OFFICIAL TRANSCRIPT PROCEEDINGS BEFORE

NUCLEAR REGULATORY COMMISSION

BEFORE THE ATOMIC SAFETY AND LICENSING BOARD

DKT/CASE NO. 50-247 SP and 50-286 SP

TITLE CONSOLIDATED EDISON COMPANY OF NEW YORK (Indian Point Unit 2) - POWER AUTHORITY OF THE STATE OF NEW YORK (Indian Point Unit 3)

PLACE White Plains, New York

DATE February 9, 1983

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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

BEFORE THE ATOMIC SAFETY AND LICENSING BOARD

- -x

In the Matter of:	:	Docket Nos.:
CONSOLIDATED EDISON COMPANY OF NEW YORK (Indian Point Unit 2)	:	50-247 SP
POWER AUTHORITY OF THE STATE OF NEW YORK (Indian Point Unit 3)	:	50-286 SP

- -x

Ceremonial Courtroom
Westchester County
Courthouse
111 Grove Street
White Plains, N.Y.

Wednesday, February 9, 1983

The hearing in the above-entitled matter convened, pursuant to notice, at 9:00 a.m.

BEFORE:

JAMES GLEASON, Chairman
Administrative Judge

OSCAR H. PARIS
Administrative Judge

FREDERICK J. SHON
Administrative Judge

APPEARANCES:

On behalf of Licensee, Consolidated Edison Company of New York:

BRENT L. BRANDENBURG, Esq.
Assistant General Counsel
STEVEN SOHINKI, Esq.
Consolidated Edison Company of New York, Inc.
4 Irving Place
New York, N.Y. 10003

On behalf of Licensee, the Power Authority of the State of New York:

JOSEPH J. LEVIN, Esq.
PAUL F. COLARULLI, Esq.
Morgan Associates, Chartered
1899 L Street, N.W.
Washington, D.C. 20036

On behalf of the Nuclear Regulatory Commission Staff:

JANICE MOORE, Esq.
HENRY J. McGURREN, Esq.
Washington, D.C.

On behalf of the Intervenors:

Council of the City of New York:

CRAIG KAPLAN, Esq.

Friends of the Earth, Inc., and New York City Audobon Society:

RICHARD HARTZMAN, Esq.

New York Public Interest Research Group:

JOAN HOLT, Esq.

APPEARANCES: (Continued)

Union of Concerned Scientists:

JEFFREY M. BLUM, Esq.
STEVEN SHOLLY, Esq.

West Branch Conservation Association:

ZIPPORAH S. FLEISHER
443 Buena Vista Road
New City, N.Y. 10956

C O N T E N T S

C O N T E N T S (Cont'd)

E X H I B I T S

P R O C E E D I N G S

JUDGE GLEASON: Shall we proceed, please.

Mrs. Moore, do you want to call your witnesses.

MS. MOORE: The Staff calls Roger Blond and Frank Rowsome to the stand.

(Witnesses sworn.)

Whereupon,

ROGER BLOND

and FRANK ROWSOME,

called as witnesses by counsel for the Regulatory Staff, having first been duly sworn by the Chairman, were examined and testified as follows:

DIRECT EXAMINATION ON BEHALF

OF THE REGULATORY STAFF

BY MS. MOORE:

Q Mr. Rowsome, would you please give your name and business address.

A (WITNESS ROWSOME) Frank H. Rowsome, III, Nuclear Regulatory Commission, Washington, D.C.

Q Okay. Mr. Blond, would you state your name and address, please.

A (WITNESS BLOND) Roger M. Blond, U.S. Nuclear Regulatory Commission, Washington, D.C.

Q Mr. Rowsome, what is your position with the

NRC?

A (WITNESS ROWSOME) Since the last time I was here testifying, my assignment with the NRC has changed. I am now the Assistant Director for Technology in the Division of Safety Technology in the Office of Nuclear Reactor Regulation.

Q Mr. Blond, what is your position with the NRC?

A (WITNESS BLOND) I am the Section Leader in the Division of Risk Analysis of the Office of Research in the U.S. Nuclear Regulatory Commission.

Q Gentlemen, do you have before you a document entitled "Direct Testimony of Frank Rowsome and Roger Blond Concerning Commission Question No. 1"?

A (WITNESS ROWSOME) Yes, we do.

Q Was this testimony prepared by you or did you participate in its preparation?

A (WITNESS ROWSOME) Yes.

A (WITNESS BLOND) Yes.

Q Do you have any additions or corrections to this testimony?

A (WITNESS ROWSOME) Yes. There's an errata sheet dated February 7, 1983.

Q Does that errata sheet contain all the additions and corrections to your testimony?

A (WITNESS ROWSOME) Yes, it does.
Q With these changes to your testimony, is the testimony true and correct to the best of your knowledge, information and belief?
A (WITNESS ROWSOME) Yes, it is.
A (WITNESS BLOND) Yes, it is.
Q Do you adopt this testimony as your testimony in this proceeding?
A (WITNESS ROWSOME) Yes.
A (WITNESS BLOND) Yes.
MS. MOORE: Copies of this testimony have been delivered to the Board, to the parties and to the court reporter. I now ask that the testimony, the errata sheet, and the attached professional qualifications be received into evidence and bound into the record as though read.
JUDGE GLEASON: Is there objection?
(No response.)
JUDGE GLEASON: Hearing none, the testimony, the errata sheet and the professional background of the witnesses will be received into evidence and bound into the record as if read.
(The documents referred to, the testimony, errata sheet and the professional qualifications of Messrs. Rowsome and Blond, received in evidence, follow:)

UNITED STATES OF AMERICA
NUCLEAR REGULATORY COMMISSION

BEFORE THE ATOMIC SAFETY AND LICENSING BOARD

In the Matter of	)	
	)	
CONSOLIDATED EDISON COMPANY OF NEW YORK (Indian Point, Unit No. 2)	)	Docket No. 50-247-SP 50-286-SP
	)	
POWER AUTHORITY OF THE STATE OF NEW YORK (Indian Point, Unit No. 3)	)	

DIRECT TESTIMONY OF FRANK ROWSOME AND ROGER BLOND
CONCERNING COMMISSION QUESTION 1

Q.1 Mr. Rowsome, please state your name and business address for the record.

A.2 My name is Frank H. Rowsome, my business mailing address is U.S. Nuclear Regulatory Commission, Washington, DC 20555.

Q.2 Please identify your position with the NRC and describe your responsibilities in that position.

A.2 I am Deputy Director of the Division of Risk Analysis within the Office of Nuclear Regulatory Research. I assist the Director in planning and managing the research program in risk assessment, probabilistic safety analysis, operatons research, reliability engineering, and related regulatory standards development.

Q.3 Have you prepared a statement of your professional qualifications?

A.3 Yes, the statement of my professional qualifications is attached to this testimony.

Q.4 Mr. Blond, please state your name and business address for the record.

A.4 My name is Roger M. Blond. My business address is the U.S. Nuclear Regulatory Commission, Washington, D.C. 20555.

Q.5 Please describe your position with the NRC and describe your responsibilities in that position.

A.5 I am the Section Leader for the Accident Risk Section of the Reactor Risk Branch of the Division of Risk Analysis of the Office of Research. In that position, I am responsible for providing technical and managerial direction in developing methods and research in accident risk analysis and in performing applications in risk assessment.

Q.6 Have you prepared a statement of your professional qualifications?

A.6 Yes, a the statement of my professional qualifications is attached to this testimony.

Q.7 What is the purpose of this testimony?

A.7 The purpose of this testimony is to introduce the NRC Staff testimony bearing upon Commission Question One concerning the risk arising from possible accidents at Indian Point Units 2 and 3. We shall outline the risk-related testimony, provide background information about the role of risk assessment techniques, and review the status of NRC Staff studies of risks posed by potential accidents at Indian Point Units 2 and 3.

Q.8 How is the whole of the testimony on risk organized?

A.8 The testimony is organized according to the following outline:

I. INTRODUCTION TO TESTIMONY ON THE RISK-RELATED QUESTIONS

II. Unused heading.*

III. STAFF EVALUATION OF RISK POSED BY INDIAN POINT UNITS 2 AND 3.

A. Accident likelihood

B. Radiological Releases

C. Accident Consequences

IV. SUMMARY RESPONSE TO THE COMMISSION QUESTIONS ON RISK

A. Staff answers to Commission questions on risk

What risks are posed by potential accidents at Indian Point Units 2 and 3 as they were when the IPPSS was done and as they will be in 1983?

B. How will the risk change with --

improvements in Emergency Preparedness?

C. Accuracy of the Staff Risk Predictions .

V. CONTENTIONS AND BOARD QUESTIONS

* Note that II is an unused heading. (Material prepared for Section II has been merged with I for clarity and continuity)

Q.9 How has the staff interpreted the phrase, "serious accidents... including accidents not considered in the plant's design basis", which appears in Commission Question 1?

A.9 The staff has taken the phrase "serious accident" to be synonymous with severe core damage and core melt accidents. Concerns with the safety of the plant center upon the highly radioactive fission products that accumulate in the reactor fuel as the byproduct of the energy-releasing nuclear reaction. The core of an operating nuclear power plant contains radioactive materials which, if ineffectively contained, can cause substantial harm to workers, and to the population and environment in the vicinity of a plant. Even after an operating reactor is shut down, a mechanism for releasing radioactivity exists. This is called the "afterheat" or "decay heat" produced in the fuel after the nuclear chain reaction ceases. This afterheat diminishes gradually once a nuclear reactor is shut down, but within the first hours or days after shutdown, the afterheat released within the fuel has the potential to melt the fuel and breach each of the several barriers used to obstruct the release of radioactive materials. Such a phenomenon may take place if the afterheat in the fuel is not dissipated in controlled ways. Because a "core melt" accident has the potential to release large quantities of radioactivity, it is the principal cause for concern among potential nuclear reactor accidents.

There is a spectrum of accidents involving the reactor that stop short of severe core damage. The design basis accidents are among these.* Some of these accidents entail releases of very small amounts of radioactivity, but the releases are far too small to pose a risk to public health and safety, principally because too little of the fission products escape from the fuel to give rise to appreciable doses or contamination offsite. Some of these accidents can do costly damage to plant equipment, but the risk posed by offsite radiological effects is negligible for these accidents which stop short of severe fuel damage. All of the accidents at domestic light water reactors (and at foreign light water reactors of domestic design) that have occurred have been of this very much less type, except for the accident at Three Mile Island.

There are several places in a nuclear plant where radioactive materials are stored in addition to the reactor core. These include the spent fuel storage facilities and the radioactive waste handling and treatment facilities. Studies such as the "Reactor Safety Study" (WASH-1400) and have shown unambiguously that both the risk posed by accidents and the potential hazards from these materials should an accident occur are far lower than for core melt accidents. Thus we can confine our attention to the spectrum of accidents involving severe core damage or meltdown.

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*For purposes of regulatory accident analysis and siting considerations. This is a non-mechanistic assumption. Realistic risk analysis, on the other hand, postulates core damage only in scenarios entailing inadequate core cooling. The design basis accidents, mechanistically analyzed, do not entail severe core damage.

Q.10 What is meant by "risk" in the context of nuclear reactor accidents?

A.10 The technical meaning of risk, as used by the NRC staff, is very much like the definition in common English usage. Risk is a measure of danger that is proportional to both likelihood of accidents and the severity of the consequences. That is, risk is likelihood multiplied by consequences. We speak of distinct measures of risk for each type of undesirable consequence: early death, early injury, delayed (latent) cancer, genetic effects, or property damage.

Mathematically, risk may be portrayed as a graph that displays the severity of the outcome of an accident, e.g., number of casualties, versus the likelihood or frequency of an outcome of at least that severity or greater severity. These graphs are often called "CCDFs," a shorthand term for their formal statistical name: Complementary Cumulative Distribution Functions. A simpler mathematical measure of risk is a single number: the expected value of risk. This is an annual average consequence of accidents. For example, an accident that causes one thousand casualties and occurs once in a thousand years has an expected risk of one casualty per year. So, too, does an accident that occurs once a year and causes one casualty. Thus, the expected value of risk, though simpler than a risk graph like a CCDF, carries less information.

Q.11 What is risk assessment?

A.11 Risk assessment is the discipline of constructing mathematical models that estimate risk. In the context of nuclear reactor accidents, risk assessment entails three principal stages of analysis. In the first stage, accident sequences are identified and the likelihood of occurrence of each sequence is estimated. In the second stage, estimates are made of the damage to the reactor fuel, the success or failure of the containment systems, and the quantity of radioactive materials released to the air and/or groundwater. In the third stage, estimates are made of the consequences beyond the site boundary from the radioactivity released.

Q.12 How are possible accident sequences identified and estimates obtained of the likelihood of each?

A.12 The process of identifying the variety of reactor accidents involving the reactor core begins with catalogs of initiating events, which are disturbances in reactor operation that have the potential to lead to releases of radiation were it not for the intervention or presence of safety systems. Next, the accident sequences are classified according to whether or not systems that can affect the course of the accident operate successfully. Event trees are used in both the qualitative cataloging of accident sequences and in the evaluation of their likelihood. Generally, there is one event tree for each distinct class of initiating event. The event trees display branch points which depict alternative accident sequences. A branch is shown for each system or function whose success or failure leads the incident down a different path.

Thus each route through an event tree depicts a distinct accident sequence.

Next, models of system reliability are constructed. This is commonly done with "fault trees" that trace the origin of system failures to discrete component failures or human errors. A completed fault tree is nothing more than a gigantic sentence in formal logic which can be read: "The system will fail if A happens or if B happens or if C and D happen together or if...," and so forth. These trees catalog the variety of ways a system or group of systems can be disabled. These trees can also be translated into mathematical models which calculate the probability of system failure as a function of the probabilities of the many potentially contributing component failures or human errors.

Finally, the catalogs of initiating events, event trees, and fault trees, are assembled into a mathematical model that permits accident sequence likelihoods to be calculated from initiating event frequencies, component failure rates, and human error rates.

For more detailed information about accident sequences and their probabilities, see Section III.A.

Q.13 What does the containment analysis portion of a reactor risk assessment entail?

A7. The objective of this phase of the risk assessment is to calculate for each accident sequence the timing, quantity, and form of radioactive materials released from the plant. This is done with conventional deterministic (rather than probabilistic) computer-aided calculations of the physical and chemical processes taking place during the evolution of the accident. An analysis is made of the status of core cooling to identify if and when the core overheats. If the analysis predicts core damage or meltdown, then one proceeds to calculate the timing, form and quantity of radioactive material released from the fuel. The slumping of the fuel within the reactor vessel and the attack of the molten core upon the reactor vessel are also analyzed to explore the timing and physical phenomena associated with vessel failure. These analyses generate predictions about releases of steam, energy, and fission products, and about the possibility of missiles from the more violent vessel failure modes. This information is treated as input data to an analysis of the phenomena taking place within the containment building. Further analyses calculate the pressures, temperatures, and composition of the atmosphere within the containment building as the accident evolves. These analyses consider whether or not the containment building remains isolated, whether or not coolers or water sprays are operating within the containment building, whether or not the molten fuel reacts violently with water in the bottom of the reactor cavity, whether or not the molten fuel is cooled there or attacks the concrete floor of the containment building, and so forth. The endpoint of these calculations is an evaluation of whether or not the containment building fails, how it fails, when it fails, and what materials and energy escape from it to the biosphere.

For more detailed information on containment analysis, see Section III.B.

Q.14 What does the consequence analysis portion of a reactor risk assessment entail?

A.14 The objective of the consequence analysis portion of a reactor risk assessment is to predict the likelihood and extent of early deaths and injuries, delayed deaths and injuries, and offsite property damage caused by reactor accidents. These predictions are made by first calculating the dispersion of radioactive materials released to the open air or subsoil. These calculations are essentially deterministic, although a probabilistic treatment is given to the variety of weather conditions at a site which might prevail at the time of releases. They result in estimates of the likelihood and severity of radioactive exposure at locations outside the reactor site via the air, air-to-ground, or liquid (groundwater) pathways. These calculations result in predictions of the doses of radiation to which members of the population might be exposed at various locations over time. Since these models predict the timing as well as the location of potential doses, a mathmetical model of emergency response can be and generally is employed that accounts for removal of people, either before the radioactive plume arrives or after it passes.

Dose-response relationships are employed to translate calculated doses into effects such as death, radiation illness, cancer, or genetic effects. Assessments of property damage are calculated in terms of the losses sustained in relocating people from contaminated areas, of the interdiction of contaminated foodstuffs, and of post-accident cleanup outside the perimeter of the licensee's property.

Risk assessments do not normally count the losses suffered by the plant owner in the property damage assessments. These losses typically would include lost capital investment in the facility, replacement power, and onsite cleanup.

It is worth noting, however, that these losses by the plant owner are predictably much larger than those suffered by the public due to radiation releases for all but the most extremely severe of potential reactor accidents.

For more detailed information on consequence analysis, see Section III.C.

Q.15 What are the principal strengths and weaknesses of reactor risk assessment?

A.15 Reactor risk assessments derive most of their strengths and weaknesses from the attempt to give a comprehensive, balanced, realistic model for predicting reactor risks. Probabilistic risk assessment (PRA) is the only known form of reactor safety analysis that can treat multiple component failures, system interactions, and human error in an integrated way. PRA is also unique in providing a comprehensive context for each aspect of reactor accident susceptibility.

The attempt to be comprehensive, however, is bought at a considerable price. The rich variety of root causes, accident sequences, and natural phenomena can only be modeled in a highly approximate way. PRAs are uniquely valuable for their ability to model the integration of very complex phenomena, but the many approximations make PRAs imprecise.

PRAs are constructed on a coherent, logical framework on which is stretched a fabric of numerous, often-simplistic approximations. There are some holes in the fabric, as well. For example, we do not know how to predict the likelihood of sabotage attempts. We have not yet mastered the art of including the contributions to reactor accident susceptibility made by those design errors that are not revealed by either design documents, surveillance tests, or reactor operations. We are not very good at predicting the likelihood that operators might misdiagnose an incident, and so employ the wrong procedures.

Such limitations make PRAs rather unreliable at predicting the precise magnitude of risk. They are, however, very successful at identifying many--if not all--of the ways a reactor may be vulnerable enough to severe accidents to warrant shutdown or remedial action. They are also extremely valuable as a method with which to estimate the importance of safety issues. A large number of inferences can be drawn from PRAs on ways to improve reactor safety. PRAs also provide a tool with which to evaluate reactor safety issues originating elsewhere even though the issue may not have been modeled in the PRA. PRAs provide an objective framework for putting reactor safety issues in context.

Q.16 What is the history associated with the Commission's investigation into the risk at Indian Point?

A.16 In the spring of 1980 the Nuclear Regulatory Commission was evaluating the actions of the Director of the Office of Nuclear Reactor Regulation with respect to continued operation of the Indian Point Unit 2 and Unit 3 nuclear power plants. As part of that evaluation the Commission asked for the formation of a Task Force of its own staff, with help from others as needed to develop information on the relative risk of the Indian Point power plants and other matters related to consideration of shutting these plants down. The probabilistic Analysis Staff of the Office of Nuclear Regulatory Research was asked to participate in that Task Force for technical work in risk assessment. The Task Force was formed and instructed by Commission Order on May 30, 1980 to provide insight into the risk posed by the Indian Point power plants compared with the other power plants that were licensed for operation. The Task

Force made its report to the Commission by written report SECY-80-283 on June 12, 1980. This Task Force Report was later published as NUREG-0715 in August 1980. The report NUREG-0715 is adopted as part of this testimony with respect to those portions which deal with accident risks. Some further analyses, not described in SECY-80-283 or in NUREG-0715, were completed after SECY-80-283 and presented to the Commission in oral briefing on June 26, 1980.

Q.17 What occurred after the release of the Reactor Safety Study?

A.17 In the several years following WASH-1400 there was much debate about the probabilistic risk assessment (PRA) methodology that was developed. The debate culminated in the publication in September 1978 of NUREG/CR-0400, the Risk Assessment Review Group Report (Lewis Committee Report). This report was the principal basis for a Commission statement on January 18, 1979, on the use of PRA. Contrary to widespread belief, neither the Lewis Committee report nor the Commission statement disavowed the Reactor Safety Study or the use of PRA. What was disavowed was the short Executive Summary of WASH-1400, which was judged to be an inadequate representation of the Reactor Safety Study. Both the Lewis Committee Report and the Commission statement encouraged careful use of probabilistic risk assessment, especially for setting priorities for regulatory attention. The Three Mile Island (TMI) accident, which came just over a month after the Commission statement, was a graphic example of the need for substantial changes in regulatory emphasis. Probabilistic risk assessment offers a more rational alternative to understanding the safety significance of a reactor design and site. Since the TMI accident there has been increasing use of PRA in regulatory activities, typical of this was the Commission's specific call for risk analysis in the Indian Point case.

Q.18 What reactors have been analyzed using PRA methods?

A.18 The growing use of PRA since the TMI accident has led to the conduct of many risk studies, which have often interacted with one another even before publication of their results. This interaction is especially important in the case of the Task Force study of Indian Point risk.

The Reactor Safety Study evaluated two reactors as surrogates for the first 100 U.S. power reactors, a Westinghouse 3-loop pressurized water reactor (PWR), Surry, with subatmospheric large dry containment; and a General Electric boiling water reactor (BWR), Peach Bottom, with a Mark I pressure suppression containment. Because of the diversity of reactor design, the NRC initiated the Reactor Safety Study Methodology Applications Program (RSSMAP) to study other reactor types with essentially the same methodology. Four plants were selected: a 4-loop Westinghouse PWR, Sequoyah-1, with an ice-condenser containment; a 2-loop Babcock & Wilcox PWR, Oconee-3, with a large dry containment; a 2-loop Combustion Engineering PWR, Calvert Cliffs-1, with a large dry containment; and a General Electric BWR, Grand Gulf-1, with a Mark III pressure suppression containment. The resources available did not permit analysis of these plants to the same extent as was done for the Reactor Safety Study so the basic event trees and fault trees of the Safety Study were adapted to these plants based on information obtained from the Final Safety Analysis Report and from some plant visits. Much of the work was concentrated on the analysis of the different containment types. The partial results of the RSSMAP studies which were available in May 1980 played an important part in the Task Force's Indian Point risk assessment. Three of the four RSSMAP reports[1/] have now been published and the fourth[2/] is in final review.

Still another plant risk assessment program has been started by the NRC, the Interim Reliability Evaluation Program (IREP). The first phase of IREP was the study of one plant, a Babcock & Wilcox PWR, Crystal River-3, with a large dry containment. The draft report on Crystal River-3 was available in May 1980 and provided some additional understanding of the important accident conditions for the Indian Point study. The Crystal River-3 IREP report has since been

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1/ NUREG/CR-1659, Reactor Safety Study Methodology Applications Program, Part 1 of 4, Sequoyah #1 PWR Power Plant; Part 2 of 4, Oconee #3 PWR Power Plant; Part 4 of 4, Grand Gulf #1 BWR Power Plant.

2/ NUREG/CR-2515, Crystal River-3 Safety Study, Vols. 1 and 2, January 1982.

published.[3/] The IREP is now in its secon⁴ phase with a standardized methodology guide and the trial study of four plants nearly complete.[4/] One of the Indian Point plants was to have been included in IREP but the owners of both elected to do a separate, more comprehensive risk study, since IREP focuses principally on systems analysis and the calculation of core melt[5/] probability.

Q.19 What was the Indian Point Short Term Risk Study?

A.19 The purpose of this study was to perform a short-term risk evaluation of the Indian Point 2 and 3 plants. This evaluation was to be used in developing a risk perspective for this high population density site and to identify improvements in design or operation which in the interim have the potential for risk reduction while a more thorough evaluation by the NRC staff and licensee were performed. The short turnaround time allowed for this effort required the use of a simplified approach. As such, the probabilistic risk

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3 The four IREP plants are Browns Ferry-1, Calvery Cliffs-1, Millstone-1 and Arkansas-1. Two have been published.

4/ It should be noted that the analysts who performed the Reactor Safety Study defined core melt as the failure to deliver prescribed core cooling. As the TMI accident demonstrated, degarded core colling can exist for hours without full-scale core melt. Nevertheless, most PRA analysts do not attempt to distinguish severe core damage from core melt and follow the Reactor Safety Study practice of treating degraded core cooling as synonymous with core melt.

5/ The Staff has performed a comparison of the benefits of new mitigation systems in NUREG-0850 and the Licensee has performed a detailed PRA.

estimates obta:.:d are subject to considerable uncertainty, including possible error or inaccuracies. However, the risk perspectives obtained, when combined with good engineering judgment, represent a useful guide to identifying relative risks and potential risk reduction measures.

Q.20 What was the technical approach taken in the study?

A.20 The technical approach taken in this study was to use event tree methodology combined with insights on dominant accident sequences obtained from WASH-1400, RSSMAP, and IREP programs to identify and probabilistically quantify the accident sequences for the Indian Point 2 and 3 designs. These previous risk studies have shown that a handful of accident scenarios would most likely define and dominate a reasonably complete set of core-melt scenarios for a PWR design. Against this experience the Indian Point designs were briefly reviewed. Particular attention was given to identifying common interactions which could affect more than one vital system or could be caused by a single initiating event. The designs were also surveyed for single point vulnerabilities in systems or potential human errors which might significantly influence the likelihood of accident sequences. This information was used in the development of event trees to identify the accident sequences appropriate to the Indian Point plants.

Insights from the previous risk studies and accident sequence analogies were also used in the development of containment failure mode probabilities and fission product releases. During the course of this brief review, no risk-significant differences between Indian Point 2 and 3 were identified.

Q.21 What specific event trees were developed in the study?

A.21 Event trees are developed from specific accident initiating conditions. Loss of coolant accidents (LOCA) and transient events (e.g., loss of offsite power) represent the two dominant types of initiating events considered in the Indian Point study. Four event trees were constructed to define the set of accident sequences which could result from significantly different initiating events. Considering the Indian Point design, the LOCA initiators were divided into three event trees, namely: (1) large and intermediate size pipe breaks greater than 2" diameter; (2) small pipe breaks less than 2" diameter; and (3) high to low pressure system interface ruptures (Event V). Transient events analyzed in the study included failure to "scram" and those events which would cause interruption of main feedwater (including loss of offsite power).

Q.22 What was the core melt probability estimate?

A.22 A rough estimate of the overall core-melt probability at Indian Point was made for the plant prior to changes ordered by the Director, NRR, in February 1980. The estimated probability, which has been corrected for minor arithmetic error, is about $4x10^{-5}$ per year. Due to the February 1980 orders, several improvements to plant operation were identified and a reevaluation of the core melt probability was made assuming these improvements were incorporated. The revised core melt probability was estimated to be about $1x10^{-5}$ per year.

Q.23 What are the limitations associated with the approach that was taken?

A.23 This short-term risk study of the Indian Point plants has notable limitations because of the methodology used. This study is incomplete; in effect it only looked for dominant risk where dominant risk had been found before in previous assessments of PWRs. If the Indian Point plants suffer from some unique vulnerability which has not been identified before, then this study would not discover it. Thus, this study has a bias which would underestimate the risk of the Indian Point plants.

Accident sequences can also be initiated by external events such as earthquakes, fires, and nearby explosive or toxic chemical hazards. Such accident sequences would be similar in nature to the LOCA and transient sequences; however, it is much more difficult to develop quantitative estimates for the probability of occurrence of external events. It should be noted that the Indian Point short-term risk study analyzed only the risk of internal events. It did not include treatment of external common cause events such as earthquakes and fires because such events were not treated well in WASH-1400 and not treated at all in the RSSMAP and IREP studies. Probabilistic risk analysis of external common cause events is not yet as well developed as PRA for internal events. Attempts to quantify external events made in previous risk assessment (e.g., WASH-1400, Zion Probabilistic Safety Study) have shown the annual probability of occurrence for such events to be low (i.e., 10-5 - 10-7) in comparison to the probability estimates that have been made for accident sequences associated with LOCAs and transients (i.e., 10-3 - 10-4).

In addition to the above limitations, it must be emphasized that the short-term study of the Indian Point plants took about two man-months of effort for the entire analysis. For comparison, the Reactor Safety Study Methodology Application Program took about one to three man-years of effort per plant and the Interim Reliability Evaluation Program took about eight to ten man-years of effort per plant. The Indian Point Probabilistic Safety Study, IPPSS, has been estimated to have taken 50 man-years of effort. As will be attested to in forthcoming testimony, the IPPSS represents a significant improvement in our level of understanding about the plants' design and operation. Included in the analysis is a comprehensive study of both Units 2 and 3 and a state-of-the art external hazards common cause analysis which harbored the dominant contributors to the risk at the plants.

Q.24 Please provide a brief introduction to the work done by the staff in the last two years to update the analysis of the risks posed by accidents at Indian Point Units 2 and 3 that was presented to the Commission in the summer of 1980 and published as NUREG-0715.

A.24 In the spring of 1980, Harold Denton, Director of the Office of Nuclear Reactor Regulation, charged the staff with preparing an analysis of the desirability of backfits to the Indian Point and Zion plants. The basis for this action was the evidence that these plants may pose a disproportionate share of the societal risks compared with other commercial nuclear power plants by virtue of the comparatively high population density surrounding these plants. The objective of these studies was to determine if retrofits to these plants were warranted to improve the capability of the plants to mitigate the consequences of core melt accidents, in order to reduce the risk so that these plants no longer pose a disproportionate share of the risk - if, in fact, they do.

Thus, the staff embarked on a project to evaluate the effectiveness and reliability with which the containment systems at Indian Point and Zion could bottle up severe reactor accidents, and to evaluate the risk reduction potential associated with a number of hypothetical retrofits including:

(1) filtered, vented containment systems,
(2) combustible gas control systems, and
(3) core retention devices.

The original product envisioned for these studies was a report or series of reports laying the technical groundwork for a regulatory decision on retrofitting the plants.

While these studies were getting under way, the Commission chartered a task force which developed perspectives on the risk posed by the possibility of accidents (the results were published as NUREG-0715) and issued the Memorandum and Order of January 8, 1981 establishing this hearing.

These ongoing studies of accident mitigation were continued with an enlarged scope: they were to constitute the technical basis with which to provide the staff's answer to the Commission questions on risk posed to this board, as well as meet the original goal of evaluating mitigation concepts.

The principal thrust of these studies remained on the mitigation retrofits until the fall of 1981, and culminated in the publication of 'JREG-0850, Vol. 1 "Preliminary Assessment of Core Melt Accidents at the Zion and Indian Point Nuclear Power Plants and Strategies for Mitigating Their Effects," November 1981.

Since that time, the studies have been refined, improved, and expanded to address the questions on risk before the board.

These staff studies have focused upon two of the three principal phases of a reactor risk assessment: the containment analysis and the consequence analysis. The staff has developed a position on these two aspects of the risk that is fully independent of the Indian Point Probabilis ic Safety Study submitted by the licensees.

The technical material in Section III.B and III.C of this testimony on the containment analysis and on the consequence analysis will describe the staff analyses and also contrast them with the licensees' corresponding analyses.

The remaining principal element of any PRA is the classification of severe accident sequences leading up to a challenge to containment systems and the evaluation of their likelihood. The scope of this portion of a PRA entails the evaluation of the susceptibility of the plant to the possible occurrence of core damage or core meltdown accident. The NRC Office of Nuclear Regulatory Research has considerable experience conducting such studies of a variety of nuclear power plants but has not done such a study of Indian Point, beyond the short term study described in NUREG-0715.

In NUREG-0850, the need for accident likelihood information was filled by the severe accident susceptibility assessment in NUREG-0715 and later improvements on it done in much the same way. That is, it was presumed that the risk-dominant accident sequences at Indian Point were the same as those found in full PRAs of other, similar reactor plants, but the reliability of the systems whose failures give rise to these accident sequences was reevaluated to reflect the specifics of system design at Indian Point. The Staff analysis of the probability of system failure for NUREG-0715 is documented in Appendix 1 of NUREG-0773. The Staff has been well aware that this approach to severe accident susceptibility analysis might miss vulernabilities not previously highlighted in PRAs of other plants and planned to revise our assessment in the light of the Licensee's study and our critical review of it.

Since the Licensee's study is more comprehensive than the Staff's prior assessment, we have adopted their assessment of core-melt accident sequence likelihoods as a starting point in developing our own.

The Staff contracted with Sandia National Laboratory to critique the accident likelihood portion of the IPPSS and to prepare an improved estimated of the likelihood of severe reactor accidents at Indian Point Units 2 and 3. The Staff has also reviewed both the IPPSS and the Sandia Draft Letter Report.

The Staff has drawn upon the SNL work, our own reviews, and upon the IPPSS in developing our estimates of the likelihood of severe reactor accidents.

Q.25 What is your summary impression of the Indian Point Probabilistic Safety Study?

A.25 The Indian Point Probabilistic Safety Study (IPPSS) is a more comprehensive reactor risk assessment than any published in the U.S. heretofore. It employs approximations that, on balance, are no less conservative than those employed in PRAs done by or for the staff. The licensees deserve a great deal of credit for tackling and publishing this massive and pioneering safety analysis.

The study is noteworthy in that it gives a more thorough accounting than prior PRAs have done of accident initiating events, both those originating in the plant and those due to external events such as earthquake or storms. The Indian Point Probabilistic Safety Study, and the sister study done on Zion, have also broken new ground in the thoroughness with which the challenges to containment by severe reactor accidents have been investigated. So, too, has the staff study of containment challenge phenomena. The IPPSS has also pioneered a technique for the propagation of uncertainties that can accommodate the effects of modeling approximations, phenomenological uncertainties, and completeness as well as the more commonly treated statistical uncertainties.

All the generic limitations of the state of the art in risk assessment described above apply to this study. Many of the approximations that form the fabric of the risk predictive models are known to be pessimistic. Some others are known to be optimistic. For some others, we do not yet know whether the models are conservative or optimistic. We shall know more after our critical review is completed, but the state of the art is such that many of the approximations in the models cannot be unambiguously identified as optimistic or pessimistic.

Q.26 Does this conclude your testimony?

A.26 Yes.

PROFESSIONAL QUALIFICATIONS
FRANK H. ROWSOME, 3rd
U.S. NUCLEAR REGULATORY COMMISSION

I am Frank H. Rowsome, 3rd, Deputy Director of the Division of Risk Analysis in the Office of Nuclear Regulatory Research. I have served in that capacity since joining the NRC in July 1979. The work entails planning, budgeting, managing and staffing the Division. Much of the work of the Division is devoted to research in reactor accident risk assessment. The remainder entails risk assessment applied to non-reactor aspects of the nuclear fuel cycle and to standards development related to system reliability or risk.

I received a bachelor's degree in physics from Harvard in 1962. I studied theoretical physics at Cornell, completing all requirements for a Ph.D except for the dissertation in 1965. From 1965 to 1973, I taught and engaged in research in theoretical physics at several colleges and universities.

In 1973 I joined the Bechtel Power Corporation as a nuclear engineer. My initial assignment was to perform accident analyses for nuclear plant license applications. After six months in that job, I was transferred to a newly formed group of systems engineers charged with developing for Bechtel a capability to perform risk assessments and system reliability analyses of the kind the NRC was then developing for the Reactor Safety Study. In that capacity I performed reliability analyses of nuclear plant safety systems, developed computer programs for system reliability analyses, performed analyses of component reliability data, human reliability analyses, and event tree analyses of accident sequences. I progressed from nuclear engineer, to senior engineer, to group leader, to Reliability Group Supervisor before leaving Bechtel to join the NRC in 1979. In this last position at Bechtel, I supervised the application of engineering economics, reliability engineering, and analysis techniques to power plant availability optimization as well as nuclear safety analysis.

While serving as Deputy Director of the Division of Risk Analysis (and its anticedent, the Probabilistic Analysis Staff), I also served as Acting Director (7 months), acting chief of the Reactor Risk Branch (9 months) and acting chief of the Risk Methodology and Data Branch (4 months).

This experience has given me the practitioner's view as well as the manager's view of those facits of reactor risk assessment entailing the classification of reactor accident sequences, system reliability analysis, human reliability analysis, and the estimation of the likelihood of severe reactor accidents. I have the manager's perspective but not the practitioner's experience with those facits entailing containment challenge analysis, consequence analysis, and risk assessment applied to other parts of the nuclear fuel cycle.

My role in the development of testimony for this hearing has been as coordinator of the preparation of testimony on risk and one of the coordinators of the technical critique of the licensee's "Indian Point Probabilistic Safety Study." I am not an expert on the design or operation of the Indian Point plants.

List of Publications

1. "The Role of System Reliability Prediction in Power Plant Design," F.H. Rowsome, III, Power Engineering, February 1977.

2. "How Finely Should Faults be Resolved in Fault Tree Analysis?" by F.H. Rowsome, III, presented at the American Nuclear Society/Canadian Nuclear Association Joint Meeting in Toronto, Canada, June 18, 1976.

3. "The Role of IREP in NRC Programs" F.H. Rowsome, III, U.S. Nuclear Regulatory Commission, Washington, D.C. 20555.

4. "Fault Tree Analysis of an Auxiliary Feedwater System," F.H. Rowsome, III, Bechtel Power Corp., Gaithersburg Power Division, F 77 805-5.

PROFESSIONAL QUALIFICATIONS
ROGER M. BLOND
U.S. Nuclear Regulatory Commission

I am Roger M. Blond, Section Leader of the Accident Risk Section, Reactor Risk Branch, Division of Risk Analysis, Office of Research. I have been with the NRC since August 1974. In my present position, I am responsible for providing technical and managerial direction in developing methods and research in accident risk analysis and in performing applications in probabilistic risk assessment. This work includes: (1) developing risk models for calculating the physical processes and consequences of reactor accidents; (2) rebaselining accident consequences and reactor risk; and (3) developing value/impact analysis methods for reactor design improvements.

In addition to the Section Leader position, I have the following responsibilities:

- I am the Chairman of the International Benchmark Exercise on Consequence Modeling, sponsored by the Committee on the Safety of Nuclear Installations, of the Nuclear Energy Agency, Organization of Economic Cooperation and Development. As Chairman, I am responsible for organizing and directing the comparison study which includes the participation of 30 organizations representing 16 countries. The study was chartered to compare the large number of computer models that had been developed to calculate the offsite consequences of potential accidents at nuclear power facilities.

- I am responsible for developing the technical rationale for the development of improved siting criteria. This work includes the development of a set of representative potential reactor accident source terms, and a full parametric study of all the factors important to siting considerations from the risk perspecti .

- I am a member of the Technical Writing Group of the IEEE/ANS PRA Procedures Guide - NUREG/CR-2300. This effort is developing a source document on PRA techniques. I am a co-author of the consequence modeling sections of the report.

- I am a member of the Department of Energy Working Group on Probabilistic Risk Assessment.

- I am a member of the NRC Incidence Response Center's Emergency Response Team.

In addition, I am directly involved in the development of a technical rationale for the NRC's Safety Goal, emergency planning and response, and numerous issues and questions which continuously arise in risk assessment.

I am also a lecturer on consequence modeling and accident analysis for the NRC Training Course on Probabilistic Safety and Reliability Analysis Techniques, for the IAEA Training Course on Nuclear Power, and for the George Washington University Seminar on Probabilistic Risk Assessment.

Risk Analyst

Before being selected for the Section Leader position, I was Senior Risk Analyst in the Office of Research. I was responsible for the following areas:

1. Consequence modeling research and development;
2. Performing and reviewing probabilistic risk assessments;
3. Siting and emergency planning and response criteria development; and
4. Integrating probabilistic risk assessment techniques into the regulatory and licensing process.

1. Consequence Modeling Research and Development

   I was responsible for revising the consequence model that was developed for the Draft Reactor Safety Study. During the course of that effort, I developed the following modeling approaches and techniques which were used for the final Reactor Safety Study consequence model (CRAC) and are documented in Appendix VI of WASH-1400 and the CRAC User's Guide:

   1. Meteorological sampling technique;
   2. Diffusion modeling technique;
   3. Time-varying meteorological model;
   4. Depletion approach;
   5. Finite cloud correction model for gamma shine;
   6. Economic model;
   7. Statistical sampling technique;
   8. Emergency response model;
   9. Property damage model; and
   10. Population treatment.

After the completion of the Reactor Safety Study, I developed the following modeling techniques which have been incorporated into the CRAC-2 computer code and documented in the CRAC-2 User's Guide:

1. Revised comprehensive emergency response model;
2. Importance sampling for meteorological data and terrain diffusion model;
3. Revised dosimetry and health effects review; and
4. Comprehensive results display package.

I also performed numerous sensitivity and parametric studies on the models and input used in the consequence model and was responsible for an extensive research program to investigate the significance of various related phenomena to risk. This research involved from five to ten contractor personnel. I also have been responsible for preparing and defending the research program and budget in consequence modeling and emergency planning before the Senior Contract Review Board and the Advisory Committee for Reactor Safeguards.

2. <u>Performing and Reviewing Probabilistic Risk Assessments</u>

I was responsible for all of the risk calculations performed for the final Reactor Safety Study. At the completion of the study, I responded to critiques and questions concerning Probabilistic Risk Assessment from within the NRC, Congress, other Federal agencies, contractors and vendors, intervenors, state and local governments, utilities, and foreign governments. I have also performed risk studies or comparisons for the following analyses:

1. Task Force Report on Interim Operation of Indian Point;
2. Indian Point and Zion Site Risk and Alternative Containment Concepts Study;
3. Hatch consequence study;
4. Three Mile Island Potential Accident Consequence Study and Source Term Study;
5. Generic Environmental Statement on Mixed Oxide consequence study;
6. Anticipated transients without SCRAM consequence study;
7. Diablo Canyon Risk Assessment review; and
8. Clinch River Breeder Reactor consequence analysis review.

I have been responsible for advising and reviewing the following foreign risk assessments:

1. Norwegian Energy Study
2. Swedish Reactor Safety Study
3. German Reactor Safety Study
4. British Windscale and PWR Inquiries

In addition, the Norwegian Government personally invited me to Norway to review the approach and assumptions used in their study.

3. <u>Siting and Emergency Planning and Response Criteria Development</u>

I was the research consultant and member of the NRC/EPA Task Force on Emergency Planning. For the work of the Task Force, I was responsible for formulating the rationale for the emergency planning basis criteria and was the principal author of the Task Force Report or Emergency Planning (NUREG-0396). I also was responsible for developing the Emergency Action Level Guidance (NUREG-0654, Appendix 1) which establishes consistent criteria for declaring emergencies based upon plant parameters.

I performed a study on the cost/benefit of issuing Potassium-Iodide to the general public. Based on this report (NUREG/CR-1433), Potassium-Iodide is not being stockpiled for public distribution. In addition, I have performed numerous studies on emergency protective measures such as sheltering versus evacuation. I also developed the Three Mile Island Emergency Contingency Plan at the time of the accident.

I developed a ranking of high population sites which has been used to designate potentially high risk contributors.

4. Integrating Probabilistic Risk Assessment Into the Regulatory Process

I have provided technical direction on consequence modeling to the regulatory and licensing process for the following areas: Perryman Alternative Site Review; Environmental Impact Statement for Class 9 Accidents; Liquid Pathway Generic Study; in understanding the course and importance of potential accidents; and in source term development. I have on numerous occasions presented the results of my work on consequence modeling and emergency planning and response to other Offices within the agency, other organizations, the Advisory Committee on Reactor Safeguards, and the NRC Commissioners.

Science Applications, Inc. (SAI), April 1973 to April 1975, McLean, Virginia

I was involved with the design and implementation of two major projects.

The first project was the Atomic Energy Commission's Reactor Safety Study. I was a research analyst involved in developing and applying reliability methods in reactor accident sequence quantification and error/uncertainty propagation. I also was given responsibility for the development of an improved consequence model for the final version of the study.

The second project was the Federal Trade Commission's Market Basket Survey. This survey was designed to statistically determine a "typical" market basket of food for the average family and have an accurate comparison of grocery store pricing. I was retained as an expert consultant to the F.T.C. and helped design and implement the survey and analysis techniques.

Computer Sciences Corporation - August 1970 to April 1973, Arlington, Virginia

I was a task leader with Computer Sciences Corporation where I worked on the general support contract for the National Military Command System Support Center (NMCSSC) in the modeling and gaming department. I designed, implemented, and documented the Data Base Preparation Subsystem of the QUICK Reacting General War Gaming model. I was task leader for the QUICK production support task with responsibilities for maintenance and production support of the model and the associated damage assessment models. I was chosen as War Gaming Analysis Section representative to study and evaluate the consolidation and conversion of the Antiballistic Missile System (ABM-I) and QUICK Strategic War Gaming Models.

Imcor-Glenn Engineering, Inc. - June 1968 to April 1970, Rockville, Maryland

Imcor-Glenn Engineering, Inc. Operations Supervisor, Programmer - I was contracted to work for the Naval Ships Research and Development Center on testing and evaluation of the Small Boats Project (PCF) and on the Sonar Dome Project. I was also contracted to the Naval Research Laboratory as site team leader for testing and evaluation of Ultra High Frequency Radio Wave Study. As operations supervisor for the Data Division of Imcor, I was responsible for programming and quality control of processed data.

