of 28 psig at 1350 hours on 28 March, the previous day. I concluded instantly without further discussion that the spike was caused by hydrogen ignition in the containment, that therefore the mushiness in the primary system had to be due to the presence of hydrogen gas loose in the primary system, that the hydrogen was from a zircalloy-water reaction and that we had to get the hydrogen out. The spike looked like those we used to calculate for hypothetical hydrogen ignition in containment except it came down faster. Containment pressure was subatmospheric which could be due to having used up oxygen by burning hydrogen. I asked the young engineer for another pressure reading and he pointed to the wide range trace at the bottom of the same chart. I asked for building temperature traces. They were confirmatory.

I asked for xerox copies and stepped back into the shift supervisor's office where Tom Crimmins was with several others and told him that there had been hydrogen ignition in containment, that there was a hydrogen bubble in the primary system, that we had to measure it and that we had a fighting chance to get it out because hydrogen "diffuses like a shot." The great sense of urgency to measure the size of the bubble derived not only from wanting to confirm or refute its presence but also to find out whether it was growing, to find out whether it was then large enough to interfere with reactor coolant pump operation on which core cooling then depended, and to estimate whether the core could be uncovered by bubble growth if depressurization occurred by failure of pressurizer heaters or a critical seal or valve. While the term bubble was used then, as it is now, we knew it could be several or many bubbles in a number of places.

One aspect of the events just described may need explanation at this point before resuming the account of what happened next. Sardonic doubt was once exhibited in my presence as to how the meaning of the spike could be rapidly apparent among the many things going on. I think the question of why I recognized it whereas others apparently hadn't deserves consideration, and the answer, I believe, is at least three-fold.

First, on the 29th, puzzles had been accumulating all evening. The primary system acted as though steam was in it outside the pressurizer but temperatures were too low. The waste gas tanks were full but we did not know why. Lots of radiation was loose in containment, but we did not know what the fuel damage was like. And we felt a great urgency to get answers. The visual image of the recorder trace resembled graphs of calculated hydrogen pressure spikes I had seen before and that image was the trigger which made all the then-known pieces of the puzzle fall in place. This kind of thinking is intuitive, not analytical in the pedestrian sense. But, I believe it is a well recognized psychological process.

The second factor is background. Although I am a licensed nuclear engineer, my degree is in chemical engineering and I worked in that field and chemistry for five years during which

I had personal experience with both the potential for and the actuality of fires and explosions. During the early years of nuclear reactor design we were especially sensitive to the possibility that metals such as aluminum, stainless steel and zircalloy used as fuel cladding could react with water at high temperatures to produce hydrogen and destroy the cladding. Later on, accident analyses such as those for TMI, included consideration of these reactions as well as hydrogen production in containment by radiolysis and by reaction of spray water with aluminum and zinc. Those familiar with these analyses knew the aluminum source was over-estimated and radiolysis was slow. Most operators and many engineers did not have this kind of background then and so probably were not as sensitive to the possible meaning of a pressure spike.

The third factor is stress. Although I am not an expert in this area, I know from experience that except for those who freeze, acute stress makes one especially alert to start with but dulls analytical and physical capabilities fast. Stress is especially high if one can't figure out what is going on. The operators and most others present upon my arrival in the control room had been under high stress for long periods. Some of them had not slept much, if at all, in about two days. We, on the other hand, while under high stress, were relatively fresh, better able to interpret the more obscure clues such as the spike.

Given these three factors, I do not find it surprising at all that the situation developed the way it did. I don't find it surprising in such a complex, confusing, unprecedented and on-going situation that it took a combination of circumstances a d a fresh look to recognize the significance of what may at first have appeared to be a spurious instrument reading among hundreds of other readings and alarms and plant control problems. I say this because I have a recollection, imprecise as to time, that mention was made among many other things in my presence at some point on March 29 of a containment pressure recorder spike said to be a spurious indication: e.g., caused by a voltage anomaly in instrumentation. I recall being skeptical of that explanation. In all the discussions, however, no one had exhibited or implied in my presence any recognition of the significance of the containment pressure spike. Nor did I pause to reflect on my skepticism at the time and, indeed, until the graph of the spike was shown to me which prompted the reaction described above.

And this leads back to the story. I knew from personal experience that under high stress one tends to lock-on to a perception of reality which, even if the best available, may be wrong. I had been trained to recognize and handle such situations. So even though we felt great pressure to act, Tom Crimmins and I forced ourselves to take the time to review the facts and test the logic of the hypothesis about the spike and related matters. When the hypothesis held up, I called someone and asked for the best man available to help us. Shortly afterward at about 2330 Mr. Jim Moore, an experienced GPU engineer arrived.

The three of us sat in the shift supervisor's office trying to figure out how to measure bubble size. Finally, after what seemed a long time but probably was not, Jim Moore said, "Boyle's law ought to work" and I recall thinking, perhaps saying almost before he had finished, "And the pressurizer is the piston." Boyle's Law states that, other things being equal, the volume of a perfect gas is inversely proportional to absolute pressure. Although other things were not equal and hydrogen is not quite a perfect gas, it was obvious that the volume of a bubble, if there was one in the primary system, could be measured approximately by measuring the difference in system pressure caused by a given difference in pressurizer level. I asked Joe Logan, the TMI-2 Superintendent, to change level to get about a 100 psi pressure differential. Operations said they had some data like that from the previous day. I asked that it be "QA'd," that is, verified before we used it and then commandeered the open telephone line to Lynchburg from a B&W engineer and made two urgent, highest priority requests of Don Nitti and Jim Taylor whom I found at the other end:

First: What is the free volume under the head of the reactor pressure vessel down to the top of the nozzles?

