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	)	50-286-SP
OF NEW YORK (Indian Point, Unit 2)	)	
	)	
POWER AUTHORITY OF THE STATE	)	
OF NEW YORK (Indian Point, Unit 3)	)	

DIRECT TESTIMONY OF FRANK H. ROWSOME
CONCERNING IV.C ACCURACY OF THE RISK ASSESSMENTS

Q.1 State your name and position with the NRC.

A.1 My name is Frank H. Rowsome. I am Deputy Director of the Division of Risk Analysis in the Office of Nuclear Regulatory Research.

Q.2 What are your responsibilities in that position?

A.2 I assist the Director in planning and managing the research group in risk assessment, probabilistic safety analysis, operations 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.

IV.C Accuracy of the Licensee and Staff Risk.

Q.4 What is the purpose of this testimony?

A.4 The purpose of this testimony is to discuss the accuracy of the IPPSS and the Staff assessment of risk.

Q.5 Please summarize your conclusions.

A.5 The uncertainties in the predictions of risk posed by severe reactor accidents at Indian Point Units 2 and 3 are large. We have been unable to pinpoint the absolute risk.

Q.6 How has the staff treated uncertainties in its calculations of severe accident risk posed by Indian Point Units 2 and 3?

A.6 The Staff has not attempted to formally calculate the uncertainties in our own risk calculations for Indian Point principally because there are many sources of uncertainty, such as modeling approximations and completeness issues for which the uncertainty cannot be mathematically derived.

It is possible to perform sensitivity studies to gauge the effect of specific sources of uncertainty. The staff has done this in some cases that are documented in NUREG/CR-2934 or in Section III of this testimony. It is also possible to translate ones judgment about the magnitude of uncertainty contributors into a mathematical model that can be used to develop an analysis of the accuracy of the bottom line risk predictions. This has been done in the IPPSS. We see some merit in using engineering judgment to arrive at a comprehensive, albeit subjective, treatment of uncertainties in this way. On the other hand, the staff has not attempted a subjective assessment of this kind.

Our principal approach to the treatment of uncertainties has been to give a qualitative account of the sources of uncertainty throughout our testimony on risk.

Q.7 Please describe the treatment of uncertainties in the Indian Point Probabilistic Safety Study.

A.7 The licensees have mathematically propagated quantitative estimates of the uncertainties throughout their risk calculations. The Indian Point Probabilistic Safety Study and the Zion Probabilistic Safety Study are the first PRAs in which this has been done. Statistical uncertainties were inferred using engineering judgment from their failure rate data base. Some accounting is given of correlated failures of multiple components within each safety system and of completeness problems in modeling the reliability of each system. Both coupled and random sources of variance were incorporated in the models of seismic fragility. Engineering judgment was used to portray the uncertainties originating in the estimation of fission product releases and consequences. All these sources of uncertainty were combined using numerical integration to yield an estimate of the range of potential error in the bottom-line risk predictions.

Q.8 What is your opinion of the IPPSS estimates of uncertainty?

A.8 The treatment of uncertainty in the IPPSS is the most comprehensive, quantitative assessment of uncertainties that has been given in a PRA to date. Nonetheless, I believe that it is plausible that the actual risk might be outside the range of risks identified in the IPPSS.

Q.9 Why do you feel that the uncertainty range calculated in the IPPSS is too narrow?

A.9 Both Robert Budnitz and Ben Buchbinder have testified in IIIA above that the uncertainties in their review areas (earthquakes, other external events, and fires) may be broader than the rather broad uncertainty bands used to describe these analyses in IPPSS principally because the methodological limitations and sensitivities of these pioneering calculations are not well-understood. In addition, James Meyer has testified in IIIB above that some of the models of core melt phenomena used in IPPSS represent one among a variety of possible courses accidents might take, so there appears to be greater uncertainty in the likelihood and character of containment failure modes than IPPSS takes into account. The judgmental treatment of uncertainties in the quantities of radioactive materials released and the consequences of releases employed in the "U-factors" in the Level II analysis in the IPPSS is quite simplistic, though not necessarily in error.

There are some other shortcomings in the treatment of uncertainties in the IPPSS that originate in modeling approximations or completeness issues. Among these are the omission of a model of sabotage, possible modeling errors in the event trees, and common cause failures other than those originating in the external events or fires.