Awards, Honors, and Publications

I received the NRC Special Achievement Award on October 29, 1976 and a NRC High Quality Award on May 11, 1978. I was a session chairman in Consequence Modeling for the American Nuclear Society/European Nuclear Society Topical Meeting on Probabilistic Risk Assessment, September 20-24, 1981 in Port Chester, New York. I was also a session chairman for the American Nuclear

Society Review Conference on the PRA Procedures Guide, April 1982, in Arlington, Virginia. For this conference, I organized three formal debates on current issues in consequence modeling. I have published numerous papers and reports in probabilistic risk assessment, consequence modeling, siting, emergency planning and response, and on the source term. A list of all publications is attached.

Education

I was awarded a Batchelors of Science in Computer Science in 1970 and a Masters of Science in Operations Research in 1973 from the American University in Washington, DC.

AUTHORED OR CO-AUTHORED THE FOLLOWING PUBLICATIONS

"Relationship of Source Term Issue to Emergency Planning," EPRI/NSA Workshop on Technical Factor Relating Impacts from Reactor Releases to Emergency Planning, Bethesda, MD, January 12-13, 1982.

Reactor Safety Study, WASH-1400, Appendix II and VI.

Nuclear Energy Center Site Survey Study, NUREG-001, Exhibit A, Section 6, part IV, "NEC Accident Risk Analysis."

Reactor Accident Source Terms: Design and Siting Perspectives, NUREG-0773, draft.

Regulatory Impact of Nuclear Reactor Accident Source Term Assumptions, NUREG-0771, April 1981.

Task Force Report on Interim Operation of Indian Point, NUREG-0715, August 1980.

Planning Basis for the Development of State and Local Government Radiological Response Plans in Support of Light Water Nuclear Power Plants, NUREG-0396, December 1978.

Emergency Action Level Guidelines for Nuclear Power Plants, NUREG-0610 (Appendix 1 of NUREG-0654, November 1980).

"Consequence Analysis Results Regarding Siting," 1981, Water Reactor Safety Meeting, Gaithersburg, MD.

"Calculations of Reactor Accident Consequences: User's Guide," draft.

A Model of Public Evacuation for Atmospheric Radiological Releases, SAND78-0092, Sandia Laboratories, Albuquerque, NM, June 1978.

Examination of the Use of Potassium Iodide (KI) as an Emergency Protective Measure for Nuclear Reactor Accidents, NUREG/CR-1433, SAND80-0981, Sandia National Laboratories, Albuquerque, NM, March 1980.

"Radiation Protection: An Analysis of Thyroid Blocking," IAEA International Conference on Current Nuclear Power Plant Safety Issues, Stockholm, Sweden, October 20-24, 1980.

"International Standard Problem for Consequence Modeling: Results," International ANS/ENS Topical Meeting on Probabilistic Risk Assessment, Port Chester, NY, September 1981.

"Recent Developments in Consequence Modeling," presented at the Jahreskolloquium PNS, Kernforschungszentrum Karlsruhe, Federal Republic of Germany, November 1981.

"International Standard Problem for Consequence Modeling," International ANS/ENS Topical Meeting on Probabilistic Risk Assessment, Port Chester, NY, September 20-24, 1981.

"Environmental Transport and Consequence Analysis," International ANS/ENS Topical Meeting on Probabilistic Risk Assessment, Port Chester, NY, September 20-24, 1981..

"Weather Sequence Sampling for Risk Calculations," Transactions of the American Nuclear Society, 38, 113, June 1981.

Calculations of Reactor Accident Consequences, Version 2: User's Guide, NUREG/CR-2326, SAND81-1994, Sandia National Laboratories, Albuquerque, NM, (to be published).

"Investigation of the Adequacy of the Meteorological Transport Model Developed for the Reactor Safety Study," ANS Topical Meeting on Probabilistic Analysis of Nuclear Reactor Safety, Newport Beach, CA, May 8-10, 1978.

USNRC, "Environmental Transport and Consequence Analysis," Chapter 9 and Appendices D, E, and F in PRA Procedures Guide, Review Draft, NUREG/CR-2300, 1981.

Overview of the Reactor Safety Study Consequence Model, U. S. Nuclear Regulatory Commission, NUREG-0340, 1977.

UNITED STATES OF AMERICA
NUCLEAR REGULATORY COMMISSION

BEFORE THE ATOMIC SAFETY AND LICENSING BOARD

In the Matter of	)	
	)	Docket Nos. 50-247-SP
CONSOLIDATED EDISON COMPANY	)	50-286-SP
OF NEW YORK (Indian Point, Unit 2)	)	
	)	
POWER AUTHORITY OF THE STATE OF	)	
NEW YORK (Indian Point, Unit 3)	)	February 7, 1983

ERRATA SHEET

FOR

DIRECT TESTIMONY OF FRANK ROWSOME AND ROGER BLOND
CONCERNING COMMISSION QUESTION 1

P.1 - A2: DELETE : Present answer entirely
REPLACE WITH - As of February 6, 1983, I became Assistant Director for Technology, Division of Safety Technology, Office of Nuclear Reactor Regulation. In that position I manage the work of the Safety Program Evaluation Branch and of the Reliability and Risk Assessment Branch.

P.2 - A6 - Line 1: CHANGE - "Yes, a the statement" TO - "Yes, a statement"

P.3 - A8 Line beginning IV: CHANGE - "To the Commission Questions on risk" TO - "To Commission Question One on Risk"

P.5 - Line 10: CHANGE - "less" TO - "less severe"

Line Sixteen - CHANGE - "and have shown..." TO - "and NUREG/CR-0603 "A Risk Assessment of a Pressurized Water Reactor for Class 3-8 Accidents" October 1979, have shown..."

Footnote: CHANGE - "For purposes" TO - "severe core damage is postulated to occur in design basis accidents for purposes...'

P.16 - Line 4: CHANGE - "Safety Study" TO - "Reactor Safety Study"

P. 16 - Lines 10 and 11: CHANGE - "Three of the four RSSMAP reports 1/ have now been published and the fourth 2/ is in final review." TO - "The four RSSMAP reports 1/have now been published."

P.14 - Line 4: CHANGE - "this testimony" TO - "the background for the Staff's Testimony"

P.16 - Line 18: CHANGE - "IREP report" TO - "IREP report 2/"

P.16 - Footnote 1/, last line: CHANGE - "Plant." TO - "Plant, Part 3 or 4 Calvert Cliffs #1."

P.17 - Line 1: CHANGE "published 3/" TO - "published"

P.17 - Lines 3 and 4 : CHANGE - "complete 4/" TO - "complete 3/ and 4/"

P.17 - Footnote 4: CHANGE - "degarded" TO - "degraded"
CHANGE - "colling" TO - "cooling"

P.25 - Line 12: CHANGE - "Sandia Draft Letter Report" TO - Sandia Report NUREG/CR-2934."

P.26 - Lines 18 and 19: DELETE - 'We shall know more after our critical review is completed, but "

Professional Qualifications of Frank Rowsome, 3rd,

P.2, ADD - As of February 6, 1983 I have been transferred from the assignment of Deputy Director of the Division of Risk Analysis,Research to the position of Assistant Director for Technology, Division of Systems Technology, Office of Nuclear Reactor Regulation.

BY MS. MOORE: (Resuming)

Q Mr. Rowsome, would you provide a brief summary of this testimony.

A (WITNESS ROWSOME) Yes. The purpose of this testimony is to introduce the Staff testimony on the risks posed by severe reactor accidents at Indian Point Units 2 and 3. It gives the technical and historical background to the Staff studies of risk at Indian Point.

In particular, the material under Roman III gives the Staff analysis of the risk: III.A, a section on on accident character and damage state likelihoods; III.B, containment analysis; III.C and D, consequence analysis and risk results.

The pieces of testimony under Roman IV are the interpretation of the risk results in response to Commission question 1.

MS. MOORE: Your Honor, the witnesses are now available for cross-examination.

JUDGE GLEASON: Mr. Blum?

MR. BLUM: Thank you, Your Honor.

CROSS-EXAMINATION ON BEHALF OF INTERVENOR

UNION OF CONCERNED SCIENTISTS

BY MR. BLUM:

Q Mr. Blond, Mr. Rowsome, neither of you is a betting man, are you?

A (WITNESS ROWSOME) No.

A (WITNESS BLOND) Not particularly.

Q Yesterday we had a great deal of philosophical discussion which got into questions of epistemology, essentially. I'd like to bring us back more in the direction of physics for a moment. Both of you have had considerable training in physics, have you not?

A (WITNESS ROWSOME) I have.

Q What I want is for one of you to give a brief description of the Heisenberg uncertainty principle as a takeoff for discussion.

A (WITNESS ROWSOME) All right, I can attempt to do that. The Heisenberg uncertainty principle applies to simultaneous measurements of position and momentum, and it is a consequence of the wave mechanical origin of quantum mechanics, and it asserts that one cannot in a system described by wave mechanics simultaneously identify both the position and the momentum of the corresponding particle with arbitrary precision.

Instead, the limit of accuracy is given quantitatively by Planck's constant.

JUDGE SHON: Mr. Blum, I think the physicists like to call it the principle of indeterminacy these days, instead of uncertainty. They like the idea that things are indeterminate, rather than uncertain.

MR. BLUM: Thank you, Judge Shon.

BY MR. BLUM: (Resuming)

Q Am I not correct that there is also a certain kind of crude layman's interpretation of the indeterminacy principle, that there are certain kinds of particles that are so small that our ability to measure them physically has limits such that we cannot assess certain physical properties with determinacy?

A (WITNESS ROWSOME) The better layman's analogy I think is to say that in attempting to measure the very tiny, the process of measurement itself disturbs the system to the extent that it isn't the same after measurement as it was before.

Q Thank you.

What I would now like to ask is whether there might not .. some analogue to this principle in risk assessment, such that we might at some point be dealing with numbers so small that measurable differences are overwhelmed by various artifacts of the methodology of risk assessment?

A (WITNESS ROWSOME) That is not really a good analogy with Heisenberg. There might be a good analogy with the Heisenberg uncertainty principle in the fact that, having learned something about the risk profile of the plant doing a PRA, the plant risk is no longer quite the same because of the educational process operating on those who own and operate the plant and those who regulate it.

But I see no analogy in the small numbers calculated for details of the risk distributions.

Q You don't see any process inherent in the process of calculating or estimating risks that could produce some sort of tendency to change the numbers in some sort of way? You can't conceive of anything like that?

A (WITNESS ROWSOME) Well, it is certainly true that the extremely small probabilities are a good deal less probable than the higher probabilities. To be accurate about truly minuscule numbers is hostage to completeness and accuracy to a greater extent than the higher estimates are.

Q Yes, so certainly with completeness and accuracy and probably also uncertainties relating to input data and modeling uncertainties, there is some quantum of uncertainty out there that possibly would not be captured by the uncertainty. That would be fair to say, would it not?

A (WITNESS ROWSOME) I don't really like the quantum analogy, but there are inaccuracies in risk assessment, yes.

Q Perhaps if I said there is some quantity of uncertainty, the amount of which is unknown, but it is in some sense not captured by the methodology?

A (WITNESS ROWSOME) The methodology is characterized by uncertainties, that is true. And they are proportionately bigger for the tiny probabilities than they are for the larger probabilities.

Q What I would like both of you to give a little thought on is what methodology there might be for determining when we have passed over the line such that the measurable differences between 10^{-12} and 10^{-10} are now so small that they are in some sense overwhelmed by the uncertainties that cannot be quantified.

A (WITNESS ROWSOME) I can start giving out my answer while Roger thinks about his. I gather you asked that question to both of us.

I don't think you can generalize on the basis of the number, like saying 10^{-10} is credible and 10 is not. It depends on how you arrive at the number. If you get to a number like 10^{-12} from a series of truly independent, individually rare events, you may be able to know 10^{-12} quite accurately or have a great deal of confidence that 10^{-12} is a correct description of a scenario, entailing many random uncoupled rare coincidences.

There are other circumstances where 10^{-12} originates from a single estimate of a not very well known parameter, in which case it would be uncertain, be very uncertain and very highly suspect. I think you have to, in judging whether small numbers are credible or not, you have to make a case by case decision depending on how those numbers were arrived at.

Q Mr. Blond, do you want to add anything?

A (WITNESS BLOND) I believe that the work that you are really trying to focus on, the interaction or interdependency between the analysis and the numbers that we are trying to really strive to analyze, is an evolving process, something that for the past ten years or so we have been making significant strides to improve, improve our understanding, improve our techniques, the statistical measures which we use to analyze the processes.

One of the more important aspects of the work, as we have seen it, is in the quality assurance process that one goes through to try to assure that the risk analysis is not interfering with the process itself, but is improving the understanding of the plant performance and the numbers which we are trying to analyze.

There is a question of art versus science somewhat in the analysis, and it is very important that we try to improve our approaches, improve our understanding of the low probabilities, to the point where we have confidence in what we're doing. And I think that is -- the Indian Point risk assessment as such has made a very significant stride in that attempt.

Q Could you say more of what you talked of as the problem of art versus science?

A (WITNESS BLOND) Well, in any analysis approach, especially one that is evolving, the analysts are in a learning process, and there needs to be an evolution of this type -- it will force itself in many ways -- to move from the technician who analyzes a system to somebody that can take a step back and say, this is a more complete view, which a risk assessment tries to focus on a problem.

And I believe we are moving from the point of having this technology in the realm of an art to more of a science.

Q You say we are moving in that direction. I assume that implies that we're not completely there yet.

A (WITNESS BLOND) There is an education process that would be involved and that process is now under way, and I believe the reflection of the day that it becomes a science is a reflection of when courses and material are being taught, so that people can easily obtain the skills necessary. Right now the skills in this area are in a fairly limited technical community.

Q This is now addressed to both of you. It would be fair to say that one principle theme affecting how low numbers can get before being overwhelmed by indeterminacy would be the amount of experience that we have already had with the thing being quantified; that would be correct, would it not?

A (WITNESS ROWSOME) With the elements that go into the quantification.

Q Which could in some sense be equated with the quality of our data base?

A (WITNESS ROWSOME) Yes.

Q Another thing would be the rigorousness of the methodology by which we assess uncertainty, would it not?

A (WITNESS ROWSOME) Not necessarily. To do an accurate calculation of a very low, a low frequency, a low frequency event, one needs either a well-established discipline for doing it or the performance of a number of sensitivity studies to verify that there are not unpleasant surprises lurking in the model, sensitivities one wasn't aware were there, or an analysis of propagation of uncertainties or some mix of those techniques or those resources, to do it reliably.

Q Couldn't those, what you call propagating uncertainties and sensitivity analyses in some sense be grouped in the category of establishing a more rigorous methodology for quantifying uncertainty?

A (WITNESS ROWSOME) Perhaps, although the sensitivity studies do not necessarily entail quantification.

Q Well, let me ask you to turn to page 12 of your testimony, which I will assume both of you will agree, this is a reasonably sophisticated discussion of some of the kinds of problems involved in probabilistic risk assessment, would you not?

A (WITNESS ROWSOME) Well, it is somewhat superficial. I don't know whether I would give it the adjective "sophisticated" or not. I believe it to be correct, but one could elaborate for years on points here.

Q We'll try to keep this to half a day at most.

In general, it is the position of both of you that the authors of IPPSS deserve some praise for their attempt to be comprehensive; that is correct, is it not?

A (WITNESS ROWSOME) That is correct.
A (WITNESS BLOND) Yes.
Q And IPPSS is in some respects more comprehensive than many of the PRA's that preceded it; is that correct?
A (WITNESS ROWSOME) That is correct.
Q For example, a more extensive treatment of external events is one major --
A (WITNESS ROWSOME) That is true.
Q -- step in that regard.
Your seventh line down --
JUDGE GLEASON: Which page?
MR. BLUM: I'm sorry. We're on page 12 still.
BY MR. BLUM: (Resuming)
Q The beginning of the first full paragraph on the page, you state, "The attempt to be comprehensive, however, is bought at a considerable price."
Now, I know there would be a double meaning here, that IPPSS was quite expensive, costing well in excess of $3 million. But that is not what you mean here, is it?
A (WITNESS ROWSOME) No, it isn't.
Q Could you tell us more specifically what you do mean by considerable price?

A (WITNESS ROWSOME) I think the next sentence does. Would you like for me to paraphrase it?

Q Why don't you elaborate on it. You can start out by reading the next sentence and then just go on.

JUDGE GLEASON: He doesn't have to read it, Mr. Blum. We all have the testimony. Just let him explain it.

WITNESS ROWSOME: The variety of root causes of accidents and the numbers of sequences -- I mean this in the chronological sense -- in which faults can develop in the plant, the number of paths by which faults can propagate through the network of systems and the like, are quite numerous and quite complicated, and to model each and every one of these in the maximum richness of detail that could in principle be done if one had millions of man-years to devote to it is simply not practical.

One makes approximations, typically conservative approximations, to simplify the problem to the point that it is manageable, that the mathematical models are solvable. One makes these approximations to render the process tractable and require finite resources.

In so doing, one loses some of the nuances of the model. I think of a risk assessment as being a complex evaluation tool from which a great variety of inferences can be drawn, different inferences, exercise these approximations in different ways. Any particular inference is likely to be sensitive to a small fraction of these approximations and largely insensitive to the vast array of other approximations in the PRA model.

So that when drawing inferences from a PRA it is incumbent on the decisionmaker, whoever is drawing those inferences, to try to identify which of the assumptions and approximations in the PRA the inference is sensitive to, and to establish the reliability of that inference in the immediate context of the decision or the inferences he is trying to draw, rather than trying to make a general statement about is it right or is it wrong.

We know they have enough approximations, the PRA's do, that they are not perfect by any means. Like any approximate model, they can be used inside their domain of reasonableness. For the approximations they are used outside that domain.

So I suggest the reliable use of PRA's entails identifying those elements of the modeling approximations to which the inference in question is sensitive.

Q Mr. Blond, what does the concept of a

considerable price mean to you?

A (WITNESS BLOND) I think perhaps an example might be useful to try to explain in my mind what it was intended here. I can point specifically in the Indian Point risk assessment to the consequence work that was done, which is an area that I do have some expertise in.

The Pickard, Lowe and Garrick team that worked in this area went to considerable expense to expand the consequence calculation to --

JUDGE GLEASON: Let's go off the record.

(Discussion off the record.)

JUDGE GLEASON: All right, Mr. Blum, let's proceed, please.

WITNESS BLOND: As I was saying, there was a tremendous effort involved in trying to move the methods into another level of analysis, another level of detail. The example I was giving or started to give was in the consequence modeling area.

For the reactor safety study, we developed a model which used a fairly simplistic treatment of the manner in which the radioactive material will be dispersed and moved into the environment when it is released from the power plant. We assumed that it would move essentially in a straight line and downwind in one direction only.

The Indian Point probabilistic safety study decided that that was not a sufficient treatment, that you could do better. Our analysis when we developed the techniques went into more detailed models, in which you can actually model the trajectory and movement of the plume.

At the time that we developed the approaches we determined that it was not necessary, in our minds it was not necessary, to go to this more difficult effort, more sophisticated level of detail, and there was a considerable price to pay, both in terms of our efforts and the computer utilization that would be required in doing such a calculation.

The Indian Point analysis went into this more difficult level of detail and actually modeled the trajectory of the plume by allowing it to change as the wind directions would change. As we now can compare the differences in that level of detail, we can come to a better appreciation of what that more sophisticated level of treatment does afford you, what benefit has that given in terms of the answers that we were generating.

BY MR. BLUM: (Resuming)

Q So Mr. Blond, I take it that your use of the word "price" is somewhat different, that you do mean it in some sense as an expense, an expenditure of the labor, time and money for computer runs; is that correct?

A (WITNESS BLOND) There is a judgment that must be made in any analysis in terms of how much detail do you really have to go to. The modeler, the person that is responsible for interpreting the information and trying to relate it to the problem at hand, has continuously to make such judgments as to what detail he goes to.

This is the whole process of evolving an art, and the question of expertise comes into play, of discussions with panels of people, whatever, are all brought into the question.

Q But to use the concept of price as it is being used here in "considerable price" means some sort of expenditure of labor, time and money; is that correct?

A (WITNESS BLOND) No. No, it is not. It would be the price in the terms of the level of sophistication of the model.

Q So the price is that you have a less sophisticated kind of model than you would like to have; is that correct?

A (WITNESS BLOND) What you would like out of any model is to duplicate reality.

JUDGE GLEASON: Aren't you just talking about a benefit to effort ratio?

WITNESS BLOND: That is very true.

JUDGE GLEASON: We understand what it is, Mr. Blum. Why don't you go on.

BY MR. BLUM: (Resuming)

Q All right. Let me return to Mr. Rowsome. The attempt to be comprehensive, however, is bought at a considerable price. Mr. Rowsome, are you familiar with the Sandia -- you are familiar with the Sandia evaluation in the IPPSS study, are you not?

A (WITNESS ROWSOME) I think you are speaking of NUREG-2934, yes.

Q Yes, that is correct. And you are familiar with some general criticism with regard to modeling of external events, are you not?

A (WITNESS ROWSOME) Yes.

Q And the gist of that criticism was that the models and probabilities attached were often much more simplistic than would be desired, was it not?

A (WITNESS ROWSOME) I am not sure that is an accurate paraphrase. I think the principle concern was that the models, being out at the limits of the state of the art, were not very well understood, very mature. We are not fully aware of their sensitivities.

Q Mr. Rowsome, isn't it fair to say that by the phrase "attempt to be comprehensive is bought at a considerable price," what is really meant there is that by being comprehensive one is forced to take on certain difficult areas that cannot really be modeled very accurately or numbers assigned precisely?

A (WITNESS ROWSOME) Yes, yes.

Q And in general, the modeling of external initiating events would be one area like that, would it not?

A (WITNESS ROWSOME) Yes.

Q What would be another area?

A (WITNESS ROWSOME) There are elements of likelihood, accident likelihood estimation, that have that character beside external events: the subtle causes of common mode failure, human cognitive behavior. There are also elements of containment analysis and consequence analysis that have those attributes.

Q Well, let me ask you to give some examples and be more specific about subtle causes of common mode failure. Which of these would be examples that could not be modeled very accurately or precise numbers attached?

A (WITNESS ROWSOME) Fire, for example, in-plant fire.

Q And why is that true of in-plant fire?

A (WITNESS ROWSOME) You will get much more expert testimony on that subject than I can give when our team with Ben Buchbinder will be testifying in Section III, I believe III.C of our testimony.

Q Do you have any opinion why that is true of fire?

A (WITNESS ROWSOME) Because the probability of initiation and propagation and the spread, if you will, of the fire, of fires, cannot be very accurately projected.

Q And that is true, you said, both with regard to initiation and spread both?

A (WITNESS ROWSOME) Yes, that is my understanding.

Q Are there any other examples of subtle common mode failure besides fire?

A (WITNESS ROWSOME) There are other examples, but I'm not sure any of the others are anywhere near so important as the ones we have already identified.

Q Going on to the second point which you raise, was human cognitive error. Why can't that be modeled? Well, first give us an example of something that can't be modeled precisely.

A (WITNESS ROWSOME) I don't believe the current state of the art -- although here I should point out again, we're going to get rather more expert testimony than I can give on the same subject in the III.A testimony.

But since you want my opinion, the problem of estimating the likelihood that a team of operators will misconstrue a pattern of evolving symptoms of an accident in progress and lock onto an erroneous hypothesis of what is happening cannot be predicted very reliably today, according to my understanding of the state of the art.

Q Would it be simplistic in your opinion to treat the actions of different plant personnel present as independent events, as a way of calculating the likelihood that four people would latch onto the same wrong hypothesis?

A (WITNESS ROWSOME) Strict randomness I think would lead to an optimistic estimate of human error probability.

Q A particular methodology where you say, what is the probability of all four operators just together reaching the wrong hypothesis and modeling that by saying, one-tenth is the chance the first operator gets it wrong, one-fifth is the chance that the second, one-tenth for the third, and one-tenth for the fourth, and then to derive the probability by multiplying one-tenth by one-fifth times one-tenth times one-tenth, that would be a rather simplistic approach to the problem, would it not?

A (WITNESS ROWSOME) It really depends on the context. The multiplication of probabilities makes great good sense, but the development of the conditional probabilities of the second man making the mistake, given that the first one has done so, is a difficult problem and I wouldn't want to endorse your numbers offhand.

Q Do you recall being asked about that methodology in your deposition?

A (WITNESS ROWSOME) Yes, I do.

Q And do you recall generally what you said about it there?

A (WITNESS ROWSOME) In general terms, yes.

Q Do you recall using the word "simplistic"?

A (WITNESS ROWSOME) I don't recall whether I did or not. It's altogether possible I may have.

Q Let me show you the bottom of page 75.

A (WITNESS ROWSOME) If you'll give me a minute, I will retrieve my copy of the deposition.

Q Okay, if you'd like to.

(Pause.)

A (WITNESS ROWSOME) Yes, I have page 75.

Q Okay. Did you not say, beginning on line 19 of page 75, "I think it would be too simplistic to say for the whole sequence this is a meaningful way of modeling it. You have to look at what the guy thought in the first five minutes or what led him to think in the next five minutes, and so forth, in order to deal with the cognitive problem, the time dependence of the evolution of symptoms and how the operators behaved. So I would be inclined to think that to be too simplistic a model to be very trustworthy in a context like TMI, regardless of whether I thought your numbers were high or low or indifferent"?

A (WITNESS ROWSOME) Yes, I said that. I still believe it.

Q Thank you. And at that time in the deposition, it did refer to the method of calculating?

A (WITNESS ROWSOME) It referred particularly to the cognitive problem of developing a hypothesis of what was happening in the plant, given that one did not have procedures aimed at diagnosis of what was happening at the plant, but which were presumptuous of the accident.

Before TMI it was commonplace in nuclear power plants to have an emergency procedure for loss of coolant accident and an emergency procedure for steam line break and for each of a number of other accidents. The procedures did not describe or instruct the operators on the pattern recognition problem of identifying what accident they had. It presumed that they knew what accident they had.

In that context, in which an operator was faced by a pattern of symptoms which according to his training and his procedures was unprecedented, unfamiliar, he had to jump to a hypothesis.

Q Thank you.

MS. MOORE: Mr. Chairman, the witness should be allowed to finish his answer.

JUDGE GLEASON: Finish your answer.

MR. BLUM: I think the question has been answered.

MS. MOORE: He has been answering a question. He is entitled to finish.

BY MR. BLUM: (Resuming)

Q Well, certainly if you want to finish answering the question.

A (WITNESS ROWSOME) The essence of my answer is simply that my comment that the probability modeling you were describing was too simplistic applies to a particular class of problems and not generally to the issue of estimating human reliability.

Q Thank you.

You've also stated, have you not, that the Advisory Committee on Reactor Safeguards has ben chiding your Division for not tackling the problem of sabotage in PRA's; is that correct?

A (WITNESS ROWSOME) That's right.

Q Well, first, what is your Division?

A (WITNESS ROWSOME) Right now it has changed since I spoke to you last. I am now in NRR. I'm now in the Division of Safety Technology.

Q The Division to which you were referring was what?

A (WITNESS ROWSOME) The Division of Risk Analysis in the Office of Research.

Q Could you tell us more about the chiding that ACRS has given to the Division of Risk Analysis?

A (WITNESS ROWSOME) Oh, a couple of times over the last three or four years, I believe it was Dr. Okrent, perhaps other members of the ACRS, urged us to take on the problem of sabotage using PRA techniques.

Q Can you tell us more specifically what urged?

MS. MOORE: Asked and answered, Mr. Chairman.

WITNESS ROWSOME: I don't recall that there was a more specific message.

BY MR. BLUM: (Resuming)

Q Did they convey a belief that bottom line risk numbers for PRA's would be substantially more accurate if sabotage was included?

A (WITNESS ROWSOME) I don't recall that they did.

Q Do you recall what the purpose of their asking sabotage to be included was?

A (WITNESS ROWSOME) I don't recall their identifying one. From the context I infer -- and this is presumptuous on my part -- that Professor Okrent found that we could gain some insights with which the safeguards program could be more sharply focused or better tuned. But that is a presumption on my part. I don't really remember the details or context in which he did urge us to take up the problem.

Q It is your belief, is it not, that studies of sabotage in probabilistic risk assessment could be more useful for designing specific plant improvements to lower the risk of sabotage than for achieving accurate bottom line risk numbers?

A (WITNESS ROWSOME) More useful, I believe is your statement? Yes, I believe it would be more useful in the design and operation of plants than in bottom line risk assessment.

Q But you also believe this could be done more successfully, do you not?

A (WITNESS ROWSOME) I think inferences about the sabotage problem would be more reliable in the context of plant design and operation than in the context of bottom line risk assessment, yes.

Q Just to get that point a little clearer: you are stating that the use of PRA for the purpose of designing safety-related improvements would be a more reliable and valid use of PRA than for setting out bottom line overall risk numbers?

MR. BRANDENBURG: I object to that question. Mr. Chairman, he's characterizing the witness' testimony. It is clearly confined to the area of sabotage.

JUDGE GLEASON: Hes's asking the question, if that is what he said. So let him respond.

WITNESS ROWSOME: It is a little bit of a general question. I am inclined to agree with it, but I have a feeling that we could come up with exceptions if we tried.

BY MR. BLUM: (Resuming)

Q First, could you explain why you agree with it?

A (WITNESS ROWSOME) Well, as I mentioned before, I think of a risk assessment as a very complex evaluation model, from which a very large number of inferences can be drawn. Some of these inferences are much more reliable than others, and while I am sure examples would come up in the course of this testimony and the testimony of others, I think it difficult to generalize among those many, many uses, many, many inferences.

Q When you mentioned four areas of relatively grave uncertainty, the first being subtle causes of common mode failures, the second being operator error, the third was having to do with the modeling of containment response, what is there that's very much on the uncertain side in modeling the containment?

MS. MOORE: Objection, Mr. Chairman. I believe that is somewhat of a mischaracterization of the witness' testimony. I don't believe he said "grave uncertainties."

BY MR. BLUM: (Resuming)

Q Do you recall the exact word that you did say?

A (WITNESS ROWSOME) I don't offhand.

JUDGE GLEASON: Are you referring to his testimony?

MR. BLUM: The answer that he gave about ten questions back.

JUDGE GLEASON: Do you remember what you said ten questions back?

WITNESS ROWSOME: I remember the context, but not the words. But I think I have a simple answer to it, and that is, I will be testifying or I will be subject to cross-examination on my testimony under Roman IV, Section C, on the uncertainties in risk analysis later in this two-week period, where the subject is dealt with in far greater detail than in the piece of testimony on which we are being crossed today.

Since you will have a shooting license at that time, why don't we deal with it at that time.

BY MR. BLUM: (Resuming)

Q All right, I'll be willing to accept that.

What was the fourth one after containment response? Was it consequence modeling?

A (WITNESS ROWSOME) I think I said there are some elements of containment analysis and some elements of consequence analysis that have the attribute of substantial uncertainty.

Q And which of the consequence analysis would you attribute that characterization to?

A (WITNESS ROWSOME) Oh, parts of the dispersion model dealing with things like particle size and perhaps spontaneous plume range.

Q What about presumed rates of evacuation and relocation of people?

A (WITNESS ROWSOME) Again, we had testimony on that subject in Roman IV of the material on Commission question 1.

Q Do you consider that to be an area of substantial uncertainty? Well, let me address that to both of you, since Mr. Blond may have expertise in this area.

A (WITNESS ROWSOME) In the bottom line risk, no.

JUDGE GLEASON: Excuse me. Mr. Blum, I really have to inject at this point. If you are going to be asking questions which are better asked in connection with testimony that is subsequently to be gone over, all we're doing is wasting everybody's time and prolonging the time which the Board has set aside for the hearing on this question.

And you just cannot ask anything of any witness that you might want to ask of any witness at any time. You are required to stay within the confines of his testimony.

MR. BLUM: I'm doing that, Your Honor. Mr. Rowsome said with regard to containment analysis he would be testifying more substantially later on, and I chose to defer that area until later on. I did not understand Mr. Rowsome to be saying that he would be testifying on the consequence analysis.

JUDGE GLEASON: But there are witnesses that are going to testify on tnat.

MR. BLUM: But the problem is, I was asking the meaning -- I asked the meaning of Mr. Rowsome's statement in his testimony. He gave me four examples with reference to it, and I was asking about those four examples.

I can withdraw the question and deal with that later.

JUDGE GLEASON: I really have to urge on you that we must get on with this proceeding. And you know, if we're going to continue at this pace then the Board is just going to have to put some time constraints in, which up to this point we have not wanted to do. But we must proceed at a faster pace than we are going.

If you would just stay with his testimony, it would be helpful to everyone.

I might also say to the witnesses, if you would just answer the questions without going, you know, wherever you go beyond the questions, it would be helpful.

BY MR. BLUM: (Resuming)

Q In your testimony you cited the example of design errors that are not revealed by either design documents, surveillance tests, or reactor operations. Could you give us some examples of those, please?

A (WITNESS ROWSOME) I can't give you some examples that were never found in these processes, because we don't know about those. I believe there were one or two plants in which it was discovered that the safety features actuation system was cross-wired in such a way that division A actuated division B of the engineered safety features, and division B of the safety system actuated division A of the safety features, and in so doing introduced some unrecognized failure modes into the system.

Q Mr. Blond, do you have an example you wish to add?

A (WITNESS BLOND) No, I don't.

Q Mr. Rowsome, am I correct that you are saying there are really two types of design errors that are possible that would not be detected? One is ones that you can identify because they have been found in other plants, and a second type would be those you cannot identify because we're not aware of them because they have not been found; is that correct?

A (WITNESS ROWSOME) There is a blind spot in PRA methodology, which I think I have testified to, I'm sure I've testified to in greater detail, in Section IV.C. I'll be happy to discuss it with you then.

Q Well, if we could just wrap up the one point that we're on right now. You did say there were those two types of design errors, is that not correct?

A (WITNESS ROWSOME) I think you said that, but I don't disagree with it.

Q IPPSS assumes an absence of design errors, does it not?

A (WITNESS ROWSOME) Not altogether. The study is quite capable of having identified and correctly modeled some kinds of design errors. The methodology employed would be fairly reliable at identifying and recording the presence of some kinds of design errors, not all kinds of design errors.

Q What kinds would it not be able to identify?

A (WITNESS ROWSOME) Those with the attributes listed here in the testimony, that they are not portrayed in design documentation, they are not revealed by surveillance tests and by operating experience.

Q Thank you.

In your testimony you refer to the Lewis report's disavowal of the executive summary of

WASH-1400.

A (WITNESS ROWSOME) I believe it was the Commission that did that.

Q I'm sorry, the Commission's disavowal based on criticisms in the Lewis report; is that correct?

A (WITNESS ROWSOME) Yes.

A (WITNESS BLOND) Of the executive summary.

Q Could you address the significance of that disavowal for risk assessment generally?

A (WITNESS ROWSOME) It was simply that the executive summary was thought to be an inadequate treatment of the full study.

Q Do you see it as having no significance beyond the particular document of WASH-1400?

A (WITNESS ROWSOME) No significance beyond the executive summary of WASH-1400.

Q Mr. Blond, do you see any sort of general note of caution applying to PRA's generally toward not having types of executive summaries that oversimplify complex results in the PRA?

MS. MOORE: Mr. Chairman, I would object. The relevance of that question is minimal. It's a very broad question.

MR. BLUM: They discuss this in their testimony.

JUDGE GLEASON: I know, but really, I don't understand where Mr. Blum's view on that subject would be helpful to any of us. You know, they talk about everything in their testimony. What is the assistance that that is going to be giving to the Board? I just don't understand, Mr. Blum.

MR. BLUM: It will get at whether there is some sense in which PRA results can be misused and what is likely to contribute to their misuse.

JUDGE GLEASON: Their testimony is there. They talked about the summary. You are taking the next step, saying does every summary have deficiencies. You know, that just doesn't --

MR. BLUM: Well, if the witness sees that as not being a generally applicable word of caution for PRA's, he can simply say so and we'll be off the area. But if he does I'd like to know what he feels that it is.

JUDGE GLEASON: Well, the Board doesn't think that it's important. That is what I'm trying to tell you.

MR. BLUM: You are saying that the Board doesn't feel what is important?

JUDGE GLEASON: The answer to that question, and the question is not important. So if you are going to keep asking unimportant questions we're going to be here, you know, two years. That is what I am trying to get at.

MR. BLUM: Why is the possibility in the Commission's view on PRA results being misused unimportant?

JUDGE GLEASON: I didn't say that, Mr. Blum, and you know that I didn't say that. I said that the question you asked the witness is not important to the Board.

If you want to keep asking unimportant questions, fine. Then I'm going to be putting some time limits in on this cross-examination very, very rapidly. It is up to you to assist us in moving this thing along.

MR. BLUM: Thank you.

JUDGE GLEASON: You're welcome.

BY MR. BLUM: (Resuming)

Q In your testimony you have characterized the extent to which the Staff has performed an independent calculation of risk at Indian Point, and by that I mean independent of IPPSS, have you not?

A (WITNESS ROWSOME) Yes.

Q I would like to explore, then, in a little more detail than was possible in the limited space of your direct testimony the precise degree to which the Staff testimony does in any way depend upon the IPPSS results. Would either of you wish to begin by addressing that generally?

MS. MOORE: Mr. Chairman, that question is too general. I think the witnesses are entitled to specific questions.

MR. BLUM: I will withdraw the question and ask a more specific question.

BY MR. BLUM: (Resuming)

Q Some of the Staff's calculations were based upon numbers regarding risk that were derived by Sandia, were they not?

A (WITNESS ROWSOME) In part, yes.

Q And the Sandia numbers were not based on an independent PRA done by Sandia, were they?

A (WITNESS ROWSOME) Not a de novo independent PRA. That is correct.

Q Those numbers were done by certain recomputations based upon IPPSS, were they not?

A (WITNESS ROWSOME) ased in part upon IPPSS and in part on an inquiry by the Sandia people into the design and operation of Indian Point.

Q Well --

A (WITNESS ROWSOME) They are not wholly hostage to what they found in the IPPSS. On the other hand, they did not go back and reverify everything in the corresponding parts of the IPPSS.

Q Sandia took IPPSS as a starting point in their calculations, did they not?

A (WITNESS ROWSOME) Yes.

Q And they did recalculations for a limited number of scenarios which they identified as dominant?

A (WITNESS ROWSOME) They will be up here, I hope, today, on the witness stand.

Q Yes. Thank you.

Do you know how many scenarios they reviewed in IPPSS?

A (WITNESS ROWSOME) Not offhand. I could look it up.

Q It was around 14, though, do you recall?

MS. MOORE: Mr. Chairman, might I have a clarification, Number One, on who reviewed it, what review? The way the question was stated, it said what scenarios they reviewed in IPPSS. Who is they, is my question.

MR. BLUM: Sandia.

WITNESS ROWSOME: Sandia recalculates in some detail something between 10 and 30 classes of accident sequences, each one of which could be described as being an ensemble of hundreds of little sequences. Really, the count of the number of sequences is in the eye of the beholder.

BY MR. BLUM: (Resuming)

Q Can you give any idea or percentage, what percentage of the work in IPPSS was reviewed by Sandia in doing their recalculations?

MS. MOORE: Your Honor, we are going to have the exact people up here who did the review. I think these questions are more appropriate for them.

JUDGE GLEASON: I presume --

MR. BLUM: If they wish to withdraw testimony about the staff having performed a fully independent review of the risk at Indian Point, I am willing to accept that withdrawal. If they are not withdrawing that testimony, we are entitled to find out the extent to which it is in fact true.

WITNESS ROWSOME: You are mischaracterizing that testimony. I said we did a fully independent analysis of the containment analysis part of the consequence analysis part, which I continue to adhere to. We did not do, nor did the testimony assert that we did a wholly independent analysis of accident likelihood.

BY MR. BLUM: (Resuming)

Q So that was to a considerable extent based on IPPSS, was it not?

A (WITNESS ROWSOME) Yes.

Q Okay. Thank you. I am sorry for my confusion.

Could you identify what you describe as the fully independent review of containment?

A (WITNESS ROWSOME) You will be getting testimony on that from Dr. James Meyer, et al.

Q And the fully independent review of consequences?

A (WITNESS ROWSOME) Dr. Charda, et al.

Q In your testimony, you give a general

assessment that IPPSS overall is no less conservative on balance than the staff's work. Is that correct?

A (WITNESS ROWSOME) On balance, yes. There are areas where they use more conservative assumptions than we would have done, and areas where we use more conservative assumptions than they do. But on balance, I think that is generally true.

Q Could you identify what those two areas are, two sets of areas?

A (WITNESS ROWSOME) I would not be able to give a complete account of all of those areas of disagreement from memory. I could give a few examples, if you wish.

Q Sure. Go ahead.

A (WITNESS ROWSOME) An example in which they used a more conservative assumption than we did or got a more conservative result from their model is in the arena of containment failure, and those accidents in which the core melts early and containment heat removal has failed. An example where they used a more optimistic model than we feel comfortable with is in the area of crediting evacuation in accident sequences triggered by earthquakes.

Q Mr. Blond, can you think of any additional samples where the staff was more or less conservative than the licensees?

A (WITNESS BLOND) Again, there will be significant discussion. This will be in testimony in Section 3 in great detail.

JUDGE GLEASON: Why don't we wait, Mr. Blum?

WITNESS BLOND: There is a full comparison of the Indian Point analysis to the staff analysis in that testimony.

BY MR. BLUM: (Resuming)

Q One minor point to clear up. The discussion on Page 4 of your testimony of after heat or decay heat, I noticed, just so I don't have the wrong place for this, I noticed the same discussion is found verbatim in the testimony of Sandy Israel. Would you be the proper people to address this to, or would Sandy Israel?

A (WITNESS ROWSOME) Well, I think if we used it, we both are.

Q But it is principally your words?

A (WITNESS ROWSOME) I wrote this paragraph.

Q The gist of this discussion on Page 4 is that meltdown and breach of containment can occur even after the plant is shut down. Is that correct?

A (WITNESS ROWSOME) It is theoretically possible, yes.

Q But most of the concern in risk analysis has been devoted toward the concern about inability to shut down the plant, and that causing the --

A (WITNESS ROWSOME) In fact, that is not true. With the exception of the rather narrowly circumscribed issue called anticipated transients without scram, the whole array of accidents we are dealing with here are accidents in which the plant is successfully shut down in the sense of the termination of the nuclear chain reaction in the reactor core.

What you may be referring to is an artifact of the regulations which describe a state called safe shutdown, which means not only shutdown but a lot of other things as well, entailing being in some cases in cold shutdown, and proper decay heat removal.

What tends to be at issue in these risk assessments, what turns out to be the safety function that is of most concern is the ability to dissipate decay heat in the hours following a reactor scram or reactor shutdown.

MR. BLUM: Your Honor, I am going to need to ask for some guidance from the Board at one point. We have something that we wish to ask of Mr. Rowsome relating to the precursor study. Now, I am aware that there is more specific testimony on this at another point in the proceeding, and in some sense, that would be the most appropriate time to address it.

At this time, there is some slight uncertainty as to whether we would be allowed to participate with those witnesses, because although it does relate to Question One and risk generally, it also relates to a Board question, and that there is a rule or a guidance that intervenors should seek leave of the Board before examining on the Board questions.

JUDGE GLEASON: Does it relate to his testimony?

MR. BLUM: Yes, it does relate indirectly to his testimony. It relates to getting a handle on uncertainties, and whether the precursor study has relation to that.

JUDGE GLEASON: Why don't we hear the question and see if there is any objection?

WITNESS ROWSOME: I have a simple solution to your problem. I am testifying on uncertainties in 4-C material.

JUDGE GLEASON: Does that take care of your problem?

MR. BLUM: It sort of shifts the problem over some. All right, I will postpone that, although in general this problem will come up at a number of points.

JUDGE GLEASON: Well, we don't like to decide things in advance.

MR. BLUM: We do have a request for leave of the Board which we hope you will be able to address fairly soon on the uncertainty.

BY MR. BLUM: (Resuming)

Q Mr. Rowsome, if I could, I would like to reopen the topic of filtered venting.

I take it there is no objection.

MS. MOORE: I didn't hear a question pending.

BY MR. BLUM: (Resuming)

Q It is your position, is it not, that there are certain filtered vent conceptions that can be even more effective than anticipatory evacuation in limiting off-site radiological risk?

MS. MOORE: I object, Mr. Chairman. This witness is not testifying on filtered vented containment systems.

JUDGE GLEASON: The objection is granted.

MR. BRANDENBERG: Mr. Chairman.

JUDGE GLEASON: Please.

MR. BRANDENBERG: You ruled a split second before I started, I think.