Second: Make refined calculations of bubble size, using pressure, temperature and pressurizer liquid volume change information we would give them, taking account of gas solubility and anything else pertinent assuming the gas is hydrogen.

Jim Moore and I then made calculations of bubble size independently and got approximately the same answer. When we corrected each other we had a bubble size of 1568 cubic feet at 875 psia from data taken at 1245 on 29 March. My calculations are time marked 0235 on 30 March. Subsequent estimates from data taken about 0330 on 30 March gave a bubble volume of about 1100 cubic feet at 875 psia. We had not yet gotten proof of the interpretation of the pressure spike but the hypothesis had been greatly strengthened.

At about 0325 hours B&W called back to report the free volume in the reactor vessel down to the outlet nozzles was 1129 cubic feet. Even though the first bubble volume calculated of 1568 cubic feet was larger than this, and the second about equal, it was clear the core wasn't uncovered. Questions to Operations indicated amperage and vibration were normal for the one primary pump which was running. So there wasn't enough hydrogen to interfere with main pump operation at then current system pressure. But there was enough so that depressurization could uncover the core and defeat core cooling by methods then being used.

Shortly before 0400 after talking to B&W, I started to calculate the amount of zirconium cladding in the core which must have burned to produce enough hydrogen for global ignition in containment and for a hydrogen bubble of the size measured. I stopped before completion because of the press of urgent matters and since rough numbers and mental corrections indicated a large part or all the zirconium had burned. I didn't necessarily believe all of it had, but it was clear now that the core was very seriously damaged. That was what we needed to know at that time.

At about 0400 after discussions with Crimmins and Moore, I recommended to Joe Logan that he start venting the pressurizer to containment while holding the pressure at the then current level of about 970 psig with pressurizer sprays and heaters on as much as possible. I also asked that analyses of the hydrogen and oxygen content of the containment atmosphere be obtained as soon as possible. The venting was aimed at removing hydrogen from the primary system by steam stripping dissolved hydrogen from the hydrogen rich water brought to the pressurizer by the sprays on the assumption that the hydrogen in the bubble would "diffuse like a shot" and replace that stripped and so the bubble would gradually disappear. Venting from the pressurizer was started later on 30 March.

Containment atmosphere sampling done between 0518 and 0638 of 31 March showed residual hydrogen of 1.7% and oxygen of 16.3% by volume clearly supporting the hypothesis of a hydrogen ignition. The normal concentration of oxygen in air is about 21% and hydrogen is essentially absent. At 2338 of 1 April B&W reported by telephone that at 1550 that day the bubble in the primary system had disappeared according to volume calculations and noise measurements. This was confirmed by a graph sent to me and received at 0044 of 2 April. The disappearance of the bubble was consistent with the initial interpretation of the spike. As more information was accumulated over the next days and weeks, the initial interpretation was demonstrated without doubt to be correct.

I find it inconceivable that if anyone had known hydrogen was present in containment and had ignited, they would have concealed that knowledge from peers or managers and that the on-site technical support team would not have been told of it. No motive for concealment by those involved existed since too much was at stake including, perhaps, their lives.

Also, I find it inconceivable on other grounds that the real significance of the pressure spike was deliberately concealed by an exercise of duplicity or dishonesty. I know many of the people involved and have for years. They simply would not have done such a thing. And when I say that I include Mr. Kuhns, Mr. Dieckamp, Mr. Arnold and all of those managers and engineers with whom I worked during the accident.

In the course of working with Mr. Dieckamp during the accident, my high regard for his honesty, managerial ability and patience, which has certainly been tested under very difficult circumstances during the past five years, was reconfirmed. I might add that Mr. Dieckamp gave a great deal of personal attention to what was going on during the TMI-2 accident. He, for example, called me directly several times near midnight of Friday, 30 March when he was concerned, as we all were, about the potential for another buildup of hydrogen concentration in the containment due to venting the primary system and due to the slow radiolytic decomposition of water in the bottom of the containment building.

To recapitulate, no recognition of or even speculation about the significance of the pressure spike was expressed or implied in all of the extensive and intensive communications I heard or was party to from early morning of 28 March until the spike's significance was recognized at about 2300 on 29 March as I have described. These communications were with both senior and junior engineers, operators and managers, probably more than 50 in all. Nor did I hear about any such prior recognition from the hundreds of people I dealt with subsequently while on duty at TMI for nearly a month. Furthermore, the people I know and dealt with would not have deliberately concealed such knowledge. And I state that judgement with emphasis and without qualification.

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 f l damage than just perforation from localized overheating. Lo ized 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.

UNITED STATES OF AMERICA
NUCLEAR REGULATORY COMMISSION

BEFORE THE ATOMIC SAFETY AND LICENSING BOARD

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

CERTIFICATE OF SERVICE

I hereby certify that copies of Testimony of Herman M. Dieckamp, Testimony of William W. Lowe, Testimony of Thomas Leroy Van Witbeck and Testimony of E. L. Zebroski, dated November 1, 1984, were served on those persons on the attached Service List by deposit in the United States mail, postage prepaid, this 1st day of November, 1984.

Ernest L. Blake, Jr.

Ernest L. Blake, Jr., P.C.
Counsel for Licensee

DATED: November 1, 1984

UNITED STATES OF AMERICA
NUCLEAR REGULATORY COMMISSION

BEFORE THE ATOMIC SAFETY AND LICENSING BOARD

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