In short, it is quite plausible to us on the staff that the true risks posed by severe reactor accidents at Indian Point Units 2 and 3 might lie outside the range suggested by the uncertainty calculations in IPPSS, either toward higher or lower risks.

Q.10 Please give a technical summary of the sources of uncertainty in the staff calculations of severe accident risk.

A.10 There are many uncertainties in the risk assessment. The easiest way to address the uncertainties is to take each of the principal phases of the risk assessment in turn and ask about the uncertainties in each separately.

Q.11 How might the uncertainties in accident likelihood affect the projected risk?

A.11 There are four kinds of uncertainties or possible errors that affect accident likelihood assessments. These are (1) statistical uncertainties, originating in the fact that we cannot measure component failure probabilities or human error probabilities or other input parameters with precision, (2) modeling approximations introduced to make the predictive models tractible, (3) errors of completeness: some failure mechanisms or scenarios have been left out entirely, and (4) arithmetic errors in assembling the models.

Q.12 How might statistical uncertainties affect the estimates of risk?

A.12 Statistical uncertainties have been calculated in the IPPSS. The results can be found in Figures 8.2-1 and 8.2-4 of the Indian Point Probabilistic Safety Study (Volume 12, pp. 8.2-2 and -3). The width of the peaked curve in the graphs give the licensees' estimate of the range of uncertainty.

Errors in PRAs originating in statistical uncertainties are, in general, no more likely to lead to over-estimates than underestimates of risk.

Dr. Robert Easterling has estimated the confidence intervals associated with many Indian Point accident frequency estimates in NUREG/CR-2934. He employed the Maximus method, an adaptation of classical statistics quite different from the Bayesian statistical methods employed in the IPPSS. His results are not significantly different from those in the IPPSS, from which I infer that the choice of statistical method is not a large source of uncertainty or potential error in most of the accident sequence likelihood estimates. Dr. Easterling has identified some isolated cases, documented in NUREG/CR-2934, where the choice of the statistical model or the data is quite sensitive. There are particularly large uncertainties surrounding our estimate of the likelihood of the double valve failure responsible for the uncontained interfacing system LOCA accident sequence.

Q.13 What influence have modeling approximations on the accuracy of the projected severe accident risks?

A.13 Modeling approximations are almost always taken in the pessimistic direction. They tend to exaggerate the risk.

An example is the treatment of partial failures. Safety functions that do not work as expected but do work partially are treated as outright failures. The risk assessment treats severe core damage, such as occurred at Three Mile Island, as a full core meltdown. The influence of these modeling approximations on the bottom line risk predictions cannot be formally calculated. However, we have some experience with refining such approximations. Many practioners of risk assessment believe that the exaggeration of the risk predictions caused by modeling approximations is compensated by the errors of omission in the risk models, although there is no reason to believe that this is always or precisely true.

Q.14 What influence have errors of omission on the accuracy of the projected risk?

A.14 Errors of omission generally lead to underestimates of accident likelihood and thus underestimates of risk. We know that a number of contributory mechanisms to accidents have been left out of the risk models: sabotage, those design errors (other than in seismic fragility) that have not been revealed by documents or by in-service experience, pressurized vessel thermal shock, etc. In addition, some contributors have been given skimpy and unreliable treatment, e.g., operator misdiagnosis of accidents in progress and perhaps DC power supply failures. These may contain errors of omission. Some errors of omission, such as operator innovations to jury-rig fixes for failed equipment, lead to overestimates of risk.

The implications for the accuracy of the risk predictions are not so bleak as this list of omissions seems to suggest, however. There are two reasons why these errors of omission are unlikely to affect the predicted risk to public health and safety. First, there are a great many severe accident scenarios. Only a few of them control risk. The others are far too unlikely to make an appreciable difference. Most of the errors of omission, if corrected, would increase the likelihood of a few accident sequences from a level that is quite negligible to a level that is still very small in contribution to core melt likelihood or risk. This is not just happenstance. More care has been taken in modeling the more likely and more severe accidents, so that most of the errors of omission are in the very much less important contributors to risk.