JUDGE GLEASON: Hold just a minute, please.

MR. BLUM: I actually have no further questions at this time.

(Pause.)

JUDGE GLEASON: Let me ask Mr. Rowsome, since he is the major domo of the testimony that will follow, whether the subject of filtered vented systems is going to come up in any of the testimony that follows.

WITNESS ROWSOME: Dr. Meyer will be attesting to the continued studies of mitigation done by the staff in this package of testimony before us now in 3-B. The issue may come up, and I cannot assure you that it will, in the staff testimony under Commission Question 5.

JUDGE GLEASON: The problem is, there is a motion to strike that testimony. There is a reference in your testimony to the filtered vented containment systems, although it is just a reference, and I think, Mr. Blum, if you want to keep your question confined a little bit, I will deny the objection and permit you to ask the question.

MS. MOORE: Mr. Chairman, might I respond?

JUDGE GLEASON: Please. If you insist, I mean.

MS. MOORE: I do. Although there is a pending motion to strike Dr. Meyer's testimony, Dr. Meyer will be available for cross examination, and it is possible that even though, if the testimony were stricken, there would be ways of asking him questions concerning that subject.

JUDGE GLEASON: How is he going to be available for cross examination if his testimony is stricken?

MS. MOORE: It is not his entire testimony, and there is dispute about the motion to strike.

JUDGE GLEASON: I understand that. Which do you prefer to do, Mr. Blum? I guess you would like to keep it where you get the most benefit from it, because the Board is interested in the subject, too. Where would you prefer?

MR. BLUM: We are puzzled as to who made a motion to strike Dr. Meyer's testimony.

JUDGE GLEASON: There is a motion pending to strike Dr. Meyer's testimony filed by the licensees.

MR. BLUM: We would appreciate being served with a copy.

MR. COLARULLI: Your Honor, they have been served with a copy.

JUDGE GLEASON: We would prefer not to get into that controversy. So the question is, Mr. Blum, do you want to do it now, or do you want to wait? We prefer you not to do it both places.

WITNESS ROWSOME: A consideration you may wish to make is that I will be testifying in Commission

Question 5 on the inferences for regulatory --

JUDGE GLEASON: They may not be permitted to ask questions on cross examination. Oh, on Question 5, did you say? I am sorry. I thought you meant Board questions.

MR. BLUM: Assuming that we are permitted to ask questions under Commission Question 5, I will wait.

JUDGE GLEASON: All right, fine. Are you finished, Mr. Blum?

MR. BLUM: Yes, I am.

JUDGE GLEASON: Thank you.

Mr. Hartzman, you will keep your cross examination brief and not repetitive, won't you?

MR. HARTZMAN: Yes. I just have a few questions, which I hope will only take a short time, Your Honor.

CROSS EXAMINATION ON BEHALF OF FOE AUDUBON

BY MR. HARTZMAN:

Q Just to pursue just a step further a point raised by Mr. Blum, you indicate on Page 25 of your testimony, you state "we," and I believe you mean the staff, "have adopted their assessment," and that will be the IPPSS assessment, "of core melt accident sequence likelihoods as a starting point in developing our own."

Just to be clear, here you are referring to your own consequence and containment analysis. Is that correct?

A (WITNESS ROWSOME) No. No. We used the critique of the IPPSS as the basis for arriving at our estimates of accident sequence likelihood. They are not the same as those in the IPPSS in most cases, but we did use the IPPSS as a starting point for our investigation. That led to the numbers we are using to answer Commission Question 1.

Q And those, as modified by the Sandia critique then, are those the numbers that went into your evaluations in NUREG-0850?

A (WITNESS ROWSOME) No, they are an outgrowth that go beyond the material in NUREG/CR-2934. To that work was added some additional analysis done by the staff and some additional judgments, and the numbers we are using to answer Commission Question 1 are kind of a third generation.

If you take IPPSS as the first generation, and this document, 2934, as second generation, I would characterize the numbers we are using as third generation.

Q So your analysis of NUREG-0850 did not rely on the IPPSS evaluation or the Sandia critique of the IPPSS evalulation?

A (WITNESS ROWSOME) Not at all. That is correct.

Q So you used those evaluations just for the analyses done in the remainder of your testimony, the staff testimony, in your consequence and containment analysis?

A (WITNESS ROWSOME) The 0850 work is a progress report on a line of inquiry performed by the staff on which we will get much more testimony by Dr. Meyer at Section 3-B of the testimony, and he is far better qualified to discuss it than me.

Q Well, just my question on the adoption of these assessments from IPPSS and Sandia, for purposes of the staff analysis, would you feel that your conclusions and your analyses would be suspect if the IPPSS and Sandia numbers were unreliable or untrustworthy?

A (WITNESS ROWSOME) If the Sandia numbers were unreliable and untrustworthy, I think our numbers would probably share that attribute.

I think the review done to arrive at the Sandia report is sufficiently comprehensive that most, if not all of the hypothetical errors or distortions that might have taken place in the study, like the IPPSS, would have been found and recognized and corrected.

Q You stated in your testimony that what was disavowed by the Lewis Committee report was the executive summary of the reactor safety study.

A (WITNESS ROWSOME) Not true. It was not the Lewis Committee. It was the Commission, in response to the Lewis Committee report.

Q On Page 14 of your testimony, you state that both the Lewis Committee report and the Commission statement encouraged careful use of probabilistic risk assessment, especially for setting priorities for regulatory attention. Did the Lewis Committee address the issue or problem of using PRA as a bottom line evaluation of risk of a nuclear plant?

A (WITNESS ROWSOME) In a general sense, no. In the specific sense of the reactor safety study, yes. They concluded that the uncertainties were larger than those identified in the reactor safety study.

Q I would just like to get your opinion on one recommendation from the Lewis Committee report on Page XI of that report, and it states, "In general, avoid use of probabilistic risk analysis methodology for the determination of absolute risk probabilities for subsystems unless an adequate data base exists and it is possible to quantify the uncertainties."

MS. MOORE: Mr. Chairman, could the witnesses see that report?

JUDGE GLEASON: Does the witness need to see that report?

WITNESS ROWSOME: I don't know whether we will or not. It depends on where the question goes. We don't yet need it, but it may happen.

JUDGE GLEASON: All right. Let's go on.

BY MR. HARTZMAN: (Resuming)

Q But is this statement a recommendation as to the use of --

A (WITNESS ROWSOME) I think you are right. It is.

Q Let me complete my question. The use of PRA as a bottom line analysis for the risk of operating a nuclear power plant?

A (WITNESS ROWSOME) It sounds that way.

Q So it would be your sense from this recommendation that the Lewis Committee did express concern about the use of PRA for bottom line risk evaluation of a nuclear power plant. Is that correct?

A (WITNESS ROWSOME) Seems so, yes.

JUDGE SHON: Mr. Hartzman, that statement, if you will excuse me, was quite heavily qualified.

MR. HARTZMAN: I am aware of that.

JUDGE SHON: They implied that if you had proper data base and things like that, you could well do this, so it might be, if it isn't now, a condemnation.

MR. HARTZMAN: My question -- I indicated they expressed concern, a great concern.

JUDGE SHON: Thank you.

BY MR. HARTZMAN: (Resuming)

Q So if the results of the IPPSS and the numbers that you used from the Sandia critique of IPPSS were to be found untrustworthy or unreliable, this kind of concern expressed by the Lewis Committee would apply to the staff analyses. Is that not correct?

A (WITNESS ROWSOME) Yes.

Q On Page 7 of your testimony, in Answer 11, you begin by stating that risk assessment is a discipline of constructing mathematical models that estimate risk. Do you recognize a distinction between risk and uncertainty as two different concepts?

A (WITNESS ROWSOME) Yes.

Q Could you explain your understanding of that difference?

A (WITNESS ROWSOME) Risk is a concept which entails likelihood and severity of outcomes in mathematical terms, something like probability times frequency times consequences. One can attribute uncertainty to the estimation of probability or consequences, so one can speak of uncertainty of risk.

Q Might it be the case in doing probabilistic calculations, determination of risk by multiplying your probability times consequences, might it be that there was such a wide band of uncertainty that you cannot conclude what the risk is?

A (WITNESS ROWSOME) One frequently finds himself in the position that one cannot determine with precision what the risk is.

Q What do you mean by precision?

JUDGE GLEASON: What do you mean by precision?

MR. HARTZMAN: Well, is he talking about -- you know, when we are talking about a band of uncertainties, and he is saying that there might be situations in which the result is not precise. I am wondering what he means by precise.

WITNESS ROWSOME: I would refer you to my testimony in Section 4-C of the uncertainties in the staff testimony.

BY MR. HARTZMAN: (Resuming)

Q Well, that is just for a hypothetical. Assume there was an uncertainty band on a probability of six orders of magnitude. Would you be able to state with some confidence what the risk of that occurring would be?

A (WITNESS ROWSOME) To that precision, yes.

Q Of six orders of magnitude?

So that when you state on the bottom of Page 12 of your testimony that such limitations, which I referred to before in the previous paragraph, that such limitations make PRA's rather unreliable at predicting the precise magnitude of risk, so you refer that there might be situations in which there is just too much uncertainty to rely on it for determinations of risk? Is that what you mean by that statement?

A (WITNESS ROWSOME) It depends on the accuracy you need for the decision process you are making. I am identifying here that there is always an uncertainty associated with reactor risk assessment. It is a substantial uncertainty, and if one is trying to draw an inference that requires precision greater than that available in the analysis, then you can't do it. If you have a decision algorithm that can accommodate uncertainty, you may well be able to make the decisions you need to make.

Q I think this may be my last or next to last question.

So that you then go on to say that they are successful in identifying many, if not all of the ways a reactor may be vulnerable enough to severe accidents to warrant shutdown or remedial action. They are also valuable as a method with which to estimate importance of safety issues. Is that -- When you say that their value as a method of estimating the importance of safety issues, is that a method of refining, do you mean by their means of refining how the problems and bugs in the system can be ironed out and eliminated?

A (WITNESS ROWSOME) Among other things, yes.

Q So that among other things, a PRA may be useful for that kind of debugging project, even though it may not be useful for overall evaluation of the risk in the system. Is that correct?

A (WITNESS ROWSOME) That could be the case, yes.

Q And that would determine how uncertain your risk estimates are. Is that correct?

A (WITNESS ROWSOME) It might be a consequence of the uncertainty. I don't think it would determine the uncertainty.

Q Let me just rephrase my question. If the uncertainty in your bottom line risk numbers are such that you may not be able to rely on a PRA for overall evaluation of risk, it may still be useful for debugging?

A (WITNESS ROWSOME) Yes.
Q And that is what you meant in making this distinction in your testimony?
A (WITNESS ROWSOME) Among other things, yes.
Q Are you aware that PRA's have been used in airplane design to work out bugs in the design process?
A (WITNESS ROWSOME) I am aware that it has been done. I am not familiar with the details.
Q Do you know whether it was used for debugging while still in the testing phase, or at a time when it was already in commercial planes in commercial use?
A (WITNESS ROWSOME) Both. Roger points out to me that these are -- the aerospace application is to system reliability analysis and the like, and is probably not properly characterized as a comprehensive risk assessment.
MR. HARTZMAN: Thank you. I have no further questions.
JUDGE GLEASON: Any redirect at this point, Ms. Moore?
MS. MOORE: I have no redirect at this point.
JUDGE GLEASON: Mr. Brandenberg, would you like to proceed? A very few questions, right?
MR. BRANDENBERG: I will make every effort to accommodate you, sir.

CROSS EXAMINATION ON BEHALF OF CONSOLIDATED EDISON

BY MR. BRANDENBERG:

Q Dr. Rowsome, welcome back to these proceedings. Brent Brandenberg, you will recall, attorney for Con Edison.

Dr. Blond, I will be asking you some questions on behalf of Con Edison.

Gentlemen, my first question relates to your Answer 15, which starts at Page 11 and continues on, in which you address the -- what you characterize as the principal strengths and weaknesses of PRA, and my question is whether you are familiar with the Commission orders establishing this proceeding that were dated January 8th, 1981, and September 18th, 1981.

A (WITNESS ROWSOME) I have read them, but not recently. My recollection is a little fuzzy.

Q Do you recall the general words in both of those orders to the effect that while PRA did indeed have strengths and weaknesses, it nonetheless comprised the best means available for evaluating the risk of nuclear power plants? Do you recall words to that effect?

A (WITNESS ROWSOME) It would not surprise me if they were there, but I cannot validate that from my memory.

Q Dr. Blond?

A (WITNESS BLOND) I would have the same response.

Q Do you generally agree with that statement?

A (WITNESS ROWSOME) Yes.

A (WITNESS BLOND) Yes.

Q Moving on to Page 12, if we may, which is also part of your Answer 15, I would like to ask you a question about the last sentence in the last full paragraph, which states as follows: "We are not very good at predicting the likelihood that operators might misdiagnose an accident and so employ the wrong procedures."

You are referring, are you not, to PRA techniques and their ability to evaluate these events?

A (WITNESS ROWSOME) I should probably constrain that to the staff as the "we."

Q As a general rule, when attempting to model operator error using PRA techniques, would you agree that there would be a convergence between what you would expect to see in real practice regarding the incidence of operator error and the accuracy of modeling of such error that would be achievable through PRA in the situation where there was increased operator training relating to the diagnosis of accident initiators?

A (WITNESS ROWSOME) Yes.

Q Now, has there in fact been substantially increased operator training relating to the initiators for accidents as part of the Commission's overall post-TMI response approaches?

A (WITNESS ROWSOME) Yes.

Q And similarly, would you agree as a general principle there would be a convergence between the actual likelihood of operator error and the ability to model such error in PRA techniques in the situation where there was an extensive program of simulator training for the operators whose performance we were attempting to model?

A (WITNESS ROWSOME) Yes.

Q Are you gentlemen aware that simulator training is in place at both of the Indian Point units?

A (WITNESS ROWSOME) Yes.

Q You are also aware, I trust, that there were efforts to model operator error in the IPPSS study, and indeed Dr. Swain and others will be addressing that later in this proceeding? Is that correct?

A (WITNESS ROWSOME) That is correct.

Q Based on this, gentleman, do you have a general view or confidence level, and I am not really seeking precision, but do you have a general view as to the success which the IPPSS study had in attempting to model the frequency of occurrence of operator error?

MS. MOORE: Mr. Chairman, I would object. This is not the panel that --

JUDGE GLEASON: I was going to make the same objection, Ms. Moore. I am glad you did.

Mr. Brandenberg, you are falling in the same trap, I might say, the problem of wasting time, as did Mr. Blum.

MR. BRANDENBERG: I will withdraw the question.

BY MR. BRANDENBERG: (Resuming)

Q Gentlemen, my next question relates to Answer 23 in your testimony that appears on Page 20. And as I understand the context of this answer, it relates back to your discussion of the Indian Point short term risk study which we have referred to in this proceeding as the 60-day study, and I believe your discussion of that topic begins on Page 17.

Am I correct that Answer 20 on Page 20 indeed relates to the so-called 60-day study?

A (WITNESS BLOND) Yes, it does.

Q I was particularly interested in your passage, actually, the last two sentences in that answer, that starts, "If the Indian Point plants suffer," et cetera, et cetera, through to the end of that question.

And my question is, if, rather than suffering some unique vulnerability, the Indian Point plants had some unique strength that was not revealed by using the well-trodden path from WASH-1400 that was used in the 60-day study, better pipes, more reliable pumps, something like that, would that have created a bias that would have tended to overestimate the risk of Indian Point plants?

In other words, is this not a two-way street here?

A (WITNESS ROWSOME) Yes, it could be.

Q Gentlemen, later, in answer to your question on Page 21, at approximately the middle of the page, you state that the IPPSS study was estimated to have taken 50 man years of effort. My question is whether either -- I am going to ask you first about the Sandia review, and then the staff's internal review -- if either of you gentlemen have any comparable approximate estimation of the amount of man months or man years, whichever index you would like to use, for the Sandia review of the IPPSS study.

A (WITNESS ROWSOME) Well, counting both Sandia and staff review of the IPPSS, I would guess it is in the neighborhood of five to ten man years.

Q Do you have any ability to distinguish between the Sandia review on the one hand and the IPPSS review on the other? I only ask because we are dealing with a separate battery of witnesses, really. Or is that a cleavage you don't feel comfortable making?

MS. MOORE: Mr. Chairman, I would object on the grounds that I don't see really how this question is relevant to the subject of the witnesses' testimony.

JUDGE GLEASON: Well, I keep making the same point, Ms. Moore, but nobody is listening to me.

Do you want to answer his question?

WITNESS ROWSOME: I would say there have been about two staff years per Sandia year. I have not looked up the numbers.

BY MR. BRANDENBERG: (Resuming)

Q Thank you.

My next question relates to Page 24 of your testimony. It appears on the top of Page 22. And we are in a bit of a time warp here, but as I understand the historical context, your answer speaks to the period in the spring of 1980, long before the IPPSS project got started.

I am particularly interested in your passage that -- this is the second sentence in the answer -- "The basis for this action was the evidence that these plants may pose a disproportionate share of the societal risks compared with other commercial nuclear power plants by virtue of the comparatively high population density surrounding these plants."

Now, do I correctly understand that this was the prevailing view in February, 1980?

A (WITNESS ROWSOME) It was the prevailing view that they might do so by virtue of that factor.

Q Now, was this possibility the result of some staff analysis performed at about that time, that is, about February, 1980, which modeled the Surrey plant, if you will, that is, the WASH-1400 plant, with all of its frequencies of error and so forth, but artificially transposed that Surrey plant to the Indian Point site, and it really modeled all of the accident sequences and their frequencies and so on that were found to have been used in WASH-1400 and modeled them instead on the Indian Point site?

Is that, generally speaking --

A (WITNESS BLOND) The work had been done probably two years prior.

Q Now, subsequent to 1980, did the staff have occasion to model both the plant specific as well as the Indian Point site characteristics in a way that combined the actual Indian Point plants, if you will, with the actual Indian Point site?

A (WITNESS ROWSOME) Was your question prior to this date?

Q No, subsequent to this date. Well, I will be more specific. Was that done in the interim operations task force report that you refer to later, which is 0715?

A (WITNESS ROWSOME) Yes.

Q Did the conclusions of that report, the NUREG-0715, essentially disprove the possibility that Indian Point plants may pose a disproportionate share of societal risks compared with other plants?

A (WITNESS ROWSOME) "Disprove" is a little strong. Established the plausibility that they might well not.

Q Did the NUREG-0715 report find as one of its principal conclusions that the overall risk of the Indian Point plants was about the same as a typical reactor on a typical site?

A (WITNESS BLOND) Yes.

A (WITNESS ROWSOME) Yes.

JUDGE GLEASON: Mr. Brandenburg, we are well familiar with these studies. If you want to keep piling them in the record, why, go ahead. It's not helping the Board any.

BY MR. BRANDENBURG: (Resuming)

Q There was a good deal of questioning yesterday, and you were indeed asked some questions by Mr. Blum, on the question of sabotage and its treatment in PRA, and my question relates to the PRA procedures guide, NUREG-2300. Are you familiar with that document, gentlemen?

A (WITNESS BLOND) Yes.

A (WITNESS ROWSOME) Generally, yes.

Q Do you have a copy of that with you here today?

A (WITNESS BLOND) Yes, we do.

Q Did you review that in connection with the preparation of your testimony?

A (WITNESS ROWSOME) I did not.

A (WITNESS BLOND) Not specifically, no.

Q Do you have any understanding as to the statements made about the modeling of sabotage using PRA techniques in the PRA procedures guide?

MR. BLUM: I would object. This is beyond the scope of the testimony.

MR. BRANDENBURG: I believe it relates to questions that Mr. Blum posed to these witnesses, Mr. Chairman.

JUDGE GLEASON: Did you ask anything about sabotage, Mr. Blum?

MR. BLUM: I didn't ask anything about the modeling of sabotage in the PRA procedures guide, which these witnesses have not used in connection with their testimony.

JUDGE GLEASON: Mr. Blum is correct.

MR. BRANDENBURG: In that case, Mr. Chairman, I have no further questions.

JUDGE GLEASON: Power Authority?

MR. COLARULLI: Your Honor, the Power Authority has no questions.

JUDGE GLEASON: Any cross or redirect?

MS. MOORE: I have no redirect, Mr. Chairman.

JUDGE GLEASON: The Board has a few questions.

BOARD EXAMINATION

BY JUDGE SHON:

Q First of all, Mr. Blond or Mr. Rowsome, in the course of cross-examination Mr. Blum asked you a few things about the ACRS. Now, we are aware that at least two ACRS members had some rather harsh words about the present state of the art about PRA. They in fact called it a sham, if I'm not mistaken, in writing; is this correct?

A (WITNESS ROWSOME) I believe they did say that, in the particular context of measuring compliance with safety goals and not in a broader context, but yes, I am aware of that.

Q What is the Staff's attitude toward this ACRS position that indeed it is not a proper way of approaching safety goals? Do you think that this is a reasonable position or is it extreme or what?

A (WITNESS ROWSOME) I believe it is the position of the Staff that it is a little presumptuous to imagine that the state of the art is such that we could reliably measure compliance with thresholds of acceptable risk.

Q I am struck by one thing, and this is quite another subject, actually. Are you familiar with Godel's theorem, G-o-d-e-l?

A (WITNESS ROWSOME) Yes, I'm aware of it. I am not very up to speed on it.

Q Something you said on page 12 reminded me vaguely of it, the statement of yours that an attempt to be comprehensive is bought at a considerable price. Godel states that for a system of logic, which PRA certainly is, you cannot be both comprehensive and totally consistent.

A (WITNESS ROWSOME) That's correct.

Q Do you suppose you're bumping into that rather than Heisenberg?

A (WITNESS ROWSOME) I don't think we've reached that point yet. I think we run into practical limitations before we get to the Godel limitations.

Q I notice Mr. Brandenburg pointed out the statement that you made also on page 12 to the effect that we are not very good at predicting the likelihood that operators might misdiagnose an accident and so employ the wrong procedures. And when asked to identify the "we" you said you meant the Staff.

I notice that in the previous testimony that we have had from the writers of the IPPSS, they told us that for matters concerning operator response and human response they relied on a document, NUREG/CR-1278, "Handbook of Human Reliability Analysis, Emphasis on Nuclear Power Plant Applications."

Since that has a NUREG number, it would seem to me that it has a modicum at least of Staff sanction.

A (WITNESS ROWSOME) Yes.

Q Now, my question is, if the Staff doesn't believe that these kinds of numbers are very reliable, then what about the fact that other people seem to be relying on a Staff document or a document sanctioned by the Staff?

A (WITNESS ROWSOME) Yes. You'll have more expert testimony on the subject by the authors of that document a little later, but I am sure you want me to address the question independently.

There are some areas of human reliability analysis that are much more reliable, much more trustworthy, than others. The kinds of things we can do well are the assessment of errors in maintenance and surveillance, such as inadvertently leaving a valve in the wrong alignment after maintenance, small departures from accepted and appropriate procedures, a maintenance man who grabs for the wrong switch or the wrong valve or miscalibrates something.

That kind of thing can be done fairly coherently, where the data base is imprecise but there is a data base. Where human reliability analysis becomes less reliable is in the realm of the broad cognitive pattern recognition problem mentioned before and, for that matter, in the realm of the creativity that an operator can bring to bear, the imaginative fix that wasn't in the procedure, the imaginative jury-rigging of equipmoent to get one through a tr ing time.

We know such things happen. We cannot model them and have not attempted to model them in the PRA.

Q With regard to sources of error in the PRA and the general reliability of the PRA and the Indian Point probabilistic safety study in particular, it seemed to me at least, as one member of this Board, while listening to what the witnesses yesterday had to say and what you had to say that there are two fundamental kinds of errors.

One is a dubiousness about the data base that can be assigned to individual parameters that enter into a calculation, and this dubiousness can be carried through the calculation in a very simple manner. It is more arithmetic than I care to do with a pencil and paper, but it can be carried through to produce a folded error making use of some assumed distribution for the parameters and you get a distribution and hence 90 percent confidence from the final answer.

But there's another kind of error, and that is the kind in which one says there's the complete omission of a sequence or something. The witnesses yesterday made some attempt to talk about that, but I think didn't address it fully.

What assurance do we have that that doesn't just completely swamp the kind of 90 percent confidence that you calculate from the base there, for example, the fact that you just missed something?

A (WITNESS ROWSOME) I believe the cumulative effect of modeling approximations in completeness issues does in fact swamp in most cases the uncertainty originating from statistics, input data.

Q You think it does?

A (WITNESS ROWSOME) I think it does.

Q If that is true, then let me put to you a hypothetical theorem that I've heard before, that there is some bottom noise level, if you want, some background level of human error and human mistakes, that says the best a human being or a group of human beings can do is some fixed number, that the chance that things will go wrong is like one in a thousand, like 10^{-3} or something.

And if you do a calculation that shows the chance that something will go wrong is one in a million, then all that really shows is that the chances are a thousand to one that you've missed something. Is it possible that we're confronted with a situation like that, that these one in a million years, one in a hundred million years, one in a billion year figures that we have been talking about for severe accidents are the result of just not having gone down the right path?

A (WITNESS ROWSOME) Well, it is certainly true that the smaller risk prediction from the PRA is, the less confidence I have in its accuracy. But as I pointed out earlier, when you are calculating individual accident sequences there are some occasions in which you find a large number of coicidences that you know are pure coincidences. They are not coupled. And while you can have a great deal of confidence in a very small number for an individual accident sequence, but generalizing to a plant, you are right.

I do think that it is difficult to attribute reliability to the very small answers that have come out of some.

Q One last question or possibly a pair of questions on a totally different subject. This is a question I should perhaps really have asked the witnesses yesterday. I had noted it down. But as experts in probabilistic risk assessment and what has been done on the PRA's that we have seen, I think one of you two gentlemen could probably set me straight on it.

We discussed, particularly with Dr. Kaplan yesterday, if you were here, we discussed in some detail the quantity lambda or the failure rate of various devices.

A (WITNESS ROWSOME) Yes.

Q And he spoke of lambda as if it were simply a number, a concept. Now, it seems to me that is not necessarily true. Indeed, lambda may be a function of time, in that the usual case is that lambda is high when a device is first installed and being debugged, and that it drops to some constant level, and then when the things starts wearing out lambda goes back up again. You have what they sometimes call a bathtub-shaped curve. It goes down, levels, and then goes back up.

When the lambdas were selected to go into the IPPSS and subsequent studies, were they simply treated as constants or was some allowance made for the fact that equipment does wear out and that the failure rate, the probability of failure at any rate, goes up very high for old equipment?

A (WITNESS ROWSOME) You will get from both the Licensees and the Staff better witnesses than we to answer that question. So I suggest you bring it up. But I think I know the answer and I think that is that no explicit accounting for wearout or burnin problems was made, and that the statistics of failure since the plant went into service were just treated as though it were an estimate of the uniform hazard rate, uniform failure rate.

Q Well, as I say, I perhaps should have asked that yesterday, but I'm sure we'll get it from some other witness. That seems to be rather an odd approximation, because if you ever tried to drive an old car or run around the block with an old body you discover that things do wear out.

A (WITNESS ROWSOME) It was our philosophy in giving the Staff answer to the Commission question that we were attempting to calculate the risk at particular periods of time. We present two families of results: one our version of the IPPSS, that is, one version of the risk as we believe it was true of the plants as they were designed and operated in 1981; and another number that credits some fixes made since and that we believe to be applicable to 1983.

We have not projected future wearout effects or the learning curve, that things may get better. So that I would say that our own risk projections would become progressively less reliable the further in the future one goes.

Q I guess I'm almost as troubled by the notion that this accident will only happen once in 100,000 years, but that figure is only good for the next year and a half. I'm troubled by that. You know, there's only a one in a million chance, but there's a chance in ten that I'm wrong, that we had yesterday, if something was internally inconsistent.

Well, thank you. That's all I have.

JUDGE GLEASON: The witnesses can step down.

We'll now take a very short break and proceed with the Staff's next witnesses.

(Whereupon, at 11:02 a.m., the hearing in the above-entitled matter was recessed, to reconvene at 11:23 a.m. the same day.)

(11:23 a.m.)

Whereupon,

DENNIS C. BLEY

DENNIS C. RICHARDSON

and STANLEY KAPLAN,

recalled as witnesses by counsel for the Regulatory Staff, having previously been duly sworn by the Chairman, were examined and testified as follows:

(Witnesses sworn.)

Whereupon,

ROBERT E. HENRY

NICHOLAS J. LIPARULO

HAROLD F. PERLA

and RICHARD H. TOLAND,

called as witnesses by counsel for the Regulatory Staff, having first been duly sworn by the Chairman, were examined and testified as follows:

MR. BRANDENBURG: Mr. Chairman, I might introduce them before the Board asks questions. Sitting at the end here is Dr. Richard Toland. Sitting next to him is Dr. Robert Henry. Sitting next to him, Mr. Nicholas Liparulo. Next to Mr. Liparulo is Mr. Dennis Richardson, who has testified before before the panel. Next to Mr. Richardson is Dr. Dennis Bley, who has similarly testified previously. Next to Dr. Bley is Dr.

Stanley Kaplan, a previous witness. And next to Mr. Kaplan is Mr. Harold Perla.

DIRECT EXAMINATION

BY MR. BRANDENBURG:

Q Dr. Toland, starting with you, please, will you state your full name. And perhaps we can shorten this by asking you to state at the same time your place of business and your position.

A (WITNESS TOLAND) My name is Richard Toland. I'm with United Engineers and Constructors in Philadelphia, Pennsylvania. I am the Manager of the Structural Analysis Group.

A (WITNESS HENRY) My name is Robert E. Henry. I'm with Frosky and Associates, Burr Ridge, Illinois. I'm a principal with the firm.

A (WITNESS LIPARULO) My name is Kick Liparulo of Westinghouse Electric. I am Manager of Core Containment Consequence Analysis, and I am in Pittsburg.

A (WITNESS RICHARDSON) My name is Dennis E. Richardson. I work for Westinghouse Electric Corporation, Pittsburg, Pennsylvania. I am Manager of Risk Assessment Technology.

A (WITNESS BLEY) My name is Dennis Carl Bley. I work with Pickard, Lowe and Garrick in Irvine, California. I'm a consultant.

A (WITNESS KAPLAN) I am Stanley Kaplan. I am consultant to Pickard, Lowe and Garrick in Irvine, California.

A (WITNESS PERLA) I am Harold Perla. I'm a consultant with Pickard, Lowe and Garrick in Irvine, California.

Q Do each of you gentlemen have in front of you a document entitled "Licensees' Testimony on Commission Question 1 and Board Question 1.1," dated January 20, 1983?

A (WITNESS BLEY) Yes.

A (WITNESS HENRY) Yes.

A (WITNESS KAPLAN) Yes.

A (WITNESS LIPARULO) Yes.

A (WITNESS PERLA) Yes.

A (WITNESS RICHARDSON) Yes.

A (WITNESS TOLAND) Yes.

Q Did you participate in the preparation of this testimony or was it prepared under your direct supervision?

A (WITNESS BLEY) Yes.

A (WITNESS HENRY) Yes.

A (WITNESS KAPLAN) Yes.

A (WITNESS LIPARULO) Yes.

A (WITNESS PERLA) Yes.
A (WITNESS RICHARDSON) Yes.
A (WITNESS TOLAND) Yes.
Q Other than the listing of errata dated February 6, 1983, do you have any further changes, additions or corrections to this testimony?
A (WITNESS BLEY) No.
A (WITNESS HENRY) No.
A (WITNESS KAPLAN) No.
A (WITNESS LIPARULO) No.
A (WITNESS PERLA) No.
A (WITNESS RICHARDSON) No.
A (WITNESS TOLAND) No.
Q Is everything contained in this testimony true and accurate to the best of your knowledge, information and belief?
A (WITNESS BLEY) Yes.
A (WITNESS HENRY) Yes.
A (WITNESS KAPLAN) Yes.
A (WITNESS LIPARULO) Yes.
A (WITNESS PERLA) Yes.
A (WITNESS RICHARDSON) Yes.
A (WITNESS TOLAND) Yes.
Q Do each of you adopt this document as your testimony in this proceeding?

A (WITNESS BLEY) Yes.
A (WITNESS HENRY) Yes.
A (WITNESS KAPLAN) Yes.
A (WITNESS LIPARULO) Yes.
A (WITNESS PERLA) Yes.
A (WITNESS RICHARDSON) Yes.
A (WITNESS TOLAND) Yes.

MR. BRANDENBURG: Mr. Chairman, this document was received into evidence by this Board yesterday. I will not move that it be accepted once more, of course.

These witnesses are now available for cross-examination.

JUDGE GLEASON: Is there, Mr. Brandenburg, one of these witnesses who wants to summarize what this testimony consists of?

MR. BRANDENBURG: Dr. Bley can, I think, summarize the area of concentration of this panel, and also Mr. Richardson for Westinghouse.

WITNESS BLEY: Thank you. The testimony of this panel addresses analysis in the IPPSS and since the publication of IPPSS. Specifically, it describes the selection of initiating events, the analysis of the response of the plant systems and operators to those initiating events, and the containment response analysis.

With respect to initiating events, we developed a complete set of initiating event groups, complete in the sense that before severe core damage can develop one or more of these groups must occur. We searched further for what I will call common cause initiating events, events which cause either more than one of the initiating event groups to occur or both cause an initiating event and to some extent degrade the performance of the systems useful in controlling the progress of the event sequences.

This process included reviewing systems design and systems interactions, as well as postulating severe environmental events or what in IPPSS is called external events.

With respect to the plant response analysis, we performed a detailed event sequence analysis that included supporting systems and data analyses and the response of the plant and structures to external initiating events. The result of this phase of our work is a set of frequencies for reaching various plant damage states, for which essentially all of the events within them, the sequences within them, look nearly the same to the containment response analysis.

Mr. Richardson will continue our summary with a description of the containment response work.

WITNESS RICHARDSON: In the core and containment area, our testimony will show that we have very high confidence, that we are very certain that the Indian Point containments, Unit 2 and 3, are most difficult to fail. This can be broken up generally into two areas.

For those degraded core sequences that don't bypass the containment, when we have minimal containment safeguards available, we believe it is almost impossible to fail the containment, looking at the analysis and the research conclusions we have. Basically a realistic assessment of the degraded core situations where we have minimal safeguards shows that the maximum expected pressure is less than the design basis pressure, around 60, 62 psi.

When we add great conservatism to this in terms of adding on the hydrogen burns and at the worst times adding in the steam spikes and other conditions that you will see in the testimony, even then we wouldn't expect the maximum containment internal pressure to go above 100 psi.

To repeat one more time, our most realistic, our best estimate analysis, would show that the maximum pressure would not even exceed design basis, and that with added conservatisms and nonrealistic models added to this, would show that the containment pressure even in these cases would probably not exceed 100 psi. We believe that the containment adds safeguards to this plant in terms of public risk, that even with uncertainties, perhaps initiating event frequencies or frequencies of core melt scenarios, that the containment itself will not fail.

Now, in cases where we do not have containment safeguards or heat removal systems available, even then we are sure it will take a very long time, in terms of many hours, for the containment to fail, which contributes greatly to the reduction in terms of public risk.

So I guess the story that really is in the testimony in terms of core and containment is our certainty and our expectation that the Indian Point containments are very difficult to fail. They will not fail under most circumstances that would be credible in terms of degraded core, and they add a great mitigation in terms of public health and safety.

JUDGE GLEASON: All right, thank you.

Mr. Blum, if this array of witnesses doesn't discourage you, would you like to proceed?

MR. BLUM: Mr. Hartzman is prepared to go forward now. We thought it would be faster.

CROSS-EXAMINATION ON BEHALF OF
INTERVENORS FOE AND AUDUBON

BY MR. HARTZMAN:

Q I will address this to the panel. I'm not sure whether perhaps more than one individual will answer or help answer this question.

JUDGE GLEASON: If we could kind of get some rule of operating that when you say something like that it doesn't require seven answers, it really would be helpful. So try to answer with a couple of good ones.

WITNESS RICHARDSON: We will, Your Honor.

JUDGE GLEASON: Is that satisfactory?

MR. HARTZMAN: That is satisfactory.

BY MR. HARTZMAN: (Resuming)

Q What are the major uncertainties that you have identified in the plant analysis, in the IPPSS?

A (WITNESS BLEY) Within the plant analysis there are a variety of uncertainties, all of which contribute to the uncertainties in our final answers. Maybe the best approach would be to talk in terms of several different plant damage states.

For the case of the interfacing systems LOCA, the major uncertainties -- and they are very large -- have to do with the plausibility of the failure modes of the valves involved in the failures. We are looking at a situation where no valves in service in power plants have ever failed in this mode, the rupture of a disc through that is seated. We are sure that the chance is very low that this will happen, but because it has never happened the uncertainties are rather large.

That is the first one. With respect to events leading to the plant damage states that eventually go into the 2RW release category, the uncertainties are primarily coming from our analyses of external events -- seismic, fire and wind -- and the uncertainties are both in the frequencies of the initiating events itself, very rare events in themselves, and for the case of seismic and wind the fragilities of equipment to those initiating events.

And finally, for the cases in which the containment remains intact, the categories 8A and 8B, the uncertainties are primarily coming from uncertainties in initiating event, frequency of internal events, and uncertainties in the failure rates of specific pieces of equipment.

Q Does any other panel member wish to add to that?

A (WITNESS KAPLAN) Yes, I would like to add something on this whole notion of large uncertainties. This relates to Dr. Bley's answer right now and also to the question you put to Mr. Rowsome earlier in your sixth order of magnitude notion.

This has to do with what is a large uncertainty. If you know that the frequency of an event is no bigger than 10^{-6}, but it might be as low as 10^{-12}, there you have six orders of magnitude. On the other hand, the whole range of that uncertainty is less than one times 10^{-6}.

So is that a big uncertainty or is that a little? So if you look at it that way it is a very small uncertainty. So just the point is, when you get to very small quantities, in terms of orders of magnitude or multiples, you can have many, many multiples, many orders of magnitude, but that is still a very, very minute quantity.

So percentagewise you can have huge uncertainties, percentagewise, but it amounts to still a very small uncertainty.

Q But Dr. Kaplan, at those very small probabilities wouldn't your analysis be more demanding in terms of trying to get a reliable number at those very low levels?

A (WITNESS KAPLAN) No. Here again, it depends on what you mean by reliable. If you are content with the statement that it is less than 10^{-6}, end. If you want to know the difference between 10^{-9} and 10^{-10}, if that is an important difference to you, that is a very demanding question.

But the statement that it's in the neighborhood of 10^{-9}, 10^{-10}, or that it's less than 10^{-6}, that may not be very demanding.

Q So would it be the upper probability estimate that is of greatest importance in that analysis, then?

A (WITNESS KAPLAN) What is of greatest importance is the whole curve. Now, if you know that the upper end of the curve is somewhere in the range of 10^{-6}, it may not be of much importance to you to know whether the lower end of the curve is 10^{-12} or 10^{-15} or 10^{-50}. Do you get my point?

Q Would it be more important to know -- well, when you say it may not be important, is that for -- for what purpose?

A (WITNESS KAPLAN) For the purposes of decisionmaking, which is the only purpose for which we do this kind of analysis in the first place.

Q But when you have numbers at that level, even when you're trying to make decisions, is it more important to look at that upper probability estimate or at a median or mean or some average like that?

A (WITNESS KAPLAN) It is important to look at the whole curve, which is the whole story.

Q When you say it is important to look at the whole curve, could you explain what you mean by that?

A (WITNESS KAPLAN) Yes. I thought I explained that a lot yesterday.

The whole curve is the language in which we communicate our whole state of knowledge about the frequency in question. That is the whole truth, and in making decisions one should be aware of the whole truth. So that is what I mean when I say look at the whole curve.

Q You are aware of the principle of maximizing expected utility, are you not?

A (WITNESS KAPLAN) Yes.

Q And when you talk about taking decisions on the basis of these probabilities, it would be through that principle, is that not correct?

A (WITNESS KAPLAN) When I talk about making decisions?

Q Yes.

A (WITNESS KAPLAN) No. I personally don't use or push or advocate that way of making decisions.

Q What method do you advocate?

A (WITNESS KAPLAN) I say you calculate the probability curves for each branch of the decision tree, assess your uncertainty on all the outcomes, costs, benefits, risks, think of it as, if you like, a triplet of probability curves against costs, benefits, and risks. Look at that triplet for each of the decision options.

Clearly that is crucial. If you're making the decision, you have to similarly evaluate costs, benefits and risks of all options. You look at that triplet of curves for each option and you make your choice.

Q How do you make your choice?

A (WITNESS KAPLAN) There is no "how to." At that point you express your preference for the set of triplets. That is not a mechanical ritual. Preference is not something that comes out of a formula. You have to --

Q In your triplet where you weigh costs, benefits and risks, you do not put your preferences into that calculation, but this is made subsequent to that calculation?

A (WITNESS KAPLAN) That is true. The preference is not part of the probability curve against the cost, benefit and risk.

Q So you would not use the principle of choosing that option which maximizes your expected value; is that correct?

A (WITNESS KAPLAN) Of utility?

Q Of utility.

A (WITNESS KAPLAN) I personally don't make a big issue of the expected utility idea or the utility idea at all. I think it is a useful conceptual notion, but in terms of actual decisionmaking I don't stress the actual putting forth of the numerical utility function in doing, you know, the so-called multivariate utility and all of that.

Our experience is, my experience is, in decisionmaking that by the time you get to the stage of the decision where you have quantified what I call the hard science part of the problem, the costs, benefits and risks and the uncertainties involved in each of that, by that stage the decision is usually obvious. And if it isn't, if two branches of the decision are roughly of equal preference, that is also an answer to you the decisionmaker. You've got kind of a wash with those two.

Q Well, if you've got kind of a wash, how do you know? How can it be a matter just of preference, you know, to choose one or another, if it's just a wash?

A (WITNESS KAPLAN) When you said it's a wash, you've said that it doesn't matter to you one way or the other.

Q So it is not a question of ambiguity. It is just a question of not mattering to you.

A (WITNESS KAPLAN) That would be the definition of saying it is a wash. You see, at that point you express your preference.

Q Okay. So that --?

A (WITNESS KAPLAN) Usually my experience is one stands out.

Q So if you were to use the Indian Point probabilistic safety study conclusions as to risk and, let's say, you evaluate the upper and lower probabilities and you ended up in your decision approach that you could not tell one way or another from those numbers what to choose, those numbers would not be relevant; is that not so?

A (WITNESS KAPLAN) No, that is not so. If I can try to read your question back as I understand it, if you are saying that you go into a decision problem and two branches of the decision in your scheme of preference are of equal preference, that the numbers you use in evaluating those two branches, that the triplets of probability curves, if you are saying they're not relevant to the decision, I don't agree. They are relevant.

It is those probability curves which have led you to the position that these two branches stand out from the others and that they are essentially of equal preference.

Q I don't want to carry this very long, because I know I'm almost through with this. Since you do have, like in your risk curves, you have upper bound estimates and you have midrange and lower bound estimates, what if you were to be trying to make a decision on the basis of these different numbers, upper bound and let's say the median, and you were to look at that, and as you calculate your utilities one option -- you might have an option on whether or not the plant should continue to operate, and another option the plant should be shut down; and that if you use the upper bound probabilities you would get an answer that you're going to get consequences that would lead you, with your preference, given your preferences, to want to leave the plant open, and that -- let me withdraw that.

A (WITNESS KAPLAN) Can I answer the first part of your question?

Q I don't know what my question was now.

Given your preferences, you are willing to accept certain kinds of consequences or not, and if you calculate -- you're willing to -- you know, your preferences are that you may be willing to accept or the community may be willing to accept certain numbers of fatalities or latent cancers or certain economic costs.

And you look on your risk curve and you get a frequency for those kinds of consequences and you have a confidence interval placed on them. How would you decide, if you were to use the upper probability and the decision there was that if it would not meet your preferences you should shut down, but if you used the median value it would say keep the plant open? Then how would you use those numbers to decide what to do?

MR. COLARULLI: Your Honor, I would object to the question. I'm not sure it's a question. It's very confusing and convoluted. It is just too confusing to be answered.

MR. HARTZMAN: I would say we should see if the witness finds it confusing.

MR. HARTZMAN: Your Honor, the point is, for the record, the question is too confusing to be understood.

JUDGE GLEASON: Maybe his answer can simplify the question.

WITNESS KAPLAN: Yes, I think it relates back to the previous question. When you have an uncertainty about a frequency, you have a probability curve expressing that uncertainty, you don't make a decision based on any one percentile of that probability curve. You make it based on the whole probability curve, which expresses your whole state of knowledge.

I feel like I'm repeating myself, but that is the answer.

MR. HARTZMAN: I feel I'm repeating myself. I think I've gone far enough.

BY MR. HARTZMAN: (Resuming)

Q Again, this may be -- I'm not sure which member of this panel can answer this next question. What uncertainties have you identified in the containment analysis, in the IPPSS? What major uncertainties?

