HISTORY ON SAFETY RESEARCH PROGRAMS
AND
SOME LESSONS TO BE DRAWN FROM IT

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One of the benefits of passing years, if there are any benefits at all, is that one is then permitted to stand up in front of a group of younger people such as those here and tell how things happened before those people were around. In fact, there will even be occasions when there is a captive audience, such as this one. Even maybe an audience of younger people who paid for the privilege, so to speak.

I am going to take full advantage of the situation by going over some ancient history of the safety research program, telling how some things began, drawing some conclusions, and even giving some recommendations. That is what I have learned to do as a member of the Defense Nuclear Facilities Safety Board; we exist to make recommendations to the Secretary of Energy. And incidentally, if the first person singular is used a lot in what follows, that's mostly because I am trying to tell of things in which I had some part and so can speak with authority.

Let me just state at the outset that this meeting is, I believe, the twenty-six in a series that I began in 1973. I'll say a little more about that later.

We start the story in the middle 1960s, a little more than thirty years ago. I was Chairman of the Advisory Committee on Reactor Safeguards (ACRS) at the time. The initial flood of orders for new nuclear plants was just beginning; designs were quite mobile. The Regulatory Staff of the Atomic Energy Commission (AEC), later to become the Nuclear Regulatory Commission, was quite small. A few of the staff members came to a meeting of the ACRS and told us that some scoping calculations made by theorists at the National Reactor Testing Facility at Idaho (as it was then called) indicated that if a reactor completely lost its supply of cooling water the temperature of the fuel would continue to rise until the core melted, and then it would melt through the vessel and fall on the floor of the containment building, and it would keep on going. Thus was born the "China Syndrome." I left ACRS a little later, but stayed in touch.

The next important step in the sequence occurred when a Committee was appointed by Harold Price, who was then Director of Regulation for the AEC, to investigate the reality of the scoping calculations. This was a group headed by William Ergen of Oak Ridge. The Ergen Report confirmed the reality of the threat, and strongly stressed the absolute importance of prevention of loss of coolant through what are now called "engineered safety features," which reactor designers had already begun to include in their designs. Incidentally, the fact that the designers were proposing these features on their own, with no prior urging, has convinced me later that the astute theorists at commercial reactor designer companies had already reached the conclusions on their own.

The Ergen Report led the Reactor Development Division of AEC to initiate several programs at the Idaho Facility, to test performance of Emergency Core Cooling Systems (ECCS) for Pressurized Water Reactors (PWRs). Loop sized experiments called "semiscale" were begun, to be followed by the LOFT experiment, where a small PWR was to be subjected to loss of coolant along with function of a scaled emergency core cooling system. Programs for testing emergency core cooling of Boiler Water Reactors were run by industrial groups at the Moss

Landing Facility of the Pacific Gas and Electric Company. Some related experiments were run with electrically heated bundles of simulated fuel elements at Oak Ridge and Westinghouse; the last set were jointly funded by AEC and Westinghouse, and they explored effects that might result from damage to fuel cladding at the elevated temperatures and reduced pressures of a loss of coolant accident. All of these operations were in accordance with a program plan formulated by a group at the Idaho facility.

At the time, the primary attention of the Reactor Development Division was focussed on development of the Fast Neutron Breeder concept, and the design of the Fast Flux Test Facility being built at Hanford. A prevalent view was that if nuclear plants were built and operated according to strictly enforced standards, there would be no loss of coolant accidents, or any other large accidents.

A number of the scientists engaged in the safety research programs chafed under what they perceived as inadequate attention to research programs to determine effectiveness of systems designed to mitigate the accident if it were to start. The unrest reached the ears of Ralph Nader, who then instituted a suit to require shutdown of all operating nuclear reactors, on the grounds that their safety was not ensured. To address the question, the Atomic Energy Commission called a public hearing to explore the question. The courts deferred consideration of shutdown of the reactors pending outcome of the hearings.

As is customary, regulatory hearings are of the judicial form. That is, intervenors act like prosecutors, and they can call their own witnesses and can cross-examine other witnesses. They did so with a vengeance in the ECCS hearings (as they were called). There were about 26,000 pages to the hearing record. I know, because I had to read every one.

A number of researchers in the programs testified, many of them expressing views that information underlying effectiveness of the ECCSs was not adequately reliable or even that it showed the analytical tools for designing the systems were wrong. Early on, the five members of the Atomic Energy Commission headed by Jim Schlesinger realized that they were not going to be able to deal with a judgement on the results of the hearing. They asked two people, Herbert McPherson of Oak Ridge and the University of Tennessee, and me, to follow the hearings and to propose a response for the Commission to make.

The two of us after long and hard work drafted a report which the Commission accepted and issued verbatim as their opinion. The central feature of that opinion was a set of requirements on computer codes that AEC would permit being used to justify effectiveness of Emergency Core Cooling Systems described in license applications. The Regulatory Staff converted these to the Appendix K to 10CFR50.

Appendix K became notorious after a time, because it was so prescriptive on acceptable features of computer codes used to design ECCSs, and contained such conservatism. Unfortunately, all of that was necessary at the time, because the prescribed technology had to circumscribe features of the technical understanding of phenomena as they were brought out by the hearing.

