it so far and that's certainly a draw-back. I mean, you need other people to also do that. And in so doing, of course, you have to investigate other things also, like perhaps safety culture and things like that.

And another interesting exercise would be

to go back and look at actual incidents, like what happened at Davis-Besse, what happened at Three Mile Island or other places, and take the various models and compare the two now, sort of like precursor study but for human reliability and say, "Well, at this point at Davis-Besse, there was reluctance to initiate bleed and feed. If I look at SLIM mod, does this allow something like that? And if so, how? If I look at THERP, does it allow for this? If so, how?" In that way, we will start giving these models a certain degree of realism, I think, because so far I don't know if anybody has done that. EPRI, of course, is trying to use simulators and so on, which is certainly something that is useful, but I think looking at real incidents would be very illuminating.

DOCTOR TAYLOR: There's great value in the real incidents. The difficulty is the statistics. I mean, there's just not enough of them to put that together in some quantitative form. You need both.

And again, I think the benchmark exercise that was done here was really not defined well enough that you could conclude something from it. The conclusion you draw is there's enormous variations, and I personally believe they're not as big as would be shown because the benchmark process was not

effective. I mean, the issue of the reality of a plant, to the best I understand, very little visitation of the plant that was in the benchmark was made. So how would you expect to get --

DOCTOR APOSTOLAKIS: I also want to make another comment. I talked to two of the major developers of models, Alan Swain, who is behind THERP, and David Embrey, who was a principal behind SLIM mod, and I mentioned to them the Ispra exercise that I've shown you and what they thought about it. And they both felt that the exercise is not worth a damn, that they felt that here was a bunch of engineers who don't understand anything about human factors trying to become experts at human factors.

So there are objections to what I've shown you. I mean, it's not that the exercise itself was the most objective thing in the world. But I think that the conclusions that there is significant model uncertainty are there. I mean, you cannot really dispute that.

COMMISSIONER CURTISS: Two other quick questions, Doctor Taylor, I guess, on your presentation as I look at the comparisons between WASH-1400 and NUREG-1150 for the two plants that you compared. I guess the layman's question that jumps

out at me is do you attribute the improvements to modifications and fixes that have been made in those plants in large measure or to development?

DOCTOR TAYLOR: The core damage frequency heavily associated with modifications that were made. Another important factor that bears on the Surry result is the recognition after WASH-1400 that the failure pressure of the Surry containment -- I think that's pretty typical of the PWR dry containments reinforced concrete types -- is 130 pounds gauge per square inch rather than the 80 pounds that were in WASH-1400. That had a substantial influence as well.

COMMISSIONER CURTISS: Okay.

DOCTOR TAYLOR: There's quite a variety of modifications going up and down in terms of their-- the modifications improving the core damage frequency, new insights such as the pump seal issue increasing the picture of core damage frequency and then in the containment performance, a very substantial effect of the higher strength of containment that had been previously understood.

COMMISSIONER CURTISS: Okay. One final question here on the materials that you left us at the beginning. I've just had a chance to look through them very quickly. But the one that I'd ask for your

specific response on now, if you're prepared, is actually the comment at the bottom of the first page on the BWR Mark I containment issue. Would you expand on that particular comment?

DOCTOR KOUTS: Well, the thing that struck us most about this was that the curves showing the results of expert opinion elicitation and the uncertainty distributions associated with the issue of the BWR Mark I containment showed very strong evidence of two different distributions being mixed together. That is the distribution functions were very strongly double humped. This implied to us a greater degree of lack of information than presence of information and a need for the Commission to resolve its view of what is actually taking place here. That is, devote more attention to removing the double hump by removing the lack of information.

I know that this is not very helpful advice, but that was the best we could come up with. This was one of several instances of presence of double humped distribution functions which made it very clear to us that the expert opinion elicitation process has some very difficult problems to address with respect to this and emphasizes the need to pay strong attention to the composition of the panels from

which expert opinion elicitation is to be drawn. Now, perhaps better attention to the panel composition in a case like this might have been very helpful.

COMMISSIONER CURTISS: Okay. All right.

DOCTOR KOUTS: Other people may have some comments.

DOCTOR TAYLOR: Very well, Herb. There is a section in the report that attempts to discuss the median versus mean and the potential use of both along the lines that if you're doing the engineering side of the job, you'll tend to look at the median because it's the center of the nest of results and uncertainties. In the severe accident evaluation, say for severe accident management, you're searching for what is your best estimate of the condition of the plant with which you could guide operators to take action to stop the progression of the accident. Again, you'd lean toward the median.

Now, if your focus is, "I want to be sure of the safety of the system," it, of course, would move to, "I want to see conservatism," and the mean will draw out the conservatism, and in the case of the expert opinion situation will amplify the radical opinion on the conservative side. My own judgment is you should look at that for sure and then when you see

something, a bimodal distribution, focus in on how did it happen and what are the underlying causes of it and make a final decision whether that level of conservatism is warranted. That's, I suppose, what I'd like to see myself in the seismic area.

COMMISSIONER CURTISS: I see what you're saying.

DOCTOR KOUTS: We gave a substantial discussion of a number of related things like should one use weight factors in expert opinion elicitation? Should you use something like the olympic voting system where you throw out the extremes? Should you -- there's a long discussion of this general subject of how you should interpret results when there are broad and bimodal distributions and you may find that --

COMMISSIONER CURTISS: I'll look forward to seeing the report.

Let me just close by thanking all of you, as well as your colleagues who aren't here, for the valuable effort. It's been most enlightening, I think, for the Commission as a whole and certainly for me.

COMMISSIONER ROGERS: Well, I've just got a couple of little ones. When do you expect to have

your final report out?

DOCTOR KOUTS: We expect to release it ourselves in about ten days.

COMMISSIONER ROGERS: Oh, very good.

Just a question as to whether you have any thoughts on how best to access this enormous amount of information that's in this study in the future. Do you have any specific recommendations? One hopes that this doesn't go on the shelf someplace and some of the very interesting background material that's in the report. Do you have any suggestions as to how that might best be accessed?

DOCTOR KOUTS: We did not address that. We discussed it at the very beginning of our Committee deliberations.

COMMISSIONER ROGERS: And gave up.

DOCTOR KOUTS: And gave up, yes.

DOCTOR LeSAGE: There have been some suggestions of some sort of computer cataloging of what's in there some way to make it accessible, but that's something we didn't as a Committee really address.

COMMISSIONER ROGERS: Well, it's certainly something, I think, we'd appreciate any thoughts that you have on it.

On behalf of my fellow Commissioners and myself, I want to thank the members of the Peer Review Committee who have been able to be here today for an excellent and informative briefing and discussion of the results of the review. The validation of this major NRC effort by an independent group with outstanding technical credentials should contribute substantially to our ability to use the results of this effort to fullest advantage. We foresee important potential applications for NUREG-1150 in a variety of purposes, both internal and external to the NRC.

While we're certainly aware of the limitations of the methods used in the study, we believe it can have an important influence in the use of PRA methods in such areas of identifying plant operational features or practices that have an adverse impact on plant safety and of making decisions on the priority and resource commitments that should be made to different NRC and licensee activities. The affirmation of the methods of this study by the peer review committee will allow us to proceed on such efforts with greater confidence and authority.

We further anticipate continued work to improve and refine selected aspects of this type of

analysis and we will appreciate any guidance the Committee can offer us in this area as well.