A (WITNESS RICHARDSON) I'll address that question first. In the containment analysis, we had a containment event tree that basically derives the conditional probabilities of, given a degraded core state, what is the conditional probability of some release category.

Okay, in the IPPSS there are figures and data and everything to give our best estimate for a realistic assessment of this containment event tree and the conditional probabilities therein. There is also an upper and a lower bound on this.

Now, the bounds on this -- first of all, let me address the containment event tree itself. It not only had in it what we would consider credible, realistic paths based on the laws of nature; we also included in the containment event tree paths that we felt were not credible, just to put them in because there were other people who thought about these types of paths. So they were also included in the point estimate analysis on the containment event tree.

The upper and lower bounds, we would go into the event tree and put variances on our probabilities and assume conditions we felt were not credible, that could not happen in terms of the laws of nature. And even with these variances on the probabilities, we would recalculate the containment event tree and we would find very little changes in terms of the final conditional probabilities for the rerelease categories. That is in general what we found.

And then for the particulars of what some of these variances were in terms of the uncertainties that we included in the containment event tree analysis, I will ask Mr. Liparulo and Dr. Henry to reply to that.

A (WITNESS HENRY) As Mr. Richardson has just pointed out, the containment event tree clearly delineates the uncertainties associated with the various nodes in the event tree. I think if you go through the event tree and come down to some very simple issues that have to be addressed, those of hydrogen burning in the containment which relate to how much hydrogen is produced as a result of this postulated accident sequence, and then also whether or not the debris is cool in the containment.

We have identified not only uncertainties we think are reasonable; if anything, we feel even more certain than what is listed in the report. We have also provided comparisons with all available experimental results.

One of the issues that I think was addressed in the report again would provide for one of the uncertainties, and again we gave very little credit, that of in-vessel coolability. Given the state of a degraded core, did the operator take any action to stop the accident while the material was still in the reactor vessel?

We gave only a credit of one out of ten. Certainly the experience of TMI shows us the operator can do that, and reviewing any operator procedures and what he would have in his availability we felt he was stronge than that, although we did not give it any additional credit.

A (WITNESS LIPARUTO) I don't have much to add to the previous discussion, except to say that the containment event tree in the IPPSS clearly states what phenomena and what uncertainties were part of these phenomena. And we were relatively insensitive, as Dr. Henry said, to the majority of the nodes in the containment event tree in terms of containment failure probability.

Q Dr. Kaplan, you recall yesterday I asked you if you were familiar with a work entitled "Games and Decisions" by Luce and Raiffa?

A (WITNESS KAPLAN) Yes, I recall.

Q And you indicated you had heard of the work, but I believe had not read it; is that correct?

A (WITNESS KAPLAN) That is correct.

Q Do you recognize it as a classic in the field of decision theory?

A (WITNESS KAPLAN) I believe that is so, yes.

Q You indicated in your resume that you are currently president of Kaplan and Associates.

A (WITNESS KAPLAN) That is correct.

Q A consulting firm specializing in risk analysis and applied decision theory. Is that correct?

A (WITNESS KAPLAN) That is correct.

Q And I think you also indicated yesterday that you are unfamiliar with the distinction between the decision problem under risk and the decision problem under uncertainty. Is that correct?

A (WITNESS KAPLAN) I am unfamiliar with the words that you are using. The underlying concept I may well be familiar with. I don't know quite what you mean by that.

Q Mr. Richardson, you indicated that you are risk assessment technology manager at this long-named division of Westinghouse Electric Corporation.

A (WITNESS RICHARDSON) That's right.

Q Are you aware of the lawsuit Con Edison has brought against your company with regard to the Indian Point plant facilities?

A (WITNESS RICHARDSON) I am aware there is such a lawsuit.

Q Do you know at all what the allegations are in that lawsuit, the charges against Westinghouse?

A (WITNESS RICHARDSON) No, sir.

MR. HARTZMAN: I have no further questions.

JUDGE GLEASON: Mr. Blum?

CROSS EXAMINATION ON BEHALF OF UCS

BY MR. BLUM:

Q This will be addressed principally to Dr. Bley and Dr. Kaplan, as I will begin with a discussion of plant analysis.

Do you have with you a copy of IPPSS?

A (WITNESS BLEY) We do.

Q Could you find in that copy Tables 8.3-3 and 8.3-2, which is in Section A on Page 8..3-14?

MR. BRANDENBERG: Mr. Blum, could you identify the volume of IPPSS?

MR. BLUM: That would be Volume 12, I believe, the summary volume.

MR. BRANDENBERG: We will need a moment, Mr. Chairman, to procure copies for the witnesses and for counsel.

MR. BLUM: An alternate source of the same information would be UCS Exhibit, I think, 5.

(Pause.)

WITNESS BLEY: Would you repeat the table numbers?

BY MR. BLUM: (Resuming)

Q 8.3-2 and 8.3-3.

I am sorry. The page numbers are 8.3-14 and 8.3-15.

JUDGE GLEASON: Go ahead, Mr. Blum.

BY MR. BLUM: (Resuming)

Q These tables list your summary probabilities of core melt for Indian Point Units 2 and 3, do they not?

A (WITNESS BLEY) They list our summary mean frequencies for core melt.

Q Yes. Thank you. And the tables also break up the outcome of core melt such that there are four categories which result in containment failure and two which result with containment intact. Is that correct?

A (WITNESS BLEY) That is correct.

Q One can determine the probability of, according to IPPSS, of core melt leading to containment failure by looking at the percentage of total core melt that is captured in the two categories, containment intact, can one not?

A (WITNESS BLEY) Again, the mean frequencies.

Q Yes, for the mean frequencies.

Looking at Indian Point Unit 2 -- this is table 8.3-2 -- the total frequency of core melt is 4.7×10^{-4}. Is that correct?

A (WITNESS BLEY) That is correct, in the IPPSS.

Q And in the IPPSS, if we add the two categories for containment intact we would receive a number approximately 1.4×10^{-4} for mean frequency, would we not?

A (WITNESS BLEY) That is correct.

MR. BRANDENBERG: Mr. Blum, excuse me for interrupting, but I really must ask, are you asking these witnesses questions about the IPPSS in its unamended form, or as amended by Amendment 1 to IPPSS?

MR. BLUM: I am asking them about IPPSS as it was initially delivered. We received amendments only a few days ago. If the amendments make substantial changes in the bottom line figures of IPPSS, I would request that witnesses be made available for cross examination on those at a later time. We can discuss this after the session, if you want.

MR. BRANDENBERG: These witnesses are here to testify on precisely those subjects, Mr. Blum.

MR. COLARULLI: Your Honor, these are the witnesses involved in Amendment 1.

JUDGE GLEASON: I would just suggest that if the amendments have changed the figures, all the witness has to do is point it out.

MR. BLUM: Your Honor, I would object to having to cross examine on the amendments to IPPSS at the present time, given the time which they were delivered to us and the absence of time in which to review them. I would be willing to cross examine on that at a later date, but not now.

JUDGE GLEASON: You can raise that objection at the time that it is appropriate to raise it. All I am suggesting is that that is one way of handling it from the witness point of view. If they don't want to handle it that way, there are other ways. You know, they can handle it. But I am not trying to take away your right to object to anything. Let's proceed.

BY MR. BLUM: (Resuming)

Q I would like to reserve the right to cross examine on amendments to IPPSS, but using IPPSS as it was originally delivered and published, the -- I believe you answered the previous question affirmatively, that the two categories combined for containment intact were added to mean values of one, approximately 1.4×10^{-4}, correct?

A (WITNESS BLEY) I did.

Q So according to this, containment will be intact slightly over 30 percent of the time in core melts, and it will fail in slightly under 70 percent of the time. Is that correct?

A (WITNESS BLEY) That was true when IPPSS was published. Here is a place where the amendment is quite different.

Q I will discuss the amendment later, but I am asking you specifically about --

MR. COLARULLI: Your Honor, I object.

JUDGE GLEASON: Hold it a minute, please. The Reporter cannot report four people at once. Let us let the witness respond the way the witness wants to respond, and then you can make your objections. And then we will listen to the person that has asked the question.

MR. BLUM: Your Honor, I presume --

JUDGE GLEASON: Please let the witness respond to that question the way the witness wants to respond.

WITNESS BLEY: Yes, Your Honor. The statement as Mr. Blum made it was true for the published version of IPPSS, but it is not true for the amended version, because the external events which are the dominant contributors to these containment failure cases are no longer as frequent as they were before the fixes that have been installed at the plant.

MR. COLARULLI: Your Honor, just as a point of information, the copy of the IPPSS that the witnesses --

that Dr. Bley has in front of him does not have included in it the amendment, and we are now getting Amendment 1, so that at the moment Dr. Bley does not have Amendment 1 in front of him.

JUDGE GLEASON: It presents some difficulty, obviously, from Mr. Blum's point of view, from the Board's point of view, and the witnesses' point of view.

MR. COLARULLI: Well, Your Honor, can I just say, the testimony as filed reflects Amendment 1.

JUDGE GLEASON: The testimony reflects Amendment 1, but when did Mr. Blum receive Amendment 1?

MR. COLARULLI: Your Honor, he received it, he personally received it -- I delivered it to him a week ago Tuesday. Prior to that time, Dr. Weatherwax was sent a copy directly.

MR. BLUM: Dr. Weatherwax has stated as of the time of his deposition that he had not in fact received any copy of Amendment 1 to IPPSS. I did receive it last week. I can't vouch for Tuesday.

MR. COLARULLI: Mr. Blum, it was the day of the deposition.

JUDGE GLEASON: All right. Let us proceed. If you received it a week ago, on the questions you ought to be reflecting that.

MR. BLUM: Pardon me?

JUDGE GLEASON: I say, if you received the document a week ago, and the testimony reflects the amendments, then you ought to be asking questions off the IPPSS as amended. And the only way that we can proceed if you are not able to do that is to ask the questions as you can, and let the witnesses respond that they are amended and to what extent they are amended. Let's proceed that way.

MR. BLUM: The form in which the testimony has been delivered makes it quite difficult to sort out what was in Amendment 1 from what was in the published version of IPPSS. I do not recall the precise date last week at which time I first received a copy of Amendment 1. It was substantially after the deadline for filing testimony before the licensees, and I have not been able to review that, and did not have notice that Amendment 1 was going to be substantially changing their major figures.

I would at this time like to cross examine simply on IPPSS as unamended, and if there is no other point in the hearings where we can hear from the licensee's witnesses on the amendments, that will simply be unfortunate all the way around, but at this point I would like to speak or to address the unamended version of IPPSS that was published.

JUDGE GLEASON: You may address it in any way that you want. You cannot restrict the witness from answering in the way that he has incorporating the amendments, particularly when the testimony before you has such amendments incorporated within it.

BY MR. BLUM: (Resuming)

Q Mr. Bley -- I am sorry, Dr. Bley, turning to Table 8.3-3, for Indian Point Unit 3, these tables appear to show, do they not, that the probability of containment failure, given a core melt, is approximately on the order of one-third. Is that correct?

A (WITNESS BLEY) The mean frequency appears to be on the order of one-third in the published IPPSS. It is on the order of one-fifth to one-tenth in the revised version. That is true.

A (WITNESS KAPLAN) Can I say something to that? Can I add something to that?

Q Go ahead.

A (WITNESS KAPLAN) Your use of the term, the probability of containment failure given a core melt, is a little misleading, because that conjures up mainly internal events leading to core melt. In that case, there is a very low -- given that core melt, there is a very low probability of containment failure.

What you are seeing here are the contributions of external events. External events are "common cause" events, these large seismic events, for example. Both cause core melt and failure of containment, late overpressure failure of containment.

MR. BLUM: Your Honor, at this time I would move that the schedule be amended to allow for cross examination on Amendment 1 of IPPSS during the second week of hearings, that some perhaps reasonably small portion of time be set aside for this.

It appears that the licensees have pursued a strategy of, after having had two years to prepare this study, changing their bottom line figures at the very last minute, while they also slip in a schedule to have cross examination of all of their witnesses pushed to the front of the hearings in order to successfully elude any probing search of the bottom line figures.

I would move that the schedule be amended at this point.

MR. BRANDENBERG: Your Honor, I would like to respond.

MR. COLARULLI: Your Honor, the Power Authority --

JUDGE GLEASON: Do both of you want to respond at once?

MR. BRANDENBERG: No.

MR. COLARULLI: No.

MR. BRANDENBERG: I think Mr. Blum's request betrays a fundamental misapprehension of what the testimony is on the one hand and what the IPPSS is on another. There are a plethora of literature resources that have been relied upon by all of the witnesses who have appeared here that provide underpinnings of their testimony.

However, the testimony of this panel is the testimony. It is the document entitled Licensees' Testimony on Commission Question 1 and Board Question 1.1, dated January 24. This document reflects the current state of risk assessments for the Indian Point plant as of January 24th, 1983.

It was filed timely unlike, I might point out, the testimony of some of Mr. Blum's witnesses, and Mr. Blum has had precisely the amount of time in order to prepare for his cross examination on these risk figures contained in this testimony as was contemplated by the original Board's schedule.

So, I find that Mr. Blum's motion was without merit, and we at Con Edison would oppose it vigorously.

MR. COLARULLI: Your Honor, if I could just add, as stated, the testimony that was hand delivered, served on January 24th, a courtesy copy given to Mr. Blum on January 25th, included Amendment 1, and Mr. Blum has had that amount of time to examine the testimony.

MR. BLUM: That is --

MR. COLARULLI: Second, the schedule on Question 1, Your Honor, as the licensees understood, we had in effect the initial burden of at least going forward on some of the evidence, and that is why licensees put these panels on first.

Third, as far as the actual amendment to the IPPSS, at the same time that I spoke with staff concerning providing the Board a copy of the amendment, we also directed that a copy be forwarded to Mr. Weatherwax out in California, and we hand delivered a second copy to Mr. Blum. So, I do not believe that we have in any way attempted to delay the expeditious timing of this proceeding.

MR. BLUM: I would note simply that some of Mr. Colarulli's factual assertions are incorrect according to my state of knowledge. Beyond that, regardless of what motivation we impute to the licensees, one would have to say that if there is no further cross examination on Amendment 1, that they have been successful in dramatically changing their bottom line numbers at the last minute and shielding this from cross examination.

MR. BRANDENBERG: Only if the last minute, Mr. Blum, is the deadline for filing Question 1 testimony, I would have to agree with you.

MR. BLUM: That was not what --

JUDGE GLEASON: Could we put a period at the end of this tete-a-tete? The motion is denied, Mr. Blum. If there is any opportunity some time next week when we finish all the other witnesses, and there is time left, we will bring back a witness who can talk about the amendment, and you have had a further opportunity for you to look at the amendment, we will do so.

We will not do so in any way of expanding or extending the schedule.

MS. FLEISHER: Your Honor, may I add a little P.S. to all of this? I purposefully didn't talk while you were considering it, but I do think that the schedule that we have been operating under, and I have refrained from making any comment about it sooner, has been prejudicial.

I think it has been very difficult for those of us who are in the high school league and not in the jets league to keep up with the material that we have been getting in the form that we have been getting it, a big pile of paper with a rubber band around it. We have had to separate it out ourselves and staple it. We do have testimony that doesn't even go according to panel. It doesn't even go according to witness.

And I think we are being put to a great deal of, what do I call it, discomfort by the licensees and also by the staff, and it is not the way these things usually function, and I think they should be reprimanded, and that we should be allowed to have the witnesses we need, if two or three of these gentlemen could come back and answer Mr. Blum at a later time.

Certainly the date, January 24th, was not the date I received any of this. I received my testimony February 1st. It is a great deal of difference in time. And I haven't even read all of it. And it is boring to sit here and not know what is going on, and it is the fault of the licensees.

JUDGE GLEASON: All right, Ms. Fleisher. Thank you for your comments.

JUDGE SHON: Just to clarify this in my own mind, as I understand it, the difference between the IPPSS as it was originally published and the IPPSS with Amendment 1 is that the document including Amendment 1 makes allowance for certain fixes that have either been proposed or accomplished which will reduce certain accident probabilities. Is that correct?

WITNESS BLEY: With respect to bottom line numbers, that is absolutely correct. There were some other corrections in that amendment, but they had no substantial effects on the results. The things that did were actual changes to the plant.

JUDGE SHON: Have those changes to the plant actually been accomplished?

WITNESS BLEY: As I spoke yesterday, some of them, I know, have been. I am not certain that all have been.

JUDGE SHON: That is the impression I got yesterday, and since that seems to be true, I think somewhere in our record we should have some feeling for or some definite record of what the plant looks like now, and what it is likely to look like in the immediate future when we are making our decision.

In other words, I don't want to make the decision based on a hypothetical plant that has fixes in it that haven't been done or perhaps not even capable of being accomplished. They haven't been thoroughly engineered yet, or something like that.

I would like to know what of this has been accomplished, when the remainder of it will be accomplished, and exactly which bottom line numbers each thing influences. Can we get some sort of information on that?

MR. BRANDENBERG: Judge Shon, I believe that Dr. Bley has that information with respect to Unit 2.

WITNESS PERLA: I would like to speak to two items that represent modifications, principally for the purpose of improving seismic capacity of the plant. One is, for Unit 2, the major contributor, the dominating contributor to the seismic initiated risk was the impact between the Unit 2 control building with the Unit 1 facility, the control building superheater combination, and to alleviate that problem.

The modification that has in fact been completed was to widen the gap and to put in seismic bumpers, rubber material between the two structure, so that it would essentially attenuate the impact between the two.

The consequence of that as reflected in Amendment 1 was to substantially reduce the seismic contribution.

A second analysis that was included in Amendment 1 was as a result of the NRC staff and Sandia reviews and analysis of the ceiling in the control room of Units 2 and 3, and while the revised analysis appears in Amendment 1 and does not particularly evaluate a modification, but simply points out the additional contribution as a result of potential failure of those ceilings, Consolidated Edison has in fact made a modification to the ceiling structure so that it precludes the failure as analyzed and essentially reduces to what it was before the contribution of that ceiling failure.

Essentially, those are two completed modifications for Unit 3. While the analysis has been completed, it is my understanding that the modification, while not yet having taken place, is about to take place in the near future.

JUDGE SHON: The modification to the control room ceiling?

WITNESS PERLA: Yes.

JUDGE SHON: Those are the chief differences, is that right, the seismic coupling between Unit 1 and the control room and the control room ceilings in both Units 2 and 3?

WITNESS PERLA: Those are the two in the seismic area. There were modifications made in the -- to alleviate the contribution of fire to core melt frequencies. Dennis?

WITNESS BLEY: I am not certain. I believe those are completed, but we will have to refer to the licensees and come back with that. I don't know with certainty.

JUDGE SHON: We would like to know.

JUDGE GLEASON: I guess Dr. Shon is asking the extent of the amendments encompassed in the amendments, and have you responded to that, how many changes have been made?

WITNESS BLEY: Yes, Your Honor. The key changes are the seismic changes in Unit 2 and the fire change at Unit 2. We know the seismic changes are complete. I believe the fire change is complete, but we will have to verify that.

JUDGE GLEASON: When you use a word like "key," it sends up a certain vibration. How many changes in total would there be? Singular changes?

And the reason I ask that question is, Dr. Paris has given me a letter here which I will have to ask Ms. Moore about, which tends to indicate that the amendment was only given to the Board under a cover letter of February 4, which indicates that we received in on February 7th.

And if that is the case, I think that kind of transmission problem may have occurred with Mr. Blum as well, and therefore I think that his point would be very well taken, that he has really not had an adequate opportunity to review this amendment.

Those are two unrelated questions I am asking, but if you want to respond first, Dr. Bley, about the number of changes that we are talking about.

WITNESS BLEY: I believe --

MR. COLARULLI: Your Honor, if I could just --

JUDGE GLEASON: Let's have his answer first.

WITNESS BLEY: I believe the -- Your Honor, there were just the three fixes at Unit 2 and a single fix, the fire fix, at Unit 3.

JUDGE GLEASON: Four all together?

WITNESS BLEY: Four all together.

JUDGE GLEASON: Constituting Amendment 1?

WITNESS BLEY: Amendment 1. That is correct.

JUDGE GLEASON: Mr. Colarulli?

MR. COLARULLI: Just as to the transmission of Amendment 1, of which Dr. Bley is not aware, again, I hand carried a copy of Amendment 1 to Mr. Blum, gave it to him at the deposition of Mr. Liparulo, Dr. Toland, and Mr. Richardson on February 1st. The prior week to that, Your Honor, we directed that a copy be sent directly to Mr. Weatherwax.

JUDGE GLEASON: Mr. Weatherwax is not directing this cross examination.

MR. COLARULLI: I understand, Your Honor, but he is the expert witness on PRA for the intervenors.

JUDGE GLEASON: Well, he is not here doing any cross examining. Mr. Blum is. The question is, when did he get a copy?

MR. COLARULLI: Mr. Blum has had that since February 1st, Your Honor.

JUDGE GLEASON: February 1st?

MR. BLUM: Mr. Weatherwax will be willing to swear when he arrives that as of last Saturday he had not received a copy of Amendment 1 and had not been able to review it.

JUDGE GLEASON: Ms. Moore?

MS. MOORE: Your Honor, this transmission problem between the staff and the Board was rather a unique one. The copies of the amendment were sent to us and had been distributed before it was remembered that the copies were numbered, and that the numbered copies had to match -- the amendment numbers had to match the copy numbers.

Therefore, we had to start a process of collecting all the amendments, and they will have to be redistributed. So we had to search for your copy. It was sent by letter on the 4th. We offered to hand deliver it, and I believe we were told that wasn't necessary, that Monday would be sufficient for the

Board. But I don't believe that unique problem has any relationship to whether Mr. Blum received his copy. That was an internal staff problem.

JUDGE GLEASON: I was not suggesting that it was, but I raise the question as to when Mr. Blum actually received it if that was, you know, typical of the transmission problems, and I understand it is a controlled document, and distribution is handled in a certain way.

And the only thing I can suggest is, I guess, back where we started, Mr. Blum, is that we will try to proceed the best way we can. If there is an opportunity to bring back a witness before this schedule is over, we will attempt to do so. I think if you have problems we will attempt to try to handle them in the context of information being given to you, if we can.

MR. BLUM: Your Honor, I would also move that on the information that Judge Shon has requested as to whether the various fixes have in fact been implemented, when they will be implemented, and possibly what risk reduction is attributable to each, that that be served on all parties as supplementary testimony, and that the licensees stand for cross examination on that, rather than simply giving information to the Board without any opportunity for cross examination on it.

JUDGE GLEASON: We will order that it be served on all the parties, and whether it will be considered as supplementary testimony we will decide when we look at it.

Why don't you proceed, Mr. Blum?

MR. COLARULLI: Your Honor, just for clarification, what is it exactly that you want served? Just so we know?

JUDGE GLEASON: If I understood Judge Shon, he asked for a statement as to when all of these fixes would be implemented.

JUDGE SHON: And what change each one resulted in in bottom line accident probabilities.

MR. COLLARULI: My answer to that is, it is in the testimony, Judge Shon. As we said, all the changes are reflected in the January 24 testimony.

JUDGE GLEASON: Is that latter question answered?

WITNESS BLEY: The testimony and in the amendment itself, it shows the effect of each of the changes.

JUDGE GLEASON: So then what we are asking is, when is it going to be implemented.

JUDGE SHON: And the implementation dates?

WITNESS BLEY: No, sir.

JUDGE SHON: Well, I want that, because I want to be sure that the plant that we are looking at is the plant that is really there.

MR. BRANDENBERG: I think the panel should be given the implementation dates, Judge Shon, right after the luncheon break.

JUDGE GLEASON: All right. Let's proceed.

MR. BLUM: I would also ask at this time for information as to whether there will be additional amendments to IPPSS brought before this proceeding.

JUDGE GLEASON: That is a good question. Are there to be any in the next four or five-month period?

WITNESS BLEY: There is work in progress right now, nearing completion, in several areas that I understand will be submitted as further amendments to the IPPSS.

JUDGE GLEASON: When will that be forthcoming? Do you know?

WITNESS BLEY: We don't have the exact dates.

JUDGE GLEASON: I think we were advised of this last week some time.

WITNESS PERLA: There is a sufficient amount of ongoing work that, as Dr. Garrick indicated yesterday, may go on for some extended period of time.

JUDGE GLEASON: We understand that the

probabilistic risk assessment work is an ongoing, continuing exercise, if you will, program, just as emergency planning is. All we are asking now is, when is your next amendment going to come?

WITNESS BLEY: Your Honor, we cannot give an exact date. There are a few minor things that are essentially ready now, but the more substantive work is still ongoing, and may be ready within -- some of it within a few weeks, some maybe longer.

WITNESS RICHARDSON: Just a second, Your Honor, if we could, please.

(Pause.)

WITNESS BLEY: One clarification point, I think. The current testimony that we have presented represents only information included within Amendment 1 with one exception, and that one we described under Question 2 testimony. That is that the direct seismic failure of the containment structure, the events that go into Category Z1Q, has been reanalyzed and is now not showing up as a contributor at all. That is the only thing that is not included in the amendment that is in our testimony.

JUDGE SHON: Do you know exactly what it was that changed, that made it not a contributor at all?

WITNESS BLEY: Mr. Perla can address that, and

I believe there is a letter.

WITNESS PERLA: There was a letter issued to the Commission. I am not quite sure of the date. February 4th, 5th, 6th, somewhere in there. In which it reflects the results of a recent analysis completed by the project team using more current criteria and more current design details on each of the two containments at Indian Point, and for these reasons substantially increases the capacity of both containments, to the extent that it is considered essentially not possible for them to fail under the maximum seismic conditions that are anticipated for the project, and as such, since they each represented the total contribution to Z1Q, and in part contribute to early fatalities, they have had a substantial impact for both units.

MR. COLARULLI: Your Honor, just so the record is clear, most, if not all of this additional work is not last-minute work, as Mr. Blum has characterized, but rather, has been in part a response to criticisms and reviews that staff and their consultants have prepared and some additional internal analysis based upon those highlights and criticisms and reviews.

So, in no way is it a last-minute effort by the licensees to change the numbers. Rather, it has been a part of a long, three-year process of preparing a study, receiving criticisms, looking more closely at those criticisms, and responsibly responding.

MR. BRANDENBERG: I believe Dr. Garrick yesterday remarked that PRA was a risk management tool, and I think both the licensees have used the results and the teachings of the IPPSS study to further improve the safety readiness of their plants.

JUDGE GLEASON: I really would prefer not to have these kinds of speeches go on any longer. If you do not object, I am going to call a recess now for lunch.

MR. BLUM: I have one standing objection I would like to put on the record very briefly, and then I would like to have lunch, too.

At this point I would, for the record, like to make a standing objection to all consideration by the Board of various amendments of IPPSS which have not been timely prefiled and made available for adequate cross examination.

JUDGE GLEASON: All right. That is a motion?

MR. BLUM: It is an objection.

JUDGE GLEASON: All right. We will stand in recess until 1:30.

(Whereupon, at 12:31 p.m., the Board was recessed, to reconvene at 1:30 p.m. of the same day.)

AFTERNOON SESSION

(1:40 p.m.)

JUDGE GLEASON: Can we proceed, please.

I wanted to make a very brief listing as to the why I understand the witnesses will be following each other for the rest of this proceeding, and in connection with that the Board has discussed the possibility, if we can't make better progress, of having evening sessions.

We intend to conclude this consideration of question 1 within the two-week period, and we'll just have to see how far we get today before getting our heads together to see where we're going with the rest of the schedule. But I want to put everybody on notice that there is that possibility.

After this panel is finished, we have a panel by the Staff of Israel, Hickman, Kolb, Easterling -- I can't make out my own writing -- Swain or something like that. That is followed by Butnitz, the Staff; that witness followed by Reed from the Staff.

MS. MOORE: Your Honor, if I may say, Dr. Budnitz and Dr. Reed, their separate pieces will be presented at the same time.

JUDGE GLEASON: That's good to hear.

That will be followed by Buchbinder, Kubicki of the Staff as a panel; followed by Mr. Rowsome again. That will be followed by Meyer and Pratt from the Staff as a panel. That will be followed by Mr. Levi from FOE, Mr. Farrell of FOE, to be followed by Bley, Potter, Walker from the Licensee, to be followed by -- I have Mr. Potter in several places here; maybe he appears in several -- by Stratton and Roger, and then Potter again; to be followed by Mr. Sholly for UCS, followed by Mr. Weatherwax, the panel from FOE of Fascello and Pitzioni; followed by the Staff's witness Archarya; followed by a panel of Archarya and Blond; followed by Mr. Nesse from the Staff, Mr. Soffer from the Staff, Mr. Codell from the Staff.

MS. MOORE: Your Honor, again, those people we put up, they are different people: Archarya, Blond, Nesse, Soffer and Codell.

JUDGE GLEASON: That is one panel?

MS. MOORE: Yes.

JUDGE GLEASON: Thank you.

That will be followed by Blond and Rowsome, will be followed also by Blond and Rowsome; panels on two different subjects, followed by Mr. Rowsome alone; and then Sears and Joyce; is that correct?

MS. MOORE: Yes, sir, that is correct.

JUDGE GLEASON: All right. Followed by Kaplan from the Licensee, followed by Rowsome for the Staff, followed by, I have, Potter from the Licensee. I don't know whether he fits there again. Followed by Blond, Staff, and then Richardson and Bley from the Licensee, Gardner and Sidel from FOE, Woods and Klecker from the Staff, and we finally wind up with Dupont.

That is the order we have, and as I indicated before, there may have to be a minor adjustment, depending on where we are at the time, but that in any event the Board intends to finish these witnesses in this two-week period. If that calls for night sessions, we're going to be doing it.

So would you proceed, Mr. Blum.

MR. BLUM: Yes. Could I ask when the pending motions to strike testimony of Dr. Cohen and Dr. Dupont will be heard?

JUDGE GLEASON: We will consider the one on the Licensee's motion tomorrow morning, and we will consider yours toward the end of the week.

MS. MOORE: Your Honor, does that mean the motion will not be considered at the time the witness takes the stand? It will be considered some time before that?

JUDGE GLEASON: I just assumed -- perhaps you'd rather not do it that way, but I just assumed that if the witness is not going to appear you'd like to let him know that before he got here.

MS. MOORE: Mr. Chairman, there is testimony they have not moved to strike. He'll be here anyway.

JUDGE GLEASON: All right, we can hold that motion until that time, until he appears.

MR. BLUM: I'm sorry. Is it contemplated that one of the witnesses will actually be testifying before the motion to strike is heard?

MS. MOORE: No.

MR. BLUM: Oh, I was just confused.

WITNESS BLEY: Your Honor, I received some information over the lunch break with respect to the questions the Board had asked us before.

JUDGE GLEASON: All right.

Whereupon,

DENNIS C. BLEY
DENNIS C. RICHARDSON
and STANLEY KAPLAN,
ROBERT E. HENRY
NICHOLAS J. LIPARULO
HAROLD F. PERLA
and RICHARD H. TOLAND,

the witnesses on the stand at the time of recess, resumed the stand and, having previously been duly sworn by the Chairman, were examined and testified as follows:

WITNESS BLEY: With respect to the Indian .oint 2 fixes that are described in amendment 1, the fire and seismic control building fixes were implemented prior to the unit being returned to service on December 29th, 1982. And the control room ceiling fix was implemented by February 1st, 1983.

I still don't have information on the Unit 3 fix with respect to fire.

JUDGE GLEASON: Thank you.

Mr. Blum.

CROSS-EXAMINATION -- RESUMED

ON BEHALF OF INTERVENOR UCS

BY MR. BLUM:

Q Dr. Bley, did any information indicate any fixes which have not been implemented yet?

A (WITNESS BLEY) No, those were the only fixes in amendment 1 with respect to Unit 2, and they have been completed.

JUDGE PARIS: Dr. Bley, would you summarize the situation with respect to Unit 3 as you understand it now?

WITNESS BLEY: Unit 3 has committed to accomplish the Appendix R fire fix that is described in amendment 1. I am not sure whether they have completed it or not. I believe it was in progress some time ago. I don't know the exact status.

JUDGE PARIS: And the control room ceiling, is that a problem in Unit 3 or is it fixed?

WITNESS BLEY: I don't know whether there is a fix plan for that ceiling or not. Perhaps Mr. Perla can address that.

WITNESS PERLA: My understanding is that it was planned to be accomplished in the near term, similar to what was accomplished by Unit 2, but it has not yet been accomplished.

MR. COLARULLI: Your Honor, if I could just note, we are still checking with the responsible people at the Power Authority to determine some more definite information for you concerning these plants.

JUDGE PARIS: I thank you.

JUDGE GLEASON: Mr. Blum.

MR. BLUM: Your Honor, it occurs to me that there may be one thing that has gone out of order. The panel here is the principal panel familiar with the breadth and range of the Indian Point probabilistic safety study. But unless I missed something, the Licensees have not offered the Indian Point probabilistic safety study into evidence, and I would therefore be willing to step back for a moment to allow them to do so.

(Pause.)

MR. COLARULLI: Your Honor, the IPPSS study, the Indian Point probabilistic safety study, was attached, even if somewhat symbolically, since it had already been delivered to the Board and to Mr. Blum, and there is a footnote in the testimony that says that the study is attached as an exhibit to the testimony.

JUDGE GLEASON: It has been offered as an exhibit?

MR. COLARULLI: Yes, Your Honor.

JUDGE GLEASON: Does it have a number?

MR. COLARULLI: It does not. We offered the testimony and the exhibit to it, as I understood it, together.

JUDGE GLEASON: So then we can consider it is a part of the record because it has been admitted in the testimony?

MR. COLARULLI: That is my understanding.

MR. BLUM: Just to be perfectly clear, it has been admitted as evidence and it can be cited for the truth of the matter asserted?

JUDGE GLEASON: Including the amendment.

MR. BLUM: I would objecet to the amendment being admitted at this time, but we can deal with that later.

JUDGE GLEASON: No, Mr. Blum, we won't deal with it later. The Board overrules your objection.

I presume that when you offered that it was with the amendment?

MR. COLARULLI: Yes, Your Honor.

JUDGE GLEASON: And you are objecting, Mr. Blum?

MR. BLUM: Yes, I am objecting to --

JUDGE GLEASON: Your objection is denied. It has been admitted with the amendment.

MR. BLUM: This is -- when he said admitted as an exhibit, this has been fully admitted so it may be cited in findings of fact; is that correct?

JUDGE GLEASON: Yes, that is correct.

MR. BLUM: Thank you.

BY MR. BLUM: (Resuming)

Q Dr. Bley, just to pick up on something we were discussing a second ago, the fire fixes for Unit 2 and Unit 3, these are both the Appendix R fire fixes of 10 CFR Part 50?

A (WITNESS BLEY) The fix for Unit 3 is the Appendix R fix. I cannot tell you with certainty about Unit 2's fix, if that is meant to address Appendix R or not.

Q Does anyone on the panel know?

A (WITNESS BLEY) The fix is described in the amendment, what is described in the fix and the affect on the risk. So the fact of whether it addresses Appendix R or not for purposes of our risk assessment does not really have any bearing. We looked at the exact nature of the fix in our evaluation.

Q So is the answer no one on the panel knows whether this is the Appendix R fix or not for Unit 2?

A (WITNESS BLEY) That is correct.

JUDGE PARIS: Excuse me for a moment. Your testimony on page 56 says, "The risk from fire at both units is low because of the implementation of plant modifications in compliance with the requirements of Appendix R of 10 CFR Part 50." Right about in the middle of the page.

So apparently somebody must have put that sentence in your testimony. I don't know which of you is responsible. It also implies that the Indian Point 3 fix may not be completed. It says "is in the process of implementing."

BY MR. BLUM: (Resuming)

Q So strictly speaking, the fire fixes were not implemented in response to discoveries of IPPSS; they were implemented in response to federal regulations; that is correct, is it not?

A (WITNESS BLEY) With the constraint that the affects on risk were considered when the Licensees were weighing alternative fixes that would satisfy the Appendix R criteria. So the fixes may have looked differently if the risk study had not been in place.

Q Dr. Bley and Dr. Kaplan, you recall the discussion yesterday about the prior drafts of IPPSS being disposed of, do you not?

A (WITNESS BLEY) I do.

Q And I believe you've testified that the various numbers in IPPSS would have gone through a large number of iterations, possibly as many as 10 or 15; is that correct?

A (WITNESS BLEY) Specific calculations may have gone through that many iterations.

Q You have also previously stated that you do not recall the changes in the prior drafts, with one or two exceptions that you have mentioned at a deposition; is that correct?

A (WITNESS BLEY) We spoke about that in the deposition, as I recall.

Q Yes. It is still your position that you don't recall the different numbers in the prior drafts other than those?

A (WITNESS BLEY) We have not reconstructed the history since that deposition, that is true.

Q And at the time of your deposition you said you did not know whether there may have been substantial changes and what were the major contributors to risk from one draft to another.

A (WITNESS BLEY) Mr. Blum, you mean as far as drafts, actual drafts of the IPPSS, not calculations along the way, am I correct?

Q Well, if the answer differs for those two, let me ask for both separately.

A (WITNESS BLEY) Well, I can say I know that among the very many calculations that were part of the evolution of the IPPSS study and some of the things we talked about in the deposition where extremely conservative assumptions were modified, some numbers changed very dramatically. We know that.

Once the rough calculations had been worked out to the point where we had complete draft material for the IPPSS, then I'm not -- the answer is a little different. I can't say that things changed very dramatically.

Perhaps you can add something, Dr. Kaplan.

A (WITNESS KAPLAN) I can't add, but I can confirm, once the draft was assembled, subsequent to that I don't remember any significant changes. As Dr. Bley said, in the course of evolving the numbers, numbers changed a lot, yes.

Q Are you aware that at any time during the course of the study people within the NRC may have been given a list of major contributors to risk and a ranking that was substantially different from what appeared in the published version of IPPSS?

A (WITNESS BLEY) A specific response, I am not aware of the ranking contributors to risk that were delivered to the Nuclear Regulatory Commission. I am now aware, and I did not recall it at the time of the deposition, that some early draft material on systems analyses had been transmitted to the Commission for their earlier review, to see how the progress of the study was going, and that material did not draw any bottom line results with respect to risk.

And of that material, there is only one really substantial change I am aware of, and that was the unavailability of contribution from service water system failure, and some change between that draft and the final report was due to, again as an amendment 1, changes in the plant and procedures at the plant that were implemented before the final report was published, to resolve some large contributors to unavailability of that system that the study had discovered.

Q Is anyone on the panel aware of the time at which members of the NRC may have been told, on the basis of the iPPSS study, that the ranking of contributors to risk was substantially different from the ranking that subsequently appeared in the published version?

A (WITNESS RICHARDSON) I'm not aware of any transmittal that would have gone to the NRC along that line. I believe most of the transmittals would have been published out of Westinghouse and I don't recall any.

A (WITNESS HENRY) I don't recall of any.

JUDGE GLEASON: Let's do this in a more efficient way. If anybody knows of the information raise your hand; we'll give him a mike. Otherwise we'll assume your answer is no.

Okay, Mr. Blum?

MR. BLUM: The record should reflect that the answer is no, none are aware.

BY MR. BLUM: (Resuming)

Q Do any of you know with reasonable assurance that in fact there were no such transmittals, that the list of contributors never changed substantially?

A (WITNESS KAPLAN) Is that two questions or one?

Q Good point. First of all, do any of you know with some reasonable degree of assurance that there were no transmittals of any kind to the NRC ranking different levels of risk, different contributors?

JUDGE GLEASON: Isn't that the question you just answered?

MR. BLUM: No. The earlier question was whether they know of specific changes, and they said they didn't know. I'm now trying to pin down whether they simply don't know whether there were any or whether they claim to know that there were none. That is the difference I'm trying to get.

JUDGE GLEASON: Gentlemen, once again, raise your hand if you know there were none.

Mr. Richardson knows something.

WITNESS RICHARDSON: I just wanted to make sure I understand the question. One more time, please.

JUDGE GLEASON: Rephrase the question.

BY MR. BLUM: (Resuming)

Q Does anyone on the panel know with reasonable assurance that prior to the publication of IPPSS there were no transmittals of any kind to the NRC which identify a substantially different ranking of contributors to risk than appeared in the final version of IPPSS?

A (WITNESS RICHARDSON) I can only say with reasonable assurance, I am not aware of any official transmittal to the NRC of prepublished information with respect to rankings.

Q Including unofficial transmittals?

A (WITNESS RICHARDSON) Then I can't say.

Q So the answer for the entire panel is that no one knows one way or the other; is that correct?

JUDGE GLEASON: None of them have raised their hand, so the answer is no for each of them.

MR. BLUM: Thank you.

BY MR. BLUM: (Resuming)

Q Dr. Bley and Dr. Kaplan, you are aware of the testimony of James Meyer under question 2, are you not? Excuse me. I'll address that simply to Mr. Bley.

A (WITNESS BLEY) I am aware of it. I don't recall it in detail.

Q Do you recall the point that came out in cross-examination, that there was an earlier version of that testimony which showed a risk reduction factor of 20, and that was later amended on the basis of some fixes at the plants to be more in the range of 2 to 5? Are you aware of that?

A (WITNESS BLEY) I don't remember that.

Q So you do not know whether the fixes that the Staff would have been using are the same ones that are contained in amendment 1?

A (WITNESS BLEY) I cannot speak to exactly what they used, that is correct.

Q Thank you.

In the published version of IPPSS, was credit taken for the risk reduction due to the ATWS fix at Indian Point Units 2 and 3?

A (WITNESS BLEY) The ATWS fix was assumed to be in place in the published version of the IPPSS.

Q Thank you. And in general, you stated before that the core melt probabilities in the published version of IPPSS are somewhat higher than the core melt probabilities in the reactor safety study; is that not correct?

A (WITNESS BLEY) I don't recall stating that, but it is true.

Q Thank you. However, the bottom line risk numbers in IPPSS are much, much lower than those in the reactor safety study, are they not?

A (WITNESS BLEY) In our testimony to, I believe it is, question 5 we have detailed comparisons with respect to early effects. The risk at Indian Point is the IPPSS is much lower than in WASH-1400. I don't recall exactly the comparison with respect to latent effects.

Q That will be covered in your question 5 testimony?

A (WITNESS BLEY) Yes, it will be.

Q Now, the difference in early effects, meaning early fatalities and early illnesses, is quite substantial, is it not, between IPPSS and WASH-1400?

A (WITNESS BLEY) It is.

Q On the order of how many orders of magnitude, would you say?

A (WITNESS BLEY) I can't recall that. I don't recall. I would have to look at the two curves. Our risk curves are presented in our testimony explicitly.

Q But it is fair to say that that difference is attributable, not to core melt probabilities, but to differences in the modeling of containment response analysis and site consequence analysis, is it not?

A (WITNESS BLEY) That's essentially true. The modeling through the core melt includes the modeling with respect to plant damage states, and WASH-1400 didn't lay out the analysis in exactly that form. So there might be a difference in that area, too, which I cannot speak to.

Q It is fair to say that by and large the plant analysis in IPPSS was performed by a different team of researchers than performed either the containment response analysis or the site consequence analysis. That is correct, is it not?

MR. BRANDENBURG: Mr. Chairman, I believe this entire subject matter was inquired into fully and answered yesterday by Dr. Garrick and Mr. Goeser.

JUDGE GLEASON: I've forgotten what the answer was. Respond, please.

WITNESS RICHARDSON: It was a team effort, an interaction that formed the entire study.

BY MR. BLUM: (Resuming)

Q But in terms of who had principal responsibility for the parts and did the bulk of the work on the different parts, this was a different team of researchers for the plant analysis?

A (WITNESS RICHARDSON) Are you talking about the IPPSS study?

Q Yes, I am.

A (WITNESS RICHARDSON) The IPPSS study was a very focused team that reviewed every part of the work that was done. To the best of my knowledge, there were individuals of various organizations that did a lot of the technical work, but everything that went into the report was through a team review and a team effort.

Q Dr. Fley, in addition to the overall team review, the bulk of the work that was done along the way was done by a different team of researchers, was it not, in the plant analysis, as opposed to consequence and site analysis and containment?

A (WITNESS BLEY) The plant analysis, for example, was accomplished, I would say, primarily under -- and I think Dr. Garrick said this yesterday -- under the auspices of Pickard, Lowe and Garrick. However, people from the other groups involved in the study were deeply involved in that work, spent time at our offices working with us, not only a final review but a continuing review and modeling assistance effort as we went along.

So it was indeed a team project. But as Dr. Garrick did say yesterday, various smaller groups had primary responsibility for each effort in the study. And that is true even within our own offices. Different groups within Pickard, Lowe and Garrick did the majority of the work in different areas of the plant analysis, but there was a lot of cross-effort within our own organization and with respect to the other members of the IPPSS team.

Q Roughly what percentage of the work on the containment analysis was done by Pickard, Lowe and Garrick?

