CONTINUED VIABILITY
OF
CRITICALITY CONTROL DISCIPLINES

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EH CRITICALITY SAFETY WORKSHOP
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I see by the program that I have been allotted 45 minutes for this introductory talk. I am embarrassed. I don't think I have ever talked for that long in my entire life. I don't intend breaking a lifetime habit now, and so I expect to return to you a little of that time for whatever better uses you may make of it.

Let's start by admitting that this field of criticality control is approaching a state that I saw some years back might exist in a remote future. That would be a world in which very few people would have first-hand experience with the phenomenon of criticality, and yet it would be necessary to arrive at conclusions based on a full understanding of criticality. In my mind, that would have been a very difficult if not impossible situation.

In the golden age of criticality studies, there were numerous facilities devoted to experiments on critical systems. I count some nineteen facilities at which such experiments were taking place in the United States, all operating over the same or nearly the same time period. A great many of these had the capability for conducting several critical experiment programs at the same time. You are all familiar with the existence of several kivas at the Pajarito Site at Los Alamos, and many of you will remember the lineup of critical experiment facilities at Argonne's two locations, Argonne East and Argonne West. But many others of the nineteen sites were also multiple in nature.

That meant that throughout the country there were numerous individuals who had real-life involvement with criticality almost every working day, who therefore developed what I would call a "natural feel" for the conditions necessary to sustain a neutron chain reaction. That situation first began to decay in the commercial sector, where the neutron physics design of nuclear power plants became much more repetitive, and innovations became infrequent. Neutron physics at and near design points became reasonably well understood, so that computer codes benchmarked against existing designs and against previous physics experiments were dependable for most purposes of design. Treatment of such other matters as effects of fission products and behavior with burnable poisons could be gathered from reactor operating history. That caused reactor design companies, subject to the pressure of competition and the corresponding need to save money on design, to shut down their physics experiment facilities. They came in time to depend entirely for their neutron physics on the large systems of reactor design codes which by that time were well developed, at least for the region of conditions against which they had been benchmarked. I became concerned at the prospect of a future in which reactors would be designed without input from physicists with a fully rounded understanding of the implications of the chain-reacting system with all its complex feedbacks. After all, a calculation is at best as good as the phenomena included in the analysis and their proper analytical description. If you miss something or incorrectly describe it, you will get wrong answers.

Fortunately, operation on-line with real power reactor systems seem to have provided enough on-the-job education to meet the needs of the static industry. But the problem of loss of capability did not end with shutdown of the industry's experimental research facilities. One-by-one, the critical assembly facilities that had been integral parts of the programs of the Federal government were also closed down. Until a few years ago only about one and a half remained. By that I mean there was the fully operating facility at Los Alamos, and there was also a capability sometimes called on at the Sandia Laboratory in Albuquerque. Even ten years ago, when we at Brookhaven along with colleagues at Babcock-Wilcox were developing a concept for a new reactor system and needed critical experiments to prove out some very difficult physics, we had to go to the facility at the Sandia Laboratory to have the experiments done.

Fortunately, and as is evident from the attendance at this workshop, there are still people around who have been involved in experimental research in the past. In time, however, that will no longer be the case. Reactor design capability will certainly suffer then, for I am sure that the drought in orders for nuclear plants will end one day, and nuclear will come back in a big way. Before that, however, the effect will be felt increasingly on the activities that are the concern of this workshop, which are the modes of control of fissionable material so as to preclude inadvertent criticality.

Of course, I have been invited to speak here because of my current position as a member of the Defense Nuclear Facilities Safety Board. But the remarks I make rest on a firm base of long-term activity in this field. For those who do not know of my background in this respect, I shall simply state some of it as background for what follows. I conducted research along with a group that I formed at Brookhaven National Laboratory in 1952 to study chain-reacting systems. We started with measurements of the neutron multiplying characteristics of subcritical assemblies of slightly enriched uranium rods in a light water moderator. This small experimental program grew into an extensive one providing parametric subcritical and critical studies of assemblies of varied fuel and moderators, including metallic and oxide fuel, and other fuel compositions such as uranium-plutonium alloy and U-233-thorium alloy. That set of studies lasted about 15 years. We also conducted design studies of several research reactor systems including the High Flux Beam Reactor and the Medical Research Reactor at Brookhaven. A small group of about half a dozen of us qualified to man the console of a critical assembly conducted some thousands of critical experiments. Because of our intimate familiarity with multiplying systems we also took over criticality control for the vaults used at Brookhaven for storing fissionable material. On-site were five vaults containing varied material, including a lot of natural uranium, slightly enriched uranium, several hundred kilograms of highly enriched uranium, and varying amounts of plutonium and U-233. For a time, a colleague and I also worked as advisors on criticality control at a nuclear fuel fabrication plant operated by United Nuclear in New Haven, Connecticut, where a substantial amount of fuel was fabricated for the nuclear navy. We set up a complete concept of criticality control for that plant, embodying concepts that I will be expounding on later in my remarks.