The second reason that most errors of omission are unlikely to affect offsite radiological risk lies in the spectrum of consequences of different core melt sequences. Most of the offsite risk originates in accidents in which the core melts early and the containment is either bypassed, failed early, or has no working heat removal systems. There are only a few accident scenarios that can fail so many safety functions at the same time. There are many other ways that an accident could occur that leads to core damage or meltdown in an intact and cooled containment. We have found in Sections IIIC and IVA of this testimony that such accidents have comparatively minor offsite consequences, although they leave the utility with a very costly burden of replacement power, plant damage, and cleanup.

If there were an error of omission in the PRA that does significantly impact the likelihood of core melt, it would probably affect those kinds of accidents that are comparatively well-contained. It would increase the projected economic losses to the utility in the same porportion as the overall increase in core melt frequency. However, it would have very little effect on the projected offsite radiological losses unless it happened to involve an accident sequence in which both core melt and severe containment failure happen together.

Q.15 These arguments are important because errors of omission are one of the principal reasons for doubting reactor risk assessments. Please give some examples to illustrate your point.

A.15 Let us suppose that the PRA omitted a common cause failure mechanism that makes the simultaneous failure of all three auxiliary feedwater pumps ten times as likely as the PRA suggests. Such a mechanism might be sabotage in the pump room. This would have virtually no effect on core melt frequency or risk because there are alternative ways of cooling the core when all feedwater is lost, and other failure mechanisms that affect both auxiliary feedwater and these alternate ways are controlling. The competing accident scenarios that are modeled in the PRA would still dominate both core melt frequency and risk.

Now let us suppose that the omission in the risk assessment were a failure mechanism that can defeat all three auxiliary feedwater pumps and also all three high pressure injection pumps at the same time, again ten times as often as the PRA suggests. Although this hypothetical failure mode can give rise to core melt following a loss of main feedwater, the effect on core melt frequency and risk would still be small because other failure mechanisms that are modeled in the risk assessment, such as earthquake- or fire-induced failure of all cooling systems, are still more likely and more serious.

Let us take another example. Suppose there were an upset condition that the operators might misdiagnose, so that the operators turn off the core cooling systems that are really necessary to avoid a core melt. Suppose further that the operators do not recognize their error until the core melts.

Such scenarios, if they were not quite unlikely, could increase the frequency of severe core damage or core melt above that predicted in the IPPSS. On the other hand, it would have little effect on offsite radiological risk. Containment heat removal would not be defeated by this pattern of human error. There are no upset scenarios in which the operators would judge it desirable to turn off all the containment air coolers. In addition, the containment sprays would be operable. Once the operators saw the unmistakable symptoms of severe core damage - e.g., very high radiation levels in containment - they would almost certainly start the sprays or allow them to start automatically without interfering with them. With either the coolers or the sprays operating the radiation would be well-contained. The offsite radiological effects would be roughly those of the accident at Three Mile Island.

Q.16 Under what circumstances might errors of omission in the risk assessment lead to substantial underestimates of the risks to public health and safety?

A.16 Errors of omission in the catalog of accident sequences and in the estimation of their likelihood could lead to substantial underestimates of the risks to public health and safety only if the frequency of core melt accidents which occur in conjunction with gross containment failure were substantially underestimated. Very few of the places in the accident likelihood assessment where errors of omission might reside have this character. The principal exception is in the reliability models for the power supplies that serve the actuation and control of the active engineered safety features. A massive failure of safety feature actuation could turn a simple, common plant upset event into one of the more severe reactor accidents, although there would be a long time before the release would take place.

Some accounting for such accident scenarios is given in the IPPSS and a better treatment is given in NUREG/CR-2934, but we are not so confident that the treatment is comprehensive as we are for most other potentially high-risk scenarios.

See also the testimony of Bob Budnitz (seismic and hurricane risk) and Ben Buchbinder (fire risk) in Section IIIA of this testimony. The seismic, hurricane, and fire scenarios provide the dominant contributions to the projected reactor accident risks at Indian Point. Errors of omission in these analyses might also result in underestimates of the risk.