A (WITNESS BLEY) Pickard, Lowe and Garrick had representatives of, I would say, three to five people, involved in a good many week-long sessions with the Westinghouse people, and Fausky and Associates, people involved in the work.

So I cannot give you a percentage. The majority of that work was done by Westinghouse, but we had a fair number of people spending a reasonable amount of time as the work was being conducted and in the reviewe process.

Q Would it be fair to say that between Westinghouse, United Engineers, and other non-PLG people, that would account for at least 90 percent of the work on the containment response?

A (WITNESS RICHARDSON) That might be a rough estimate, but I would have no way of exacting it to that detail.

Q Thank you.

Dr. Kaplan, returning again to these tables at 8.2-2 and 8.2-3.

(Pause.)

Do you have those tables now?

A (WITNESS KAPLAN) Yes, I do.

Q The figure in the lower right corner of 4.7×10^{-4}, and on the other table 1.9×10^{-4}, those represent mean total core melt estimated frequency; is that correct?

A (WITNESS KAPLAN) They are mean core melt frequencies.

Q And that is according to your state of knowledge?

A (WITNESS KAPLAN) That is correct.

Q And a fuller expression of your state of knowledge would be a curve which included an additional point that represented the assertion that -- excuse me -- that you were 90 percent confident that the core melt probability or the core melt frequency would be no greater than one times 10^{-3}; is that correct?

A (WITNESS KAPLAN) The curve is in here. Are you asking me to read the 90 percentile from the curve?

Q Yes.

A (WITNESS KAPLAN) I don't remember the number but I'll take your reading. It's somewhere around there.

Q Okay. And both of these -- in other words, you believe there is a higher probability that the frequency is 4.7×10^{-4}, but there is a 10 percent probability that it could be even greater than one times

10^{-3}. That is correct, is it not?

A (WITNESS KAPLAN) There's 10 percent probability that it would be greater than the 90th percentile.

Q Which corresponds to one times 10^{-3}.

A (WITNESS KAPLAN) Somewhere around -- I'm not necessarily agreeing to your number, but it's somewhere around there.

Q I think the numbers I've got were taken off some table in the vicinity of that.

A (WITNESS KAPLAN) I'm assuming you did that right. I'm just saying I don't remember the 90 percent.

Q Now, you testified earlier that your state of knowledge includes all your experience and all your judgments based on that experience, and that data and so forth; that is correct, is it not?

A (WITNESS KAPLAN) I am saying that that is what we mean by probability assignment, yes.

Q If we were to imagine some other hypothetical risk assessors that had all the intelligence and resources and data base that the authors of IPPSS do, but they were different simply in the fact that -- simply in two respects, one, they personally favored the shutdown of the Indian Point plants, which you do not;

and they were hired by, say, a regulatory agency that also favored the shutdown of the plants -- it is likely, is it not, that the total experience of these researchers would produce figures somewhat higher than yours?

A (WITNESS KAPLAN) I am not going to agree with that. If in your hypothetical example the two assessors are completely rational and have the same total information base, then they will come up with the same probability. Now, if you want to say --

Q This is based on the assumption that you are completely rational; is that correct?

A (WITNESS KAPLAN) Yes. If they are behaving rationally and they have the same totality of information, then they will come up with the same probability. Now, if they have some motives which color their rationality, then anything can happen.

Q But in your case, you believe you embody pure rationality and have no motives that are coloring your --

A (WITNESS KAPLAN) Mr. Blum --

MR. COLARULLI: I object.

JUDGE GLEASON: I think he ought to answer the question.

WITNESS KAPLAN: Mr. Blum, in this study we have done the best job we know how to assess the risks as we see them. Our job is to assess the risk, not to minimize it, not to maximize it, and not to make decisions based upon it. We are not the decisionmakers. We are the risk assessors.

BY MR. BLUM: (Resuming)

Q Yes, I know.

A (WITNESS KAPLAN) And as far as your implication here that because we are personally in favor of nuclear energy we would make these numbers look small, that is contrary to the truth. Because we are in favor of nuclear energy, we have a lot of personal interest in seeing that the plants are safe. And the state of mind of a risk analyst and the whole purpose of the study is to find things that may have been overlooked, to look in all the dark corners. That is what we mean by assessing risk.

We don't serve anybody, we don't serve our clients or ourselves or anybody, by presenting an inaccurate picture of our state of knowledge.

Q I understand that. I wasn't intending to impute any bad motives to you. I was simply inquiring as to whether there is some probability that different analysts having different viewpoints on these things would come up with different numbers, because, as you have stated before, their viewpoints and all of their experience are part of their total state of knowledge.

A (WITNESS KAPLAN) If you are conceding that they have different total states of knowledge, then indeed they could have different probability.

Q But isn't it likely in the hypothetical that I have presented that they would have somewhat different states of knowledge?

A (WITNESS KAPLAN) Well, ar two real human beings have different total states of knowledge. It is not only likely; it is observably --

Q Correct. And it's also conceivable that the difference in state of knowledge would be of some interest and relevance insofar as it pertained to different attitudes toward nuclear power, toward keeping the plant open, different presuppositions about how to save the plants?

A (WITNESS KAPLAN) Certainly presuppositions and attitudes would have relevance to the results they come up with. I'm not sure I would admit those things into the category of bodies of information.

Q But it would be in the category of state of knowledge, would it not?

A (WITNESS KAPLAN) No, no. A presupposition or an attitude, I would rule that out of the rational domain.

Q You would agree that all of you have presuppositions and attitudes, do you not?

A (WITNESS KAPLAN) I would agree that we have attitudes and presuppositions. We are human. By our professional discipline, our training, our intent to be "as objective as we can" and to not have our judgments and our assessments colored by presuppositions and attitudes. I mean, that's --

JUDGE GLEASON: Is this line of questioning going anywhere?

MR. BLUM: Yes, it is.

JUDGE GLEASON: Would you please enlighten me where it is intended to go?

MR. BLUM: If I can have about three or four more questions, that might do it.

JUDGE GLEASON: All right.

BY MR. BLUM: (Resuming)

Q So from what you said earlier, you would rule your presuppositions and attitudes outside of the rational domain; is that correct?

A (WITNESS KAPLAN) Well, what we are meaning by attitudes and presuppositions and the way we are using those words, we are using them to mean positions, attitudes, not based upon facts and experience and knowledge, but rather a predisposition from some unrelated experience or something like that.

I would rule them out, because that is the sense in which we are using those words now, as I understand the conversation. I think we are talking about things that are not rational when we're talking about attitudes in the sense we're talking about it now.

When you ask whether somebody's attitude can color his judgment, I think you're meaning the non-rational part of his consciousness.

Q What we have been trying to get at in our earlier questioning, and have not succeeded so far --

MR. BRANDENBURG: I like to hear that, Mr. Blum.

JUDGE GLEASON: Mr. Brandenburg, please.

BY MR. BLUM: (Resuming)

Q -- is to identify some objective methodology which will succeed in separating out the prior attitudes and predispositions which you term nonrational from the kernel of rationality which you would like to achieve in pure form, and we would like an explanation of that specifically with regard to the process by which uncertainty estimates were derived.

That is, was there a procedure to check the possibility of bias and subjectivity and predisposition, and can you explain why that procedure would have been successful at doing so?

A (WITNESS KAPLAN) Mr. Blum, the way in which we have separated out the various categories of information, E-1, E-2, E-3, is very carefully laid out, and I spoke to that, and I can some more if it is not clear.

Q I know. I understand that. I remember in your prior testimony. I am asking, is there anything else other than what you have said so far?

A (WITNESS KAPLAN) Sir, the E-2 and the E-3 are objective in the sense that they are agreed upon, all right. The E-1 you would call subjective in the sense that each person has his own prior, all right. What you are driving at is that different experts might have different priors and that might affect the final results, and we did deal with this yesterday.

We said we acknowledged different experts could put forth different priors, and to the extent that their priors influenced the final results, to that extent the final results could be different. Now, what we did, the priors that we used were -- reflect the judgment of the team and they reflect also a desire on the part of the team to not be biased.

They represent in general a bending over on the part of the team not to inject any prior bias into the prior. And the way they reflect that is that the prior distributions are taken broad, broader than even we think they should be.

Q Do you have anything else to add?

A (WITNESS KAPLAN) Yes. We might repeat again that peer reviewers have looked at this issue, have examined the effects of different priors. I believe Sandia did this and came up with the conclusion that had you chosen even wider priors, it would not make much difference in the final results. What else can I say?

Q All right. Mr. Richardson, with regard to the failure pressure of the containment at Indian Point, you are aware that -- are you aware of the testimony of Drs. Meyer and Pratt of the NRC staff?

A (WITNESS RICHARDSON) I am somewhat familiar. Yes, sir.

Q And you are aware that their estimate of the probability of the containment failing in different pressures is substantially different from that in IPPSS?

A (WITNESS RICHARDSON) I am aware of some analysis. I didn't believe it was substantially different, that it would make any effect on our results.

Q Well, is not the following probability distribution an approximately correct description of the graph provided in IPPSS? At 141 psia, there is 100 percent chance of the containment having failed, using onset of yielding as the criterion of failure. At 139 psia, there is approximately 10 percent chance that it will have failed. And that at 136 psia, there is approximately a 1 percent chance it will have failed. Is that correct?

A (WITNESS RICHARDSON) I believe that is the correct interpretation of that curve that you are looking at. Yes, sir.

Q That curve, could you identify where that is in IPPSS, please?

A (WITNESS RICHARDSON) I gave you the number of that right after the lunch break. I did not write it down myself. 2.5 dash something, 16, I believe.

Q 2.5-16. Does anyone have a copy of IPPSS so that we could open that up? Unfortunately, we were not able to get that volume.

A (WITNESS RICHARDSON) Yes, we have it.

Q Now, that curve which attributes a 1 percent chance of containment failure at 136 psia, was that the basis of calculations in IPPSS?

A (WITNESS RICHARDSON) This curve was used, yes, in the containment event tree results.

Q So it forms -- in some sense is a part of all the consequence and bottom line risk results of IPPSS. Is that correct?

A (WITNESS RICHARDSON) Theoretically, yes. Actually, no. We were almost totally insensitive to this curve because, as we stated yesterday, as Mr. Goeser stated yesterday, our results for almost all cases fell well below this curve.

Q But you did use the curve in all your calculations leading to bottom line risk results, did you not?

A (WITNESS RICHARDSON) Not in all the calculations, no.

Q Did you use another curve?

A (WITNESS RICHARDSON) In some cases, we had a bypass of -- we had containment bypass.

Q Well, excuse me. In the one case of containment bypass or containment overpressure failure is pretty much irrelevant.

A (WITNESS RICHARDSON) Right.

Q With that one exception, this was the curve tha you used in all your calculations?

A (WITNESS RICHARDSON) Yes, to the best of my knowledge.

Q Well, now, you are aware -- I guess you stated that you are not aware of the details of how Dr. Meyers' curve differs from your own.

A (WITNESS RICHARDSON) No, not on exact

details. No, I am not. I --

Q What I would like to have you do is look at Page 3.B-63 of the staff's testimony, where there is a comparison of these two curves.

A (WITNESS RICHARDSON) I don't have that here. Could you show that to me?

(Whereupon, the witnesses perused the document.)

Q All right. I will read off some numbers, and you tell me if any of these comparisons are incorrect from your reading of the curve. For failure at 141 psia, the staff or Dr. Meyers says 98 percent; you say 100 percent. For failure at 136 psia, the staff says 84 percent, and you say approximately 1 percent. Both of those are correct?

A (WITNESS RICHARDSON) It appears correct, yes.

Q And the staff continues on down from there to be 50 percent probability at 131 psia, 16 percent at 126 psia, and about 2 percent at 121 psia. Are those all correct?

A (WITNESS RICHARDSON) As far as I can tell, yes.

Q You are aware, are you not, that in IPPSS there are a large number of sequences where the pressure is projected to rise to some amount between 120 psia and 140 psia, are you not?

A (WITNESS RICHARDSON) Yes. I would recognize first that there was over 6,000 sequences analyzed in the containment event tree, because we followed all paths because of the conservatisms induced. We did not zero out any paths, and so there would be a large number of paths that may lead up those high pressures, but the total frequency would be very, very minute, almost zero, and I will also ask Mr. Liparulo to add to that, please.

A (WITNESS LIPARULO) As Dennis mentioned, we attempted in the analysis provided in IPPSS to provide a complete assessment of different accident sequences in the sensitivity to the assumptions in our view of the phenomenology. In order to do this, to give the reader a complete picture, we ran many cases, performed many calculations which we would describe basically to be unreasonable, but they did show sensitivity to many of our subjects.

Q I would now like to pass out a copy of Appendix 4.4.9 from IPPSS, entitled Compilation of Results.

JUDGE SHON: Mr. Blum, if you don't mind, while you are doing that, I would like to ask the panel a question.

It was my understanding from some testimony we had heard earlier on that the containment failure late overpressurization and containment failure due to high pressure in toto contributed only a small amount to the, well, prompt fatalities, for example. Is this correct?

WITNESS RICHARDSON: Yes, this is correct. A very small amount.

WITNESS KAPLAN: The prompt fatalities.

JUDGE SHON: So when it was said a moment ago with the sole exception of containment bypass you used this curve, that sole exception was the important thing.

WITNESS RICHARDSON: Oh, yes, over 95 percent of the early fatalities were derived from that sequence.

JUDGE SHON: So that what we are discussing now as far as practicalities at any rate isn't really a very important point?

WITNESS RICHARDSON: That is right, Your Honor.

JUDGE SHON: Thank you.

WITNESS HENRY: Just one further point for your information and that of the Board. I just want to make sure we don't confuse the accident sequences which are dealt with as initiators, and those sensitivity calculations, which there are many plots given in the Indian Point studies, they are two totally different failures.

BY MR. BLUM: (Resuming)

Q The claim that overpressure failures are unimportant for prompt fatalities is itself dependent upon conclusions made in your site consequence modeling, is it not?

A (WITNESS BLEY) The site consequence model is the place in the IPPSS analysis where the consequences, either latent effects or early fatalities, are calculated. With importance to that analysis is the timing of the release, and the characteristics of the source term involved. And those are to a great extent -- they come about as a result of the strength of the containment, so it really shows up in both places, and the part on consequence modeling can be covered by our panel 3 later.

Q If it turns out that either the consequence modeling or the containment response modeling is seriously flawed, then the assertion that late overpressure is unimportant for early fatalities will probably not stand up. Is that correct?

A (WITNESS RICHARDSON) As far as the

containment goes, we would think it totally incredible that the analysis is flawed in any way. So I can't even accept the basic assumption of your statement.

Q Well, let me address the question to the whole panel. I hope we can get over this one question. The assertion that late overpressure or 2RW mode of failure is unimportant for latent fatalities is arrived at through a combination of the containment modeling and the consequence modeling, and that if either of these turns out to be seriously flawed, that could result in that conclusion not being accurate. It is simply a conditional statement.

I realize you have great faith in your containment modeling, notwithstanding its difference from the staff.

A (WITNESS RICHARDSON) We agree that they are important for latent fatalities.

Q I am sorry. I meant early fatalities. Excuse me.

A (WITNESS BLEY) I guess if our analysis is wrong, and we have given analysis in great detail in IPPSS and the amendment, then it is wrong. We don't believe that it is.

A (WITNESS HENRY) Excuse me. That is also one reason we did so many sensitivity analyses in the report.

Q I take it you agree that late overpressure failure mode is important for property damage and latent health effects, including latent fatalities.

A (WITNESS BLEY) We certainly do. That is where the bulk of our contribution to latent effects is coming from.

Q Okay. At this point I would like you to turn to -- I would like to draw your attention to Table 1, and describe what this is to us.

A (WITNESS LIPARULO) Table 1 is entitled Summary of Results. What Table 1 is is, it provides a listing of the number of sensitivity studies, of the number of sensitivity studies which follow, which are presented in graphical form following these tables. These sensitivity studies were performed in order to provide the reader with a complete understanding of the sensitivity of the results to various assumptions in the model.

These cases, I should add, are generally very conservative, and do not -- do not represent our best view of what will happen in the core melt event, but they do show in general our insensitivity to the assumptions and phenomenology presented in the IPPSS study.

Q I would like one of you, whichever tabulates numbers most quickly, to go through the column that is second from the right. This column is entitled Pmax, which -- Am I correct? -- stands for maximum pressure sequence?

A (WITNESS LIPARULO) That would be the maximum pressure presented in the graph which follows.

Q And I would like you to go through and identify how many of these numbers fall between the range of 120 and 141, and then also identify how many exceed, how many equal or exceed 141.

A (WITNESS LIPARULO) I will do that. However, I think I have to discuss as I go through each case what the conservative assumptions are in the case, and say at the end that these are sensitivity studies that we are talking about.

Q Well, in the interest of getting the numbers out, let me see if I can get someone who will do it just straightforward, giving the numbers, and then we can go through different ones later on.

A (WITNESS LIPARULO) I think you would be doing a disservice just to quote the numbers.

Q I believe it is a legitimate question to ask to get the numbers. I have not gone ahead and done it. Isn't it true, and I will address this to all members of the panel, that there are eight numbers listed there which fall between 120 and 140, and then there are two that exceed 140?

A (WITNESS BLEY) Again, before we answer that, that seems to decouple the consequences of scenarios from the probability, and we are very hesitant to do that, because of the nature of these calculations.

A (WITNESS RICHARDSON) I believe we should be able to answer so that it puts things in context, not taking our answers out of context, as you are doing.

Q Well, could you first answer the question, that it is correct that there are eight between 120 and 140, and two above it, and then you can go ahead and supply the context.

A (WITNESS LIPARULO) I will trust your count.

Q Pardon me?

A (WITNESS LIPARULO) I trusted your count. I assumed you could count the numbers correctly. I see no need to redo it.

JUDGE GLEASON: They said they agree with you.

BY MR. BLUM: (Resuming)

Q Thank you.

Is there any other place in IPPSS that identifies the end point, that is, the maximum failure pressure of different scenarios?

A (WITNESS LIPARULO) I would like to correct you. These are not scenarios. These are merely sensitivity studies.

Q Right. Is there any place in IPPSS that does it for the scenarios that is a way that is not a sensitivity study?

A (WITNESS LIPARULO) The best estimate accident calculations are presented in Chapter 4, and they are in a table entitled just that, Best Estimate Accident.

Q So those are what you believe to be the most likely failure pressure rise of different accidents? Is that correct?

A (WITNESS LIPARULO) That is correct.

Q And these are the sort of maximum credible pressure for the accidents?

A (WITNESS RICHARDSON) No.

A (WITNESS LIPARULO) In some cases, they may represent very conservative assumptions. You see, when any engineer, any good engineer does a study, he always investigates the sensitivity to his assumptions in the study, and if you find that you are very, very sensitive to something that is not very far out, why then you spend a lot of time in that area.

What these numbers represent are in general very conservative calculations which we did along the way to developing the probability of containment failure, and they provided data to us, and obviously I was sensitive to whether I thought 50 percent of the core melt or the vessel failure or 51 percent, I would have a major problem, but that isn't what these studies show.

These studies showed us that we were very insensitive to a majority of our assumptions on how a core melts. That is why these studies were performed. In many cases, they are bound in cases, okay? And what I would consider bound in calculations.

Q You said you were insensitive to your assumptions about how the core melts. Is that --

A (WITNESS LIPARULO) In general, that is what I said.

Q And that was given your assumption of the failure pressure of the containment at or very close to 141 psia. That is correct, is it not?

A (WITNESS LIPARULO) If the containment failure --

Q Could you just answer? Is that correct or not?

A (WITNESS LIPARULO) You cannot get containment failure probability from containment failure. You cannot couple the two.

Q I know that, but when you said that your studies showed an insensitivity to precisely how the core melts, those studies were made with an underlying premise that the failure pressure of the containment was such as is represented by the graph that we have talked about having a very steep distribution around 141, is it not?

A (WITNESS LIPARULO) If the curve changed, the probability would change, depending on how much the curve changed. If the curve changed by a few psi, our estimate would not have changed for containment failure probability. In general, accidents can be divided into two classes, those with safeguards on and those with safeguards off. Those with safeguards on do not generally give high pressures in the containment for best estimate accident corrections. They end up around design pressure.

So, our sensitivity to the curve, Figure 2.5-1.2, is not great, since the majority of the accidents with safeguards on do not generate pressures even close to those curves. This is just a reiteration of what Mr. Goeser told us yesterday. Bob, do you have something to add to that?

A (WITNESS HENRY) As soon as you're finished, let me add something.

Q If I could ask one other question just on where we were.

A (WITNESS HENRY) Can I amplify on that slightly?

Q Oh, go ahead.

A (WITNESS HENRY) You also have to remember there are two different things that are better than these kinds of evaluations. You investigate the sensitivity to what you consider to be realistic physical processes and your uncertainties in those processes.

You also do sensitivity studies which step far outside the bounds of what is physically reasonable, and those are also considered in here just to display the sensitivity to other bounding type calculations, albeit it far outside the bounds of physical reality.

Q A difference of 15 or 20 psi in the containment failure pressure would have a substantial effect on the time at which the containment failed, would it not?

A (WITNESS LIPARULO) In the process of answering that, I'll first explain how we calculated the timing for containment failure. Containment failure was generally only calculated, only likely for delayed overpressure cases, delayed overpressure cases. At the time we were doing the study we didn't have the capability, the detailing model, to back into an accident.

As a result, we conservatively extrapolated the curves and estimated containment failure would occur at about 12 hours into the accident, realizing that this represented a major conservatism. Later calculations have shown that it's more likely that the containment would fail at about a day from these accident sequences or after a day.

So even though containment failure probabilities or failure times were based, of course, upon the calculations, the manner in which they were done was a conservative calculation of the curve in the report.

Q Do you attach specific probabilities to the different maximum failure pressures in Table 1?

A (WITNESS LIPARULO) As you asked me in the deposition, Mr. Blum, no, we don't.

Q Are different probabilities attached to pressure rises of different sequences anywhere in IPPSS?

A (WITNESS LIPARULO) As I mentioned at the deposition, there were no formal probability distributions attached to the curves presented in the IPPSS study.

Q Given the late overpressure sequences, would there not be a substantial difference in the time of containment failure if one posits failure at 141 psia as opposed to 126 psia?

A (WITNESS LIPARULO) Yes, in reality there would be. The point I was making is that if I assume for the purposes of discussion the containment failure pressure was 120 psia, given the data that I have today, not using the conservative extrapolation of the curves in IPPSS, I would predict containment would not fail at 12 hours into the accident.

Q Thank you.

Mr. Richardson, returning to --

MR. COLARULLI: Your Honor, if I could interrupt for a second. If we could take a short break. One of our witnesses would appreciate a short break.

JUDGE GLEASON: All right, let's take a short break, five minutes or so.

(Whereupon, at 2:50 p.m., the hearing in the above-entitled matter was recessed, to reconvene at 3:01 p.m. the same day.)

3:01 p.m.)

JUDGE GLEASON: Let's go, Mr. Blum.

BY MR. BLUM: (Resuming)

Q Mr. Richardson, do you recall your deposition on February 1st, 1983?

A (WITNESS RICHARDSON) Yes.

Q And at that time you stated, did you not, that you could not recall exactly how the calculations were arrived at for the particular curve around 141 psia that you put forward?

A (WITNESS RICHARDSON) Yes. I believe we talked a little bit about kind of a two-step process. It is, I have to agree, it is perhaps somewhat confusing between the deterministic analysis for the 141 lower bound and our probabilistic treatment, conservative treatment of that, and at this point I would like to ask Dr. Toland to talk about deterministic analysis, the lower bound that we used on that.

A (WITNESS TOLAND) It is important to reemphasize the 141 psia is not the failure pressure of the containment. What we chose to do was to back off from trying to determine that specifically, because of the uncertainties that would be associated with actually being able to determine failure. There's a lot of uncertainty there.

What we did instead was to choose a point, which we refer to as a limit load pressure, at which we could scrutinize this containment very, very carefully as to whether it could sustain that pressure, and that is what we did. We determined then that 141 psia was the limit pressure.

What it represents is the upper bound on what we call linear small displacement response of the structure. It is the upper bound on a response regime to pressure, for which we know very, very well how the structure will behave, how it will carry the loads, how the stresses will be distributed through the containment.

We know that our analytical tools are quite accurate in that regime. We also know the material properties very, very well. We looked very, very carefully for any secondary failure modes that might intervene and prevent us from reaching that point. We could not find any.

We are able to state with what we determined as very, very high certainty or confidence, rather, that it would not fail at that point. I told the people at Westinghouse that they could use 141 psia as a lower bound on failure if they chose; however, I could not describe where a failure would actually occur. It was their decision then to go ahead for the purposes of their safety study to assign failure to that pressure, 141.

They said the probability of failure is one for 141, and then they applied -- they skewed some distribution below that, but it is not the failure of that containment. What I represented to them was instead really the possibility of using a step function, where you literally jump from zero to one across 141. That is how the 141 was arrived at.

BY MR. BLUM: (Resuming)

Q The specific decision to not use a step function, but to rather use a very steep curve, that didn't involve calculations, did it, Mr. Richardson?

A (WITNESS RICHARDSON) Not to my knowledge. I believe that was just a conservative way of treating the information from Dr. Toland for the purposes of the study.

A (WITNESS TOLAND) Mr. Goeser of Westinghouse yesterday did give some indication of what went into that curve. I do not recall what that was, but it is in the actual testimony from yesterday.

Q But in any event, the result comes out less conservative than what the Staff comes up with and having a less steep curve; is that correct?

A (WITNESS RICHARDSON) The overall results of the study I don't believe would change at all if we had used the Staff's curve, at least the one I saw.

Q So you are saying that a difference in a failure pressure of 20 makes no difference for the overall study, or of 10 to 20?

A (WITNESS LIPARULO) I believe what we are saying, Mr. Blum, is that the curve we see here, which is representative of the Staff curve, we don't feel our results in terms of risk would be sensitive to whether we used this curve or whether we used the curve in the study.

Q This is just a general feeling, is that correct?

A (WITNESS RICHARDSON) Yes, based on our knowledge of the sensitivities in the containment event tree.

Q Dr. Toland, there appears to be some difference in how release itself is modeled in IPPSS as opposed to, say, WASH-1400. WASH-1400, for example, contemplated a sort of massive failure of the containment at some point, with a concomitant very large release; is that correct?

A (WITNESS TOLAND) It's been three years since I read WASH-1400 and that portion of it. I don't recall that specifically.

MR. BLUM: Judge Shon looks like he knows the answer.

JUDGE SHON: No, no. I had wanted to ask a moment ago, before you got off onto that question.

You said, I believe, some members of the panel, Dr. Richardson in particular, you said that you did not believe that using the Staff's curve as against what is presented here as the utility's safety study curve would make a vast difference. Is that in delayed fatalities as well as prompt fatalities?

WITNESS RICHARDSON: Yes, sir.

JUDGE SHON: An: is that because so few scenarios do wind up in that pressure range and stop?

WITNESS RICHARDSON: Basically, we feel it incredible to get our pressure up in that range, 120 psi range, unless you have no cooling, in which case you would --

JUDGE SHON: Yes, I understand that case. That's what I wanted to establish, why it was that behavior.

BY MR. BLUM: (Resuming)

Q Do you regard it as incredible that you could have no cooling?

A (WITNESS RICHARDSON) Oh, no. I was thinking of the scenarios where we had cooling. It would be almost incredible to get a pressure up tc that range. Where we didn't have cooling, obviously we did fail the containment after a number of hours.

JUDGE PARIS: I don't think you answered the question.

WITNESS RICHARDSON: Do you want to repeat your question again, please?

JUDGE PARIS: Maybe I didn't understand the question.

JUDGE SHON: As I understood what you were saying, it is that if you don't have cooling you fail the containment at 141, or any of it?

WITNESS RICHARDSON: Yes.

JUDGE SHON: If you do have cooling, you don't get up to the range of 121 to 141, so there are no scenarios where you wind up in the range where using one curve or the other would make a difference?

WITNESS RICHARDSON: Absolutely, correct.

JUDGE SHON: That is the way I understood it.

BY MR. BLUM: (Resuming)

Q Mr. Toland, the way the release is modeled in IPPSS, is it true that what happens at a failure pressure of 141 is the containment simply begins to leak? Is that correct?

A (WITNESS TOLAND) That is correct in my opinion, yes.

Q And the effect of that is there is also some higher point at which you assume the containment would actually burst, as opposed to leaking slowly?

A (WITNESS TOLAND) There's a possibility you could burst the containment. I feel it extremely improbable because in order to get there you have to deform this thing, literally ballooning it to such an extent that you're going to place significant strains on other portions of the containment which are more probable to leak, produce a leaking mode type of failure, which would limit the level of pressure you could literally accommodate.

In other words, you would be leaking this thing and not able to pressurize it any further. So the bursting of the containment in my opinion is extremely improbable.

Q And that conclusion or assertion is reflected in IPPSS, is it not?

A (WITNESS TOLAND) I have had discussions with people at Westinghouse who did the modeling as to what I believe to be the most likely, realistic and rational types of failure modes that you could have, and those are leakage modes that you would accommodate, which first are -- it's extremely improbable to get a large bursting mode type failure.

Q Does IPPSS assign probabilities to large bursting modes of release?

A (WITNESS TOLAND) Mr. Richardson would have to answer that.

A (WITNESS RICHARDSON) Two points to the response. We do in the containment event tree allow for early overpressure, because we did not allow any path to go to zero. So in those cases we would have early overpressure and large releases, which in terms of the effective releases would have to be addressed to D. Walker.

And other than that, the actual types of releases, the duration of them, whether they were puff releases or whatever, would have to really be addressed to D. Walker and the consequence panel.

JUDGE SHON: I notice that on page 87 of your testimony there is a note on the 2RW release, which I think is the kind we are discussing here, that the duration of the release is multi-phased. Could you explain that a little more? That seems to go to the detail that you're getting into here.

WITNESS BLEY: Judge Shon, our consequence panel can address that in more detail, but basically, since we expect in the delayed overpressure case that it will be a slow leakage sort of containment failure, there we would have really a continuous release over several hours. And that has been modeled as a set of three discrete releases or a multi-phased release condition, instead of a continuous one, because that was the best we could do in the consequence modeling.

JUDGE SHON: So that in your consequence modeling, in effect, when cooling fails and the presure increases indefinitely, it goes to 141 pounds or something around that and goes pssst and then stops, and then goes psst and then stops, again and again.

I don't know whether the court reporter can spell "pssst" or not.

WITNESS BLEY: That is the way it is modeled. We expect it would be a continuous release, but we have modeled it as a three-puff release.

JUDGE SHON: It doesn't just pop all of a sudden, as we were discussing a while ago as Mr. Blum was asking questions about it.

WITNESS BLEY: That is right, it is modeled as three discrete puffs to approximate the continuous leakage.

JUDGE SHON: Thank you. Mr. Blum, go ahead.

JUDGE PARIS: May I ask a follow-up question.

What do you expect the sites of these high-pressure releases to be? The sites, s-i-t-e-s.

WITNESS HENRY: I think I understand your question. Let me see if I can give some perspective this way. First, let me give you some idea of the area that would be involved, the flow area required to relieve. If the overpressure is due to steaming, under these cases you would require a hole that was about four inches in diameter. If it were due to concrete attack, the hole area might be two and a half to three inches in diameter.

As we all know, there are several penetrations that run through the walls of the containment at different elevations. If one of those were to be pulled out -- let's discuss for instance a penetration which would be about a foot in diameter. If it were to be pulled out about a quarter of an inch, it would be sufficient to alleviate the pressurization in the system.

Given all the large deformations that Dr. Tolant was mentioning, one of the penetrations would have to be the first site to go.

JUDGE SHON: Would have to be what?

WITNESS HENRY: The first site rupture, release of containment.

JUDGE SHON: That confuses me a little bit. Page 79 of your testimony, at the top of the page says, the first full sentence on that page, "The containment capability is not limited by the presence of penetrations or other discontinuities, but it is determined by the expansion of the wall area on a broad scale due to internal pressure exerting a force on the wall."

WITNESS TOLAND: Your Honor, that is associated with the 141 limit pressure. Our methodology was as follows. We established a limit pressure based on what we call the membrane response of the basic structure, which is a shell-type structure. Then what we did was we scrutinized very carefully all the discontinuity areas within that shell, again treating them basically as a shell-type structure.

Then, after that we looked at what we referred to as all the mechanical components of that, which include penetrations, piping penetrations, electrical penetrations, fuel transfer tube, the equipment hatch itself per se, the personnel airlocks themselves per se, and assured that they could also sustain that same pressure.

So the pressure then is really a limitation on the response of that basic structure, and all the other potential failure modes which might intervene to keep you from reaching that point have been scrutinized and found not in fact to precipitate a failure earlier than that.

However, recall that the response at this point in time is still what we call the small displacement response. Radial displacements are in the neighborhood of one and a half to two inches on the radius. Vertical displacements depend on where you are. It might be an inch.

If you go to the large displacement regime, which is actually going over and above the 141 psi, you are in the yielding region of the main rebar. The radial displacements will continue to increase with very small increments of pressure. Ten inches is not improbable, possibly 20 inches, you could get on the radius; two inches, three inches, four inches vertically.

These induce extremely high strains, stresses, on these penetrations. That is what I am saying. It is probable as the potential failure mode that you will induce failures at those locations, but it is beyond 141 psi.

JUDGE SHON: So in effect what you're saying is that your analytic calculation, which is essentially a linearization, a simplification, predicts that it is the region just below the atmospherical cap that goes first or that yields first, but that you anticipate that it will be actually elsewhere that actual penetrations or actual openings will occur; is that right?

WITNESS TOLAND: As you go beyond 141 and now you are starting to yield on other locations on the shell, you will start to incur these large, large deformations, with the attendant large strains on these penetrations, that is correct.

JUDGE SHON: I see. Thank you for explaining that.

BY MR. BLUM: (Resuming)

Q Dr. Toland, you have personally reviewed in some detail the containment strengths of nuclear power plants at three different sites, have you not?

A (WITNESS TOLAND) Indian Point, Seabrook, and the Washington Public Power Supply System's Units 1 and 4, that is correct.

Q And Zion in addition to that?

A (WITNESS TOLAND) I have read the Zion 1 Sargent & Lundy's analysis, Mr. Adolf Walters' analysis of that, yes.

Q Of the plants in these different sites, which is the site where the containments are weakest?

A (WITNESS TOLAND) Of those particular containments, I think they are very comparable. There is a distinguishing characteristic physically between Zion and the others. Zion is what we refer to as a post-tensional prestressed concrete containment. All the others are reinforced concrete containments. Reinforced concrete containments will behave slightly different than the Zion containment.

However, the Sargent & Lundy work produced, independent of what we did, a very comparable level of pressure that the Zion plants could sustain, comparable to what you have here at Indian Point. The same values that I would expect to see at Seabrook will be slightly higher, in part because they have a slightly higher demand for seismic, and the same is true with the Supply System's containments. There is additional seismic rebar there which will translate into some additional containment capability.

Q Have you done any additional calculations with regard to this since February 1st, 1983?

A (WITNESS TOLAND) No.

Q It is true, is it not, that on your deposition on February 1st, 1983, you stated that Indian Point had the weakest containment of the plants at those sites, did you not?

A (WITNESS TOLAND) Yes, but we're talking about small differences. I just explained to you the reason principally is the fact that both Seabrook and the Supply System containments are designed for higher seismic zones, and that translates over into more reinforcement bar into the actual shells, and that translates up for a comparable measure of this limit pressure at a higher level than what you have at Indian Point.

Q Do you recall the differences you gave in expected failure pressure for Indian Point as opposed to the others?

A (WITNESS TOLAND) I gave you a limit pressure and I said for Seabrook that I thought it might be 15 to 20, about 15 psi higher than what it is at Indian Point, that is correct.

Q Do you remember saying in the vicinity of 160?

A (WITNESS TOLAND) I stated 155 to 160 psi absolute, yes.

Q Uh-huh.

MR. BLUM: At this point we're now running into this possible problem I mentioned before, that we do have the material -- it is a little disorganized at present, but we do have it available -- whereby we could question Dr. Bley regarding pressurized thermal shock, which is relevant to, obviously, relevant to the plant analysis part.

On the other hand, this would probably come better and fit in better and would also be able to deal with Dr. Bley's pressurized thermal shock testimony if it were done at a later time under the Board question. I don't know how to read the Board's order. Certainly it is not the case that if something is relevant to a Commission question the Intervenors are disqualified from asking about it by virtue of the Board asking a question on it.

But what I don't know is whether we are obligated to ask this question now instead of what would be the more logical time.

JUDGE GLEASON: Ask them now.

MR. BLUM: Ask you on pressurized thermal shock now.

BY MR. BLUM: (Resuming)

Q Dr. Bley, you have expressed the view -- you are familiar with the document known as the response to the Sandia criticisms?

A (WITNESS BLEY) The one submitted by the Licensees?

Q Yes.

A (WITNESS BLEY) I am. It has been quite a while since I looked at it.

Q Are you prepared to discuss pressurized thermal shock now?

A (WITNESS BLEY) I think to some extent.

Q In that document you expressed the view that pressurized thermal shock is not a serious problem in your estimation; is that correct?

A (WITNESS BLEY) I think what we said was we did not think it would contribute substantially to risk of these plants, and explicit presentation of the pressurized thermal shock results would not change our risk curves. And in that regard the answer is yes.

Q In your response you cite a couple of documents from Westinghouse, but you make no response to the pressurized thermal shock precursor study that had been done; is that correct?

A (WITNESS BLEY) I guess I'm not correct on what you're talking about, the precursor study on pressurized thermal shock.

Q A study by Donell Phung, P-h-u-n-g, of Oak Ridge National Laboratories; are you familiar with that?

A (WITNESS BLEY) Not immediately. I'm aware of some work that has been done at Oak Pidge on pressurized thermal shock. I'm sure Westinghouse is, too. But that report I cannot identify.

Q So you are not familiar with its conclusion that there have been a significant number of pressurized thermal shock precursors at both Indian Point Units 2 and 3?

A (WITNESS RICHARDSON) Let me address that. Westinghouse, along with the Westinghouse Owners Group, did a study where we included all precursors for pressurized thermal shock in the form of a PRA addressing pressurized thermal shock. This was delivered to the Commission, and along with their study and other results showed pressurized thermal shock not to be a contributor to risk.

Now, in our study we basically included all the initiators that would possibly lead to pressurized thermal shock from any type of transient events, LOCA's, small LOCA's, steam breaks, feedwater breaks, and looked at their probability of causing cracks that would not stop and then cracks that would exceed the capability of the ECCS system.

And this was done in conjunction with a study by the NRC on the material properties, I believe, and the combination of that with then conclusions which said that the pressurized thermal shock was not a major risk to this plant.

There is testimony to that effect under question 1.4 that has been submitted.

Q Are you aware that one of the pressurized thermal shock precursors at Indian Point was classified as severity level 4, the highest level of severity?

A (WITNESS RICHARDSON) I'm sorry, I'm not familiar with the terminology. I don't know what you mean by "severity level 4." In terms of frequency?

Q Well, this is by the Oak Ridge study by Donell Phung.

A (WITNESS RICHARDSON) Well, I'm sorry, I can't address that. I don't -- I can't at this point.

(Pause.)

Q Which of you gentlemen is most capable with regard to seismic faults?

A (WITNESS PERLA) I can speak to that to some degree.

Q In your testimony at page 47, you assert that quakes with accelerations of .1 to .3g would not damage "even the weakest Indian Point critical structure." Have you modeled what type of quake would begin to damage various structures offsite, such as for example offsite power?

A (WITNESS PERLA) In our models that we

generated for our analysis, we conceded that one of the earliest failures would be the transformer through which offsite power is fed to the plant. As a result of that, we conceded that we in fact in any seismic event would lose offsite power.

Q In any seismic event of any size?

A (WITNESS PERLA) No, at the fragility level or -- excuse me. At the fragility level of the particular transformer. The median capacity, seismic capacity of the transformer, as I recall, is approximately .27g's. That infers that, considering the uncertainty on that fragility value, which has been described in the IPPSS, that basically earthquakes substantially below that would have zero effect on the failure of that transformer.

Q Does .27g correspond in any way to a modified Maccari intensity?

A (WITNESS PERLA) There is not an exact relationship, because it does vary from the earthquake frequency content and energy content, and it also varies between the distance at the source point of the earthquake and the point of interest. And of course, thirdly, it varies as a function of the materials between the two.

But there have been some relationships

developed that essentially suggest that that might be in the intensity 7, roughly, intensity 8, perhaps.

Q And this is a median value, you said, or mean, the .27?

A (WITNESS PERLA) .27 is the best estimate, median acceleration capacity.

Q Can you give us some estimate of the probability curve, how steep or not it is?

A (WITNESS PERLA) I don't recall it precisely, but to the best of my recollection I would say it would perhaps extend downwards, if this is your question, in reality downwards to the upper teens, perhaps .17, .18g.

Q So in IPPSS you were assigning some probability to loss of offsite power at those different intensities of earthquakes?

A (WITNESS PERLA) Yes, at those different accelerations.

Q What about gas turbines at the Buchanan station? What kind of modeling was done for them with regard to earthquakes?

A (WITNESS PERLA) We modeled the gas turbines as well, and my recollection is that we conceded their failure, and I believe it was we conceded their failure to start, which made the seismic capacity of that equipment irrelevant.

Q Did you take into account the effect of earthquakes on operator stress level so as to calculate the effect that that would have on the behavior of operators following an earthquake?

A (WITNESS PERLA) Yes, in a sense we did. We considered, quite conservatively we believe, that any activity which would threaten the control room, cause any type of failure, one of which I'll describe in a moment, would essentially disable the operators from performing their function. And the analysis specifically in which we drew that conclusion was the failure of the control building earlier because of the impact of it with Unit 1.

Q What about with an earthquake that might precipitate a loss of offsite power, but not substantially damage the plant? Was there any modeling done there with regard to the effect on operators?

A (WITNESS PERLA) The concept there, of course, is that the small earthquake that would not fail anything in the plant would certainly be of little concern to any operator as a consequence. The plant is in a position to essentially almost operate automatically.

Q This would include the situations where there has been a loss of offsite power due to the earthquake and a loss of gas turbine power?

A (WITNESS PERLA) Yes, because the diesel generators certainly provide the opportunity of supplying emergency power.

Q And you're saying a quake of that intensity would not have any noticeable effect on the operators?

(Pause.)

A (WITNESS PERLA) In our analyses, where we essentially had a seismic-initiated loss of offsite power, the following scenarios would be no different than the internal event analysis that considered a loss of offsite power. The frequency -- excuse me.

Q Go ahead.

A (WITNESS PERLA) With an exception, of course, and that is with the seismic-initiated frequency as an initiating event is substantially less than initiating frequency than a loss of offsite power due to -- from the internal analysis.

Q And you assumed, did you not, that when ceiling panels weighing 25 pounds fell down upon the operators, that these would not disable the operators unless they struck them directly with the pointed edge? That is correct, is it not?

A (WITNESS PERLA) That is not exactly true. We had considered the possibility of incapacitating the operators and that is clearly analyzed in amendment 1.

Q Well, addressing this with regard to prior to amendment 1, that is correct, that that is how you modeled it in IPPSS?

A (WITNESS PERLA) Would you restate your question?

Q That the ceiling panels weighing 25 pounds each falling down on the heads of operators would not disable the operators unless it struck them directly with the point of the panel going into their -- well, the point of the panel impacting on their head.

A (WITNESS PERLA) I don't believe that was our original analysis. In fact, I don't believe that in the IPPSS report failure of the ceiling panels is addressed. It was implicitly thought about by the team and we considered that such failures would essentially have little likelihood of incapacitating several operators in a given event.

Q Dr. Bley, do you recall anything in the response to Sandia letter report about panels falling on the heads of operators?

A (WITNESS PERLA) If I can continue discussing that point, in the response to the Sandia suggestion that that analysis be accomplished and subsequent to the issuance of the IPPSS, we did in fact analyze a failure of panels as a possible means of incapacitating operators in the units, in the control room units.

Q Thank you. I believe -- well, to move on to another area of questioning, Dr. Bley, it is true, is it not, that in IPPSS, and here I will now refer to both the original IPPSS and Amendment 1 which I have not yet read, that the way -- there is modeling of a possibility of a group of operators all achieving the wrong hypothesis and acting erroneously as a result, is by using a mathematical procedure that treats the incorrect hypothesis of each operator as an independent event. That is true, is it not?

A (WITNESS BLEY) It is not.

Q It is not? Would you describe in detail the mathematical model that is used there, please?

A (WITNESS BLEY) I will be happy to, and it goes beyond the mathematical model. The physical or functional model that is involved is one that realizes, or recognizes, I should say, that while this may have been true for all -- many operators before the Three Mile Island accident, all operators have been trained somewhat differently.