Once again, we thank you members of the Committee for coming today and we thank all the members of the Committee for the considerable time and effort they've devoted to this review and for their very valuable insights and assessments.

Thank you very much.

(Whereupon, at 3:31 p.m., the above-entitled matter was adjourned.)

CERTIFICATE OF TRANSCRIBER

This is to certify that the attached events of a meeting of the United States Nuclear Regulatory Commission entitled:

TITLE OF MEETING: BRIEFING ON NUREG-1150 PEER REVIEW GROUP STATUS

PLACE OF MEETING: ROCKVILLE, MARYLAND

DATE OF MEETING: JUNE 20, 1990

were transcribed by me. I further certify that said transcription is accurate and complete, to the best of my ability, and that the transcript is a true and accurate record of the foregoing events.

Carol Lynch

Reporter's name: Peter Lynch

SPECIAL COMMITTEE TO REVIEW THE
SEVERE ACCIDENT RISKS REPORT

Dr. George Apostolakis

Dr. Adolf Birkhofer

Dr. Lars Hoegberg

Dr. William Kastenberg

COMMITTEE (CONTINUED)

Dr. Herbert Kouts

Dr. Leo LeSage

Dr. Norman Rasmussen

Dr. John Taylor

Dr. Harry Teague

Response To The Kastenberg Review

Was Appropriate

Uncertainties

Expert Elicitation

Focus On Low Probability Tails

Is There A Cutoff

Use For Standardized Methods

Further Improvements In PRA

- A Good Report
- Responded Well To Earlier Concerns
- Generally State Of The Art
- Broke New Ground

ITS VULNERABILITIES

Treatment of Human Reliability

Elicitation of Expert Opinion

EXTERNAL EVENTS NOT FULLY TREATED

OTHER MISSING MATTERS:

Pressure Vessel Failure

Main Steam Line Failures In PWR's

Errors Of Commission

Startup And Low Power Operations

RECOMMENDATIONS

Early Publication

Additional Guidance of Seismic Matters for IPE

Seismic Research

Human Factors Research

USE OF STRUCTURED, FORMALIZED EXPERT OPINION ELICITATION:

- Both a distinctive and one of the most controversial features of NUREG-1150.
- The expert elicitation process in Draft-1 was severely criticized.

Expert judgment of some kind (informal or formal) is necessary in all PSAs because knowledge of phenomena and scenarios is not adequate to provide reliable analytical or experimental values for many parameters required in a PSA.

THE STEPS IN THE NUREG-1150 (DRAFT -2) EXPERT OPINION ELICITATION PROCESS INCLUDED:

- Selection of the expert panels.
- Training of the experts.
- Technical presentations to the experts and discussions among the experts.
- Elicitation process.
- Combining the results.

THE COMMITTEE'S MAJOR CONCLUSIONS REGARDING EXPERT OPINION ELICITATION

- The structured use of expert opinion in NUREG-1150 represents an important advance over previous applications of expert opinion.

- Specifically, Draft-2 represented a major improvement over Draft-1.

- The Committee still has concerns regarding the selection of experts.
  - Why were some selected and others not?
  - Was the mix of expertise optimum?

- Further, expert opinion, even when properly structured, should be applied with caution and the results treated with some skepticism, sinced we are dealing with poorly understood and complicated phenomena.

ADDITIONAL COMMENTS ON EXPERT OPINION

- Formal expert opinion, guided by recognized professionals, is preferable to the informal alternative.

- The training provided the experts was useful.

- The process provided a structured method for introducing additional analysis and experiments.

ADDITIONAL COMMENTS ON EXPERT OPINION (cont.)

- The process was well-documented and the documentation will be useful.
- Expert opinion may have been relied upon too heavily in some instances.
- The number of issues addressed by the experts was limited. Even so the workload on the experts was sometimes excessive.

ADDITIONAL COMMENTS ON EXPERT OPINION (cont.)

- Other issues, judged initially to be less important, were addressed by the Project Staff. Sometimes, as the study evolved, these issues turned out to be very important. The expert opinions given by the Project Staff were not well-documented.

- Alternate methods of weighting the judgments of the individual experts were not discussed.

- The NUREG-1150 expert opinion process was expensive and time consuming. Unlikely to be repeated again soon.

COMMENTS ON METHODOLOGY, SAFETY CULTURE, AND HUMAN RELIABILITY ANALYSIS

Presented to the
US Nuclear Regulatory Commission
June 20, 1990

by GEORGE APOSTOLAKIS
University of California, Los Angeles
Member, NUREG-1150 Peer Review Committee

COMMENTS ON METHODOLOGY

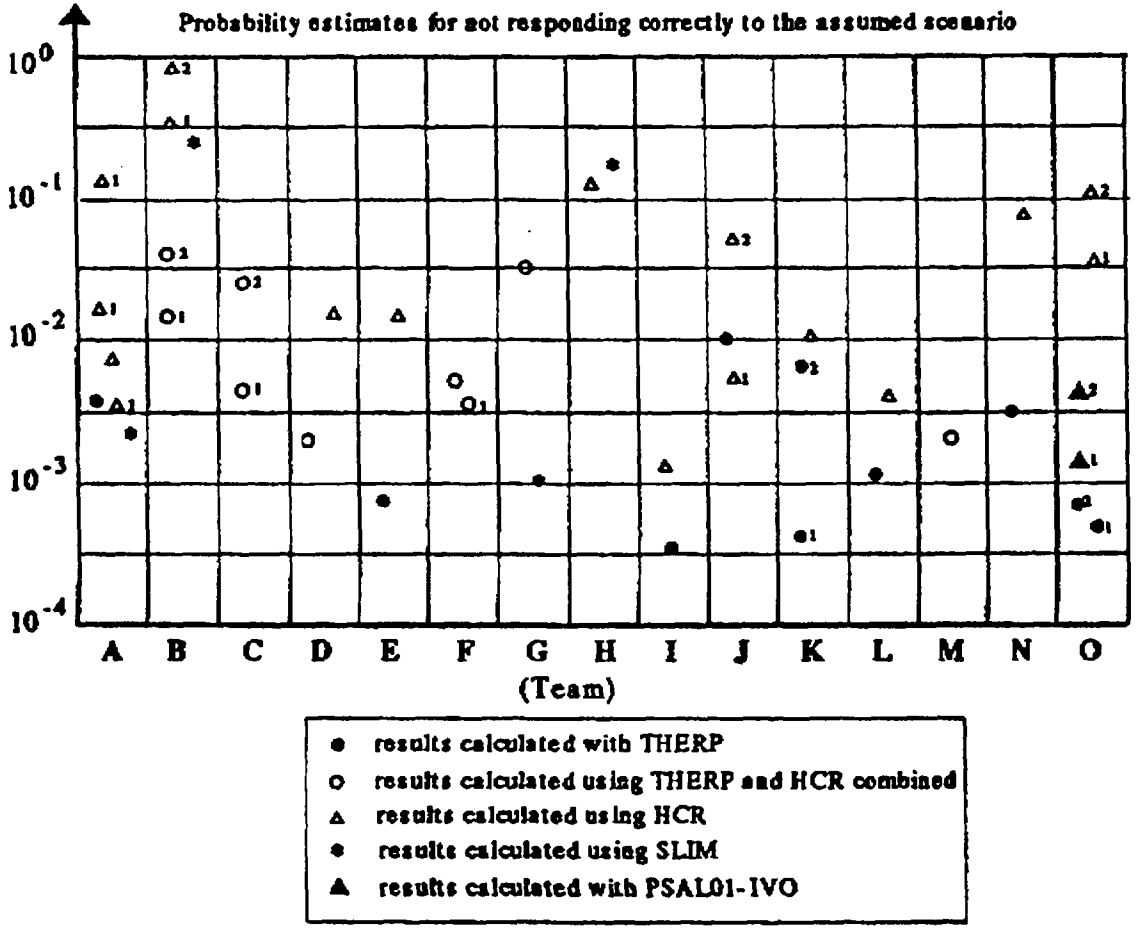
- WASH-1400 Methodology still used
- Better data available
- Improved models for external events
- Human errors during accidents