We never had an inadvertent criticality at Brookhaven. During the period of our activity, however, a number of criticality accidents occurred throughout the country, including three where fatalities occurred. There were two chemical process accidents, one at Los Alamos and the other at Wood River Junction, Rhode Island, each fatal to a chemical technician, and the SL-1 accident in Idaho, where three reactor operators were killed. Note that the first two accidents occurred during cleanup of waste containing fissile material, an activity that is now taking place at a number of the facilities of the Department of Energy.

Over the same period there were also a number of inadvertent criticalities at experimental facilities of the Atomic Energy Commission. All of these took place at facilities with designs that had profited from the lessons of the two wartime criticality fatalities at Los Alamos during the Manhattan Project, and a severe exposure of experimental staff in an accident at the Argonne National Laboratory. Most of these later accidents occurred under conditions where operating personnel were well protected from released radiation, usually by shielding.

Among my friends conducting similar experimental programs at other facilities were some who said they were not very concerned about unplanned criticality if it took place under conditions where personnel were adequately protected. One good friend said that a facility that did not have an accident once in a while was not working at its optimum. I have to admit that there is logic in that position, or at least there was a lot of logic in it at one time.

However, that is a position that cannot be defended at this time in light of prevailing attitudes on nuclear matters. A criticality accident in which no one is even slightly injured would generate enough fright through reporting by the press that the consequences on many important programs would be unacceptable. Moreover, the nature of the Department of Energy's operations has changed substantially since then. Now we are again most concerned about the possibility of a criticality during cleanup operations. In such circumstances, the shielding to protect workers would no longer be present. Severe injury or even fatality would be real possibilities. In the most general way, a criticality accident is now a complete no-no. Such an event must not be allowed to happen. That is the reason for the strong interest of the Defense Nuclear Facilities Safety Board in ensuring continued viability of criticality control disciplines, and it is also the principal force behind having this workshop.

We should talk about the reasons why criticality control today seems to require such extensive resources in DOE. Certainly the reason is the difficulty in solving the analytical problems that are posed. There are three principal causes of the difficulty. One is the variety of situations that must be analyzed. The second is the complex and varied geometries that have to be analyzed. The third is the lack of good information on the composition of so much of the material in question. I could give many examples of real world situations embodying all three problems. A couple will suffice. Many facilities have to ensure freedom from criticality accidents during storage of miscellaneous containers of contaminated material that might hold appreciable amounts of fissionable material even though they are not likely to contain much fissionable material. Uncertainty as to the contents leads to use of very conservative protective measures. Containers will also vary in size and geometrical shape, and configurations may tend to change now and then as new material is brought in and as storage arrangements change.

A second example is afforded by conditions that arise when glove boxes formerly used in handling plutonium need to be cleaned out. The nature and amounts of material in the glove boxes change with time as cleanup proceeds. Some of the more frequent criticality infractions occur under these circumstances.

These situations are among those presenting some of the greatest difficulty for estimation of criticality. Not only is there uncertainty as to the important characteristics of individual items, but interaction between items is very difficult to estimate. Some of the most abstruse theory in neutron physics deals with the magnitude of k_{eff} for sets of isolated but interacting regions containing fissionable material.

Because of the nature of difficulties I have mentioned, it has become customary to base criticality control in such circumstances on results of calculations with such codes as the Monte Carlo type Keno. These calculations are made using extreme assumptions as to amounts and distribution of fissionable material so as to bound all situations that might be encountered. Limits are set on amounts and characteristics of the materials to be dealt with. In almost every real case encountered in operations these limits are grossly conservative. Safety is achieved, but at quite a price. Not only are operations limited severely, but workers come to develop a contempt for the limits when criticality infractions do not lead to criticality accidents.