Since the Three Mile Island accident there has been much emphasis placed on not rapid recovery from accidents but, rather, ensuring a minimum set of conditions exist within the plant to protect the reactor core. Things such as the new requirements for securing safety injection at all plants are examples of this.

This new approach, which I think really harks back many years to the early use of nuclear power, is one that takes advantage of the in some ways slow response of the nuclear plant and the minimal set of things one needs to do to protect the plant, to key the operator on cues with respect to pressures and temperatures within and around the plant, which means that even if they do something wrong along the way, they are continually alerted by these cues to bring themselves back, to bring the plant back.

So we are not so interested in predicting whether or not there is a mistake made along an accident sequence. What we are interested in is whether the function that needs to be provided through the operator is eventually provided. And by "eventually", I mean within time to protect the core.

The models that look at the operators and look at their dependence or independence are mathematical models that are related to the models in the -- I forget the exact title, but it is known as the Swain Handbook on Human Reliability that we talked about or that someone else talked about this morning.

Q Dr. Bley, we are a little short on time. If I could just bring you to the bottom line, wasn't the way you calculated it for four operators present something like .10 times 1/5 times .10?

A (WITNESS BLEY) I don't remember the number, but it did not assume independence. It assumed some level of dependence among the various levels of operators -- sometimes very high dependence, sometimes low dependence -- depending on what activities the operators are expected to be involved in. It was not an independent assumption.

Q Well, I see where we are getting confused on the different meanings of "dependent" and "independent". I meant independent in a mathematical sense, that you simply multiplied one times the other.

A (WITNESS BLEY) Well, I am sorry --

Q The numbers that were input into the multiplication were meant to connote some level of dependence. What you call as a low level of dependence corresponds to the figure .10, does it not?

A (WITNESS BLEY) I don't remember the numbers, but I meant independence in a mathematical sense, which is what I described. It is independence, probabilistic independence among the operators.

Q Okay. To wrap up, Mr. Richardson, it is your position in general, or at least with regard to containment response, that any amount of conservatism is too much and that it's best to have as little as possible. That is correct, is it not? I can refresh your recollection.

A (WITNESS RICHARDSON) No, please. I am just giving some thought. I guess in my opinion when anybody is doing a risk assessment analysis, they should attempt to make it as realistic as they can within the bounds of their knowledge, with the exception where this would require perhaps more amount of effort than it would be worth from the viewpoint of what is contributing to the risk, and from that viewpoint one would probably start with a more conservative bias type of analysis, and then drive it toward a more realistic analysis based on what was contributing to the risk.

But that would be the flavor of how at least I would try to approach probably any type of a risk analysis.

Q Do you have a copy of your deposition there?

A (WITNESS RICHARDSON) No, I don't.

Q I will bring you my copy from here, but I believe at the very top of page 83 you stated, "I believe any conservatism is probably too much. As I stated before, in reality you should try to be realistic." And that was said in the context of your curve regarding the 141 failure pressure as opposed to the Staff's curve.

A (WITNESS RICHARDSON) Give me a second just to read the preamble. If I could read what I said here, it starts out, "You do feel it is desirable to have some conservatism in this, is this correct?" And we were speaking about what the NRC was doing.

I stated, "It is desirable to keep things as realistic as we can. In some cases, though, instead of wasting a lot of time and energy on exacting details, we would be somewhat conservative unless it was of great importance." And I believe that is exactly what I stated a minute ago.

Q Just to connect the two, the question is "what makes you believe that the amount of conservatism embodied in your curve is sufficient conservatism?" Answer: Mr. Richardson: "I believe any conservatism is probably too much. As I stated before, in reality you should try to be realistic."

A (WITNESS RICHARDSON) Right. In that context, we were speaking about the containment pressure failure curve.

Q Yes.

A (WITNESS RICHARDSON) And I believe -- I guess I stated with respect to that curve that probably any conservatism is probably too much, and there I believe I was referring back to UN&C calculations, which stated they thought a lower limit of onset to yield was 141. They didn't really believe it would fail at that point and so we were being ultra conservative in even having a curve below that, if I remember back to the conversation we were having.

Q And to wrap up with you, Dr. Toland, on page 15 of the UE&C appendix in IPPSS, which is the part of IPPSS where the 141 failure pressure was addressed, there is -- it is discussed on page 15. I don't know. Is there a copy of that available for the witness?

MR. COLARULLI: Mr. Blum, do you have a volume number?

MR. BLUM: Yes, Volume 8, Section 4.4. Yes, Appendix 4.4.1.

WITNESS TOLAND: Which page?

BY MR. BLUM: (Resuming)

Q Page 15 of the Appendix.

A (WITNESS TOLAND) Yes.

Q And there it is stated that the pressure of 141 psia, or 126 psig is 2.7 times the design accident pressure for Indian Point, and that this 2.7 factor directly corresponds to conservatisms in the original design. These are identified below, and listed below are six things -- first, application of load factors, for which a factor of 1.5 is given; application of capacity reduction factors, 1.11; strength of liner not accounted for, 1.15; minimum strength of material considered, 1.18; seismic rebar resisting LOCA loads, 1.12; designer conservatism, 1.06.

And when we multiply together these six sets of numbers, we come out with the number 2.7, which can then be multiplied by the design pressure to get us 141 psia. It is your position that the specific numbers -- the 1.5, 1.11, 1.15 -- make no practical sense, is that correct?

A (WITNESS TOLAND) That is not correct.

Q Well, that is what you stated in the deposition.

A (WITNESS TOLAND) No, no. In my deposition, what I told you is that your implication that I just multiplied some numbers together in order to get to the 141 was incorrect.

Q Excuse me. That is not what I am asking about. I am simply asking about these numbers. I realize you had other ways of getting to 141.

A (WITNESS TOLAND) The intent of the enumeration of these factors is to show to the experienced analyst, the knowledgeable analyst, that in fact he shouldn't be surprised that we got to 141 because this limit load, which is really the upper end of this linear response, is a direct result of all the original conservatisms that went into the design. It is as simple as that.

Q But you did make the statement that the particular breakdown of the 2.7 in this way did not make any practical sense, is that correct?

A (WITNESS TOLAND) It didn't have any consequence on the actual determination of the 141 psia. It would have no consequence in the whole safety study. It is only that it be illuminating to the more experienced analyst so that he shouldn't be surprised.

Q Do you recall using the phrase "do not make practical sense"?

A (WITNESS TOLAND) In context, no. Out of context, I don't recall. You would have to provide the context to me and perhaps I will recall.

Q I believe there should be --

A (WITNESS TOLAND) What is your question? Perhaps we can get to the heart of the matter.

Q Well, that was a fairly straightforward point. I was going to ask you that that was in fact what you said, and then you could explain further what you meant by it.

A (WITNESS TOLAND) I think I did explain what I meant by it. It has no consequence to the final source of that number. It has no consequence in the safety study itself. It is there only for edification for apparently only structural analysts.

Q But apparently it doesn't correspond to any physical reality.

A (WITNESS TOLAND) No. It only enumerates where you can actually see within the original design basis of these structures the various conservatisms which bring you up to this point. That is it. That is the sole intent of it.

Q It was on page 38. The question was, on line 12, "Do you understand my problem, what I am trying to get at, whether there is any mathematical sense assigning these numbers and multiplying them together to get 2.7?"

A (WITNESS TOLAND) Yes, that comes back.

Q Excuse me. "Witness Toland: There is no practical sense." First, that is in your deposition, is it not, that statement?

A (WITNESS TOLAND) That is correct.

Q And I take it that you calculated the 141 by some means that are not contained fully in Appendix 4.4.

A (WITNESS TOLAND) The Appendix does not enumerate the analysis that went into this, that is correct.

MR. BLUM: Okay, thank you very much. I have no further questions.

JUDGE SHON: I just have a very brief question of Dr. Toland. Throughout our entire dealing with this number of what the containment will stand we have been talking in terms of absolute pressures.

WITNESS TOLAND: That is correct.

JUDGE SHON: This sort of surprises me, because it seemed to me that it was the gauge pressure that was actually stressing the material under consideration.

WITNESS TOLAND: That is correct. In fact, our calculations were done with gauge pressures 126 psig. It was only to accommodate all the other portions of the work that we transferred it over to an absolute pressure.

JUDGE SHON: Okay, that makes much more sense.

JUDGE GLEASON: Ms. Moore?

MS. MOORE: I have a few questions.

CROSS EXAMINATION

BY MS. MOORE:

Q Gentlemen, for the SE damage state, does your analysis indicate that the cavity will be flooded during the time from vessel failure through to containment failure, or will it be dry?

A (WITNESS HENRY) You did say SE?

Q Yes.

A (WITNESS HENRY) With the safeguards or without?

Q Without.

A (WITNESS HENRY) The cavity would be initially wet due to the water that is lost from the primary system that fills the floor of the containment and also spills over the curb down into the reactor cavity instrument tunnel. After vessel failure, that water is eventually vaporized into the atmosphere.

Q Let me ask you this: In the damage state -- we are sticking with the SE damage state -- is the long-term accident progression controlled by water-core-debris interactions or core-concrete interactions?

A (WITNESS LIPARULO) Both. Of course, given that you have water in the cavity initially, the rate of pressurization is depending on the rate of steam generation, and then when the water dries up you begin to get core-concrete interaction, and then your rate of pressurization depends upon your rate of core-concrete interaction. So the rate of pressurization is a function of both phenomenon.

Q Okay. Could you tell me at what time, according to your mechanistic analysis, the containment fails in the SE damage state?

A (WITNESS LIPARULO) As I recall, we assume it failed at 12 hours into the accident. This was, as I stated before, based upon some conservative -- what we considered to be conservative extrapolations of a Class 5 event. In actuality, I believe it would be later than the 12 hours assumed in the study.

A (WITNESS HENRY) I might add to that, it is not greatly different from the transient for small breaks because it is merely an energy balance. When you provide enough energy into the containment it results in steam formation and concrete attack to initially defile the containment.

Q In your analysis, what is the content of base mat penetration for the E sequence?

A (WITNESS HENRY) Let me make sure I correctly understand your question. Base mat failure at any time, or base mat failure prior to overpressurization of the containment?

Q Let's try first prior to failure by overpressurization.

A (WITNESS HENRY) Prior to overpressurization? To the best of my recollection, it is 10^{-3}. Excuse me.

A (WITNESS LIPARULO) I would like to add one statement to that. That would present considerable conservatism in our study since if the containment would fail at base mat penetration, the containment pressure could relieve itself into the ground and thus prevent an overpressure failure, which is where all the consequences are coming from.

We decided to take this conservative approach part of the study rather than to try to define who would win the race -- containment overpressure or base mat penetration.

Q Do you have a probability for base mat penetration at any time?

A (WITNESS HENRY) Could you be more specific, if there is a note in the containment event tree for base mat penetration? Are you talking about any time during the accident?

Q After overpressurization.

A (WITNESS HENRY) After containment overpressure, that really was not treated in the study.

Q Okay. What is the --

A (WITNESS HENRY) Excuse me.

(Witnesses conferring.)

JUDGE GLEASON: Proceed, Ms. Moore.

BY MS. MOORE: (Resuming)

Q Could you tell me what is the probability of failure by overpressurization from the SE damage state?

A (WITNESS LIPARULO) I believe it is very nearly one, but I am not certain. It is .999.

Q Okay, thank you. Gentlemen, are you aware of a portion of the testimony that has been filed by licensees in this proceeding that states that further -- that there is testimony which justifies the use of smaller source terms than those previously used?

A (WITNESS LIPARULO) I am aware that is in the testimony. That will be handled by a later panel. We are really not the source term people here.

Q On page 81 of your testimony you state that source terms generally follow the approach of the reactor safety study, do you not?

A (WITNESS BLEY) If I may, this whole section is to be covered by our panel 3, and I think that would be more appropriate if we could delay it.

Q You are on that panel, Dr. Bley, are you not?

A (WITNESS BLEY) I am, but as a general member of the panel, to tie the source term mark back to the study as a whole, the real experts in this area are the other two gentlemen on the panel.

Q Okay. Then my understanding is correct that the delineation of the source term was not part of the containment analysis?

A (WITNESS RICHARDSON) If I could address that, part of the containment analysis was to sort each and every one of the sequences in the containment event tree to a release category. However, the definition of the source term related to the release category, the consequences thereof, was really established by somebody else.

All the containment event tree did was to sort the sequences into their proper release category. The somebody else, by the way, was Dee Walker, who will be on the next panel -- Panel 3.

Q Then I take it that -- well, strike that.

MS. MOORE: I have no further questions.

JUDGE GLEASON: Mr. Brandenburg?

MR. BRANDENBURG: No redirect.

MR. COLARULLI: The Power Authority has no redirect either, Your Honor.

JUDGE SHON: I would like to ask one question, probably of Doctors Bley and Kaplan. It is one that I realized a little too late yesterday that I should have asked someone on the panel yesterday. I asked it this morning of a Staff witness, but he wasn't able to give me a very good answer.

Dr. Kaplan, when we were discussing failure rates and the quantity lambda yesterday, the probability of failure of a pump, for example, we spoke as if the failure rate constant were a constant, that is, did not change with time. That is not generally true. It might be true in some cases, but as a rule you had a typical situation where things have high failure rates at the beginning and end of some fixed life, and a constant in-between.

Does the Indian Point probabilistic safety study take account of this variation of failure rate, particularly wear-out -- the fact that things get on and the bearings on that pump we talked about get rusty or something like that over a number of years? Does it take account of that and, if so, how?

WITNESS KAPLAN: Dr. Bley is just itching to answer this question.

JUDGE SHON: I thought he might be.

WITNESS BLEY: I think I would like to address it from the practical point of view of the study and systems analysis, and then maybe Dr. Kaplan can add something on the mathematical side.

These two plants have rather substantial operating experience at this time. The components within the plant are not all new. Some are new; some are old. They have been changed out. We are addressing the plants as they are and we expect they will continue to be for some time.

That means they are undergoing continual maintenance. Our failure data includes failures that really are burn-in kinds of failures on new equipment. It includes some wear-out kinds of failures. The plant right now is a collection of old and new and continuously maintained. And if wear-out begins to occur with some component that has not seen it before, the maintenance program adapts to that new situation and brings that back into status with the rest of the plant.

Therefore, I think the way we have handled it, with constant failure rate assumption, with substantial uncertainty, is the best model for the plant in the way we have built our whole analytical model. We don't track individual components as they are taken out and replaced over the life of the plant.

So I think it is not only the most practical, but probably the best way to address that situation for those plants as they exist now. Now that would not necessarily apply to a completely new plant for its first year or two of operation, nor for something -- some other sort of facility that does not have a continuing maintenance program of the sort that we do find in these plants.

JUDGE SHON: So in effect what you are saying is the population for which you originally drew your failure rate data represents a mix of old and new equipment, that this is a mixture of old and new equipment, so it has some average failure rate that you think is a constant under maintenance and proper replacement conditions. Is that about it?

WITNESS BLEY: That is about it.

JUDGE SHON: Thank you.

WITNESS KAPLAN: That answer is sufficient, but I will add a few more points anyway. First of all, a lot of the equipment is standby equipment that doesn't get used and doesn't age on that basis.

JUDGE SHON: It might age. You know, things rust.

WITNESS KAPLAN: All right. All right.

JUDGE SHON: My car doesn't start well if I let it sit for a long time.

WITNESS KAPLAN: But even the standby equipment is maintained and tested. The other point is, with respect to one of the most important category of events, namely initiating events, that we do consider in assessing the frequency of those events, that we consider the whole history of our plant and other plants, without a time dependency model.

But if you did include a time dependence in the model, what you would probably find is that the frequency of those events goes down as we all get smarter and operators get smarter about where those events come from and learn how to make them less frequent.

JUDGE PARIS: To anyone on the panel, I have the following question: Are the risk estimates in the IPPSS and the amendment for Indian Point Unit 3 based on the assumption that all of the fixes, including the fire fix and the control room ceiling fix, have been completed?

WITNESS PERLA: I believe that is correct.

JUDGE GLEASON: Gentlemen, you are excused.

Ms. Moore, would you call your next panel of witnesses, please? Thank you, gentlemen.

(The witnesses were excused.)

JUDGE GLEASON: Gentlemen, would you rise and we will swear you in.

Whereupon,

SANFORD ISRAEL,

JACK HICKMAN,

GREGORY KOLB,

ROBERT G. EASTERLING,

and

ALAN D. SWAIN

were called as witnesses by counsel for the NRC Staff and, having been duly sworn by the Chairman, were examined and testified as follows:

MR. COLARULLI: Judge Gleason, possibly if I could make one statement to follow up Judge Paris' so that at least the record is complete concerning those two items in amendment 1, just a very brief statement --

JUDGE GLEASON: I'd rather you wait until he gets here. He will be back in a minute and we will stop at that point.

Ms. Moore, please proceed.

MS. MOORE: Mr. Chairman, might I have a moment?

JUDGE GLEASON: Yes.

(Pause.)

JUDGE GLEASON: Would you like to give that explanation, Mr. Colarulli?

MR. COLARULLI: Yes. It is just a brief statement.

As has been testified to fixes at Indian Point Unit 3, the control room ceiling and the fire fix were modeled in Amendment 1. The Power Authority -- presently, Indian Point Unit 3 is not operating today and the Power Authority presently intends to have both of those fixes in place before startup.

JUDGE GLEASON: All right, Ms. Moore, are you ready?

MS. MOORE: Yes.

JUDGE GLEASON: If you could, at some point, with one of your witnesses who is coming forward, have him put that information in the record somehow, even though you stated it. You are counsel. We would appreciate having that. Is that all right?

MR. COLARULLI: I am not sure if any of the witnesses have that information, but we will certainly try to figure out a way of doing it.

JUDGE GLEASON: All right. Thank you. We might have to call one to put him on very briefly and excuse him.

Go ahead, Ms. Moore.

DIRECT EXAMINATION

BY MS. MOORE:

Q Mr. Israel, would you state your name and business address?

A (WITNESS ISRAEL) My name is Sanford Israel. I am with the Nuclear Regulatory Commission in Washington, D. C.

Q Mr. Hickman, would you state your full name and address?

A (WITNESS HICKMAN) My name is Jack Hickman. My business address is Sandia National Laboratories, Division 9412, Albuquerque, New Mexico.

JUDGE PARIS: I'm sorry. I missed your name, sir. Could you give it again?

WITNESS HICKMAN: Jack Hickman.

BY MS. MOORE: (Resuming)

Q Mr. Kolb, could you please state your name and business address?

A (WITNESS KOLB) My name is Gregory Kolb. My business address is Sandia National Laboratories, Division 9412, Albuquerque, New Mexico.

Q Dr. Easterling, would you do the same thing, please?

A (WITNESS EASTERLING) My name is Robert Easterling. My address is Division 7223, Sandia National Laboratories, Albuquerque, New Mexico.

Q Mr. Swain, would you do the same?

A (WITNESS SWAIN) My name is Alan Swain. I am in Division 7223, Sandia National Laboratories, Albuquerque, New Mexico.

Q Mr. Israel, what is your position with the NRC?

A (WITNESS ISRAEL) I am a risk analyst with the

Liability Risk Assessment Branch, Division of Safety Technology.

Q Mr. Hickman, would you identify your position with Sandia?

A (WITNESS HICKMAN) Yes. I am supervisor of the Nuclear Fuel Cycle Safety Division.

Q Mr. Kolb, would you identify your position at Sandia?

A (WITNESS KOLB) I am a member of the technical staff within the Nuclear Fuel Cycle Systems Safety Division.

Q Dr. Easterling, would you identify your position at Sandia?

A (WITNESS EASTERLING) I am a member of the Statistics Committee, Human Factors Division.

Q Dr. Swain, would you identify your position at Sandia?

A (WITNESS SWAIN) I am a staff member, a member of the Division of Statistics, Computing and Human Factors.

Q Gentlemen, do you have before you a copy of a document entitled "Direct Testimony of Sanford Israel, Jack Hickman, Gregory Kolb, Robert G. Easterling, and Alan D. Swain, Commission Question 1 and Board Question 1.1"?

A (WITNESS ISRAEL) Yes.
A (WITNESS HICKMAN) Yes.
A (WITNESS KOLB) Yes.
A (WITNESS EASTERLING) Yes.
A (WITNESS SWAIN) Yes.
Q Was this document prepared by you or did you participate in its preparation?
A (WITNESS ISRAEL) Yes.
A (WITNESS HICKMAN) Yes.
A (WITNESS KOLB) Yes.
A (WITNESS EASTERLING) Yes.
A (WITNESS SWAIN) Yes.
Q Do you have any additions or corrections to this document?
A (WITNESS ISRAEL) Yes. We do have corrections to the document.
Q Could you please state those corrections for the record?
A (WITNESS ISRAEL) On page four, line five, the word "grantification" should be "quantification".
JUDGE GLEASON: Excuse me. Which page? "Grantification" should be "quantification".
WITNESS ISRAEL: On page ten, lines 6, 7, and 8, essentially the last sentence in that paragraph, should be stricken, and in its place the following: "Of

the four, two we found to be reasonable, one resulted in a higher human error probability estimate, and one of them resulted in --

MS. FLEISHER: Excuse me. Could you speak up? I can't hear.

JUDGE GLEASON: Would you start over again?

MS. FLEISHER: And give the source again?

WITNESS ISRAEL: I am sorry. Page ten, lines 6, 7, and 8, the last sentence in that paragraph, that should be stricken and in its place the words: "Of the four, two we found to be reasonable, one resulted in a higher human --

JUDGE GLEASON: You'll have to go slower if you expect us to write this in.

WITNESS ISRAEL: Pardon?

JUDGE GLEASON: You'll have to go slower if you expect us to write this in.

WITNESS ISRAEL: I'm sorry.

JUDGE GLEASON: "Two we found to be reasonable."

WITNESS ISRAEL: Comma, "one resulted in a higher human error probability estimate, and one of them --

MR. BRANDENBURG: Excuse me. Can we take it again from "human", please?

JUDGE GLEASON: Higher human error probability.

WITNESS ISRAEL: "Estimate, and one of them resulted in a lower estimate."

JUDGE GLEASON: All right, if I could read it now, it says, "Of the four, two we found to be reasonable, one resulted in a higher human error probability estimate, and one of them resulted in a lower estimate." Is that right?

WITNESS ISRAEL: Correct.

Table 1 on page 12, the second line entry, Core Melt without Containment Cooling, we go over to the third column, which is under Sandia Estimates IP-2, and where it says 6.7(-7), it should read "6.1(-7)."

Those are all the corrections.

BY MS. MOORE: (Resuming)

Q Gentlemen, with these corrections, is this testimony true and correct to the best of your knowledge, information and belief?

A (WITNESS ISRAEL) Yes.

A (WITNESS HICKMAN) Yes.

A (WITNESS KOLB) Yes.

A (WITNESS EASTERLING) Yes.

A (WITNESS SWAIN) Yes.

Q Do you adopt this testimony as your testimony in this proceeding?

A (WITNESS ISRAEL) Yes.
A (WITNESS HICKMAN) Yes.
A (WITNESS KOLB) Yes.
A (WITNESS EASTERLING) Yes.
A (WITNESS SWAIN) Yes.
MS. MOORE: Copies of this testimony have been delivered to the Board, the parties and the court reporter. I ask that the testimony and the attached professional qualifications be received into evidence and bound into the record as though read.
JUDGE GLEASON: Is there objection? Hearing none, the testimony will be received and bound into the record as if read.
(The testimony of Messrs. Israel, Hickman, Kolb, Easterling, and Swain follows:)

UNITED STATES OF AMERICA
NUCLEAR REGULATORY COMMISSION

BEFORE THE ATOMIC SAFETY AND LICENSING BOARD

In the Matter of	)	
	)	Docket Nos. 50-247-SP
CONSOLIDATED EDISON COMPANY OF NEW YORK (Indian Point, Unit 2)	)	50-286-SP
POWER AUTHORITY OF THE STATE OF NEW YORK (Indian Point, Unit 3)	)	

DIRECT TESTIMONY OF
SANFORD ISRAEL, JACK HICKMAN, GREGORY KOLB,
ROBERT G.EASTERLING, AND ALAN D. SWAIN
COMMISSION QUESTION 1 AND BOARD QUESTION 1.1

Q.1 Please state your name and business address for the record.

A.1 My name is Sanford Israel. My business address is U.S. Nuclear Regulatory Commission, Washington D.C. 20555

Q.2 Please identify your position with NRC and describe your responsibilities in that position.

A.2 I am a Risk Analyst in the Reliability and Risk Assessment Branch of the Division of Safety Technology within the Office of Nuclear Reactor Regulation of the Nuclear Regulatory Commission. My responsibilities are to provide risk perspectives based on a review of core-melt sequences and system reliabilities for various assigned tasks.

Q.3 Have you prepared a statement of your professional qualifications.

A.3 Yes, I have prepared the statement of my professional qualifications attached to this testimony.

Q.4 Please state your name and business address for the record.

A.4 My name is Jack Hickman. My business address is Sandia National Laboratories, Division 9412, Albuquerque, New Mexico.

Q.5 Please identify your position with Sandia and describe your responsibilities in that position.

A.5 I am Supervisor of the Nuclear Fuel Cycle Systems Safety Division at Sandia National Laboratories. In that position, I am responsible for the performance, evaluation and application of nuclear power plant system reliability analysis for programs being performed for the Nuclear Regulatory Commission.

Q.6 Please describe your education and professional qualifications.

A.6 A copy of my professional qualifications is attached to this testimony.

Q.7 Please state your name and business address for the record.

A.7 My name is Gregory Kolb. My business address is Sandia National Laboratories, Division 9412, Albuquerque, New Mexico.

Q.8 Please identify your position with Sandia and describe your responsibilities in that position.

A.8 I am a member of the technical staff within the Nuclear Fuel Cycle Systems Safety Divison. I am responsible for the performance and review of nuclear power plant systems reliability analyses which are a part of several research and technical assistance programs funded by the Nuclear Regulatory Commission.

Q.9 Please describe your education and professional qualifications.

A.9 A copy of my professional qualifications is attached to this testimony.

Q.10 Please state your name and business address for the record.

A.10 My name is Robert G. Easterling. My business address is Sandia National Laboratories, Albuquerque, New Mexico.

Q.11 Please identify your position with Sandia and describe your responsibilities in that position.

A.11 I am a staff member in the Reliability Department. My activities include research in statistical data analysis and in the application of statistical techniques to reliability and risk assessment.

Q.12 Have you prepared a statement of your professional qualifications?

A.12 Yes, I have prepared the statement of my professional qualification attached to this testimony.

Q.13 Please state your name and business address for the record.

A.13 My name is Alan D. Swain. My business address is Sandia National Laboratories, Albuquerque, New Mexico.

Q.14 Please identify your position with Sandia and describe your responsibilities in that position.

A.14 I am a member of the technical staff in the Statistics, Computing, and Human Factors Division where I provide technical leadership for the human factors group. My activities include the identification of the potential for human error in complex systems, the provision of design recommendations to reduce this potential, and the grantification of the human error potential for reliability and risk assessment studies.

Q.15 Have you prepared a statement of your professional qualifications?

A.15 Yes, I have prepared a statement of my professional qualifications attached to this testimony.

Q.16 Mr. Israel, what is the purpose of this testimony?

A.16 The purpose of this testimony is to provide frequencies of plant damage states, caused by internally initiated events excluding fire and sabotage, that are used in James Meyer's testimony on radiological releases. This testimony also provides an assessment of the IPPSS results in the area of plant damage states caused by internally initiated transients and accidents.

Q.17 Mr. Israel, what is the scope of this testimony?

A.17 This testimony discusses the probabilistic treatment of internally initiated reactor transients and accidents (except those initiated by fire and sabotage) leading to core melt. It assumes that the IPPSS is an accurate reflection of the current, as-built-and-operated plants (except for the contemplated modifications identified in the licensees' response to the NRC Staff First Set of Interrogatories Concerning Questions 1 and 2, dated June 25, 1982).

This testimony does not cover the adequacy of the probabilistic treatment of fires in IPPSS which is discussed in the testimony of Benjamin Buchbinder et al, or adequacy of the probabilistic treatment of external events which is discussed in the testimony of Robert J. Budnitz. This testimony does not cover sabotage. Further the issue of pressurized thermal shock is discussed in the testimony on Board Question 1.4 and the issues of steam generator tube rupture is discussed in the testimony on Board Question 2.2.1.

Q.18 Please discuss what is meant by plant damage state.

A.18 A plant damage state is a group of accident sequences that result in core melt and have a common containment condition such as containment bypass prior to core-melt or core-melt with or without containment cooling available.

Q.19 Why does the Staff emphasize core melt accidents in it's risk assessment?

A.19 The core of an operating nuclear power plant contains radioactive materials which, if ineffectively contained, can cause harm to the population and environment in the vicinity of a plant. Even after an operating reactor is shut down, a mechanism for releasing radioactivity exists. This is called the "afterheat" or "decay heat" produced in the fuel after the nuclear chain reaction ceases. This decay heat diminishes gradually once a nuclear reactor is shut down, but within the first hours or days after shutdown, the decay heat released within the fuel has the potential to melt the fuel and breach each of the several barriers used to obstruct the release of radioactive materials. Such a phenomenon may take place if the decay heat in the fuel is not dissipated in controlled ways.

Because a "core melt" accident has the potential to release large quantities of radioactivity, it is the principal cause for concern among potential nuclear reactor accidents.

We do not differentiate between severe core damage and core-melt events and we will refer to both types as core-melt events. This assumption is conservative because the potential radioactive release for events that stop at severe core damage may be less than that for core melt events. The analyses have not been refined to differentiate the fraction of events that may terminate at severe core damage.

The timing of the core melt with respect to containment failure has an impact on the consequences because it affects the radioactive releases to the environment. If the containment building remains intact following core melt, the potential radioactive releases would be reduced and the consequences to the public would be small compared to sequences where the containment building fails above ground level or is bypassed prior to core melt or shortly thereafter.

Q.20 How were the plant damage states derived?

A.20 The Licensees performed a risk assessment of their plants (IPPSS) which included the frequencies of transients and accidents resulting in core melt. The Staff contracted with Sandia National Laboratories to review the core melt analysis portion of IPPSS and to derive modified estimates for core melt sequences as appropriate. Sandia grouped the core melt sequences into plant damage state categories based on the availability of containment cooling following core melt.

Q.21 Messrs. Kolb/Hickman, please describe the scope of the Sandia review of internally initiated events presented in the IPPSS and your findings.

A.21 The Sandia review of the IPPSS is presented in NUREG/CR-2934, Review and Evaluation of The Indian Point Probabilistic Safety Study. The IPPSS estimated the frequency of several hundred core melt accident sequences initiated by internal events. Because of the very large number of sequences considered in the report, and the time limitation placed on our review, it became necessary for us to focus on a subset. The subset which received the most extensive review were those identified in the IPPSS to dominate the internal event core melt frequency or the important plant damage state frequencies. There were 18 of these sequences, 10 for Indian Point 2, and 8 for Indian Point 3. The review of these sequences relied heavily on our past PRA experience. This experience aided us in searching for subtle methodological problem areas, potential omissions, and important analysis assumptions which could have a significant impact on the sequence frequency estimates. Our review discussed in NUREG/CR-2934 also entailed an evaluation of the basic building blocks of the IPPSS internal event analysis; namely, initiating events, fault trees, event trees, human errors, data, common cause and sequence analysis. These building blocks were reviewed to determine if possible errors, unrealistic assumptions, or omissions made by the IPPSS analysts could allow additional sequences to the above mentioned 18 to become important.

Q.22 Based on your review of the IPPSS, what is your overall impression of the internal event analysis presented in that study?

A.22 It is our opinion that the IPPSS internal event analysis is a state-of-the-art analysis performed by competent analysts and from it much is to be learned. Their treatment (modeling, assumptions, completeness, etc.) is comparable with other state-of-the-art probabilistic risk assessments. We did identify areas in our review where we thought alternative modeling or calculational techniques should be considered; and based on these, calculated revised estimates of the dominant accident sequence frequencies.

Q.23 With reference to the internal event building blocks mentioned previously, describe the major findings of your review.

A.23 Initiating events are plant occurrences which require a rapid reactor shutdown and subsequent safety system operation to prevent core melt. We found the initating events covered in the IPPSS to be relatively complete and their frequency estimates to be reasonable compared to those addressed in previous PRA's. However, an exception to this was found. We found no indication that the IPPSS considered an initiating event caused by a pipe break in the component cooling water system. This event was found in our review to be an important contribution to the core-melt frequency.

Fault trees are logic models which describe the various ways safety systems can fail. We reviewed all the fault trees presented in the IPPSS and found them in general to be a reasonable representation of the Indian Point safety systems. However, we felt some changes in the logic structure of the fault trees for the service water system, auxiliary feedwater system, and fan coolers were appropriate. The IPPSS analysts agreed with this conclusion and we factored these changes into our plant damage state frequencies. System unavailabilities presented in our evaluation NUREG/CR-2934 compare reasonably with estimates from other studies. We also noted that the IPPSS analysis was unduly conservative by not taking any credit for the main feedwater system to remove decay heat following a reactor scram.

Event trees are logic models which delineate the various combinations of safety system failures following an initating event leading to core melt and/or containment failure. These combinations are known as accident sequences. We reviewed all of the event trees and found the structure of most to be appropriate. We made changes in several for purposes of calculating our revised estimates. They were: 1) steam generator tube rupture, 2) loss of service water, 3) loss of component cooling water, and 4) anticipated transients without scram (ATWS). Differences in 1) and 4) above had the most impact on the results. The IPPSS steam generator tube rupture event tree did not include a containment bypass sequence caused by a stuck open secondary safety valve. The IPPSS ATWS event tree gave credit for an ATWS fix for which the utilities decided to defer implementation. We performed a revised analysis which considered sequences involving a steam generator tube rupture and stuck open safety valve, as well as, ATWS sequences which did not include the fix. We quantified these sequences and included them in our final plant damage state frequencies.

The human is the most difficult nuclear plant "system" to analyze. He can have both a positive and negative influence on the course of various accidents. Because of the very large number of human activities possible, we focused our human error review on those activities identified in the IPPSS to have a major impact on the dominant accident sequences. This investigation revealed that either no or limited procedures existed for several of the activities, e.g., feed and bleed core cooling, loss of component cooling water. In our revised estimates, we assigned bounding human error probabilities for these situations. Four activities which were important and identified as having emergency procedures were reviewed in some depth. These activities dealt with switchover from injection to recirculation following a LOCA. Of the four, our revised estimates resulted in higher human error probability estimates for two of them and lower estimates for two of them.

We commend the IPPSS for greater use of plant specific initiating event and component failure data than found in many past PRA's. Our evaluation indicated that the Bayesian methodology produced reasonable point estimate failure probabilities based on our comparison of the IPPSS estimates of the dominant accident sequences to estimates based primarily on the IPPSS--reported data. Our evaluation also considered statistical confidence limits on the occurrence frequencies of these sequences. These limits, based primarily on IPPSS--reported data, identify a range of sequence frequencies that are consistent with the data considered. We found that the IPPSS point estimates generally fell within these ranges.

Common cause events result in failure of multiple safety systems or subsystems which compromise designed redundancy. A common cause failure could be the result of a test or maintenance error, a common support system, or an unidentified cause. The IPPSS modeled the more important of these common cause failures. The IPPSS analysts, however, did not have available at the time they performed the work some common cause data sources which have recently been made available to us (e.g., Common Cause

Fault Rates for Pumps, EGG-EA-5289 and Precursors to Potential Severe Core Damage Accidents, NUREG/CR-2497). These sources suggest higher common cause system failure in some cases. Our revised results take into account these more recent common cause data sources.

Sequence analysis requires the logical combining and quantifying of the initiating event and the fault trees for each accident sequence defined by the event trees. At this point in our review, we requantified the dominant accident sequences based on the results of the review of the basic building blocks of the IPPSS. Each accident sequence was then assigned to a plant damage state. Comparing our results with those found in the IPPSS reveals some differences.

Q.24 What are the internal event plant damage state frequency estimates derived by the Sandia review, and how do they compare with the IPPSS estimates?

A.24 Table 1 summarizes the revised frequency estimates and compares them to the IPPSS. These frequency estimates were extracted from Tables 5.2-1 and 5.2-2 in our final report, NUREG/CR-2934, "Review and Evaluation of the Indian Point Probabilistic Safety Study," December 1982.

Q.25 What are the uncertainties in the results?

A.25 Uncertainties can be divided into three types: data, modeling, and completeness. Each of these types is addressed below with respect to internally initiated events.

Table 1

IPPSS and Revised Internal Event Plant Damage State Comparison

	IPPSS Estimates		Sandia Estimates	
Plant Damage State	IPS	IP3	IP2	IP3
Containment Bypass Prior to Core Melt	4.6(-7)	4.6(-7)	2.1(-7)	2.1(-7)
Core Melt Without Containment Cooling	1.1(-6)	7.1(-7)	6.7(-7)	5.7(-7)
Early Core Melt With Containment Cooling	5.4(-5)	1.8(-5)	1.2(-4)	1.8(-4)
Late Core Melt With Containment Cooling	3.4(-5)	1.1(-4)	1.0(-4)	1.0(-4)
Steam Generator Tube Rupture With Stuck Open Secondary Safety Valve	--	--	5.2(-7)	2.0(-7)

Data

Data uncertainties arise from a lack of infinite data pertaining to initiating event frequencies and subsystem and component failure probabilities. These types of uncertainties were evaluated for our revised list of Indian Point dominant accident sequences by using the Maximus methodology, referenced in our report, to calculate statistical confidence limits for the frequency of these sequences, and for the plant damage state frequencies. The results are presented in Chapter 5 of NUREG/CR-2934.

Modeling

Modeling uncertainties stem from the inadequacy of the PRA logic models to perfectly represent reality. Some of the more important modeling uncertainties are evaluated in NUREG/CR-2934 via a sensitivity study. The sensitivity issues addressed there are: 1) feed and bleed core cooling, 2) core melt/systems interaction, and 3) reactor coolant pump seal LOCA.

Completeness

Uncertainties associated with completeness are related to the inability of the analyst to evaluate perfectly and exhaustively all contributions to core melt because of oversights due to lack of knowledge or the limited scope of the analysis. We identified some areas where the IPPSS internal event analysis appeared incomplete. (Two of the more important areas were discussed previously; namely, the component cooling water system pipe break and steam generator tube rupture with a stuck open secondary safety valve.) Although we believe our revised estimates reflect a state-of-the-art level of completeness for PRA's, there is no guarantee that our review, which was based largely on our own PRA experience, was complete in an absolute sense.

Q.26 Mr. Israel, what core-melt frequencies for internal events are used in the NRC Staff's response to Commission Question 1?

A.26 The estimated core-melt frequencies (for internal events) used in the Staff's response to Commission Question 1 are presented in Table 2, as a function of plant damage state.

Under Containment Bypass Prior to Core Melt, the estimated frequency of interfacing LOCA (4×10^{-7}) is a compromise between the original IPPSS values (5×10^{-7}, Table 8.3-9 Event 24 and Table 8.3-10 Event 15), the initial Sandia reestimate (3 to 5×10^{-7}, draft Sandia report) and the final Sandia estimate (2×10^{-7}). The estimates are sensitive to the different models used to describe an interfacing LOCA, an event which has not occurred. The NRC Staff has not performed a separate review to differentiate between the various results because of the small difference among the estimates. So, we are using an estimate somewhat biased to the high side, for purposes of developing a risk estimate.

The estimated core-melt frequency for a steam generator tube rupture event with a stuck-open secondary relief valve was developed by the NRC Staff as discussed in Testimony for Board Question 2.2.1. The NRC Staff's evaluation considered multiple tube ruptures, while Sandia's evaluation considered only a single tube rupture. The multiple tube rupture events yielded higher estimated core-melt frequencies.

Table 2 Estimated Core Melt Frequencies For Internal Events

Plant Damage State	IP2	IP3
Containment Bypass Prior to Core Melt		
Interfacing LOCA	4×10^{-7}*	4×10^{-7}
SGTR	2×10^{-6}	2×10^{-6}
Early Core Melt without Containment Cooling	6×10^{-7}	6×10^{-7}
Early Core Melt with Containment Cooling	1.2×10^{-4}	1.8×10^{-4}
Late Core Melt with Containment Cooling	1×10^{-4}	1×10^{-4}

*Frequencies are events per reactor-year.

The estimated core-melt frequencies for Early Core Melt without Containment Cooling, Early Core Melt with Containment Cooling, and Late Core Melt with Containment Cooling were obtained from Sandia's reevaluation of the dominant sequences presented in IPPSS (Table 5.2-1 and Table 5.2-2 of the Sandia report, NUREG/CR-2934). Core melt sequences with containment cooling available generally involve LOCA's. The Sandia estimates for these categories considered additional aspects not considered in IPPSS in some of the sequencies. The Staff believes that the Sandia analyses for these categories are a better representation of the plants (within the scope of the study); however, the core-melt frequencies may be somewhat high because of the LOCA's frequencies used. The LOCA's frequencies used are from the IPPSS and represent point estimates that are higher than those used in previous PRAs. Because the plant damage states (core-melt with containment cooling) are not dominant contributors to offsite consequences, as shown in J. Meyer's testimony, the NRC Staff did not try to resolve what LOCA's frequencies are most appropriate for risk assessments.

I cannot confirm that the estimates of core-melt frequencies presented in Table 2 are correct in an absolute sense because of uncertainties associated with completeness, data, and modeling. The overall core-melt frequencies for internally initiated events appear to be consistent with other studies within the scope of the analysis performed and appear to be reasonably developed within the state-of-the-art based on the Sandia review of IPPSS.

Q.27 Does this conclude your testimony?

A.27 Yes

SANFORD L. ISRAEL

Professional Qualifications

I am a Risk Analyst in the Reliability and Risk Assessment Branch, Division of Safety Technology, Office of Nuclear Reactor Regulation. I am responsible for evaluating the reliability of nuclear power plants, identifying dominant risk sequences associated with plant operation, and assessing the relative importance of safety issues and proposed plant modifications.

I received a Bachelor of Science Degree in 1958 and a Master of Science Degree in 1959 from MIT. Both these degrees were in Mechanical Engineering.

From 1960 to 1966, I was an engineer with Nuclear Development Associates (later known as United Nuclear Corporation) where I was initially involved in test programs related to two-phase flow and hydrogen thermal conductivity. Subsequently, I was responsible for the thermal-hydraulic design of fuel for light water reactors.

From 1966 to 1974, I was Manager of the Thermal-Hydraulic Section at United Nuclear Corporation (later known as Gulf United Nuclear Fuels Corporation) where I supervised test programs, computer code development, and analysis related to the thermal-hydraulic design of light water reactor fuel.

In June 1974, I accepted employment with the Atomic Energy Commission (now the Nuclear Regulatory Commission) in the Reactor Systems Branch where I was responsible for reviewing various safety systems and analyses in the Sequoyah, North Anna, Floating Nuclear, and Alan S. Barton plants. In 1976, I was appointed Section Leader in the Reactor Systems Branch where I supervised the activities of several professionals who reviewed systems important to safety for conformance to the regulations, Standard Review Plan, and guidelines.

In 1979, I served on the Bulletins and Orders Task Force which developed and implemented recommendations based on concerns that were identified in the TMI-2 accident.

In May 1980, I joined the Reliability and Risk Assessment Branch where I have developed risk perspectives for several safety issues such as lightning strikes and the necessity of PORV's and have developed a position paper on the National Reliability Evaluation Program.

Jack W. Hickman

Educational and Professional Qualifications

Jack W. Hickman is Supervisor of the Nuclear Fuel Cycle Systems Safety Division of Sandia National Laboratories in Albuquerque, New Mexico. In this position, he is responsible for the performance of a variety of reliability and probabilistic risk assessment (PRA) programs under sponsorship of the U.S. Nuclear Regulatory Commission (NRC). Past and current programs include responsibility of performance of the risk assessments of light water reactor power plants in the Reactor Safety Study Methodology Applications Program (RSSMAP) and the Interim Reliability Evaluation Program (IREP). In addition, Mr. Hickman is or has been responsible for programs involving the use of risk assessment to address generic and plant specific issues before the NRC. Generic issues have included underground siting, auxiliary feedwater availability, DC power system reliability, and station blackout frequency. He is also currently responsible for a risk based evaluation of the issues identified in the Systematic Evaluation Program and the NRC probabilistic risk assessment training program and occasionally lectures on fault tree analysis and PRA for George Washington University. He serves as Chairman of the Technical Writing Group preparing the Industry/NRC PRA Procedures Guide and has responsbility for the IREP PRA Procedures Guide. He occasionally consults for the Advisory Committee on Reactor Safeguards (ACRS) on the subject of probabilistic risk assessments.

Mr. Hickman received his MS Degree from the University of New Mexico and BS Degree from Oklahoma State University, both in electrical engineering.

LIST OF PUBLICATIONS

1) Dominant Accident Sequences for an Ice Condenser PWR Plant, S.V. Asselin, J.W. Hickman, et al., ANS Winter Meeting, November 1978.