SAFETY CULTURE

- Plant policies may set priorities of operator actions long before emergencies (e.g., reluctance to initiate bleed-and-feed cooling at Davis Besse, June 9, 1985)
- A fundamental management responsibility is the establishment of a safety culture governing the actions and interactions of all individuals and organizations engaged in activities related to nuclear power (IAEA INSAG).
- Management quality not in current PSA.

HRA

- HRA for routine actions is state of the art.
- For recovery actions, ASEP procedures were used.
- Alternate models were not included,i.e., model uncertainty was ignored.
- CEC-JRC Ispra HF-RBE demonstrated the importance of model uncertainties.



A. Poucet, "The European Benchmark Exercise on Human Reliability Analysis," PSA '89, Pittsburgh, April 2-7, 1989.

HRA EXAMPLE

- SLC system manual initiation (ATWS).
- Grand Gulf: THERP methodology, SNL 0.0001
- Peach Bottom: Time reliability curves, BNL, 0.02.
- No significant differences between the plants.

HRA (cont.)

- Errors of commission due to misdiagnosis were not investigated.
- Other PSAs have done so, e.g., those for the Oconee and Seabrook plants.

Major Technical Progress Has Been Achieved Since WASH-1400

- Improved data and methodology has given greater insight on prevention of severe accidents
- A radical increase in experimental data and analytical methods has permitted more effective evaluation of severe accident mitigation

WASH-1400--NUREG-1150 Comparisons

- Core damage frequency
- Accident progression
- Containment performance
- Severe accident source terms
- Off-site consequences

(Comparisons only for the Surry PWR and the Peach Bottom BWR)

Core Damage Frequency
(per reactor year)

	WASH-1400	NUREG-1150
Surry	6×10^{-5}	2.3×10^{-5}
Peach Bottom	2.9×10^{-5}	1.9×10^{-6}

Accident Progression
(Contribution of Individual Scenarios to Core Damage Frequency for Surry)

	WASH-1400	NUREG-1150
LOCA	64%	15%
Transients and station blackout	28%	77%
Containment bypass	8%	8%

Accident Progression
(Contribution of Individual Scenarios to Core Damage Frequency for Peach Bottom)

	WASH-1400	NUREG-1150
LOCA	3%	6%
Transients and station blackout	97%	94%

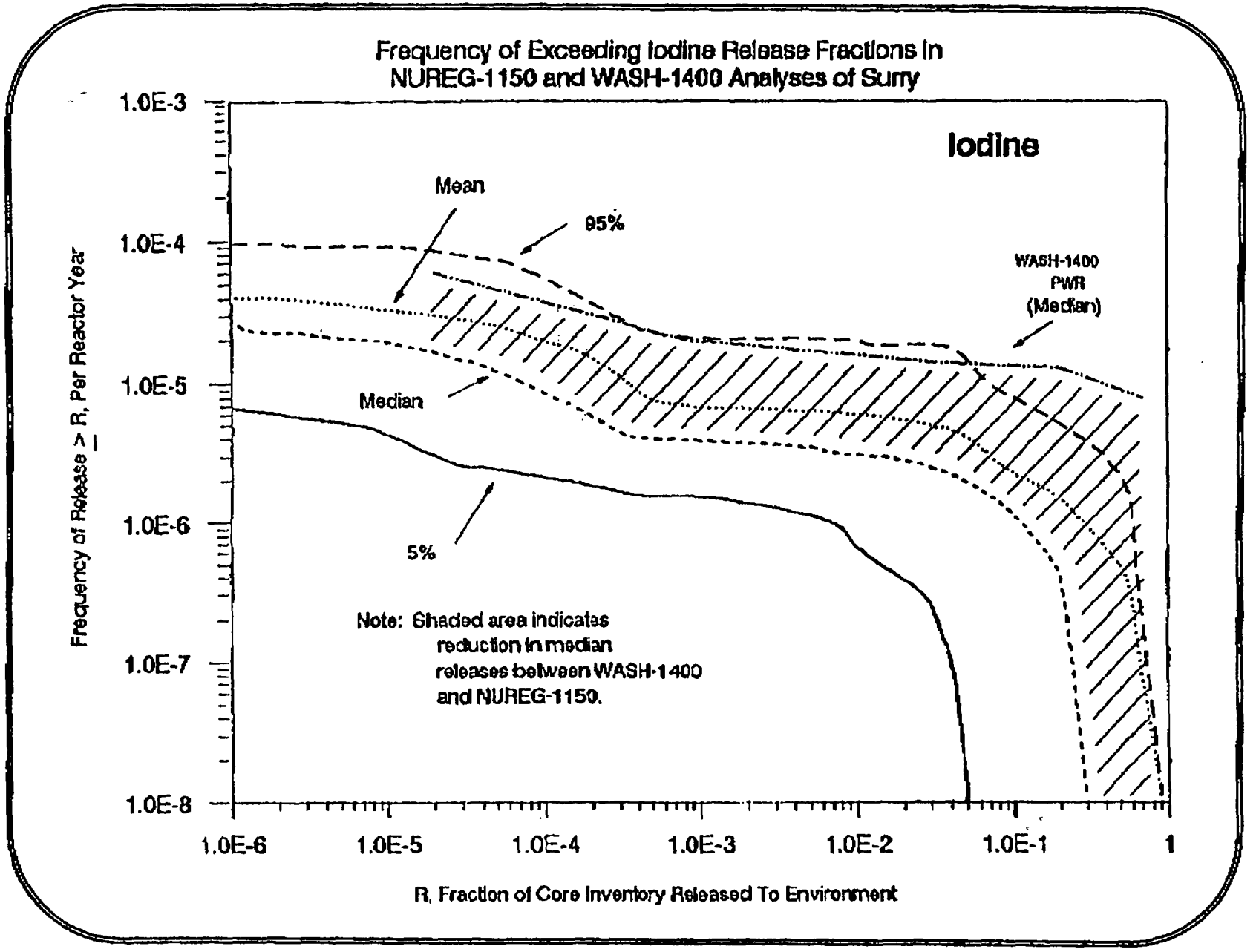
Containment Performance
(% of Accidents Resulting in Containment Failure)

	WASH-1400	NUREG-1150
Surry		
1. Containment failure	100	19
2. Containment intact	0	81
Peach Bottom		
1. Containment failure	100	74
2. Containment intact	0	26

Severe Accident Source Terms: NUREG-1150 Compared to WASH-1400

Median radionuclide releases from Surry of:

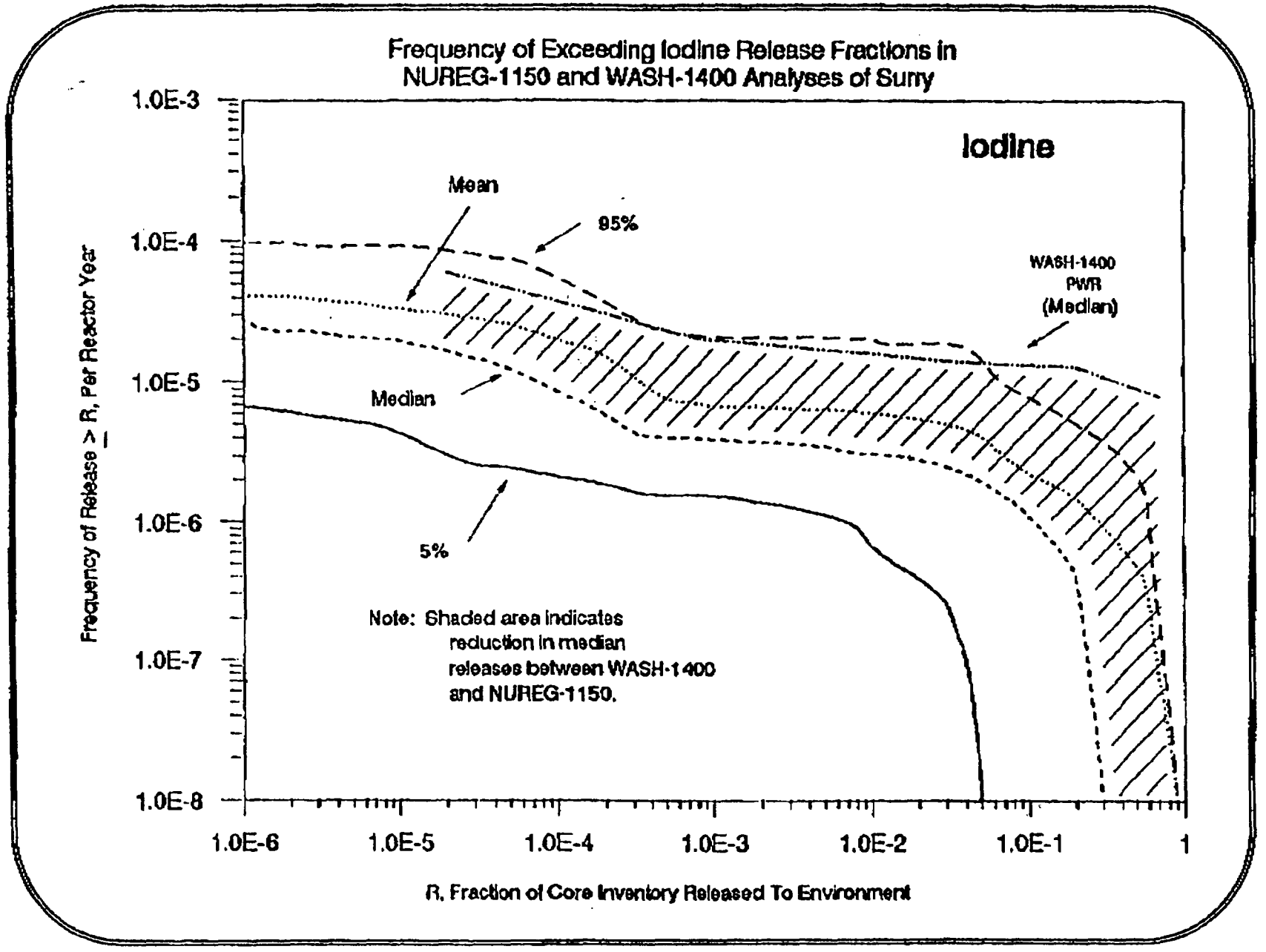
- 10% or more of core inventory of iodine or cesium: 10 times lower
- 1% or more of core inventory of strontium: ~1000 times lower
- 0.04% or more of core inventory of lanthanum: ~1000 times lower

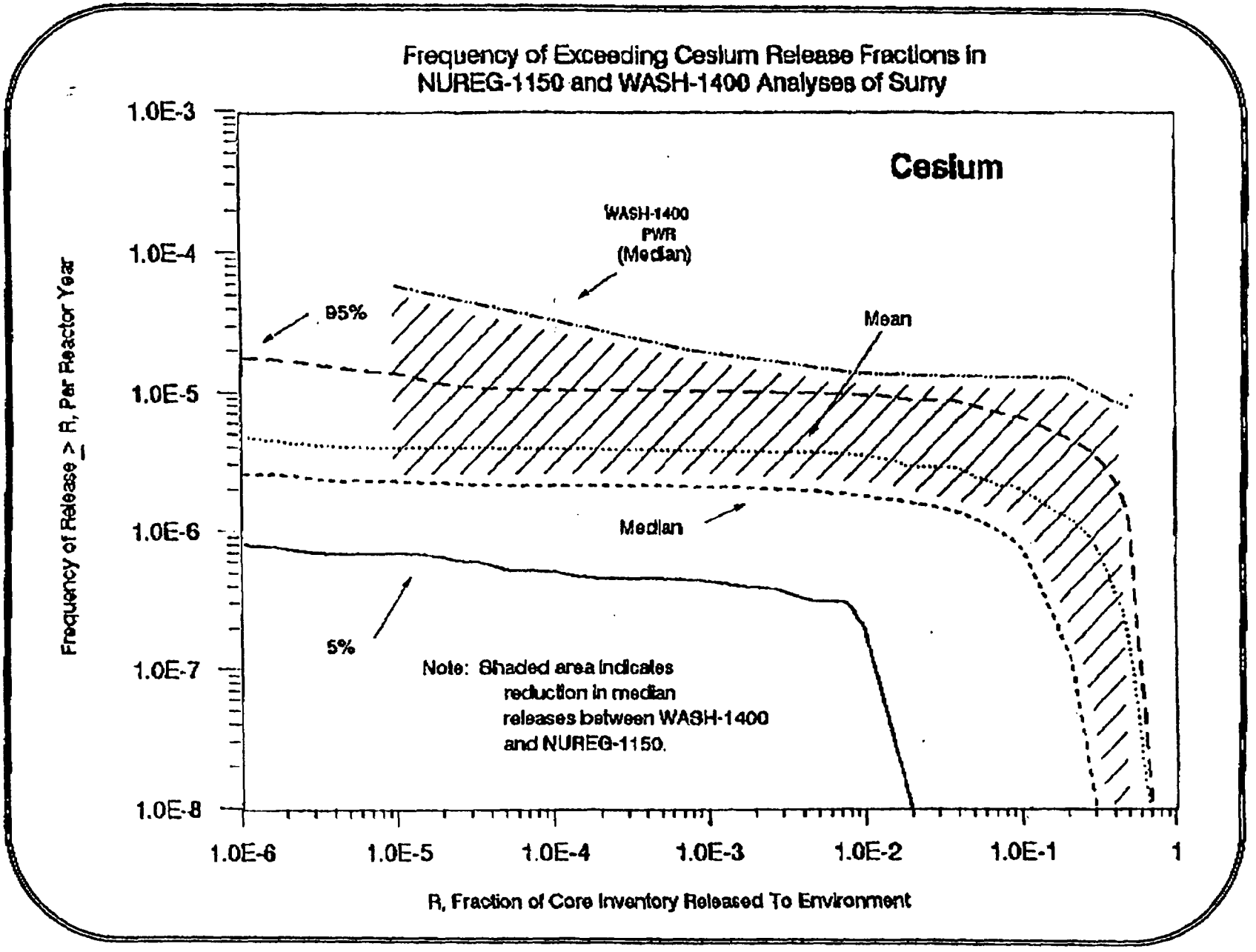

Frequency of Exceeding Iodine Release Fractions in
NUREG-1150 and WASH-1400 Analyses of Surry
Iodine
Mean
95%
WASH-1400
PWR
(Median)
Median
5%
Note: Shaded area indicates
reduction in median
releases between WASH-1400
and NUREG-1150.
Frequency of Release > R, Per Reactor Year
1.0E-3
1.0E-4
1.0E-5
1.0E-6
1.0E-7
1.0E-8
1.0E-6
1.0E-5
1.0E-4
1.0E-3
1.0E-2
1.0E-1
1
R, Fraction of Core Inventory Released To Environment