The opinion also promised a safety research program to firm up the basis for acceptable features of the computer codes, and to permit relaxation of the requirements where that became possible. The courts concluded that the Commission's opinion satisfied requirements for the time being, and noted that the promise of the safety research program offered to solve the problem in the long term. Conduct of the safety research program came to be called "paying off the mortgage on the power reactor program." The mortgage was to be considered paid off when the ECCS computer codes were regarded as reliable, and when they predicted acceptable behavior of the systems.

By this time Schlesinger had left the Commission and Dixie Lee Ray had become the Chairman. She drafted me to head the research program as Director of a new Division of Reactor Safety Research. After an ineffective struggle to escape I agreed to do so. With the argument that the Regulatory Staff needed to have an independent basis for deciding the validity of the technical propositions it had to consider in reactor licensing, I managed to obtain resources needed for a rapidly growing program of "confirmatory research." Though the central feature of that program was development of new computer codes for predicting response of reactor systems to ECCS, development of input data needed for the codes, and testing of prediction capability in experiments up to and including operations with the LOFT facility, other components were added, including such topics as structural reliability of piping and vessels, and safety of fast breeders and High Temperature Gas-cooled Reactors. I would be remiss if I did not mention the singularly important contributions by Dr. Long Sun Tong in achieving the goals of the research on water reactor safety.

I left the program in 1976, a year after it had been transferred to the new Nuclear Regulatory Commission. It continued to be enlarged under Saul Levine and Bob Budnitz, and the mortgage was finally paid off by the series of LOFT tests, where ECCS performance was conservatively predicted by safety codes which had been developed at Los Alamos and Idaho. I won't go into details here because many of you participated in these phases of the program, and I was off doing other things.

The important thing to note is that the nuclear power plants operating today owe their continued existence to the safety research program that is represented here. You developed an entirely new branch of engineering: that of nuclear power plant safety.

Now I'll turn to another part of nuclear plant safety in which I've been a fringe player since its beginning. This is risk assessment.

In 1956 the first commercial power reactors were about to go on line. Congress struggled with legislation to protect the fledgling industry from any financial cataclysm from accidents to nuclear power plants. The Commission asked Brookhaven National Laboratory to conduct a study to estimate the consequences of such an accident. A group was formed within my Division at Brookhaven to attempt this. The result, published as a report numbered WASH-740, was not very helpful, because it concluded that if the containment worked, there would be no damage to surrounding population, and if containment failed, only the upper bound of consequences could be estimated, from liberation of all fission products in the form of noble gases, a large fraction of the radioiodine, and some fraction of the other fission products. The public damage from that would be enormous. In a rare example of common-sense legislation, Congress issued what became known as the Price-Anderson Act to provide the protection needed by the industry, and in doing so it ignored the upper limit estimates of WASH-740. That Act established a fund containing contributions by nuclear utilities of a portion of their revenues from nuclear electricity. The fund was for coverage of expenses following a conceptual accident, and for public compensation in that event. It was enough for the industry to go ahead with development of the field.

In 1966 the Act had to be renewed, and a second study at Brookhaven for this purpose led to a conclusion that by and large the earlier results could not be improved on. Some years later, as the time approached for a second consideration of renewal of the Act, Harold Price called me to ask if I personally would head a new study, also to estimate probabilities of accidents. I said I did not think that could be done, but suggested Norman Rasmussen of MIT as one who could try. Well, you all know the results; Norm with the help of Saul Levine and other contributors did an incredible job. He succeeded in accomplishing the impossible, and WASH-1400 was born.

Dixie Lee Ray also asked me to fold management of Rasmussen's project into my new Division of Reactor Safety Research, which was easy since Saul Levine was already my deputy. Before the Report was issued, I read every word of it and its voluminous appendices, and I did much of the technical editing on it. After I left the safety research program, I continued active in numerous meetings and discussions on WASH-1400 and the successor program that led to NUREG-1150.

This is all by way of my leading up to some remarks on the uses of risk assessment. There is a lot of discussion these days directed to risk-based regulation. I must say that there is more danger of over-use of this concept than there is to be attached to its under-use.

Levine and Rasmussen were always firm in their statements that the large error bars should be the basis for avoiding use of bottom line estimates of risk. Risk estimates should be used primarily as a basis for regulation that remains solidly founded on mechanistic requirements, with emphasis on <u>a basis</u> rather than <u>the basis</u>. I thoroughly agree. Risk analysis can be very powerful in a number of ways. It can be used to identify vulnerabilities in design and operation of plants by searching for major contributors to accident probability and consequences. It can be used as an aid to choice among alternatives in design or operation because many contributors to error bars cancel out in comparisons of risk. But where risk analysis is used in such ways, the conclusions should always be subjected to reality checks. The question should be asked: does this result make sense? If not, what is the source of the problem? It is not only important that regulators recognize such points; it is important that they continue to pass the information on to the less-technically-prepared groups that often try to push use of risk methods too far. I read that there are now pressures from some parts of Congress to do more risk-based regulation. What do they mean?

Some final remarks. The first of these meetings of the reactor safety research community in 1973, was attended by only a sparse number of researchers from a few laboratories. This was just after the trauma of the ECCS hearings, and attendees were afraid to talk about their research. When I asked for people to speak, I got only silence. I had to harangue the group and tell them that I not only had to hear what they were doing but I insisted that they had better start publishing the results in the open literature or there would be hell to pay. A few people then began to talk about their work, and a trickle of publications began to appear in subsequent months, that later swelled to a torrent. Well, this meeting like so many of its predecessors shows that the problem of reluctance in 1973 no longer exists. Hallelujah!