November 1, 1984

UNITED STATES OF AMERICA
NUCLEAR REGULATORY COMMISSION

BEFORE THE ATOMIC SAFETY AND LICENSING BOARD

In the Matter of	)	
	)	
METROPOLITAN EDISON COMPANY	)	Docket No. 50-289 SP
	)	(Restart-Management Remand)
(Three Mile Island Nuclear Station, Unit No. 1)	)	
	)	

TESTIMONY OF E. L. ZEBROSKI

My name is Edwin L. Zebroski. My current position is Chief Nuclear Scientist at the Energy Study Center, a part of the Electric Power Research Institute (EPRI) in Palo Alto, California. EPRI is the research arm of the electric utility industry. Prior to joining EPRI, I held various design and development positions in Stanford Research Institute, (Physics Department), and in the General Electric Company, Research Laboratory, and in the Nuclear Energy Division. My training includes degrees in Science from the University of Chicago and the University of California. I am a registered Professional Engineer, and a member of the National Academy of Engineering. I have authored or co-authored over 120 technical publications and patents relating to the basic and applied science of nuclear energy. A major area of my specialization during the period 1965-1976 was the behavior of nuclear fuel under various operating conditions, including transients and accidents.

The purpose of my testimony is to cover three main points, based on my personal observations and involvement as a member of the Industry Advisory Group, convened at Three Mile Island in the early days following the accident:

1. The extent to which there was a rapid learning curve evident in the days immediately after the accident, in respect to organizing, and interpreting, the large volume of plant data, and in sorting out different views and speculation as to the extent and nature of the damage to the reactor, by focusing on generation of hydrogen as illustrative of this learning curve.

2. The extent to which related uncertainties remained for months after the accident, reflecting the limited general state of knowledge of severe core accidents at that time.

3. The extent and nature of the involvement of Mr. Herman Dieckamp in the activities and technical discussions of the Industry Advisory Group during the period of my observation.

At the time of the TMI-2 accident, I was Director of the Nuclear Systems and Materials Department at EPRI, which conducts research and development programs aimed at improved lifetime, reliability, and cost-effectiveness of components, fuels, and systems of Nuclear Power Plants. (Mr. Dieckamp was generally aware of these programs through his participation in prior years in two of the advisory committees which serve EPRI.) At a Research Advisory Committee meeting in Scottsdale, Arizona, Dr. S. Bartnoff of GPU reported to the Committee on March 29 and again on the morning of March 30, that an incident had occurred at TMI-2. Later in the morning of March 30, Mr. Culler, the President of EPRI, reported to the same meeting on a phone call from Mr. Dieckamp which indicated that the situation had deteriorated relative to the perceptions on the previous day and that technical support help from EPRI was needed. Mr. Culler agreed to send technical assistance to TMI, initially consisting of Mr. Milton Levenson, then Director of the Nuclear Division at EPRI, and myself. Mr. Dieckamp outlined four basic tasks which needed technical support; I was asked to undertake the first task which was Core Damage Assessment.

After a conference call on March 30 with Mr. Robert Keaten of GPU, I traveled to TMI, arriving on the morning of March 31. Office and conference space was made available at the National Guard Armory adjacent to the Harrisburg airport. An initial meeting to review the situation was organized and the technical review discussion was led by Mr. Dieckamp on the afternoon of March 31. Sometime during March 31, I became aware of the pressure spike which occurred shortly before 2:00 p.m. on March 28, 1979. I remained at TMI intermittently for the next four weeks, serving as co-leader of the Industry Advisory Group which was assembled to provide calculations, and evaluation of options for maintaining control and safety of the reactor system.

My investigative efforts on core damage at TMI during the initial days following the accident centered on several questions: namely, (1) the postulated hazard from the gas bubble in the reactor, (2) the possible extent of core damage, and (3) possible means for removing the gas bubble.

The gas bubble evident in the reactor was postulated to be potentially subject to ignition and explosion creating a sense of immediate potential for catastrophe. This potential apparently was first postulated about March 30th, and was reported in the national media with banner headlines. Various people from national laboratories discussed the explosive potential. The President's Science Advisor was reported to have commented that New York City and Philadelphia might be exposed to severe radiation if the bubble were to explode. A helicopter reportedly was dispatched to bring sacks of oxygen-absorbing chemicals (like sodium hyposulfite, a chemical used in photography).