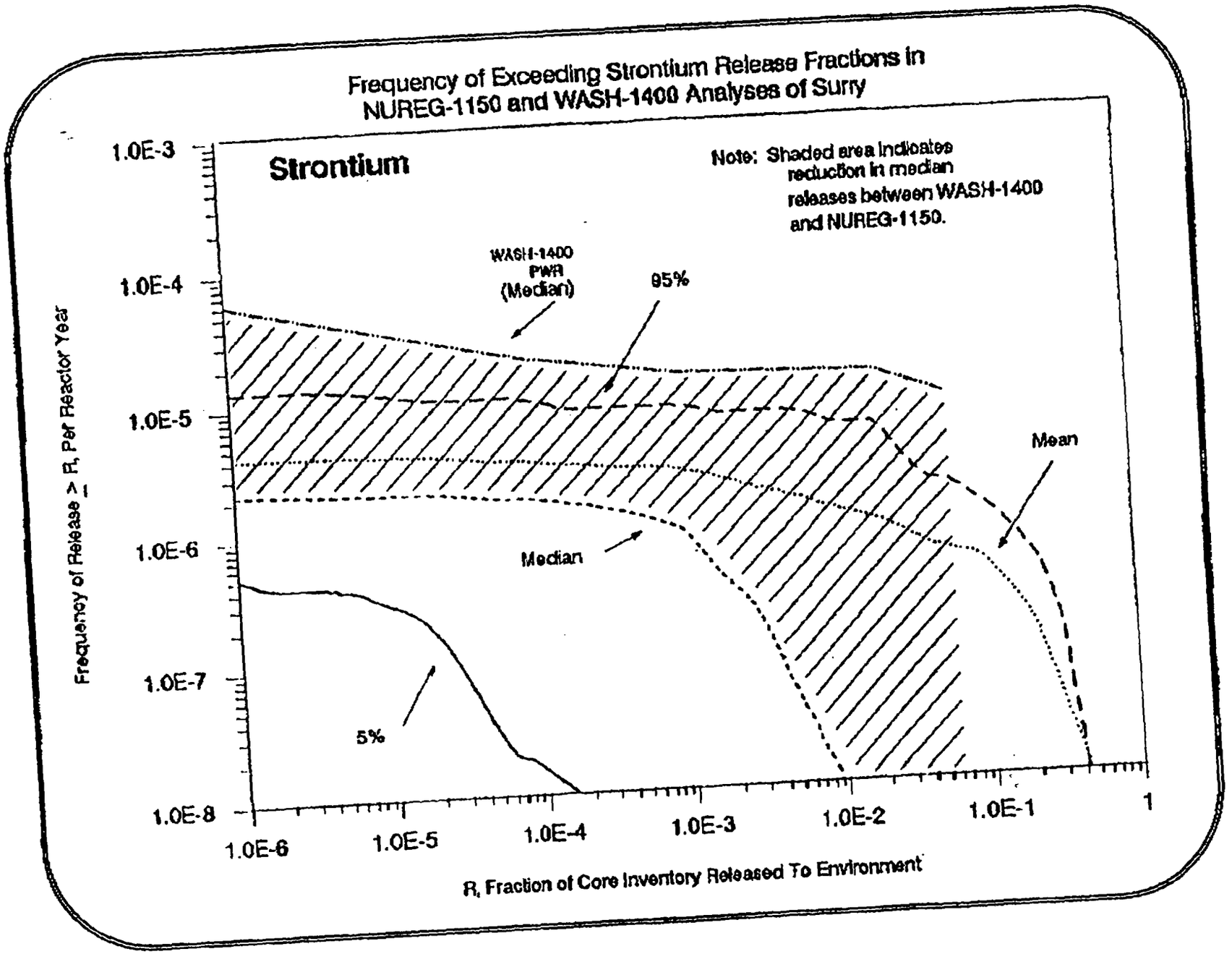
Severe Accident Source Terms: NUREG-1150 Compared to WASH-1400

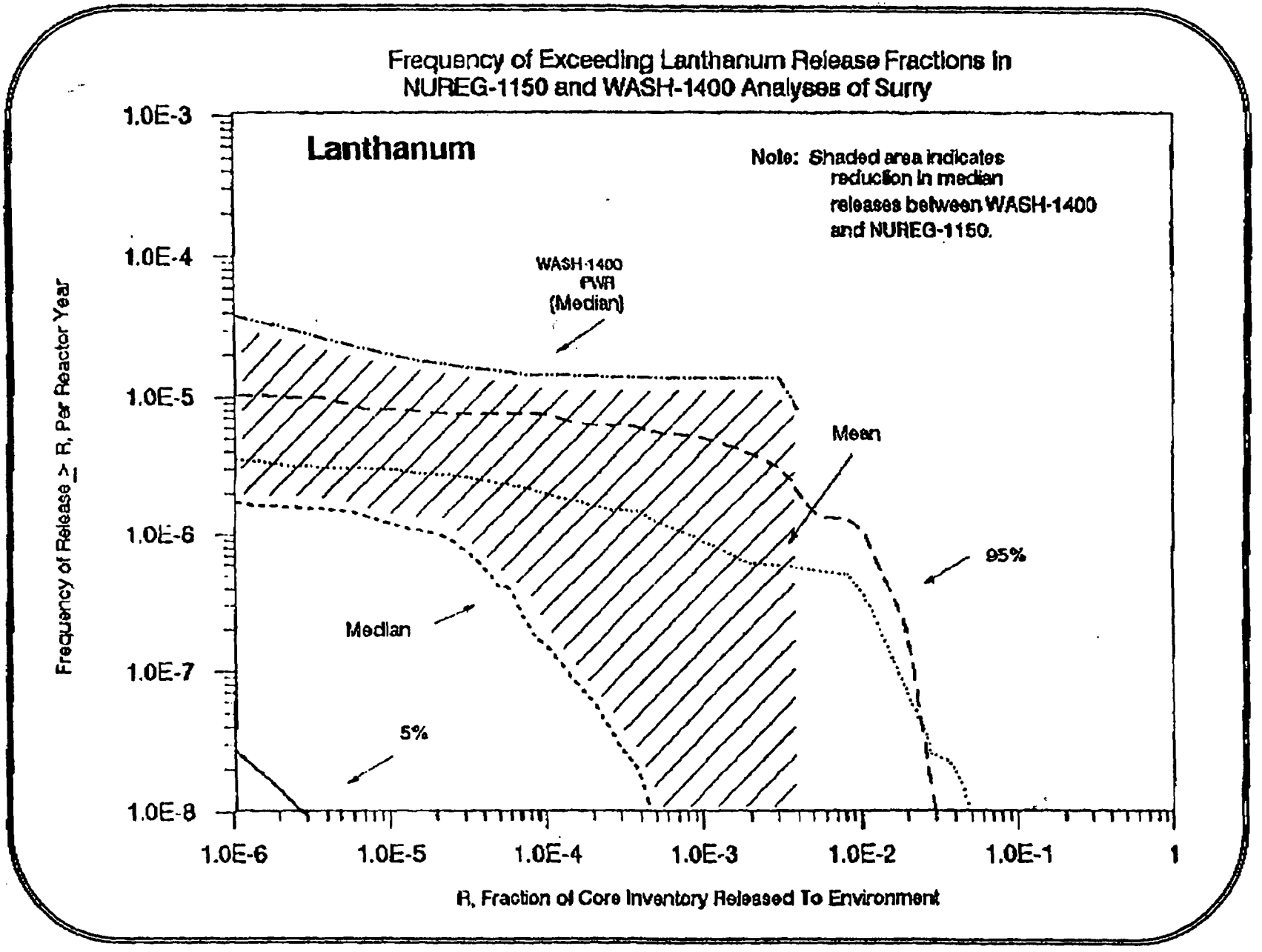
Median probabilities that radionuclide releases from Surry exceed specific fractions of core inventory, given a severe accident:

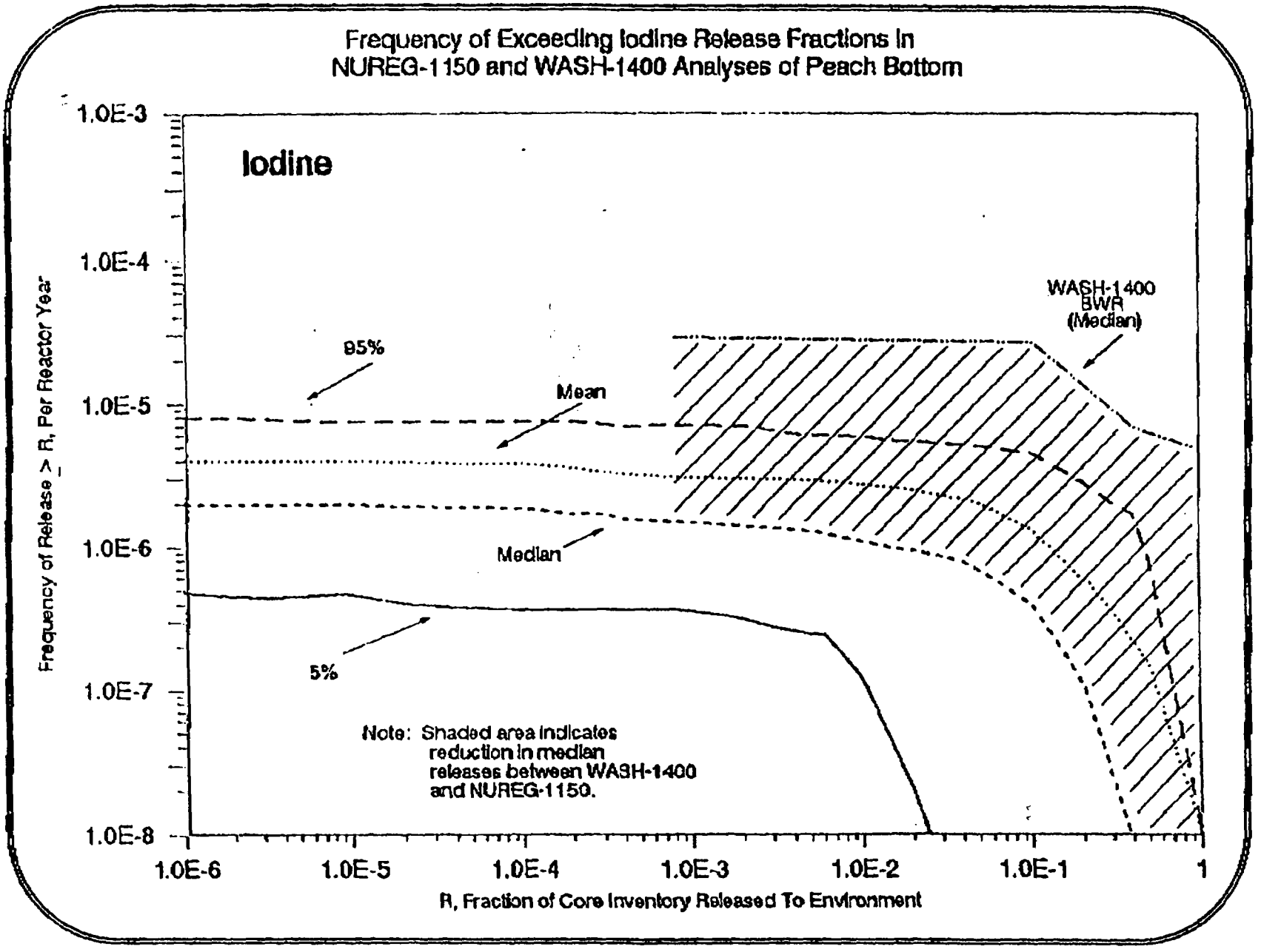
- that 10% or more of iodine or cesium would be released: ~5 times less
- that 1% or more of strontium would be released: ~1000 times less
- that 0.04% or more of lanthanum would be released: ~500 times less