2) System Event Tree Analyses for Determining Accident Sequences that Dominate Risk in LWR Power Plants, S.V. Asselin, J.W. Hickman, et al., ANS Topical Meeting on Probabilistic Analysis of Nuclear Reactor Safety, May 1978.

3) The Reactor Safety Study Methodology Applications Program: Sequoyah #1 PWR Power Plant, NUREG/CR-1659, SAND-80-1897, February 1981.

4) Development and Organization of the Industry/NRC PRA Procedures Guide, International ANS/ENS Topical Conference on Probabilistic Risk Assessment, Hickman.

5) PRA Procedures Guide, Review Draft, NUREG/CR-2300, April 1982.

6) An Assessment of Auxiliary Feedwater Systems, M. Taylor, D. Carlson, M. Cunningham, S. Asselin, J. Hickman, G. Kolb, ANS Transactions, Vol. 33, 1979.

Gregory J. Kolb

Educational and Professional Qualifications

Gregory J. Kolb is a member of the Nuclear Fuel Cycle Systems Safety Division of Sandia National Laboratories in Albuquerque, New Mexico. In this position, he is responsible for the performance and review of nuclear power plant systems reliability analyses which are a part of several research and technical assistance programs funded by the Nuclear Regulatory Commission (NRC). Mr. Kolb has acted as systems analysis team leader for several nuclear power plant probabilistic risk assessments (PRA). Most recently he was the principal investigator for the Arkansas Nuclear One risk assessment as part of the Interim Reliability Evaluation Program. Prior to this assignment, he acted as team leader for the Oconee and Calvert Cliffs PRA as part of the Reactor Safety Study Methodology Applications Program. In addition, he was part of the Crystal River PRA analysis team and was one of the principal reviewers of the Zion PRA. Besides PRA activities, Mr. Kolb has been involved in the technical review of the "Rogovin Study" analysis of the accident at Three Mile Island, and a program which investigated the reliability of several nuclear power plant auxiliary feedwater systems. He has published several papers and reports in the field of PRA.

Mr. Kolb received a BS degree in Engineering from California State University Northridge in 1975 and a MS degree in Nuclear Engineering from the University of Arizona in 1977.

LIST OF PUBLICATIONS

1) The Reactor Safety Study Methodology Applications Program - Oconee Results. S.W. Hatch, G. Kolb - ANS Transactions, Vol. 38, 1981.

2) LWR Core Meltdown Accident Sequencer Phenomenology, P. Cybulskis, R. Wooton, G. Kolb, ANS Transactions, Vol. 41, 1982.

3) Reactor Safety Study Methodology Applicators Program: Calvert Cliffs #2 PWR Power Plant, S. Hatch, G. Kolb, R. Wooton, P. Cybolskis. NUREG/CR-1659, May 1982.

4) Reactor Safety Study Methodology Applications Program: Oconee #3 PWR Power Plant, G. Kolb, S. Hatch, P. Cybulskis, R. Wooton, revised May 1981, NUREG/CR-1659.

5) Interim Reliability Evaluation Program Analysis of the Arkansas Nuclear One-Unit 1 Nuclear Power Plant, G. Kolb, NUREG/CR-2787, June 1982

6) Insights from the Arkansas Nuclear One Unit 1 IREP Analysis, G. Kolb, Proceedings of the International ANS/ENS Topical Meetings on PRA, September 20-24, 1981, Port Chester, NY.

7) Systemic Event Tree Methodology Employed in the Interim Reliability Evaluation Program, G. Kolb, proceedings of the International ANS/ENS Topical Meeting on PRA, September 20-24, 1981, Port Chester, NY.

8) An Assessment of Auxiliary Feedwater Systems, M. Taylor, D. Carlson, G. Kolb, M. Cunningham, J. Hickman, S. Asselin, ANS Transactions, Vol. 33, 1979.

9) Arkansas Nuclear 1, Unit 1, Risk Analysis Results, by G. Kolb and D. Kunsman, International Meeting on Thermal Nuclear Reactor Safety, August 29 - September 2, 1982, Chicago.

DRF

PROFESSIONAL QUALIFICATIONS OF

ROBERT G. EASTERLING

Robert G. Easterling has been a staff member in the Reliability Department of Sandia Laboratories, Albuquerque, since August 1967, except for January to June 1974 when he was a visiting lecturer in the Department of Statistics, University of Wisconsin, Madison, and from June 1975 to June 1977 when he held the position of Statistical Adviser in the Applied Statistics Group of the Nuclear Regulatory Commission. He received his B.S. in mathematics and his M.S. and Ph.D. in statistics from Oklahoma State University in 1964, 1965, and 1967, respectively.

He is a Fellow of the American Statistical Association and has served in various organizational positions including president of the Albuquerque chapter. He is editor of the applied statistics journal, TECHNOMETRICS, and has written articles which appear in various statistical, reliability, and quality control journals and conference proceedings.

His activities at the NRC and Sandia have included consulting and research in statistical data analysis and in the application of statistical techniques to reliability and risk assessment. Publication and presentations in the area of nuclear risk assessment include:

"Probabilistic Analysis of Common Mode Failures." Proceedings of ANS Topical Meeting on Probabilistic Analysis of Nuclear Reactor Safety, May 1978.

"Statistical Problems in Nuclear Regulation," with R. H. Moore, Annual Meeting of the American Statistical Association, August 1978.

"Some Statistical Aspects of Uncertainty Analysis," 1978 ANS Annual Meeting.

Review of Anatomy of Risk, by W. D. Rowe, TECHNOMETRICS, May, 1980, p. 278, 279.

"Statistical Problems in the Assessment of Nuclear Risks," Annual Meeting of the American Statistical Association, August 1980.

"Comments on the Bayesian Method for Estimating Reactor Core Melt Frequency." Nuclear Science and Engineering, 1980, p. 202.

"Discussion of Conover/Iman Paper (Small Sample Sensitivity Analysis Techniques for Computer Models, with an Application to Risk Assessment)." Communications in Statistics, 1980.

"Some Observations on: Reliability Problems in Power Generator Systems." To appear in Proceedings of the 1981 DOE Statistical Symposium.

ALAN D. SWAIN, PH.D.

Dr. Swain is a member of the technical staff in the Statistics, Computing, and Human Factors Division at Sandia National Laboratories, Albuquerque, New Mexico, where he provides the technical direction for the Human Factors Group. In addition to his Sandia responsibilities, he is a regular lecturer at the University of Wisconsin - Extension, and he lectures annually in Europe.

He has been active in the nuclear weapons field since 1954 and in the nuclear power field since 1968. He has advised government authorities in England, Scotland, France, Germany, Denmark, Norway, Sweden, Finland, Italy, and South Africa on methods to reduce serious human errors in the operation of nuclear power plants (NPPs) and on methods to quantitatively assess the influence of human errors in these plants. In 1979 he met with the Swedish Commission on Evaluating Nuclear Power to advise them of the kinds and relative costs of human factors improvements in Swedish NPPs that could materially reduce the risk of human-induced failures in these plants. Before and after the Three-Mile Island accident he has assisted the Nuclear Regulatory Commission (NRC) by evaluating the impact of potential human errors in responding to possible transient conditions in NPPs.

He was responsible for the human reliability analysis in WASH-1400. The human reliability analysis model employed was THERP*, developed by Dr. Swain in the early 1960's for applications to weapon systems, and is now widely employed for a variety of man-machine systems. In Section 6.1 of Appendix III** the rationale for the high estimates of human failure probabilities in WASH-1400 is stated in terms of the poor human factors practices and design features in NPPs.

In 1975 he followed with a study*** of the Zion NPP in which detailed human factors problems were described (which are characteristic of all presently operating plants) plus suggestions for inexpensive changes in on-site practice, equipment, and operating procedures which would result in substantial improvement in human reliability. Subsequent experience in the Zion and other NPPs indicates that these suggestions have not been acted upon.

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* Swain, A. D., A Method for Performing a Human Factors Reliability Analysis, Monograph SCR-685, Sandia National Laboratories, Albuquerque, NM, Aug. 1963, 62 pp.

** "Human Reliability Analysis", Section 6.1 in Appendix III - Failure Data, of WASH-1400 (NUREG-75/014): Reactor Safety Study - An Assessment of Accident Risks in U. S. Commercial Nuclear Power Plants, U. S. Nuclear Regulatory Commission, Wash. D.C., Oct. 1975, pp III-59 to III-69 (written by A. D. Swain and H. E. Guttmann).

***Swain, A. D., Preliminary Human Factors Analysis of Zion Nuclear Power Plant, NUREG76-6503, U. S. Nuclear Regulatory Commission, Wash. D.C., Oct. 1975, 81 pp.

Dr. Swain's major effort in NPP research is the Handbook of Human Reliability Analysis With Emphasis on NPP Applications*, a nearly four-year research effort sponsored by the Probabilistic Analysis Staff, Office of Nuclear Regulatory Research, US NRC. The Handbook consists of models of human performance, estimates of human error probabilities (an uncertainty bounds) for NPP tasks, and a human reliability method and technique to apply the data and models to estimate the influence of human errors on safety and reliability of NPP operations. The models are unique in the field of human behavior in that they can be used to predict a wide variety of human behavior in an applied setting, they are testable, and they are modifiable as better data on human performance in NPPs become available. The Handbook is serving as the method for assessing the influence of human errors in the NRC's Interim Reliability Evaluation Program (IREP), a program to quantitatively assess the risk to the public of a sample of operating US NPPs.

Dr. Swain received his PH.D. in experimental psychology in 1953 from the Ohio State University. In 1950 he participated in the Psychology Corporation's study of the effectiveness of flight simulation, the first quantitative assessment of this training technique. From 1952 to 1958 he was with the American Institutes for Research. His research included applications to maintainability design and techniques and to training and training devices. From 1958 to 1961 he was with Dunlap and Associates, Inc., where he designed training programs, course curricula, and training aids and devices for the U. S. Navy, including the nuclear submarine program. In later work for this company, he designed the human reliability program for the Air Force's Manned Orbiting Laboratory. In 1961 he joined the Reliability Analysis Department at Sandia National Laboratories where most of his work has been in human engineering and human reliability analysis in nuclear weapons and nuclear energy. During this time he was Visiting Professor at the University of New Mexico for three years. Since 1972 he has spent up to one-fourth time lecturing for the Department of Energy, the Nuclear Regulatory Commission, the University of Wisconsin, and various foreign agencies. He is the author of numerous publications, including several chapters in books and his own books.

Dr. Swain is a Fellow of the Human Factors Society, a Senior Member of the American Society for Quality Control, an ASQC certified reliability engineer, and a certified psychologist in the State of New Mexico. He is a member of the Group of Experts on Human Error Data and Assessment of the Committee on the Safety of Nuclear Installations, Organization for Economic Co-Operation and Development, with headquarters in Paris. He meets annually with European experts in human reliability to assess their use of his methods and to advise them on human factors problems in nuclear power plants.

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* Swain, A. D. and Guttmann, H. E., Handbook of Human Reliability Analysis With Emphasis on Nuclear Power Plant Applications (Draft for Public Review), NUREG/CR-1278, Office of Nuclear Regulatory Research, U. S. Nuclear Regulatory Commission, Wash. D. C., Oct. 1980, approx. 600 pp.

January 1982
A. D. Swain

A LIST OF HUMAN FACTORS PUBLICATIONS OF SANDIA NATIONAL LABORATORIES

Items marked with an asterisk, an "at" sign, a plus sign, or a number sign can be obtained as follows:

(*) - National Technical Information Service, Springfield, VA 22161
(@) - Division of Technical Information and Document Control, U.S. Nuclear Regulatory Commission, Wash., DC 20555
(+) - the periodical indicated
(#) - the publisher indicated

The remaining items can be obtained (if copies are available) from Dr. A. D. Swain, Div. 1223, Albuquerque, NM 87185.

Items with SCTM, SCR, SCDC, SCDR, SLA, or SAND designations are Sandia National Laboratories' reports.

(*) 1. McCornack, R. L., Inspector Accuracy: A Study of the Literature, SCTM-53-61(14), February 1961, 29 pages.

(*) 2. Swain, A. D., System and Task Analysis: A Major Tool for Designing the Personnel Subsystem, SCR-457, January 1962, 26 pages.

(*) 3. Rook, L. W., Reduction of Human Error in Industrial Production, SCTM-93-62(14), June 1962, 29 pages.

(+) 4. Swain, A. D., "Reliable Systems Versus Automatic Testing," in Proceedings of the Ninth National Symposium on Reliability and Quality Control, Institute of Radio Engineers, New York, January 1963, pp 380-390. (Also SCR-582.)

(*) 5. Swain, A. D., Altman, J. W., and Rook, L. W., Human Error Quantification: A Symposium, SCR-610, April 1963, 20 pages.

✓(*) 6. Swain, A. D., A Method for Performing a Human Factors Reliability Analysis, SCR-685, August 1963, 62 pages.

(+) 7. Swain, A. D., "Human Factors in Design of Reliable Systems," in Proceedings of the Tenth National Symposium on Reliability and Quality Control, Institute of Electrical and Electronic Engineers, New York, January 1964, pp 250-259. (Also SCR-748.)

✓(*) 8. Swain, A. D., THERP, SCR-64-1338, August 1964, 12 pages.

(+) 9. Rook, L. W., "Evaluation of System Performance from Rank-Order Data," in Human Factors, 1964, 6, pp 533-536. (Also SC-DC-64-1119.)

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MS. MOORE: Mr. Israel --

JUDGE PARIS: Ms. Moore, could I interrupt for a moment. I would like to note for the record that, as I recall, three or four years ago, perhaps two or three years ago, I attended a course sponsored by the Department of Energy having to do with human factors engineering, taught by Dr. Swain in Bethesda. The course lasted for one week.

JUDGE GLEASON: Did you pass the course?

JUDGE PARIS: I received a certificate.

JUDGE GLEASON: Ms. Moore, back to you.

MS. MOORE: Your Honor, at this time I would like to mark as Staff Exhibit 5 for identification a document entitled "NUREG/CR-2934, Review and Evaluation of the Indian Point Probabilistic Safety Study."

JUDGE GLEASON: What will it be marked as?

MS. MOORE: Staff Exhibit 6.

JUDGE GLEASON: All right, that'll be marked as Staff Exhibit 6.

MS. MOORE: For identification.

JUDGE GLEASON: For identification.

(The document referred to was marked Staff Exhibit No. 6 for identification.)

BY MS. MOORE: (Resuming)

Q Mr. Hickman, did Sandia National Laboratories conduct a review of the Indian Point probabilistic -- a portion of the Indian Point probabilistic safety study?

A (WITNESS HICKMAN) Yes, we did.

Q And was that review embodied in a published document?

A (WITNESS HICKMAN) Yes.

Q Is the title of that document NUREG/CR-2934, Review and Evaluation of the Indian Point Probabilistic Ssfety Study?

A (WITNESS HICKMAN) Yes, it is.

Q Do you have a copy of that document before you?

A (WITNESS HICKMAN) Yes.

Q And that document embodies your review?

A (WITNESS HICKMAN) Yes, it does.

MS. MOORE: Mr. Chairman, at this time I would like to move that Staff Exhibit No. 6 be admitted into evidence.

JUDGE GLEASON: Is there objection?

Hearing none, the exhibit will be received into the record as evidence.

(The document referred to, previously marked for

identification as Staff
Exhibit No. 6, was
received in evidence.)

BY MS. MOORE: (Resuming)

Q Mr. Israel, could you please give a brief summary of this testimony.

A (WITNESS ISRAEL) Yes. The purpose of this testimony is to provide frequencies of plant damage states caused by internally initiated events that are used in Jim Meyer's testimony on radiological releases. This testimony also provides an assessment of the IPPSS results in the area of plant damage states caused by internally initiated events.

MS. MOORE: Your Honor, the witnesses are now available for cross-examination.

JUDGE GLEASON: All right. Mr. Blum, have you got your energy back?

MR. BLUM: In the interest of saving time, I'd like to propose something a little unusual, and that is, we passed out what I guess will be marked for identification UCS Exhibit 8, which is a couple of portions of a deposition taken of the four Sandia witnesses, and these are specifically the portions where Dr. Easterling has explained what is a fairly sophisticated analysis of uncertainty, both in general and in the IPPSS, and potential problems posed by Bayesian methods, the extent to which these were or were not important in IPPSS specifically.

In the cross-examination, to get to the ultimate conclusion we have to go through about three levels, the first of which is getting down completely and understanding the general approach that Dr. Easterling has given. If necessary, I can go through and replicate this in cross-examination.

I am feeling a little tired and fuzzy now, and have some trepidation that I would sort of be butchering a very elegant and distinguished witness by having to do that. What I would prefer to do is that, if everyone including the witness could read this and then the witness be given the opportunity to say whether he objects to any particular parts of it or that there is anything that he would change, anything he would want to add and so forth, and then the questioning can proceed with this in the record, and can probably then be accomplished in about one-eight of the time it would otherwise take.

I realize that is not strictly how things are done in courts, but I think probably it would be in the interest of all concerned to do that here.

MR. COLARULLI: Your Honor, the Power

Authority would object to that procedure, especially if it is a complex, as Mr. Blum described, sophisticated discussion. That is the kind of discussion that should be had on the record here, Your Honor, through questions and answers.

It is very possible that Staff would want to follow up. It's possible Licensees would want to follow up. And Mr. Blum's procedure really short-circuits a process that should be drawn out in terms of questions and answers, and not just trying to put in several pages from a deposition which is not part of this record.

MR. BLUM: We would not object to sharing this time with other parties, in case the Staff wished to ask clarifying questions, and especially the Board or even the Licensees. It just seems that for getting at the ultimate issue that has to be gotten out, if we can do the first two-thirds of it in a way that's both less prone to confusion and much more efficient, at least we should take a shot at doing that.

I guess maybe people might want to read it and then decide whether they feel it is the type of document --

MS. MOORE: Mr. Chairman, the Staff would object. I think that if Mr. Blum had wanted to do this there was ample time to give us notice, so that someone could read the deposition pages thoroughly and make that kind of decision. It was not given to us, and I believe that the proper way to proceed here is cross-examination.

The witness has been made available. He is not unavailable. Therefore, the deposition is not properly used to admit into the record. And if he wants to use it for impeachment that is a different story.

This deposition was not taken with the idea that it would be used in lieu of the witnesses' testimony and cross-examination. Therefore, we should not proceed that way.

MR. BLUM: One other possibility is that we could reverse the order and allow the Licensees to cross-examine first, and this would allow more time for thorough review. I guess if we thought of this particular objection we might have given it out this morning.

JUDGE GLEASON: Could I understand a little more fully, Mr. Blum, what the purpose is that you wish to make of the deposition? Do you wish to impeach the witness with respect to it?

MR. BLUM: No, in this case it is not to impeach the witness. It is simply to give the witness an opportunity to get out a fairly intricate series of premises and to -- it is essentially an aid to developing something that is sufficiently complex that if there's any frustration and confusion along the way of developing it the Board is liable to become somewhat impatient with it, although the Board would at least have this to see where we're going with it.

MR. BRANDENBURG: Mr. Chairman, it seems for all the reasons Mr. Blum has stated that this is exactly a subject matter that all of us in the room should hear here. I would join in the motions of the Power Authority and the Staff.

JUDGE GLEASON: Let's take a few minutes to read it.

(Pause.)

JUDGE GLEASON: Let's go back on the record.

Mr. Blum, there's no way we can let this in under the procedure you suggest. I sympathize with the physical aspects of your problem. If you'd like to take another break in the proceeding, we'll be glad to do that. We can ask Mr. Sholly if he wants to ask some questions.

I don't think we can admit it.

MR. BLUM: Well, I can proceed, I think, and replicate it.

CROSS-EXAMINATION ON BEHALF

OF INTERVENOR UCS

BY MR. BLUM:

Q Dr. Easterling, do you recall your deposition of January 12, 1983?

A (WITNESS EASTERLING) Yes.

Q And are you generally in agreement with what you said at that deposition?

A (WITNESS EASTERLING) Generally. I have a list of typos and some corrections.

Q But substantively, as far as you've been able to read in what was marked as UCS Exhibit 9 for identification, you are generally in agreement with that?

A (WITNESS EASTERLING) As far as I read, yes.

Q You would agree, would you not, with the statement that the field of analysis of PRA is still undergoing significant development? In particular, there is no generally accepted rigorous mathematical basis for uncertainty analysis?

A (WITNESS EASTERLING) Yes, I agree.

Q You also agree that there's some mathematical approaches that are available for this, but none of them are generally accepted; is that correct?

A (WITNESS EASTERLING) Correct.

Q What would you describe as some of the mathematical approaches?

A (WITNESS EASTERLING) Some of the mathematical approaches to uncertainty?

Q Yes. Is Bayesian methodology one of these?

A (WITNESS EASTERLING) Yes.

Q And is that essentially the methodology on which IPPSS is based?

A (WITNESS EASTERLING) Yes.

Q In what sense are there controversial areas with regard to Bayesian methodology?

A (WITNESS EASTERLING) The main areas of controversy concern the use and choices of prior distributions.

Q Would there also be controversial areas with regard to what is called state of knowledge, degree of belief, however you want to express it?

A (WITNESS EASTERLING) That is what I meant by prior distributions, yes.

Q And why are people uneasy, why are some people uneasy, with Bayesian methodology?

A (WITNESS EASTERLING) There are a couple of reasons. One is, can you really translate knowledge, a state of knowledge, into a quantitative expression with a probability distribution? Can that translation be made? That's the main controversy, I think, the main reservation.

Q In your mind, how difficult is this to do?

A (WITNESS EASTERLING) In my mind it is quite difficult to do it, to do it convincingly.

Q And why is that?

A (WITNESS EASTERLING) Because you are taking a sort of vague -- you're taking pieces of information which are expressed in various ways, which are in your mind, and you are converting those to numbers. That is not a direct one to one process.

Q So there is a problem of what are to a considerable extent subjectively based conclusions coming off of objective numbers; is that correct?

A (WITNESS EASTERLING) I didn't follow that.

Q Let me withdraw that question.

But aren't there things built into the Bayesian methodology that in practice alleviate that problem?

A (WITNESS EASTERLING) Yes. The Bayesian methodology, there are features that can alleviate the problem. Namely, when you have adequate data to make your results genuinely insensitive to the choice of prior distribution.

Q Do these necessarily alleviate the problem?

A (WITNESS EASTERLING) No.

Q Why not?

A (WITNESS EASTERLING) Because some prior distributions may not be overcomeable by certain amounts of data.

Q Can you think of an example using pumps, say?

A (WITNESS EASTERLING) No.

(Laughter.)

Q Could I refresh your recollection with page 33 of your deposition.

(Pause.)

Q Can you think of an example with pumps now?

A (WITNESS EASTERLING) Okay. The example given on page 33 had to do with, you know, hypothetically translating what a person might know about pumps to a prior distribution.

Q Is there some effect that by simply equating the hunches with the actual numbers, as is done in Bazian methodology, that what happens is that the uncertainty implicit in the process of forming the hunches gets masked?

A (WITNESS EASTERLING) That is possible.

Q When you say it is possible, does that mean you have instances where it is not true?

A (WITNESS EASTERLING) No.

Q So you think that it would be generally true of the Bazian approach?

A (WITNESS EASTERLING) What do you mean by it?

Q The uncertainty implicit in forming the hunches and then having the numbers be your hunches, that that uncertainty tends to get masked by using the Bazian approach.

A (WITNESS EASTERLING) Yes

Q This is generally true of the Bazian approach. Is that correct?

A (WITNESS EASTERLING) I think so, though in my deposition I added the fact that you could put priors on your priors on your priors ad infinitum.

Q But then wouldn't you just have the same problem all over again?

A (WITNESS EASTERLING) Yes.

Q Was this done in the IPPSS study, that they had a two-level prior distribution, one on top of the other?

A (WITNESS EASTERLING) No.

Q In terms of the component events, what did they have?

A (WITNESS EASTERLING) They had prior distributions specified for each component and failure mode.

Q Just one prior distribution?

A (WITNESS EASTERLING) Yes.

Q And what do they have in terms of initiating events?

A (WITNESS EASTERLING) Essentially it got down to one prior, but it took two steps to get to it.

Q For the component prior distributions, it was a matter of them choosing what their data base was to develop the prior distribution and then going from there based on the data. Is that correct?

A (WITNESS EASTERLING) Yes.

Q The kinds of uncertainties you are describing here, are these obviated by having a posterior distribution as well?

A (WITNESS EASTERLING) No.

Q Why isn't it?

A (WITNESS EASTERLING) Because the posterior distribution is a function of the prior distribution.

Q Are you saying that the posterior simply propagates the uncertainty in the prior? I realize there are different meanings to the word "propagate."

A (WITNESS EASTERLING) What I am saying is that the uncertainty represented by the prior is then merged with the data using posterior or updated distribution.

Q It is true, is it not, that risk analysis has not yet gotten to the point of being able to perform comprehensive uncertainty analyses?

A (WITNESS EASTERLING) I agree with that.

Q Is there anything foreseeable on the horizon that is going to make us much better off than we are now with respect to uncertainty analyses?

A (WITNESS EASTERLING) I don't see anything like that.

Q What are your own interests in this regard?

MS. MOORE: Mr. Chairman, that question is so vague.

JUDGE GLEASON: It is a pretty broad question.

BY MR. BLUM: (Resuming)

Q What would you most like to see done with regard to improving a situation with uncertainty analysis?

A (WITNESS EASTERLING) On Page 56, I think my own interests are getting a more visible role for actual data.

Q And by that you mean actual experiential data?

A (WITNESS EASTERLING) Yes.

Q And you would use that to create an estimate of uncertainty? Is that correct?

A (WITNESS EASTERLING) I would use that in evaluating uncertainty, yes.

Q So what you are really trying to do in essence is to have a more comprehensive and complete quantification of uncertainty. Is that correct?

A (WITNESS EASTERLING) No, I am just saying I would like to see a bigger role for data in whatever is done.

Q Well, what you would like to see is the assessments of uncertainty being more frank than they are now with regard to uncertainties created by a lack of posterior data. Is that a fair statement?

A (WITNESS EASTERLING) No. I don't really understand that statement.

Q You would agree that what we are dealing with here is not really a problem of honesty, would you not?

It is more a question of adequacy of expressing actual uncertainties?

A (WITNESS EASTERLING) I would agree.

Q And in what sense are expressions of actual uncertainties inadequate?

A (WITNESS EASTERLING) You would have to give me specific examples. I just can't answer that.

Q Well, for example, there are three types of uncertainties, are there not?

A (WITNESS EASTERLING) At least.

Q The three that I am familiar with include data uncertainty, modeling uncertainty, and completeness uncertainty. Would you accept that as a typology of three uncertainties?

A (WITNESS EASTERLING) Yes.

Q How effective has the PRA community been to date with regard to expressing completeness uncertainty?

A (WITNESS EASTERLING) I think completeness uncertainty is generally recognized. I think if you are asking, does the PRA community have a way of putting a number on completeness uncertainty, the answer is no.

Q Does the PRA community have convincing ways of bounding the extent of completeness uncertainty in a PRA?

A (WITNESS EASTERLING) I think to a certain extent.

Q Can you give us anything more certain on what extent or what you believe to be a valid way of bounding completeness uncertainties?

A (WITNESS EASTERLING) No, I haven't really thought about that at all.

Q Can you get anything more specific than to some extent it is possible to bound completeness uncertainties?

A (WITNESS EASTERLING) No, only in that maybe you hypothesize some event that was not included. We have a few hundred reactor years of experience. If that omission was a significant omission, then you would expect that event to happen. So that is one piece of evidence against certain types of incompleteness.

Q So to the extent that probability estimates don't get too much lower than is substantiated by the number of years of reactor experience, we have some basis for feeling confident that uncertainties are not larger than that.

A (WITNESS EASTERLING) Yes.

Q Are you aware of other convincing ways of bounding the completeness uncertainty?

A (WITNESS EASTERLING) Again, this is not an area I am really up on, but no, I am not aware.

Q Thank you.

You would agree that the PRA hasn't anything truly quantitative for the idea of completeness, with the idea of completeness for modeling uncertainty?

A (WITNESS EASTERLING) I didn't quite follow the question.

Q The problem is, I didn't either.

When you say that we don't have the ability to come up with specific convincing numbers for completeness uncertainty, you are referring to the PRA community, are you not, when you say we?

A (WITNESS EASTERLING) Yes, particularly.

Q And that would include the IPPSS study in it, would it not?

A (WITNESS EASTERLING) Yes.

Q Could you describe or give your impression of the treatment of uncertainty in the IPPSS study specifically with regard to completeness uncertainty, modeling uncertainty, and data uncertainty?

A (WITNESS EASTERLING) Data uncertainty is treated through the Bazian analysis.

Q That was one of your few answers I found disappointing during the deposition. Could you elaborate on that further?

A (WITNESS EASTERLING) You didn't give me a copy of the following page.

Q There was nothing there on the following page. I could if you want it.

MR. BRANDENBERG: Mr. Chairman, I have abided Mr. Blum thus far as he tied us up with these witnesses' testimony, but as I hear these questions more and more, I have more and more difficulty tying it up with these witnesses' direct testimony. So at this time I would like to object to this line of testimony as going beyond the direct testimony of these witnesses.

MR. BLUM: This is absolutely, completely relevant to his direct testimony and his area of expertise.

JUDGE GLEASON: The objection is denied.

MR. BLUM: Excuse me.

BY MR. BLUM: (Resuming)

Q Actually, there was something on the further page. Part of the problem is, one of the pages duplicated the previous page through a printing error.

JUDGE GLEASON: Do you have a copy of your deposition, Mr. Easterling?

WITNESS EASTERLING: Not the full -- I have it with me.

JUDGE GLEASON: Could you give it to them, and then refer to the page?

MR. BLUM: This would now be on Page 59, which follows 57, because 58 repeates Page 57.

BY MR. BLUM: (Resuming)

Q And my question would be, what additional would you say with regard to modeling uncertainty?

A (WITNESS EASTERLING) I am still not following your specific question.

Q Well, the initial question had been, what would you say about data uncertainty, modeling uncertainty, and completeness uncertainty, and you had finished addressing data uncertainty, and I am now asking, what would you say about modeling uncertainty in the IPPSS study?

A (WITNESS EASTERLING) As they were handled in the IPPSS?

Q Yes, that is correct.

A (WITNESS EASTERLING) I think the question of modeling uncertainty and completeness uncertainty are more questions in terms of the systems analysis, in which I was not involved, and so I really don't have much more to say on those topics.

Q Okay. Did they make use of something called a beta factor in the IPPSS study?

A (WITNESS EASTERLING) Yes.

Q And what is the beta factor?

A (WITNESS EASTERLING) That is a factor used in estimating the probabilities of dependent events.

Q And there have been data collected for at least some dependent events. Is that correct?

A (WITNESS EASTERLING) I think there have been some data collected which have been used to estimate beta factors.

Q But to the extent that one does not have data on that actual dependent event that is being calculated with the beta factor, this is an additional source of uncertainty in the beta factor itself, is it not?

A (WITNESS EASTERLING) Yes.

Q And the PRA community is not very advanced in its ability to quantify that uncertainty, is it, in the beta factors?

A (WITNESS EASTERLING) I can't say.

Q But in general, the validity of the beta factors and our ability to have a high degree of confidence in them is dependent upon having a data base that realistically simulates the actual condition that the beta factor is supposed to represent, is it not?

A (WITNESS EASTERLING) I am not sure that I followed your line.

Q Would it not be the case that our ability to have a high level of confidence in the accuracy of a beta factor that was assigned would depend upon there being some data base which involved events and conditions that closely simulated that for which the beta factor was being assigned?

A (WITNESS EASTERLING) That would certainly help.

Q Do you feel there would be a high degree of confidence in the beta factors without that?

A (WITNESS EASTERLING) I am not sure about using the phrase "high degree of confidence."

Q Do you feel that we could quantify -- Well, strike that.

There is also a potential problem, is there not, with regard to data that goes into the priors using a Bazian methodology? Is there not a problem with regard to selection of data?

A (WITNESS EASTERLING) I am not sure. A prior is supposed to be a representative state of knowledge prior to observing data, so I am not sure that I follow your line.

Q Well, what is there that you know of that went into the priors in IPPSS?

A (WITNESS EASTERLING) For the component data, or for the component prior distributions, sources of information included WASH-1400, IEEE Project 500 report, and some NUREG reports based on LER summaries.

Q And there was also some EPRI data base, was there not?

A (WITNESS EASTERLING) In terms of initiating events, yes.

Q What does EPRI stand for?

A (WITNESS EASTERLING) Electric Power Research Institute.

Q Thank you.

And the licensees did document, did they not, that they had used a variety of different data bases in constructing their priors in IPPSS?

A (WITNESS EASTERLING) Yes.

Q There were a number of instances, were there not, where the licensees -- I am sorry, the authors of IPPSS had a choice among different data bases, were there not?

A (WITNESS EASTERLING) That is possible. I cannot put a number on that.

Q But there were lots of instances where they selected one data base as opposed to using all the available overlapping data bases?

MS. MOORE: Mr. Chairman, is there a question pending?

JUDGE GLEASON: Yes, there is. I am not sure the answer is coming.

MS. MOORE: I didn't hear the question part of it.

JUDGE GLEASON: Well, the question part of it was, is it not -- do you understand the question?

WITNESS EASTERLING: I will repeat my answer from the deposition. In some instances, they use combinations of different bases from different data. They used information from LER summaries. They used some from WASH-1400. They combined these two sources of information pertaining to the same events to get their prior distributions.

BY MR. BLUM: (Resuming)

Q And there was the possibility of some sources in addition to the LER and WASH-1400, were there not?

A (WITNESS EASTERLING) There are other data bases around, yes.

Q And there are other data bases the licensees used in some instances. Is that correct?

A (WITNESS EASTERLING) Other than the LER's and WASH-1400.

Q Yes. I am sorry, I didn't hear that.

A (WITNESS EASTERLING) I said, other than the LER's and WASH-1400, there are some other data bases.

Q And those are the ones you identified befor as EPRI data bases and IEEE 500 data? Is that correct?

A (WITNESS EASTERLING) Correct.

Q Are you aware of any in addition to those two?

A (WITNESS EASTERLING) That they used?

Q Yes.

A (WITNESS EASTERLING) I don't recall any others that they used.

Q Are you aware of any that they could have used but did not use?

A (WITNESS EASTERLING) There is a data base referred to as NPRDS, Nuclear Plant Reliability Data System.

Q So it is your belief that they may or may not have used that? You don't know?

A (WITNESS EASTERLING) I don't know.

MR. BLUM: Ms. Moore, do you want to repeat your objection that you made in the deposition?

MS. MOORE: Ask your question.

BY MR. BLUM: (Resuming)

Q Are you aware of situations where there were data available that the authors of IPPSS did not use in forming a prior?

A (WITNESS EASTERLING) Am I aware of data bases they did not use?

Q Yes, in forming a prior.

A (WITNESS EASTERLING) Yes. I don't have any knowledge that they used NPRDS data.

Q And the NPRDS data stands for Nuclear Plant Reliability Data System. Is that correct?

A (WITNESS EASTERLING) Correct.

Q And that is an industry, NRC-supported data system, is it not?

A (WITNESS EASTERLING) Yes.

Q Mr. Kolb, do you have any information with regard to this matter?

A (WITNESS KOLB) I can read what I said in the deposition, which was, there were certain instances as we are going through the IPPSS report where we were checking LER's, and we found some cases where it looked like the data they used didn't actually match the LER data. That alerted us to, hey, maybe there's a problem here, and we went out for a couple of meetings with the IPPSS analysts and talked to them about it, and found out there was usually some good reason for them removing that for the data base. Either the design was changed, or that piece of data didn't actually apply to this plant. There was usually a good reason. I can't think of a particular reason offhand, but there was good reasons.

Q But in general they did use their judgment in screening the data as to whether it applied to the plant based on their judgment?

A (WITNESS KOLB) That's true, yes.

Q Dr. Easterling, do you have an estimate of roughly what percentage of the time there was data available that was not used?

A (WITNESS EASTERLING) No.

Q But I take it there were a substantial number of places where they didn't use all the available data; is that correct?

A (WITNESS EASTERLING) I'm not sure. Like I said, there are other data bases available in PRDS. There are a variety of reasons for not using that data.

Q But you do think that there were a number of places where they didn't use all the available data?

MS. MOORE: Mr. Chairman, I think that was asked and answered earlier.

MR. BLUM: All right.

BY MR. BLUM: (Resuming)

Q Was any systematic procedure used for choosing or selecting among alternative data bases?

A (WITNESS EASTERLING) That is not a question I can answer.

Q Did you know of any at the time of your deposition?

A (WITNESS EASTERLING) No.

Q Do you know of any now?

A (WITNESS EASTERLING) No.

Q How about for deciding whether to select one or to combine different data bases? Was there any specific procedure articulated for that?

A (WITNESS EASTERLING) Yes. I think you are referring to cases for most of the component failure parameter distributions, which involved combining information from the LER reports and from WASH-1400.

Q Isn't there some kind of mathematical problem in doing that and taking such disparate things and combining them?

A (WITNESS EASTERLING) I'm not sure what you mean by "mathematical problem."

Q Is there an element of arbitrariness involved?

A (WITNESS EASTERLING) Yes. I can refer you to the procedure in IPPSS of taking the WASH-1400 ratio of the fifth to ninety-fifth percentiles and using that as the ratio of the IPPSS twentieth to eightieth percentiles. The question arises, 20-80, why not 25-75, et cetera.

Q Would it make much difference whether it was the 75th or the 80th or the 90th percentile?

A (WITNESS EASTERLING) It depends, case by case.

Q There are some instances where it could make a great difference?

A (WITNESS EASTERLING) It is possible, yes.

Q Can you give us an example of one where it would?

A (WITNESS EASTERLING) Right, in the case of the interfacing LOCA, and the effect was rupture of two valves in RHR suction line, that using the WASH-1400 percentiles as the IPPSS percentiles, 5th to 95th percentage, the estimate obtained in that case versus that obtained using those percentiles as the 20th and 80th percentiles. The estimates differ by about three orders of magnitude.

Q Now, the general practice in most instances had been to use the 20th and 80th percentiles; is that correct?

A (WITNESS EASTERLING) Correct.

Q But in this case they chose to use the 5th and 95th percentiles; is that correct?

A (WITNESS EASTERLING) Correct.

Q And the effect of that was to lower the probability by approximately three orders of magnitude?

A (WITNESS EASTERLING) Yes.

A (WITNESS KOLB) But that calculation was redone, by the way.

Q Thank you.

As I recall, when that point was raised to Dr. Kaplan in a deposition he felt that it was a totally specious criticism, that it was simply applying an inappropriate test to a number. Would you agree with that?

A (WITNESS EASTERLING) No.

Q Dr. Kaplan felt that the figure created by three orders of magnitude created an unfair impression of lack of precision, because this was not the kind of number that should be looked at in terms of a difference of 5-95 or 20-80. Do you agree with that?

MS. MOORE: Mr. Chairman, reference is being made to a deposition and I believe he's saying something that another witness said, and this witness has never seen that deposition.

MR. BLUM: Well, it wasn't a problem the first time around, but I'll strike "Dr. Kaplan" and rephrase the question.

BY MR. BLUM: (Resuming)

Q Is the impression of imprecision created by this change from 5-95 to 20-80 being three orders of magnitude an inaccurate impression?

A (WITNESS EASTERLING) Not for the particular instance, no.

Q You believe that the impression of imprecision is accurate?

A (WITNESS EASTERLING) In that particular instance, because they had no plant specific data involved in the analysis.

WITNESS ISRAEL: May I make a comment?

JUDGE GLEASON: Go ahead.

WITNESS ISRAEL: I believe that Mr. Kolb has indicated that the analysis he is talking about, the event being the system LOCA, was that that was in the initial Indian Point probabilistic safety study. Interactions between the Sandia and the Licensee and PLG brought to light certain differences between Sandia and the PLG analysis, and they subsequently went back and redid the analysis providing additional data to almost render the point moot, I think, in terms of potential upper bound on the potential frequency for this type of an event.

So I guess we're talking about some hypothetical in the initial IPPSS analysis which has subsequently been revised in terms of the Sandia presentation, both in the draft report and also in the final report.

(Pause.)

BY MR. BLUM: (Resuming)

Q What would you have to do to know whether such differences between 30-70 and 90-10 or 20-80 and 5-95 would be substantial for all the entries on the bottom line risk tables in the IPPSS, these being Tables 8.3-2 and 8.3-3, the summary tables?

A (WITNESS EASTERLING) We essentially have to repeat the analysis, which is what we did, in terms of the dominant sequences. We repeated the analyses leaving out the distribution altogether to arrive at our bottom line figures.

Q So it would be the case that where you have adequate plant specific data you can go back and test the effect of the priors by measuring the plant specific data by themselves against the Bayesian posterior that comes out of the combining of the priors and the plant specific data; is that correct?

A (WITNESS EASTERLING) Yes.

Q But where you do not have adequate plant specific data, then you are unable to test the uncertainties attributable to the Bayesian priors; is that correct?

A (WITNESS EASTERLING) No. As long as you have some plant-specific data you can do it with data. It's not a question of adequate or inadequate. It is either none or some.

Q But if the data were far too hypothetical, if the data were far too small to be statistically reliable, then it wouldn't serve this function, would it?

A (WITNESS EASTERLING) You could still make the evaluation of what effect did the prior distribution have, yes.

Q But the problem would be, there would be so much uncertainty in any statistical extrapolation based on the limited plant specific data that it wouldn't cure your doubts as to the degree to which the prior distribution represented reality, would it?

A (WITNESS EASTERLING) I'm not sure. If you have very limited data, then your statistical confidence limits could be very wide and I guess encompass the estimates you would get from a wide variety of prior distributions.

Q Does the Bayesian method used give you much confidence in the results over and above the confidence you get simply by looking at the plant specific data?

A (WITNESS EASTERLING) No.

Q Does it give you any increase in your

confidence?

A (WITNESS EASTERLING) It gives me confidence somewhat in that plant specific data we used in a well defined manner.

Q But I'm asking you if the Bayesian methodology increases your confidence in the results in any way. What is your answer?

A (WITNESS EASTERLING) No.

Q Does the use of prior data increase your confidence in the answer?

A (WITNESS EASTERLING) It depends on the way it was used.

Q The way it was used in IPPSS, does it increase your confidence?

A (WITNESS EASTERLING) No.

Q Now it gets somewhat harder. The way you test various artifacts or uncertainties or whatever, due to the use of a prior distribution is essentially by comparing whether the prior distribution's mean results or its curve differs from that of the plant specific data, is it not?

A (WITNESS EASTERLING) That was the way we did our analysis, yes.

Q And in doing that, you picked areas where you did have sufficient plant specific data that you could do that kind of analysis?

A (WITNESS EASTERLING) Not totally. We picked areas where were the dominant plant -- or dominant sequences. There were some cases where to do our analysis we went beyond just the particular Indian Point data and used industrywide data.

Q As a prior or posterior?

A (WITNESS EASTERLING) Neither. As data.

Q Doesn't that run into the same problem as the Bayesian prior?

A (WITNESS EASTERLING) No, I don't think so.

Q Why is the Bayesian prior worse?

A (WITNESS EASTERLING) What I'm referring to, the particular example I can think of was check valve failures, which had happened very few times. So we went to the LER summaries and used the industrywide data for check valve failures in doing our alternate analysis. So I'm not -- it's completely separate from doing the Bayesian analysis.

I'm not sure I follow your question.

Q Did you use the same, say, industrywide data as the Licensees used when you did your analysis?

A (WITNESS EASTERLING) I think so. In those cases when the data came from the LER summaries, yes, but used them in a different way.

Q So if the Licensees had chosen from among prior data bases in order to closely match posterior data, that would show up in your analysis as the Bayesian method not posing any particular problem, would it not?

A (WITNESS EASTERLING) I'm not -- I don't understand your question because I don't understand what you mean by "posterior data."

Q I'm sorry, I meant plant specific data. Thank you for correcting me. I meant the plant specific data that the authors of IPPSS used in constructing their posterior.

A (WITNESS EASTERLING) And the question again was?

Q The question was, since the method for testing whether this possible problem of Bayesianism is a real problem in the IPPSS study is to in essence compare the plant specific data used in the posterior with the prior data, if the Licensees had been able to select -- I'm sorry -- if the authors of IPPSS had been able to select among data bases so as to match the posterior data, then this would show up in your review as Bayesian analysis posing no problem; is that correct?

A (WITNESS EASTERLING) No, I don't think so. That's a pretty complex thing you describe. You're saying, does our analysis indicate that somehow the data used in IPPSS were selected from available data bases. I really don't think I mentioned that at all.

Q Now, you testified before your analysis does not indicate one way or the other how the prior data, the industrywide data or whatever, the subjective estimate data, were selected. You just have what the Licensees did.