In the telephone call with Mr. Robert Keaten of GPU on March 30 (mentioned earlier) he noted that he had become aware of a gas bubble in the reactor vessel but did not know its source or its full composition. He hypothesized that it might contain some air, from air dissolved in the borated water used to assure safe nuclear shutdown of the reactor.

I stated to Mr. Keaten my belief that oxygen could not be present in the reactor vessel and that no explosion was possible. I repeated this position later in meetings with NRC people on site (Stello, Vollmer, and Mattson) during the period March 31 to April 2, quoting the extensive literature on this subject dating to the 1950's and 1960's. The basic scientific information was that the presence of even small amounts of hydrogen suppressed the effect of radiation on water. (In the absence of excess hydrogen, radiation acting on water can produce hydrogen and oxygen in a volume ratio of 2 to 1, which is an explosive mixture.)

Apparently none of the staffs or the officials of the various government agencies involved were aware that since the mid-1950's, hydrogen was routinely used in all pressurized water reactors -- both Navy and civilian power -- to prevent the formation of oxygen-hydrogen mixtures by radiolysis. I urged the NRC representatives to make telephone calls to the national laboratories (Brookhaven, Argonne, and Oak Ridge) where the scientific and test work had been done to check out this information. This work was widely published in technical papers, and covered in textbooks on nuclear engineering.

By about April 2nd or 3rd, the NRC decided that the evidence against the possibility of a hydrogen-oxygen explosion was indeed unquestionable, and the bubble was disappearing. This was announced publicly, with the comment that previous concerns of possible explosion were due to overly conservative calculations. (It was later evident that the earlier inquiries to the national laboratories did not indicate that hydrogen was present in the reactor vessel.)

During the same days I was also continuing my efforts to estimate the extent of core damage. The prevailing state of knowledge on possible reactor core damage as of 1979 was the analysis in the report Wash-1400. This report, and the related NRC calculations used in licensing, postulated that if cooling water was lost, the fuel would fail (distort and leak) due to high temperature, and that the reactor core would then proceed to melt down with extensive spread of the bulk of the radioactive elements in the fuel (up to 70% of the total). The information available to me March 30 through April 4 did not correspond to such a degree of severity. The observations available March 30 and 31, (including the pressure spike and the indications of high levels of gaseous radioactive elements, but only small amounts of iodine and cesium) was that a significant fraction of the fuel was certainly perforated, releasing most of the rare gases. The apparent evidence that only a small fraction of the iodine and cesium were released was consistent with perforation of fuel cladding, but not necessarily gross disruption or melting of fuel. If major core damage were present, a large fraction (up to 70%) of the iodine and cesium would be expected to be volatilized -- according to the prevailing calculations accepted by the NRC.

The pressure spike was evidence of the probable presence of enough hydrogen to burn, but of itself was not evidence of how much had been produced. Small amounts of hydrogen -- as low as 4% in air, are known to be capable of ignition -- which would result in a pressure pulse, even if there was simply burning rather than explosion. I was aware that hydrogen gas from gas cylinders is routinely used to provide a small amount of hydrogen dissolved in the reactor coolant. As noted earlier, the dissolved hydrogen is used to prevent the decomposition of water by radiation (radiolysis), which would otherwise form oxygen and hydrogen. There was an evident need to determine whether some hydrogen cylinders or piping might have leaked hydrogen into the containment, which then could be ignited when a relay or motor was actuated or started.

Another possible source of hydrogen was recognized to be from the reaction of zirconium with steam at high temperatures. This was also plausible but did not of itself necessarily imply more extensive fuel damage than just perforation from localized overheating. Localized overheating alone could cause clad ballooning and rupture, with or without the added effect of oxidation of zirconium.

One of the major technical surprises of the subsequent investigations of the TMI accident has been the low extent of mobility of iodine and cesium, despite what we now know to be major core damage, with oxidation of a large part of the cladding. On the basis of the licensing analyses prevalent at the time of TMI, major core damage should have been accompanied by the release of large fractions of iodine and cesium (up to 70% of total inventory) to the primary system, and from there to the containment building air, and to any leakage paths to the auxiliary building. It is now known scientifically (although not yet fully accepted for regulatory purposes), but was not known or accepted then, that iodine and cesium, under conditions prevailing in a PWR loss-of-coolant accident, have a very strong affinity for water. The relatively large amounts (over 5%) which are now believed to have escaped from the fuel at TMI-2, have remained almost entirely in the water.