We believe that the great majority of the significant accident sequences have been identified and their likelihood correctly estimated. It is widely recognized, however, that some ways that faulted conditions in the plant can propagate among systems are quite subtle and hard to anticipate. This is the heart of the systems interaction issue. The Power Authority of the State of New York has underway a program to catalog and evaluate systems interactions in Indian Point Unit 3. It will provide and interesting benchmark on how thoroughly the IPPSS managed to identify and model the more important interactions. PASNY has projected a completion date of March, 1983 for their systems interaction study.

Q.17 What impact might arithmetic errors have on the accuracy of the risk predictions?

A.17 In principle, arithmetic errors could grossly distort the results. However, a significant distortion of the risk due to arithmetic errors in either the IPPSS or the staff calculations of risk would have been conspicuous in the comparison of the two studies and against the background of other PRAs and risk research. Thus, we need not count upon formal checking procedures to exclude the possibility that arithmetic errors are responsible for large distortions of the risk profile of the plant.

Q.18 Can an upper limit on the likelihood of severe reactor accidents be drawn directly from light-water reactor operating history?

A.18 Yes, such an upper limit can be calculated, but it is not rigorously applicable to either Indian Point unit. There has been over 500 reactor years of experience in the United States. Another 500 reactor years have been accumulated in foreign reactors having a design comparable to our domestic light water reactors. In the combined experience of 1000 reactor years, there have been no core melt accidents and only one instance of severe core damage, the accident at Three Mile Island. If the industry average frequency of core melt accidents were once in a thousand years or greater, we would have seen it by now. There would have been more close calls, instances of severe core damage, or even full core melts than have taken place. (See also my testimony on Board Question 1.2.)

Reactor risk assessments have predicted core melt frequencies in the range of once in a thousand reactor years down to once in several hundred thousand reactor years. Most cluster around once in ten thousand reactor years. Thus we can infer that if reactor risk assessments routinely under-predicted the likelihood of core melt by more than a factor of ten, we would have seen it by now.

These arguments suggest that the core melt frequency at each of the Indian Point plants is probably not much greater than 10^{-3} year. There are two weak spots in this logic, however. First, we have reason to believe that design differences do result in different plants having different core melt frequencies. Some plants are more susceptible than others. Second, the inference from industry experience presumes that the risk does not change with time. If the risk has declined with accumulated experience, the inference from the historical record is strengthened. If, on the other hand, wearout effects cause the risk to increase with time, the inference from industry experience is weakened. Up to this point, the risk has decreased with accumulated experience. We have no way to be sure that our increasing understanding of reactor safety and future improvements in the plants will outweigh the effects of aging, and so lead to declining risk, but it is my opinion that the risk will continue to decline.

Q.19 How might uncertainties in accident phenomena and releases of radiation affect the risk?

A.19 The uncertainties in accident processes tend to be predominantly pessimistic. It is unlikely that accident releases are as great as our model suggests; they cannot be very much larger. It is quite possible that they are substantially less. The effect of these biases is that the actual offsite radiological risks are likely to be less than we have modeled them to be.

Q.20 How did you arrive at this conclusion?

A.20 There are many known or suspected exaggerations of the risk in the calculations of the timing and quantity of fission products that would be released in severe reactor accidents. These exaggerations have been incorporated in Dr. Meyer's analysis in areas in which the experimental evidence is weak, to assure that it is quite unlikely that the release severity might be underestimated. Among the model assumptions that tend to exaggerate the severity of the release predictions are these:

1. The plateout of fission products released from melting reactor fuel on the inside of the reactor coolant system is ignored; it is all presumed to escape from the reactor coolant system.

2. The rate at which particulates in the containment atmosphere settle out - as modeled in Dr. Meyer's analysis - ignores the effect of agglomeration. Particles tend to adhere to one another and these larger, heavier particles settle out more rapidly than the smaller particles do individually.

3. The effectiveness with which water captures particulates and soluable fission products is treated conservatively. This is particularly important for those scenarios in which gasses from the melting or melted fuel percolate through water or the containment sprays operate.

4. No allowance has been made in Dr. Meyer's calculation for the filtering effect of leakage from the containment.

5. In many release scenarios, the gasses escaping from a leaking, ruptured, or bypassed containment would be released inside the Primary Auxiliary Building. No plateout, filtration, or fallout of fission products within the auxiliary building is assumed.