But the test you use for whether there's some sort of statistical contamination coming in from the priors is ultimately a test of, how much do the priors differ from the posterior plant specific data; that is correct, is it not?

A (WITNESS EASTERLING) Our evaluation had to do with comparing the IPPSS estimates to estimates we calculated based on plant specific data in the main, and our evaluation was that the point estimates given in the IPPSS were consistent with what the data indicated.

Q Let's try once more with a question. The consistency you are referring to, one of the things that could produce that consistency, is it not, is if the authors of the study were aware of plant specific data and went back and selected prior data or made prior subjective estimates in place of data that in fact resembled the plant specific data in their averages?

That is one possible way that the study could come out to show that Bayesian methodology introduces no contamination?

A (WITNESS EASTERLING) That is a possible way.

Q Okay. A way to know whether that in fact happened -- well, one bit of evidence that would be relevant for knowing whether that in fact happened would be to look at the various prior drafts and what situation the priors and posteriosrs, the priors and the plant specific data and the posterior, were at different times, to see whether one discovers some evolution toward trying to harmonize the plant specific data and the priors.

That could conceivably be detected in prior drafts, could it not?

A (WITNESS EASTERLING) It is conceivable, but it could also be misleading.

Q I'm sorry, I didn't hear you.

A (WITNESS EASTERLING) I said it's conceivable, but it could be misleading.

Q Okay, that's fair.

(Pause.)

Q Mr. Kol, what was your first -- do you recall what your first re onse was when you saw the containment numbers coming in from the authors of

IPPSS?

A (WITNESS KOLB) The containment numbers? Could you be more specific?

Q Well, with regard to when the containment of Indian Point is first predicted to deform in any way and with regard to how this was represented by the Licensees.

MS. MOORE: Mr. Chairman, objection. That is outside the scope of the direct testimony of these witnesses.

MR. BLUM: I withdraw the question.

JUDGE GLEASON: I think we could do without a judge in this proceeding.

I better take that back. Some of the Board members might agree with me.

BY MR. BLUM: (Resuming)

Q Mr. Kolb, what is the normal practice of Sandia with regard to whether to retain draft material as the study is in progress?

A (WITNESS HICKMAN) I'd like to try to answer that. To my knowledge there is no laboratory-wide policies concerning the retentions of drafts.

Q Mr. Kolb, do you know of any studies in Sandia where the practice has been to systematically destroy the drafts at the end of each stage of the analysis, moving from one cut to a later one?

I am sorry. I was asking specifically Mr. Kolb. You can supplement after he answers.

A (WITNESS KOLB) Systematically, no. However, as a matter of convenience, and to avoid further dissemination of bad information or wrong information, I personally have on occasion just gutted my office and cleaned out all my files.

Q So you are saying this is frequently left to the decision of the individual researcher?

A (WITNESS KOLB) Yes.

Q But there is not an agency-wide policy --

A (WITNESS KOLB) No.

Q -- to ask all researchers to destroy their data?

A (WITNESS KOLB) No.

MR. BLUM: The record should reflect the witness is shaking his head no.

WITNESS HICKMAN: If I may add to that, there are also occasions when an individual, a project leader or a supervisor may decide to destroy the drafts for that reason, so I think the answer is that it is very individual and situation-specific.

BY MR. BLUM: (Resuming)

Q And Sandia's practice, Mr. Hickman, this non-retention of documents, might be done in some areas of the study, but it is not conceivable that this could always be the case, that whenever conservatism was removed, you no longer needed any trace of the earlier version. Is that correct?

I can show you Page 75, 74, 75 of the deposition, if you want to refresh your recollection.

A (WITNESS HICKMAN) If you could ask the question again, maybe I could follow the question.

JUDGE GLEASON: Mr. Blum, if you are asking quetions which are covered in the deposition, why don't you just ask them directly off the deposition so they know what they are talking about? If they said something wrong then and there you can prove it.

MR. BLUM: Okay.

BY MR. BLUM: (Resuming)

Q Could you read on Line 19, beginning on Page 74, the question and then the answer that follows on the top of the page?

MS. MOORE: Mr. Chairman, I would ask, is this deposition now being used for impeachment?

MR. BLUM: Right now it is.

JUDGE GLEASON: I think it is, but I think he can find that out much more directly by just asking the question, and see if he still subscribes to that statement or whatever.

MS. MOORE: Then I would ask that Mr. Blum read the question.

BY MR. BLUM: (Resuming)

Q I am beginning on Line 19 of Page 74. "Question: But for Sandia's practice, this would be done in some areas of the study, but it is not conceivable that this could always be the case, that whenever conservatism was removed, you no longer needed any trace of the earlier version. Is that correct? Answer (Witness Hickman): That is correct. Answer (Witness Kolb): Right."

Do you two gentlemen agree with that earlier answer?

A (WITNESS KOLB) Yes.

Q It is true, is it not, that Sandia had originally offered to review the containment analysis portions of IPPSS as well as the plant analysis, is it not?

MS. MOORE: Mr. Chairman, that is beyond the scope of their direct testimony. I fail to see the relevance.

JUDGE GLEASON: Well, I think he is trying to impeach the witnesses, so he can go beyond the scope.

Is that what you are trying to do?

MR. BLUM: No.

JUDGE GLEASON: Then the objection is granted.

BY MR. BLUM: (Resuming)

Q There were some earlier studies done by some people in Sandia which came up with a substantially lower containment failure pressure for Indian Point than is in IPPSS, were there not?

MS. MOORE: Objection, Mr. Chairman. It is beyond the scope of this witness's testimony.

JUDGE GLEASON: I think it is relevant.

MR. BLUM: This is now for impeachment.

JUDGE GLEASON: It is an acceptable question. You can answer, please.

WITNESS HICKMAN: I am frankly not sure what previous studies were done on the containment area. We did not review the containment analyses in the IPPSS, and did not pursue that.

BY MR. BLUM: (Resuming)

Q Yes, I know, and the reason you did not review it is that the staff specifically asked you not to. That is correct, is it not?

MS. MOORE: That is the correct. It is the same question asked in a different form.

MR. BLUM: This now goes to a systematic bias. What I am trying to show is that there were earlier results that came up that were too pessimistic, and following those, for whatever --

JUDGE GLEASON: The objection is denied. The witness can respond to the question.

MS. MOORE: Mr. Chairman, there has been no foundation laid.

JUDGE GLEASON: He is trying to find out what the basis of his statements are, Ms. Moore. He is entitled to do that.

BY MR. BLUM: (Resuming)

Q Do you recall the question?

JUDGE GLEASON: He has probably lost the question at this point.

BY MR. BLUM: (Resuming)

Q The question is, the reason that Sandia did not review the containment analysis portions of IPPSS is that the NRC staff, which was doing the hiring, specifically asked you not to. That is correct, is it not?

A (WITNESS HICKMAN) I don't know whether they specifically asked us not to. What they did do was describe that area of the IPPSS which they wished us to study, and that was not included.

Q But you had volunteered to do the containment analysis as well, had you not?

A (WITNESS HICKMAN) We as a laboratory had done that, yes.

Q I am sorry. I didn't hear that.

A (WITNESS HICKMAN) Yes.

A (WITNESS ISRAEL) May I comment further on that?

Q Well, let me get back to one missing link, and then you can.

MS. MOORE: Mr. Chairman, these witnesses are testifying as a panel.

JUDGE GLEASON: I know, but, Ms. Moore, he is entitled to restrict questions to a particular member of the panel if he wants to, and that is what he wants to do.

BY MR. BLUM: (Resuming)

Q Is any member of the five of you on the panel aware of any earlier studies by Sandia which came up with a somewhat more pessimistic failure pressure for Indian Point than was ultimately used in IPPSS?

A (WITNESS EASTERLING) No.

A (WITNESS ISRAEL) No.

A (WITNESS HICKMAN) No.
A (WITNESS SWAIN) (Nods negatively.)
A (WITNESS KOLB) (Nods negatively.)
Q All right. Thank you.
In general, there was informal contact between Sandia and the authors of IPPSS going on throughout the Sandia review of IPPSS, was there not?
A (WITNESS KOLB) Yes.
Q So there was a lot of give and take both ways, was there not?
A (WITNESS KOLB) What do you mean by give and take?
Q Well, for example, that you would discover a problem in the study and they would fix it. Is that correct?
A (WITNESS KOLB) It was more of, we would have a question about the study and they would answer it.
Q Were there any instances where you discovered problems and they fixed it as a result?
A (WITNESS KOLB) If they fixed it, that would imply they would have to come out with amendments. How could they fix it?
Q I see. It is your position there was no informal give and take prior to the publication of IPPSS. Is that correct?

A (WITNESS KOLB) There were discussions, questions and answers. There was nothing written exchanged between us.

Q How about orally? There were exchanges that were oral?

A (WITNESS KOLB) Yes. Yes. Questions, answers.

Q And do you know whether any changes were made in the analysis in response to any of that informal give and take?

A (WITNESS ISRAEL) Could I get a clarification of the question here? Did you indicate that there was interaction between Sandia and the licensing people prior to the issuance of IPPSS?

Q That is what I asked.

A (WITNESS KOLB) Oh, okay. No. Let's go back.

JUDGE GLEASON: You had no give and take prior to the issuance of IPPSS?

WITNESS KOLB: We had discussions with the licensee, but on discussions of a different plant.

JUDGE SHON: I think not just the licensee but the people the licensee had hired to work on the IPPSS also. There was an implication a while ago that you had conversed with someone working on the IPPSS before the

IPPSS was published, or at least that was the impression the Board got.

WITNESS KOLB: We reviewed the Zion probabilistic safety study which Pickard, Lowe, and Garrick, and the same crew that was just sitting up here before did.

JUDGE GLEASON: We are talking about Indian Point.

WITNESS KOLB: That is the only give and take we had with them prior to the publication of IPPSS, but it was on a different subject.

BY MR. BLUM: (Resuming)

Q So it is possible they could have been incorporating some suggestions for Zion into the Indian Point IPPSS? Is that it?

A (WITNESS KOLB) I don't know.

Q Pardon me?

A (WITNESS KOLB) It's possible. Anything is possible.

Q Dr. Swain, you agree, do you not, that there are certain kinds of human error problems in nuclear power plants that are not modeled very well?

A (WITNESS SWAIN) Yes, that is true.

Q And that is not a function of any inadequacies on your part so much as it is a limitation of the state of the art, the PRA?

A (WITNESS SWAIN) That is true.

Q And what would be the things that you would agree are not modeled very well?

A (WITNESS SWAIN) Well, of course, this is a relative statement, but we model how people behave in an unusual or a stressful situation much less better -- we don't model that as well as we model how people behave in more rule-based decisions, such as using a procedure to carry out some calibration task. So, how people behave in unusual situations is modeled less well than how people behave in more proscribed situations.

Q Also, the particular problem of complex cognitive error, that also is an area that is not modeled very well?

A (WITNESS SWAIN) That is part of the relative less capability of modeling how people respond in an unusual situation. In the normal routine, the cognitive element, while it may be present in some small amount, is usually not significant.

Q Dr. Easterling, you are most confident when there is actual data that risk assessments can be based upon, are you not?

A (WITNESS EASTERLING) Yes.

Q And what are some areas where we do not have actual data, but only global assessments made by experts?

A (WITNESS EASTERLING) I think the human error area is one.

Q Are there any others in addition to human error that you are aware of?

A (WITNESS EASTERLING) I can't think of any right now. I think that perhaps there are in the external events area, but I am not very aware of those.

Q Dr. Swain, the author of IPPSS did not in any way improve upon your methodology contained in the NUREG that you authored, did they?

A (WITNESS SWAIN) No, they used the basic methodology, and the estimated failure probabilities in the handbook, but they did modify them on the basis of plant-specific situations, which of course the handbook encourages people to do.

Q I believe in the deposition it came out that there were some actual data showing that the actual rates of human error were somewhat higher than as modeled in the handbook. Do you recall that discussion?

A (WITNESS SWAIN) Oh, this had to do with a particular, if I remember the conversation, this had to do with some simulator data, data collected by one of our contractors, in which they discovered in some cases certain required actions in a written procedure were overlooked, sometimes -- all of the time in several trials, and as I pointed out, these were actions which were buried in a very poorly written procedure, and also they were actions which the nuclear power plant team which was being requalified or recertified did not regard as, shall we say, critical actions, and neither did the instructors.

And so I made the statements that if you are going to use data collected in a simulator, you have to be very careful that you don't assume that all actions are equally important in responding to a simulated transient or LOCA.

Q But in this case, you felt it was best to ignore that contradictory data. Is that correct?

A (WITNESS SWAIN) I would never use that kind of data in a PRA unless it turned out -- well, you see, the problem is, the people themselves and the instructors didn't regard those actions as especially important. They could be overlooked, in other words, without any detriment to coping with a simulated unusual event.

So, data from that kind of a situation would have very limited, if any, application to the real world

PRA.

Q In general, though, the figures in your handbook are subjective estimates of experts rather than actual data, are they not?

A (WITNESS SWAIN) Well, I think maybe the deposition might have given some misleading -- a misleading implication here. It is true that the estimates in the handbook of human error probabilities are what we call derived data. Now, I am using the term derived data to differentiate it from some very nice, hard error relative frequencies that one would like to collect on errors made in large numbers of performances of many of the nuclear power plant tasks that we are concerned with in PRA's.

Since that data do not exist, then what we had to do in order to derive the estimated HEP's, human error probabilities, in the handbook was to search for some kind of data that had at least psychological or behavioral similarity to the kinds of tasks and actions that are done in nuclear power plants.

We searched far afield. We collected some information from the British. I remember going back to one experimental study conducted in 1913, I believe, which was the closest data we could find for a particular task of interest for which we were trying to develop estimated human error probabilities.

So, I would prefer to say that the estimates that we have in the handbook are based to the extent possible on either experimental data or data from other kinds of operations, such as chemical plant operations, and they are certainly based on sound psychological theory.

As I say in my lectures, I think that our data at least on an ordinal scale are right on. On an interval scale, we hope they are pretty close. And when you get to the absolute or ratio scale, you are a little bit more up for grabs. There is more uncertainty. And that is why we tend to use rather large uncertainty bounds on our estimates.

Q In the draft Sandia report, you had a criticism of IPPSS that insufficient documentation was provided to evaluate whether the possibilities for comparable cost failures from human errors were appropriately assessed. Do you recall that criticism?

A (WITNESS SWAIN) Yes.

Q And the gist of that was that they just didn't give you enough information to enable you to assess common cost failures from human errors, is that correct?

A (WITNESS SWAIN) This was in certain errors, and to me when I did not see that in IPPSS, the IPPSS report, this was a red flag, and I mentioned this to Mr. Kolb. And as I recall, in the checking we found that those particular cases either didn't make any difference, or we got information from the IPPSS team that they actually had based their estimates on carrying out tasks in such a way as to minimize common cost failures due to human error.

Q I have just about concluded, but since you were the primary expert in this area, I would like you to describe for the Board in your own words or to give the specific mathematical formula used for modeling the situation of where there are four operators or two operators and two supervisors, all of whom come up with a wrong hypothesis that leads them to act incorrectly.

Could you tell the Board how that was calculated in IPPSS?

A (WITNESS SWAIN) Well, in general, what you have to decide first of all is of the four people that could be present, are all four of them going to be present. Obviously, if something occurs one minute after some transient or LOCA has been initiated, there will not be four people present. The fourth person would be the shift technical adviser. At least normally you wouldn't take credit for four people being present very early.

So the modeling we have in the handbook -- and I must say the modeling in the draft handbook is no longer valid because of the changes in staffing required by NRC since the TMI incident -- but the modeling in the IPPSS study does reflect the possibility of having four people there, and this is very reasonable. The only question would be then at any given time into a transient or a LOCA are they making say optimistic assumptions about the number of people being present. So this was one thing I had to look at.

Then the next thing you have to look at it is for any given task that is being performed, even if four people are there or three people are there, whatever, is it reasonable to presume that all of the people there would be actively engaged in this particular task?

And then having answered that question, you have to go further and say, all right, let's assume that N number of people are interacting; what are the levels of dependence among these N people? And to do that, the IPPSS analysts used the dependence model which is in the draft issue of the handbook and made judgments as to what levels of dependence would be appropriate. For example, between the two operators in some cases they would assess a high level of dependence, which simply means that relative to people operating independently, the two operators are not, and the likelihood of one operator catching another operator's mistakes are not as high as if they were operating independently.

Q And how would that be modeled quantitatively?

A (WITNESS SWAIN) Well, if you are dealing with a fairly -- just to cite a simple answer -- if you are dealing with fairly low basic human error probabilities -- and by that I mean probabilities of no greater than 10^{-2}, then if you have a high level of dependence, we are in effect saying that the conditional probability of failure of the second person -- that is, vis-a-vis catching a mistake made by the first person -- is .5. If we are dealing with a moderate level of dependence, it is .15. That is to say, this guy will catch errors made by the others about 6 out of 7 times. And if it is a low level of dependence, we're saying that the conditional probability of failure of a second person is .05, which is saying, in effect, that about 19 times out of 20 if he is involved in the task being done by the first person, he will catch the error about 19 times out of 20. Of course, if it is zero dependence, then you can assume, you know, use the basic probability of error for both. But that is a little unusual in many cases.

And then at the opposite extreme if it is complete dependence between two people, we assume that if the first person makes a mistake, the second guy will never catch it.

Q So for the complex cognitive error, the Three Mile Island type situation, that was modeled with four people present. Let's say it was a high level of dependence, a moderate level, and two low levels of dependence.

A (WITNESS SWAIN) It depends on which tasks they were analyzing. In the cognitive aspect now, that had to do with a recognition of what the particular problem was, and then the only ones that we analyzed, which were the high pressure and low pressure problems, recirculation problems.

It was our judgment or collective judgment -- certainly I agreed with this based on the handbook -- that in the time frame in which we are talking that the probability that the people would fail to know what the problem was was negligible.

Q Right. It was calculated at let's say what, .5 times .15?

(6:00 P.M.)

A (WITNESS SWAIN) I don't remember the numbers. I would have to go back to the particular page number in our evaluation. But it turned out to be negligible, as I recall. Isn't that right, Greg?

A (WITNESS KOLB) Talking about the cognitive? Yes, I don't remember that, but yes.

Q Thank you.

The last question goes to Dr. Easterling. You stated that the Sandia review looked only at the dominant sequences, is that correct?

A (WITNESS EASTERLING) Correct.

Q And these are the dominant sequences which --

A (WITNESS EASTERLING) Well, we looked at the individual component estimates, initiating events, all those.

Q I'm sorry. You did recalculations only for the dominant sequences.

A (WITNESS KOLB) That is incorrect. We reviewed the basic building blocks of the IPPSS -- event trees, fault trees, initiating events -- and gathered all of our findings in all of the different areas and asked ourselves okay, how do these findings impact on the dominant sequences, and also, how do these findings, how can they possibly make things that the IPPSS study said were not dominant become dominant, okay. And we also identified some sequences which they did not model and include in our results.

So just to say that we looked at the IPPSS dominant sequences is a false statement.

Q I take it you would all agree that that the same generic state of the art limitations on probabilistic risk assessment that applied to IPPSS also apply to you in your work. Would you all agree with that?

A (WITNESS EASTERLING) I don't know what you mean.

A (WITNESS HICKMAN) I don't understand.

JUDGE GLEASON: They don't know what you mean, Mr. Blum.

BY MR. BLUM: (Resuming)

Q You do all agree that there are certain limitations on the accuracy of PRAs that are not simply a function of particular errors of the particular group doing PRA, but there is some limitations imposed by the state of the art itself. You would agree with that, wouldn't you?

MS. MOORE: Mr. Chairman, that is pretty general.

JUDGE GLEASON: Well, let me just answer it yes.

(Laughter.)

WITNESS HICKMAN: The general answer to the general question is yes.

JUDGE GLEASON: Is that it, Mr. Blum?

MR. BLUM: All right. I'll accept your answer, Judge Gleason.

JUDGE GLEASON: We agree to that.

MR. BLUM: Thank you.

JUDGE GLEASON: Mr. Hartzman.

MR. HARTZMAN: I have just a few questions.

JUDGE GLEASON: Good.

MR. HARTZMAN: Of course, one never knows what occurs.

BY MR. HARTZMAN:

Q I would like to refer you to page 10 of your testimony, the paragraph in the middle of the page where you are discussing your evaluation of occurrence frequencies of certain initiating event sequences following from them. And you conclude the paragraph by saying, "These limits, based primarily on IPPSS reported data, identify a range of sequence frequencies that are consistent with the data considered. We found that the IPPSS point estimates generally fell within these ranges."

Those, I take it, are the ranges which you calculated in your review of IPPSS, is that correct?

A (WITNESS EASTERLING) That is correct.

Q And the point estimates which fell within those regions, was that the median?

A (WITNESS EASTERLING) No. That was the post-data means.

Q Would the upper bound estimates of IPPSS have fallen within your ranges?

A (WITNESS EASTERLING) I don't know.

Q You did not calculate that?

A (WITNESS EASTERLING) No.

Q Can you tell where in the interval the median point estimate fell within your range?

A (WITNESS EASTERLING) No, because they didn't tabulate your medians.

Q So you would not know where their worst possible estimates would have fallen either inside or outside your range that you calculated, is that correct?

A (WITNESS EASTERLING) No.

Q Now, you indicate on -- this is in NUREG-CO2-934 -- on page 2.6-11, that indicates in the last paragraph on that page that for a large LOCA your calculations would yield an upper 95 percent statistical confidence limit of .14 occurrences per year, which is considerably more pessimistic than Indian Point's 95th percentile.

Do you know precisely what that difference is?

A (WITNESS EASTERLING) The difference between --

Q Between your calculation .14 occurrences per year and the Indian Point 95 percent.

A (WITNESS EASTERLING) It's in the IPPSS. I don't know it.

I think for a large LOCA, if I recall correctly, their 95th percentile is about 6 times 10^{-4} -- 3, excuse me, 10^{-3}.

Q And yours would be 1.4 times 10^{-2}, is that correct, or is it 10^{-1} ?

A (WITNESS EASTERLING) I'm not sure even by ours.

Q Well, on page 2.6-11 you indicate a statistical -- your upper 95 percent confidence limit of .14 occurrences.

A (WITNESS EASTERLING) No, that is not ours. If you express their posterior mean variance as effective data, you get effective data of essentially no occurrences in 21 years, which leads to that bound of .14 in our calculations. We estimated LOCA frequency based on no occurrences in 500 years, which is the preceding paragraph.

MR. HARTZMAN: I have no further questions, Your Honor.

JUDGE GLEASON: Mr. Brandenburg.

MR. COLARULLI: Judge Gleason, the Power Authority would like to proceed first.

BY MR. COLARULLI:

Q Dr. Easterling, I believe in your answers to Mr. Blum you discussed the nuclear plant reliability data system, that is correct?

A (WITNESS EASTERLING) Yes.

Q And I believe you stated that there were a variety of reasons why one would not use that data base, is that correct?

A (WITNESS EASTERLING) In my deposition I discussed that, yes.

Q What reasons do exist for not using that particular data base?

A (WITNESS EASTERLING) I think there are questions about the completeness and accuracy of that data base. It is a voluntary system. They've had lots of startup problems. And it's not what you would call a mature data base.

Q So would you recommend using that data base?

A (WITNESS EASTERLING) No.

Q On page 109 of your deposition to which Mr.

Blum referred you are discussing the differences between using a 90th percentile and 10 versus the 30-70, et cetera. I believe that you testified that you repeated the analyses independently in an effort to check the results of the IPPSS, is that correct?

A (WITNESS EASTERLING) Are you referring just to the interfacing LOCA sequences or just in general?

Q How about just the interfacing LOCAs.

A (WITNESS EASTERLING) Yes. From the original analysis published in IPPSS we did repeat the calculations using 20th-80th percentiles versus 5th and 95th percentiles.

Q And your discussion at the bottom of page 109, the top of 110 refers to that reanalysis.

A (WITNESS EASTERLING) No. That was subsequently. After that problem surfaced, as we discussed earlier, Pickard, Lowe and Garrick reanalyzed the interfacing LOCA sequence using industry data, not using the WASH-1400 assumptions. So that is the situation that we reanalyzed.

Q And what were the results of that reanalysis compared to the IPPSS?

A (WITNESS EASTERLING) On the interfacing LOCA we came out with a lower estimate than IPPSS.

Q By what factor were you lower?

A (WITNESS EASTERLING) I believe roughly a factor of two or three.

Q Could you turn to page 11 of your testimony? Actually, the bottom of page 10 and the top of page 11. I believe it is your part of the testimony in which it discusses the use of some other common cause data sources. Is that part of your testimony?

A (WITNESS KOLB) Yes.

A (WITNESS HICKMAN) Yes.

Q And did you take into your analysis these additional common cause data sources?

A (WITNESS KOLB) Yes.

Q And what impact did taking into account these additional data sources have in terms of the risk posed to the public?

A (WITNESS KOLB) It increased some of the system unavailabilities, and some of them it didn't have any effect. I don't think we specifically tried to answer that question, but those common cause data sources are factored into our results.

Q Do you know specifically what effect including these common cause data bases had on the ultimate public health risk?

A (WITNESS KOLB) We did not address the risk question. We stopped at the plant damage states for one thing.

JUDGE SHON: Sir, did the inclusion of this later data substantially -- I use the word advisedly -- affect the probabilities of severe core damage states, double them or triple them or make them ten times as much, anything like that?

WITNESS KOLB: I don't recall that as being a driving factor.

WITNESS HICKMAN: I think part of the answer is we made no attempt to determine the effect of that particular change. I'm not sure we really know the answer.

JUDGE SHON: I'm sorry, Mr. Colarulli. Go ahead.

WITNESS KOLB: Other things were more important than these common cause factors in developing the core melt probability that we predicted -- things like a pipe break in the component cooling water system. We assessed an accident initiated by that sort of a pipe break had a fairly large impact on the core melt frequency. That particular sequence the IPPSS study did not treat.

BY MR. COLARULLI: (Resuming)

Q Mr. Kolb, do you agree or are you generally comfortable with the Bayesian approach that is used in the IPPSS?

A (WITNESS KOLB) My past experience has been primarily centered around point estimates rather than statistical methods. I don't consider myself a statistician.

Q Do you have an opinion on the use of the Bayesian approach in the IPPSS?

MR. BLUM: I object. This is beyond the scope of the witness' expertise.

JUDGE GLEASON: You raised this issue itself.

MR. BLUM: I raised it specifically with Dr. Easterling. He's the statistician.

JUDGE GLEASON: They are all part of a panel, so let's hear what he has to say.

WITNESS KOLB: I'll give you my opinion. As far as predicting a point estimate it seems reasonable, but beyond that I have no opinion.

BY MR. COLARULLI: (Resuming)

Q Mr. Hickman, do you agree with it, are you generally comfortable with the Bayesian approach that is used in the IPPSS to evaluate data?

A (WITNESS HICKMAN) I think based on the work that Dr. Easterling did, to compare the result of the Bayesian methods with what he would arrive at in his techniques leads me to believe that the way it was used had little effect on the results. Therefore, I am not uncomfortable with it.

Q Mr. Israel, are you basically comfortable with the Bayesian approach used in the IPPSS, too?

A (WITNESS ISRAEL) I guess I'd have to agree with Jack that when we started out this review I wasn't aware of the two different philosophies on statistics. So I think that in terms of reviewing the IPPSS, I think we give it a fair try since we had someone who was not in the Bayesian camp, so to speak.

As a result, I think it got a very critical review in terms of the results, and the results, the overall results didn't change that much, and the changes I think that did occur in terms of core melt frequencies were not due necessarily to the statistics or the beta factor but due to more systems analyses effects and to the component cooling water. There was also analysis of the LOCA, the research situation, things of that nature. By and large it had no effect, as far as I could see.

Q Dr. Swain, do you have an opinion on this question?

A (WITNESS SWAIN) I wasn't aware that the Bayesian approach was used in my area. I just thought they used the numbers out of the handbook, and if they used the Bayesian approach, I didn't detect it.

Q Could you turn to page 15 of the testimony? This question is for anyone on the panel, but I assume, Mr. Easterling, you can answer it.

Table 2 at the top, are those core melt frequencies medians or means?

A (WITNESS ISRAEL) Those are point estimates. I can't characterize them as medians or means.

MR. COLARULLI: I have no further questions.

JUDGE GLEASON: Ms. Moore, do you have any redirect?

MS. MOORE: Is Con Edison going to cross?

JUDGE GLEASON: I assumed that you were doing the cross for both. Was I incorrect?

MR. BRANDENBERG: You are incorrect, Mr. Chairman.

JUDGE GLEASON: I am incorrect. All right, Mr. Brandenberg, go ahead.

CROSS EXAMINATION ON BEHALF OF CONSOLIDATED EDISON

BY MR. BRANDENBERG:

Q Mr. Kolb and Mr. Hickman, earlier you were asked by Mr. Blum about communications that you had with Pickard, Lowe, and Garrick during the course of your review of the Zion probabilistic risk assessment.

To put that in some sort of chronological perspective, could you tell us when the period was that you were engaged in substantial frequent discussions with the Pickard, Lowe, and Garrick firm respecting the Sandia PRA? Excuse me, I misspoke. The Zion PRA. Please forgive me.

A (WITNESS HICKMAN) The Zion PRA or the Indian Point PRA?

Q The Zion PRA. As I understood the substance of your testimony, you said you had, during the period of time that Mr. Blum was inquiring about, you had no discussions with Pickard, Lowe, and Garrick regarding the Indian Point study, but you did have discussions regarding the Zion study, and my questioning relates solely to the Zion study.

And it is, when were those discussions taking place?

MS. MOORE: Mr. Chairman, that is beyond the scope of the testimony.

JUDGE GLEASON: I don't know what that has to do with the testimony. Mr. Blum asked a different question in which he got that as an answer, but what does that have to do with it?

MR. BRANDENBERG: Well, I wouldn't have asked the question had Mr. Blum not asked it, Mr. Chairman.

JUDGE GLEASON: I realize that. But do you think that is important to getting something valuable in the record?

MR. BRANDENBERG: I think it is important enough to correct a possible misapprehension that may be in the record based upon Mr. Blum's question.

JUDGE GLEASON: Well, answer the question.

WITNESS KOLB: To put it in the context of relation to when we started the Indian Point review, we started the Indian Point review around the end of April, '82, and we conducted the Zion review from November of '81 up through March of '82, so as soon as we finished the Zion review, we had a break for a month, and then we started on the Indian Point review.

We had a meeting with Pickard, Lowe, and Garrick on Zion in January of '82.

BY MR. BRANDENBERG: (Resuming)

Q Gentlemen, I would like to ask you a question about Answer 23 to your testimony that appears on Page 8. Mr. Kolb, I believe you addressed this earlier. My question relates to the component cooling water system pipe break initiator that you evidently turned up in connection with your review, and which you state here, it was found by you to be an important contributor to core melt frequency.

And my question is, from where did you derive the frequency of pipe failure that you used in your modeling?

A (WITNESS KOLB) We extracted it directly out of the component cooling water system analysis in the IPPSS.

Q And how about the frequency of pipe failure?

A (WITNESS KOLB) That is what I am talking about. They analyzed the component cooling water system in terms of failure frequency of the system by pipe failure.

Q Do you know whether this failure frequency was for high pressure pipe failures or for low pressure pipe failures?

A (WITNESS KOLB) It was supposedly applicable to the component cooling water system, which is low pressure pipe, I would assume.

Q Now, did your analysis reveal that these pipes failed through ductile or brittle failure, or what was the failure mechanism?

A (WITNESS KOLB) We didn't try to postulate a particular failure mechanism. However, that pipe data, to the best of my knowledge, comes from WASH-1400, which was based on not only nuclear plant data but non-nuclear plant data, which conceivably could cover low pressure pipes and that sort of thing.

Also, I am not convinced that that data doesn't include other than pipe failure caused by embrittlement or overpressure. I mean, what about the possibility of a crane or something running into the pipe, or that sort of thing?

Q That was my next question. You anticipate me. Do you know whether the failure rate you used included failures due to traumatic things?

A (WITNESS KOLB) I don't know.

Q Now, after the component cooling water system pipe break that you have identified here, you took that to a core melt state. You must have, obviously, because it turned out to be a substantial contribution to your core melt frequency. How did you do that? What following series of mechanisms did you find were taking place from the point of the component cooling water system pipe break to the incidence of core melt?

A (WITNESS KOLB) That is discussed in detail in Section 4.6 of NUREG-2934, and let me summarize it. You have your pipe break. The system empties in about five to ten minutes, in that ball park. You lose your heat sink, which is required to cool the safety injection pumps and the charging pumps.

Also, you lose cooling to the thermal barriers of the reactor coolant pumps. Because you lose the thermal barrier coolers to the reactor coolant pumps, the charger pumps, you will use the seal cooling. They subsequenly fail, according to the IPPSS models, resulting in like a 1,200 gallon per minute LOCA, and since you safety injection pumps have also lost bearing cooling, they cannot respond to the LOCA, and they will fail, based on an analysis by Con Ed, in about 15 minutes.

Q So in substance, then, the component cooling water system pipe break leads to what has been described in these hearings as a reactor coolant pump seal failure?

A (WITNESS KOLB) Right.

Q Now, you mentioned the time period of 15 minutes in connection with Sandia's analysis. Did you form a point of view as to the realism surrounding the period of time after which a coolant was lost before the pump failed? The pump seals?

A (WITNESS KOLB) Pump seals?

Q You mentioned 30 minutes. I think it was 15 minutes.

A (WITNESS KOLB) It was 15 minutes for the safety injection pumps.

Q Now, have you formed a point of view as to whether that is conservative?

A (WITNESS KOLB) We don't have verifiable evidence which would repute that postulated seal failure. However, our experience with reactor coolant pumps of slightly different design and some experiments that have been run on those types of seals suggest that they will suddenly let go like it is postulated in the IPPSS.

Q Do you have an opinion as to whether the use of 30 minutes for the modeling of the failure of the reactor coolant pump seal failure is highly conservative, best estimate?

A (WITNESS KOLB) For Westinghouse pumps, I don't really know too much about Westinghouse pumps. For a Byron Jackson pump, I have seen calculations which suggest five GPM each at 30 minutes, TGPM at an hour, and it stays constant thereafter. I have -- We raised the same concern in the Zion review, and their response to us was that 300 GPM per pump is a bounding worst case analysis, and they feel it is much less.

However, they also said that they couldn't produce the calculations to support that with their flow rates.

Q Gentlemen, my next question relates to the same answer, but over on Page 9 of your testimony, the steam generator tube rupture event.

And I would like to ask you a question about Page 4.1-2 of NUREG-2934, in which you discuss the sequence.

Now, as I understand your modeling under the line of discussion here, under letter B -- this is the second line across the page -- you assumed a probability of 1.0 that the secondary system safety valves demanded open.

A (WITNESS KOLB) Yes.

Q What assumptions did you make in modeling this sequence about the failure of the residual heat removal system?

A (WITNESS KOLB) For this sequence, Sequence B?

Q Well, Sequence A. Forgive me. I moved up one line.

A (WITNESS KOLB) Well, it is annotated down here with a note at the bottom of the page, but we basically assumed that the driving failure of the system would be failure of both pumps. We assumed no operator errors or support system faults because the support systems are operating prior to the accident and establishing RHR within 12 to 14 hours, or whatever, whatever the accident model. I think it is about that.

Establishment of RHR assuming the operators do routinely through shutdown.

Q Based on these assumptions, would you characterize your modeling of the steam generator tube rupture sequences as quite conservative?

A (WITNESS KOLB) I think the thing that is probably the most conservative is that the safety valves would be demanded open in P' bability One. I think that is probably the certain assumption. But that was based -- that wasn't just a guess. That was based on the

Indian Point procedures which have the operators isolating the power operated relief valve on the secondary following a rupture. We don't think that is such a good idea.

Q My next question is addressed to you, Mr. Israel, and it relates to the passage later on in this paragraph we have just been discussing. It relates to the ATWS situation.

My question is, why, if you do know, did the Indian Point licensees prefer the implementation of the ATWS fix such as you discussed here in your testimony?

A (WITNESS ISRAEL) As far as I know, that is how I read the statement of the various letters. I guess we got one from Con Ed and a different one from PASNY on different dates. My reading of those indicated that essentially they were deferring the implementation of the ATWS until some later time.

Q Do you have any other understanding as to the circumstances under which the Indian Point licensees determined that the ATWS modification should be deferred?

A (WITNESS ISRAEL) Presumably it was described in this letter. I just don't recall what it was.

Q Can you tell us at present within the NRC what the regulatory status of the ATWS guidance was?

MS. MOORE: Mr. Chairman, I think that is beyond the scope of his testimony.

MR. BRANDENBERG: I don't believe it is, Mr. Chairman. We are discussing it right here on Page 9.

JUDGE GLEASON: What was the question again, Mr. Brandenberg?

MR. BRANDENBERG: Whether he knows what the status, the current regulatory status of ATWS guidance to licensees is within NRC.

JUDGE GLEASON: Answer yes or no.

WITNESS ISRAEL: Yes and no. In terms of the Indian Point plants, I believe that that was part of the confirmatory order dealing with the ATWS fix. I am not familiar with what specific guidance we have given to other plants. I do know that we are in the process of rulemaking on ATWS, and that issue will be, for other plants, decided once the rulemaking is finished.

JUDGE SHON: Excuse me, Mr. Brandenberg. What was the ATWS fix mentioned here?

WITNESS ISRAEL: I believe it was, provide an alternate turbine trip mechanism that would not go through the scram system.

JUDGE SHON: Thank you.

BY MR. BRANDENBERG: (Resuming)

Q Mr. Israel, we have had a lot of discussion here today about Amendment 1 to the IPPSS. Have you had an opportunity to review that document?

A (WITNESS ISRAEL) No, I have not.

Q Have you had any discussions with anyone as to whether or not the Amendment 1 of IPPSS defines that there is no effect on risk associated with removing that feature from the Indian Point PRA?

Do you have any information on that?

A (WITNESS ISRAEL) No.

Q My next question relates to the passage at the bottom of Page 9 and the top of Page 10, the sentence starting at the bottom of Page 9: "This investigation will reveal that -- limited procedures existed for several activities, feed and bleed, cooling, and loss of component cooling water."

I really don't know who to address this to. Who might I inquire of the panel is likely to be the most familiar with this passage?

A (WITNESS KOLB) I would.

Q Mr. Kolb, as I understand it, you modified the -- possibly the event tree nodes or something of that sort. In any event, you remodeled the analysis of situations in which these activities would occur based upon your finding that there was an absence of written procedures at the plant to instruct operators as to how to carry them out. Is that essentially correct?

A (WITNESS KOLB) That's true.

Q Now, do you know whether operators at the Indian Point units are currently trained and exercised on the techniques of feed and bleed and the response to loss of component cooling water situations?

A (WITNESS KOLB) We had some discussions with the Indian Point operators at both Unit 2 and 3, and found that they seemed to have received feed and bleed, core cooling training on the simulator. They seemed to be aware of the operation.

However, when I asked them, where was their procedure, they couldn't find it. In fact, it seems that their procedure was addressing another method of core cooling. Right, Sandy? They had -- restoring the main feedwater system rather than going into feed and bleed.

A (WITNESS ISRAEL) That is correct. When I toured the control rooms -- I can't remember whether it was both or one or the other of the units -- and I looked at procedures dealing with loss of main feedwater, which then gets you into auxiliary feedwater, and then if you postulate loss of auxiliary feedwater. My recollection was the procedures instructed the operators to depressurize the steam generator and to use the booster pumps as an alternate source of feedwater.

Q Did you conclude, based on this inquiry, Mr. Israel, that the operators of the Indian Point units were familiar with these techniques?

A (WITNESS KOLB) Yes, they were familiar with them, but the question that remained in our mind was, would they really go to it? I mean, if their procedures are telling them to do one thing, and the IPPSS model has them doing something else, even though they are aware of it, it put a doubt in our minds.

Q I, too, have a question on Table 2 of your testimony, which appears on Page 15, but it is somewhat different than Mr. Colarulli's. I understood you to say that these were point estimates rather than means or medians. My question is in connection with the development of this table.

Was any effort made by members of this panel to calculate uncertainty associated with these point values?

A (WITNESS ISRAEL) I guess an effort was made by the Sandia folks to calculate uncertainties related to data that is presented in the NUREG report.

Q Would you characterize these point values expressed in Table 2 on Page 15 as best estimates or as bounding calculations? What --

A (WITNESS KOLB) Point estimates.
Q All right.

Gentlemen, just one last question. You tweaked, if you will, the plant damage state frequencies in IPPSS, and I think the basis of your analysis of the IPPSS plant damage state frequencies is sort of captured here in Table 2. I had some difficulty, however, in following it through the ensuing parts of the staff's testimony in this proceeding, just because it is getting mixed up with so many other things.

I did have one question, though, and that is whether the members of the panel could characterize their judgment as to the significance to public risk, early fatalities, late fatalities, man rems, things of that sort, of the changes in estimated core melt frequencies as stated in Table 2 from those set forth in the original IPPSS study.

MS. MOORE: Objection. That is beyond the scope of this testimony.

MR. BRANDENBERG: I think we are entitled to some assessment of the significance of this to risk, Mr. Chairman. We have this analysis taken just pretty much as far as the core melt frequency and not beyond.

MS. MOORE: Mr. Chairman, I believe the staff has repeatedly said there is a risk analysis that takes it through risk, and perhaps the best person to ask is the person who works with the risk results. These members didn't offer the risk results in IPPSS.

MR. BRANDENBERG: My trouble, Mr. Chairman, is, from this point on this analysis gets mixed up with so many other things.

JUDGE GLEASON: Well, you can bring it back again when the other witnesses are on, Mr. Brandenberg.

MR. BRANDENBERG: Let me ask an alternate question then.

BY MR. BRANDENBERG: (Resuming)

Q Do any of you gentlemen have a judgment as to the likely significance of the differences in estimated core melt frequency that you have derived here in Table 2, the different in these frequencies from those in IPPSS? Do you have any estimate as to the impact that would likely have on public health at Indian Point?

MS. MOORE: Mr. Chairman, I believe it is the same objection.

JUDGE GLEASON: It is the same. Why don't you hold that question, Mr. Brandenberg? I will remember to remind you.

BY MR. BRANDENBERG: (Resuming)

Q Let me ask one other question then. Given the uncertainties in this analysis, would you characterize the differences in estimated core melt frequencies set forth in Table 2 of your testimony, the differences between these and the parallel frequencies in IPPSS, would you characterize those as substantial?

A (WITNESS KOLB) Go ahead, Sandy. You can answer first.

A (WITNESS ISRAEL) The question is, are the uncertainties associated with --

Q No, no. Would you characterize the frequencies that you found, your analysis found as set forth in Table 2 compared to the parallel frequencies that were found in IPPSS, would you characterize those differences as substantial?

A (WITNESS ISRAEL) No, not really. There is a comparison on Table 1, Page 12, the estimates. And I don't know. They go up by a factor of two or three. And that doesn't seem unreasonable, to have two different people, two different organizations --

JUDGE GLEASON: Does anyone from Sandia want to answer that question?

WITNESS ISRAEL: Pardon?

JUDGE GLEASON: Does anyone from Sandia want to respond to that?

WITNESS KOLB: Typically, the category that dominates risk are the core melt without containment cooling category and the containment bypass categories, and I think you can see in both cases our estimates are lower than what IPPSS estimated. So I would say except for maybe the steam generator tube rupture, our estimates would give a lower estimate of the risk, just based on my guess.

MR. BRANDENBERG: Mr. Chairman, I have no further questions of these witnesses.

MS. MOORE: Mr. Chairman, might I have a moment?

JUDGE GLEASON: Excuse me?

MS. MOORE: Might I have a moment?

JUDGE GLEASON: Certainly.

(Pause.)

MS. MOORE: Your Honor, I have no redirect.

JUDGE GLEASON: You have no redirect?

MS. MOORE: No.

JUDGE GLEASON: We will start tomorrow with Mr. Budnitz and Mr. Reed tomorrow at 9:00 o'clock.

Thank you, gentlemen, for your patience. We are in recess.

(Whereupon, at 6:42 p.m., the Board was recessed, to reconvene at 9:00 a.m. of the following day.)

NUCLEAR REGULATORY COMMISSION

This is to certify that the attached proceedings before the

ATOMIC SAFETY AND LICENSING BOARD

in the matter of: CONSOLIDATED EDISON COMPANY OF NEW YORK (Indian Point Unit 2) POWER AUTHORITY OF THE STATE OF NEW YORK (Indian Point Unit 3)

Date of Proceeding: February 9, 1983

Docket Number: 50-247 SP & 50-286 SP

Place of Proceeding: White Plains, New York

were held as herein appears, and that this is the Original transcript thereto for the file of the Commission.

Alfred H. Ward

Official Reporter (Typed)

[signature]

Official Reporter (Signature)