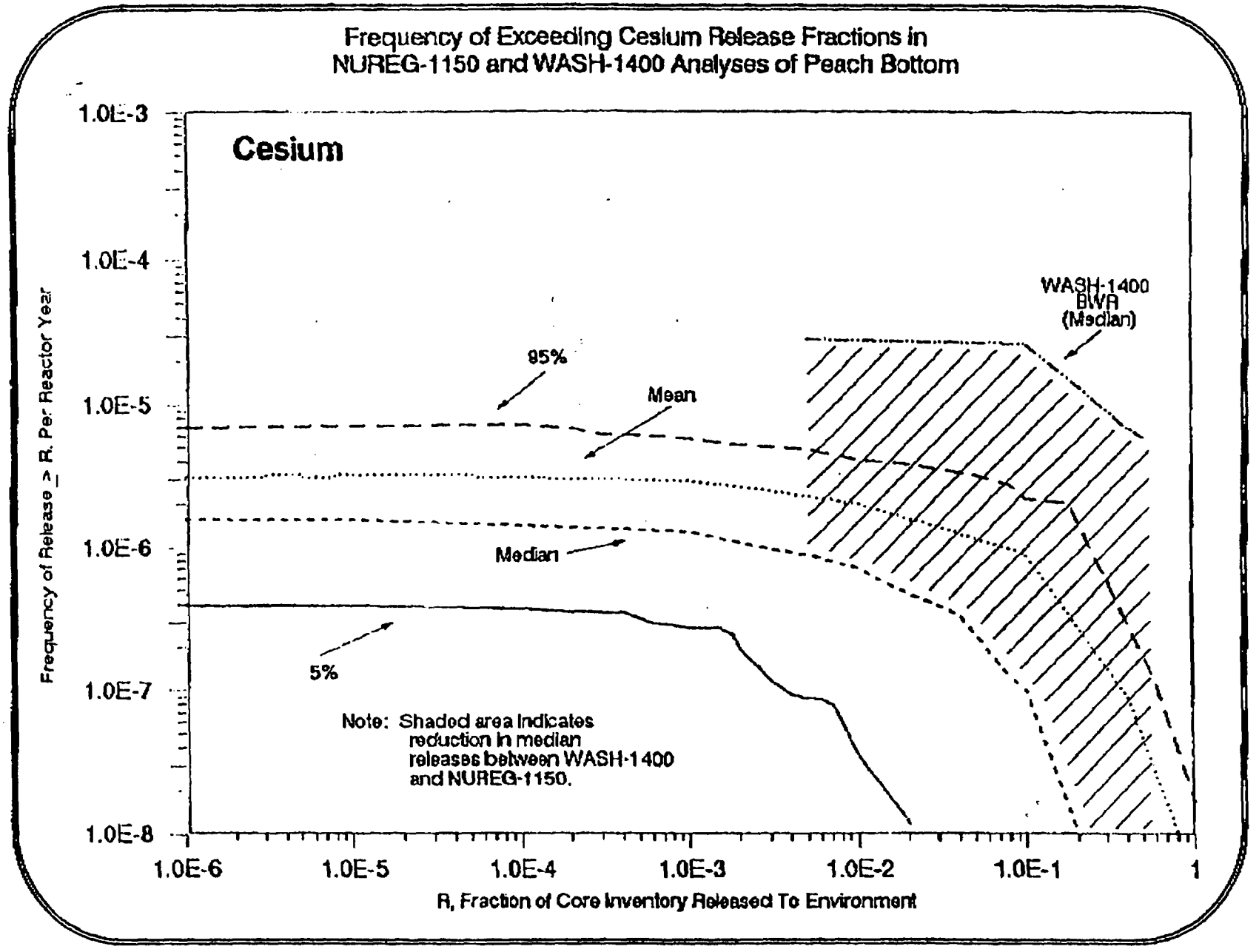

Frequency of Exceeding Iodine Release Fractions in
NUREG-1150 and WASH-1400 Analyses of Surry
Iodine
Mean
95%
WASH-1400
PWR
(Median)
Median
5%
Note: Shaded area indicates
reduction in median
releases between WASH-1400
and NUREG-1150.
Frequency of Release > R, Per Reactor Year
1.0E-3
1.0E-4
1.0E-5
1.0E-6
1.0E-7
1.0E-8
1.0E-6
1.0E-5
1.0E-4
1.0E-3
1.0E-2
1.0E-1
1
R, Fraction of Core Inventory Released To Environment

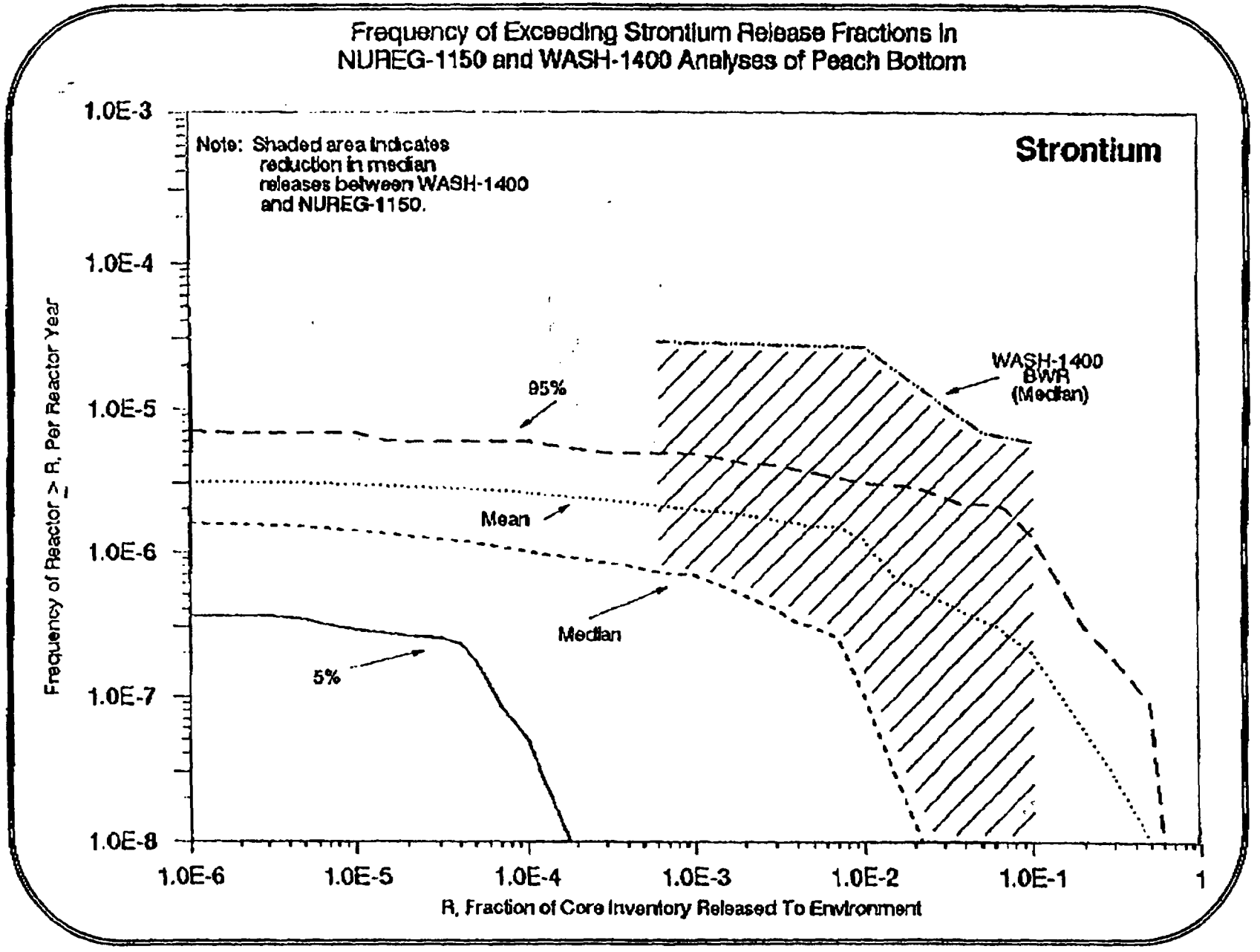

Frequency of Exceeding Ceslum Release Fractions in
NUREG-1150 and WASH-1400 Analyses of Surry
Ceslum
WASH-1400
PWR
(Median)
95%
Mean
Median
5%
Note: Shaded area indicates
reduction in median
releases between WASH-1400
and NUREG-1150.
Frequency of Release > R, Per Reactor Year
1.0E-3
1.0E-4
1.0E-5
1.0E-6
1.0E-7
1.0E-8
1.0E-6
1.0E-5
1.0E-4
1.0E-3
1.0E-2
1.0E-1
1
R, Fraction of Core Inventory Released To Environment