The small amount of iodine that did escape to the air (a small fraction of 1%) was readily detectible in the containment building and the auxiliary building. Had the postulated amounts of iodine been released, much larger emissions of iodine to the containment, and via leakage paths to the auxiliary building, would have been expected. In the absence of such observations, the expectation that core damage was limited to leakage or perforation of some fuel was plausible. If the fuel were only perforated, then it would still be possible to remove it and replace it using conventional underwater mechanical handling equipment. A small degree of fuel perforation ("leakers") is often present in the normal periodic refueling operations.

The question of how much hydrogen was evolved was the key to determining whether the core damage was limited to perforation or whether there had been more extensive or even severe disruption of the core structure. The first solid evidence of the amount of hydrogen produced came from the analysis of gas samples taken from the containment building on March 31 at 0600. These showed significant oxygen depletion (4.4% to 5.2% below the normal value in air, respectively). This corresponds to extensive reaction of zirconium (later calculated to be 45 to 52% of the core inventory). However, at the time, these results were questioned. Eight more gas samples were taken on April 1 and April 2. These showed substantially smaller oxygen depletion (average value of 2.3%, but with a wide scatter, some samples showing normal oxygen levels or higher). Later samples have confirmed that the initial values from the samples of March 31 are most likely to be valid. (There is an apparent possibility that in-leakage of air to the gas samples caused the error and scatter in the April 1-2 samples.)

Even with 50% cladding oxidation, the preservation of much of the core structure was judged to be possible. This assumed that the oxidation of the zirconium cladding produced a layer of oxide, but leaving a metal tube intact under the oxide. (Somewhat analogous to rusted iron pipe with an average of half of the iron still intact.) The near-normal readings on thermocouples in the core region also seemed to indicate that the core structure was mostly intact.

After the situation at TMI had been stabilized, late in April, 1979, the EPRI Board of Directors authorized EPRI to set up an investigation team to assess the facts, causes, and lessons learned from the accident.

This led to the organizing of the Nuclear Safety Analysis Center (NSAC) at EPRI in May, 1979, for which I was named the Director. In the next few months, a total of 80 technical people were enlisted in the investigation for a total of 12 man-years of effort. This effort produced a report (NSAC-1) issued July, 1979 on the sequence of events, with supplements in succeeding months. A final report including the supplements was distributed in March 1980. Many other investigations were proceeding which involved exhaustive interviews with plant personnel. These interviews apparently were finding a considerable range of conflicting recollections and perceptions. It was decided that the NSAC study should rely on the detailed analysis of instrument records and to avoid reliance on recollections or interpretations by plant personnel.

Accordingly, we did not interview any of the plant personnel. (We did have full support and help from GPU and plant personnel in finding and copying any instrument records and logs. This eventually amounted to over 50,000 pages of records).

Some months later, in Palo Alto, analysis of the instrument records brought out awareness (in NSAC) of an apparent thermal shock to the reactor core, possibly from a rise of water level in the core at about 7:47 a.m. on March 28. The nuclear instruments also showed a change in readings which could be interpreted as relocation of fuel by slumping or collapse of fuel rods previously embrittled by oxidation of the cladding. (I was aware of the results of two incidents in which experimental fuel was operated without adequate cooling and which resulted in fragmentation of the fuel rods.)

The NSAC analysis reported in NSAC-1 suggested that roughly the upper two-thirds of the core had been uncovered and subsequently overheated. Given that about 50% of the total zirconium was converted to oxide, the local oxidation in the upper part of the core would have to be near 100%. The fuel cladding in this region would be almost completely converted to a ceramic oxide. The sudden cooling of a hot brittle ceramic can result in fragmentation. The likelihood that core structure was preserved in this region was then recognized to be small. From this emerged the hypothesis published in NSAC-1 report, that a region of the core shaped like an inverted bell, reaching to within about 3 to 5 feet of the bottom of the core, was most likely fragmented into a rubble bed. (This analysis was confirmed conclusively only in July-August 1982, when a TV camera was lowered into the reactor core region.)

During the period that I was at TMI, Mr. Dieckamp continued to keep in touch with me and Mr. Levenson and to participate in the technical discussions after the initial meetings of the Industry Advisory Group (IAG), to which I referred earlier. There was also an operating support group at TMI led by Mr. William S. Lee (of Duke Power) for a time and then later by Mr. Byron Lee (of Commonwealth Edison Co.). Mr. Levenson and I met with this group daily to review our findings and recommendations. Mr. Dieckamp participated actively in these discussions. There were also daily meetings with the principal NRC representatives (led by Mr. Victor Stello) to discuss our findings and recommendations, also with active participation by Mr. Dieckamp.