One of my favorite stories in this regard dates to a time long ago, when my group at Brookhaven was engaged in measuring criticality of lattices of slightly enriched uranium rods in light water. Of course, maximum reactivity could be achieved only when the rods were uniformly arrayed in the moderator under optimum conditions. We were told that dissolver limits at Hanford were based on the reactivities we measured, although the geometrical and chemical situation in the dissolvers were far from the optimum conditions we had used. One of the brighter technicians in the Hanford operation decided that dissolving could be speeded up if these criticality limits were relaxed. He was discovered tossing uranium slugs one by one into a barrel of water. The story goes that he was fired forthwith, and a new and higher criticality limit was adopted for dissolver loadings.

How do we get out of this mess? In some cases we can't. But I believe that there are some possibilities. In some cases conservatism can be relaxed.

I have some examples of practices that date back to the fuel fabrication plant for which I developed criticality limits a long time ago, that might be helpful. There limits were based on two principal assumptions which have come to be customary everywhere. The first is that there should be no single clusters of fissionable material (in that case flat plates of a uranium alloy) that under any circumstances of geometry or neutron moderation by water could lead to a critical state. The second is that the individual clusters should be separated so that passive flooding would not cause separate clusters to interact neutronically The limits used in this connection were established using a simple two-group calculation that had been benchmarked against data in the literature on water-reflected uranium solutions. A safety factor of 2.2 was applied to the cross-sectional area of an individual cluster in accordance with requirements on double-batching. That led to a safe cross-sectional area of piles of plates, hence to a safe number of plates that could be stacked anywhere.

From then on control was established ergonomically. The entire fuel fabrication plant was divided into zones. Typically each zone was the location of a single process stage, such as rolling, a machining step, etc. Fissionable material could be moved from one zone to another only in carefully designed carts and by designated control personnel. Carts were so designed that they could carry alloy plates only in geometries meeting the safety criterion. This was guaranteed by their slanted trays which had fronts whose heights would only permit the maximum allowed number of plates; plates above these would slide off onto the floor. Carts were fitted with fenders to prevent their coming too close together. All material in zones could be stored only in the carts that brought the material there, because no other storage possibility was provided. Zones contained no horizontal surfaces where items could be stacked. All surfaces used for writing, for instance, were slanted so that nothing could be left on them. In some cases these design features had to be modified to allow specific processes to be performed in a zone, but in each such case another ergonomic layout was used to accomplish the safety objective. All design was directed to making processes easy to perform, but accidental criticality an impossibility.

The possibility of flooding is central to the safety criteria. There is a subtle difference between the two modes of possible flooding of single containers of plates and wholesale flooding of a processing area possibly containing interacting containers. Though we designed to avoid consequences of either mode of flooding, the second is much less likely than the first, and we could have relaxed the associated precautions if that had been of value. Giving up the small added benefit was just not worth it as far as ease of processing was concerned.

I draw several conclusions from experiences such as these. First, we were lucky to be able to design safety into the process at the very start. That permitted us to institute a control system that fit into operations in a natural way and took into account manufacturing needs. Second, that success in designing a system well suited to production was only possible through participation with operating staff. Third, the use of ergonomic methods was highly successful. I never heard any complaints that criticality requirements were hard to understand. There was just no way they could be misinterpreted or violated. Naturally, there was no criticality infraction at all, at least over the several years that I was involved. Fourth, I found that simple paper and pencil type theory was adequate for our purposes when it was adequately benchmarked against experiments. Of course, our demands on theory were not that involved, but I have always believed that it is better to use simple benchmarked theory which you understand than complex analytical methods that have never been tested under circumstances to which they are to be applied. More on that later. Fifth, it is wise to understand the nature of the conservatism built into the control system. Some of them can probably be relaxed, either through arguments as to their utility or through simple changes to design.

I feel that some of these lessons are adaptable to other more complex problems faced in the Department of Energy's operations today. The possibility of introduction of ergonomic methods is especially attractive, and so is the possibility of use of simpler computational methods. I have been actively pressing the case for the latter.

Now for some further words on computational methods. The staff of the Defense Nuclear Facilities Safety Board has recently been looking into the state of computer codes used by various adjuncts of the Department of Energy. These are codes spanning numerous engineering fields. They have found what seems to be a pervasive problem of lack of configuration control of these codes. Nobody seems to own them. There is a widespread lack of documentation of the codes as they were first written and as they may have been changed with time, and a vacuum in information on the degree of success in applications. So far the criticality codes have not been among those reviewed, but since all other codes that have been looked at have these problems, it should be expected that they have the problems, too.

It would be useful to know the history of the Monte Carlo codes used in criticality calculations, to determine how they may have been altered from any earlier versions, and whether in their present form they have been benchmarked against experimental results falling in the general range of their usage. Always be careful of the old adage, “Garbage in, garbage out.”