6. Most measures of offsite radiological risk (delayed health affects and property damage in particular) are dominated by accidents that progress through what Dr. Meyer has labeled Damage State E. These accidents entail failure of all heat removal systems - both core cooling and containment cooling systems. Dr. Meyer's analysis of the containment response to damage state E falls at the threshold between severe and benign containment failure modes. It is a borderline case whether the containment fails due to overpressure or succeeds in bottling up the fission products, gases, and steam. Dr. Meyer's best estimate suggests that 40% of these events produce gross overpressure failures of containment about 11 hours after core melt, and that 60% of these events produce very modest atmospheric releases. There is a delicate balance in this analysis in which the pressure of the gases within containment may hover for some time near the failure pressure of the containment. Small uncertainties in the calculation could throw the result toward 100% overpressure failure or 100% benign releases. In the former case, most measures of risk would increase by as much as a factor of 2.5; in the latter case most measures of risk would fall by a factor of roughly 100, i.e. to 1% of the predicted values. Thus, Dr. Meyer's central estimate, and the staff testimony on risk, is biased toward the pessimistic end of this particular band of uncertainty.

An alternative outcome for long-delayed overpressure failure of containment is the possibility that the containment might develop a slow leak a few hours or tens of hours after core melt that would suffice to prevent gross overpressure rupture. In this case the timing of the release would agree roughly with the staff calculations for late overpressure failure, but the quantities of fission products released would be less in total and very much more gradual than our model suggests.

Q.21 How do you know that the severity of releases could not be very much greater then the staff testimony suggests?

A.21 The staff testimony suggests that a very severe release takes place in roughly one out of three core melt accidents, and that a large fraction of those radioactive materials available for release are released. Even if all these materials were released in every core melt, the risk would not be more than about a factor of 10 higher than our testimony suggests. On the other hand, it is quite plausible that severe releases take place in less than 1% of core melt events, and that the severe accidents entail releases of smaller fractions of the core inventory.

Q.22 What are the effects of uncertainties in the staff consequence analysis upon the projected risk?

A.22 Among the principal contributors to the uncertainty in consequence analysis are the assumed particle size for particulate releases, the fluid-dynamics of plume rise and the possibility of spontaneous plume rain, dispersion parameterization, deposition modeling, dosimetry and health effects modeling. Section IIIC describes the uncertainties in greater detail. For a more extensive treatment of uncertainties in consequence modeling see Chapter 9 of NUREG-2300.

Q.23 In light of all these uncertainties, what do you judge the accuracy of the bottom line risk predictions to be?

A.23 I think it important to communicate my judgment of the range of uncertainty, but I do not want to portray it as anything more objective than my judgment. I arrive at a judgment of the range of possible error in the bottom-line risk predictions as follows.

I would be mildly surprised, but not very surprised, if the likelihood of the more severe releases of radiation which drive the offsite radiological risk were in error by a factor of 30 (higher) or 1/30 (lower). This can be portrayed as an uncertainty factor of $10^{\pm 1.5}$. Likewise for the quantity of fission products that might be released to the atmosphere in these accidents might range from 3 times our estimate to 1/30 of our estimate ($10^{-.5\pm 1.0}$). The several kinds of consequences have some what different uncertainty factors, but most, I believe, are predicted within a factor of 10 of the correct value, or better ($10^{\pm 1.0}$). Since risk is obtained by multiplying the likelihood by the severity of release and multiplying that by the consequences of the release, the uncertainty factors are also multiplicative.

The risk uncertainty factor is thus $10^{-.5\pm 1.5\pm 1.0\pm 1.0}$.

The three uncertainty contributors are uncorrelated so that the combined uncertainty can be estimated as the square root of the sum of the squares of the contributors:

$$\sqrt{(1.5)^2 + (1.0)^2 + (1.0)^2} = \sqrt{4.25} = 2.1$$

Thus I judge the uncertainty of our bottom line risk predictions to be roughly $10^{-.5\pm2.1}$, that is I would be mildly surprised, but not very surprised if our estimates of offsite radiological risks were too low by a factor of 40 ($10^{+1.6}$) or too high by a factor of 400 ($10^{-2.6}$ = 1/400).

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.