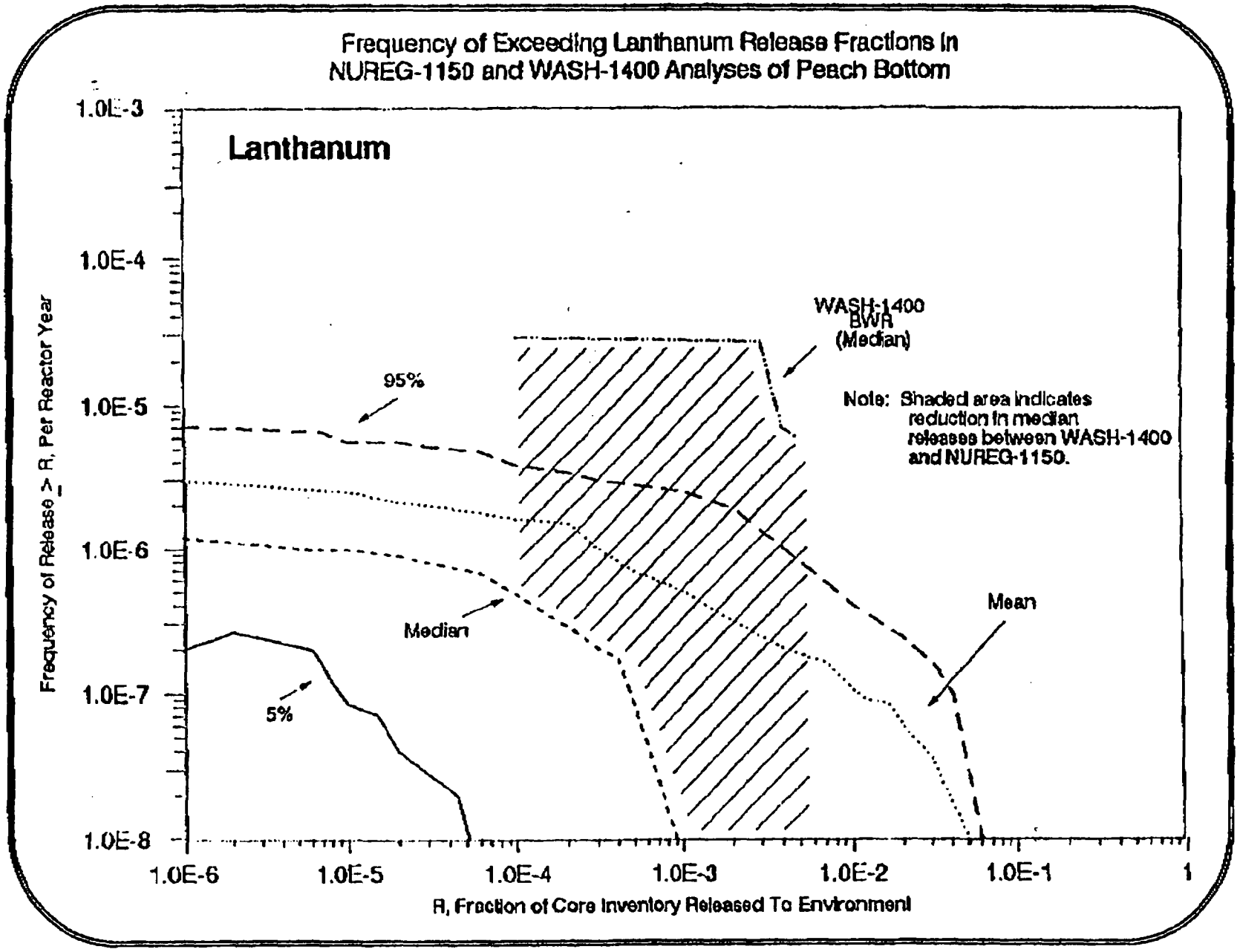

Frequency of Exceeding Strontium Release Fractions in
NUREG-1150 and WASH-1400 Analyses of Surry
Strontium
Note: Shaded area indicates reduction in median releases between WASH-1400 and NUREG-1150.
WASH-1400 PWR (Median)
95%
Mean
Median
5%
1.0E-3
1.0E-4
1.0E-5
1.0E-6
1.0E-7
1.0E-8
Frequency of Release > R, Per Reactor Year
1.0E-6
1.0E-5
1.0E-4
1.0E-3
1.0E-2
1.0E-1
1
R, Fraction of Core Inventory Released To Environment


Frequency of Exceeding Lanthanum Release Fractions in
NUREG-1150 and WASH-1400 Analyses of Surry
Lanthanum
Note: Shaded area indicates reduction in median releases between WASH-1400 and NUREG-1150.
WASH-1400 PWR (Median)
Mean
95%
Median
5%
1.0E-3
1.0E-4
1.0E-5
1.0E-6
1.0E-7
1.0E-8
Frequency of Release ≥ R, Per Reactor Year
1.0E-6
1.0E-5
1.0E-4
1.0E-3
1.0E-2
1.0E-1
1
R, Fraction of Core Inventory Released To Environment


Frequency of Exceeding Iodine Release Fractions In
NUREG-1150 and WASH-1400 Analyses of Peach Bottom
Iodine
1.0E-3
1.0E-4
1.0E-5
1.0E-6
1.0E-7
1.0E-8
Frequency of Release > R, Per Reactor Year
95%
Mean
Median
5%
WASH-1400
BWR
(Median)
Note: Shaded area indicates
reduction in median
releases between WASH-1400
and NUREG-1150.
1.0E-6
1.0E-5
1.0E-4
1.0E-3
1.0E-2
1.0E-1
1
R, Fraction of Core Inventory Released To Environment


Frequency of Exceeding Cesium Release Fractions in
NUREG-1150 and WASH-1400 Analyses of Peach Bottom
Cesium
1.0E-3
1.0E-4
1.0E-5
1.0E-6
1.0E-7
1.0E-8
Frequency of Release ≥ R, Per Reactor Year
WASH-1400
BWR
(Median)
95%
Mean
Median
5%
Note: Shaded area indicates
reduction in median
releases between WASH-1400
and NUREG-1150.
1.0E-6
1.0E-5
1.0E-4
1.0E-3
1.0E-2
1.0E-1
1
R, Fraction of Core Inventory Released To Environment


Frequency of Exceeding Strontium Release Fractions In
NUREG-1150 and WASH-1400 Analyses of Peach Bottom
Note: Shaded area indicates
reduction in median
releases between WASH-1400
and NUREG-1150.
Strontium
WASH-1400
BWR
(Median)
95%
Mean
Median
5%
1.0E-3
1.0E-4
1.0E-5
1.0E-6
1.0E-7
1.0E-8
Frequency of Reactor > R, Per Reactor Year
1.0E-6
1.0E-5
1.0E-4
1.0E-3
1.0E-2
1.0E-1
1
R, Fraction of Core Inventory Released To Environment


Frequency of Exceeding Lanthanum Release Fractions In
NUREG-1150 and WASH-1400 Analyses of Peach Bottom
Lanthanum
1.0E-3
1.0E-4
1.0E-5
1.0E-6
1.0E-7
1.0E-8
Frequency of Release > R, Per Reactor Year
WASH-1400
BWR
(Median)
95%
Note: Shaded area indicates
reduction in median
releases between WASH-1400
and NUREG-1150.
Mean
Median
5%
1.0E-6
1.0E-5
1.0E-4
1.0E-3
1.0E-2
1.0E-1
1
R, Fraction of Core Inventory Released To Environment