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Conference

Proceedings of the Nuclear Criticality Technology Safety Project

Williamsburg, Virginia
May 10–11, 1994

Los Alamos
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Abstract

This document contains summaries of most of the papers presented at the 1994 Nuclear Criticality Technology Safety Project (NCTSP) meeting, which was held May 10 and 11 at Williamsburg, Va. The meeting was broken up into seven sessions, which covered the following topics: (1) Validation and Application of Calculations; (2) Relevant Experiments for Criticality Safety; (3) Experimental Facilities and Capabilities; (4) Rad-Waste and Weapons Disassembly; (5) Criticality Safety Software and Development; (6) Cricality Safety Studies at Universities; and (7) Training. The minutes and list of participants of the Critical Experiment Needs Identification Workgroup meeting, which was held on May 9 at the same venue, has been included as an appendix. A second appendix contains the names and addresses of all NCTSP meeting participants.

INTRODUCTION

On May 10 and 11, 1994, the Nuclear Criticality Technology Safety Project (NCTSP) held its third annual meeting, this time at the Ft. Magruder Inn and Conference Center in Williamsburg, Va. The conference was broken up into seven sessions that addressed the following topics:

1. Validation and Applications of Calculations
2. Relevant Experiments for Criticality Safety
3. Experimental Facilities and Capabilities
4. Rad-Waste and Weapons Disassembly
5. Criticality Safety Software and Development
6. Criticality Studies at Universities
7. Training.

The following proceedings present the summaries or full text of most of the papers given.

This meeting marked the first time that Russian scientists participated in the proceedings. Anatoly Tsiboulia, of the Institute of Physics and Power Engineering: Obninsk, presented a paper on the development of the computer code ABBN-90. Vladimir Yuferev, of the All-Russian Research Institute of Experimental Physics (Arzamas-16), was also scheduled to give a paper presenting an overview of criticality-safety work carried out at the Institute. He was also supposed to present a paper written by colleague Yevgeny Glushkov on similar work carried out at Moscow's Kurchatov Institute. Unfortunately, last-minute visa problems prevented Mr. Yuferev from attending, so Mr. Tsiboulia presented these papers for him. In all instances, he spoke to the conference through an interpreter.

Immediately prior to the meeting, on May 9, NCTSP working groups met in session at the conference center. These working groups addressed the topics of

- Physics Criteria for Benchmarks,
- Evaluation Techniques, Parametric Studies,
- Experimental Needs, and
- Rules and Regulations Standards.

The minutes and participant list of the Experimental Needs meeting are given in Appendix I. Appendix II contains a list of the names and addresses of all the NCTSP participants.

As a final note, it should be mentioned that a partial annular eclipse of the sun, which occurred in the sky over Williamsburg during the lunch hour on May 10, prevented the conference organizers from starting Session 2 (Relevant Experiments for Criticality Safety) on time. This resulted in the session's running late, which necessitated a drastic abbreviation in the session's concluding remarks given by Burton Rothleder.

AGENDA
NUCLEAR CRITICALITY TECHNOLOGY SAFETY PROJECT MEETING
MAY 10 AND 11, 1994

TUESDAY, MAY 10

Introduction and opening remarks	Burton Rothleder (DOE/EH-64)	0830-0845
Keynote speaker	Herbert J. Kouts (DNFSB)	0845-0945

Session 1:
Validation and Applications of Calculations

Co-chairs:
Blair Briggs
(INEL)
Dae Chung
(DOE)

Speaker	**Topic**	
Anatoly Tsiboulia (Institute of Physics and Power Engineering)	Critical Experiments Analysis by ABBN-90 Constant System	1000-1030
Cecil V. Parks (ORNL)	Calculations of k_∞ for Homogenous U-235/Metal Mixtures: Will the Real k_∞ Please Stand Up?	1030-1055
Fritz Trumble (ORNL)	Criticality Safety Benchmark Evaluation Project: Recovering the Past	1055-1120
Roger Brewer (LANL)	The Impact and Applicability of Critical Experiment Evaluations	1120-1145
Lunch	Speaker: William Vernetson, past Chair of the National Organization of Test Research and Training Reactors	1145-1315

TUESDAY, MAY 10 (cont.)

Session 2:
Relevant Experiments for Criticality Safety

Co-chairs:
Burton Rothleder
(DOE/EH-64) and
Dennis Cabrilla
(DOE/EM-431)

Speaker	Topic	
Burton Rothleder Dennis Cabrilla	Introduction: Experiment Selection Procedure by the Methodology and Experiments Subcommittee (MES) of the Nuclear Criticality Experiments Steering Committee *	1315-1330
Michaele Brady (SNL)	Spent Fuel Safety Experiments (SFSX)	1330-1350
Rene Sanchez (LANL)	Proposal for Experiments with Actinide Elements	1350-1410
Richard E. Anderson (LANL)	Validation of Calculational Methodology in the Intermediate Energy Range	1410-1430
Robert Rothe (RFP)	Plutonium Solution in Concentration Range from 8 to 17 g/L	1430-1450
Richard E. Malenfant (LANL)	A Program to Evaluate Measurements of Subcritical Systems	1450-1510
J. Blair Briggs (INEL)	Absorption Properties of Waste Matrix Materials	1530-1550
Dennis Cabrilla Hans Toffer (WHC)	Alternate Measurements of Benefit to Criticality Issues at Hanford	1550-1610
Burton Rothleder	Reduction of the Experimental Burden: Calculations as Benchmarks	1610-1630
Working Group Summaries		1630-1700
Banquet	Speaker Victor Stello	1800-2000

* *See Appendix I, Attachment 3*

WEDNESDAY, MAY 11

Session 3: Experimental Facilities and Capabilities

Chair:
Richard Malenfant
(LANL)

Speaker	Topic	
Vladimir Yuferev* (All-Russian Research Institute of Experimental Physics)	An Overview of Criticality Safety Research at the All-Russian Research Institute of Experimental Physics	0815-0845
Yevgeny Glushkov* (Kurchatov Institute)	A Short Review of Critical Experiments Performed at the Kurchatov Institute	0845-0915
Richard Malenfant (LANL)	Critical Experiments, Accident Simulation, and Dosimetry at the Los Alamos Critical Experiments Facility	0915-0945

**Papers presented by Anatoly Tsiboulia*

Session 4: Rad-Waste and Weapons Disassembly

Co-chairs:
Adolf Garcia (ANL)
Ivon Fergus (DOE/EH-11)

Speaker	Topic	
Ron Knief (Ogden Environmental)	Criticality Analysis for Weapon Disassembly at the Pantex Plant – Part I: Bare Pits	1000-1020
Steve Payne (DOE/ALO)	Postulated Accident Scenarios in Weapons Disassembly	1020-1040
Steve Troyer (Batelle, Pantex)	Criticality Safety in High Explosives Dissolution	1040-1100
John Schlesser (LANL)	Next Generation Storage Facility	1100-1120
Jor-Shan Choi (LLNL)	Long-Term Criticality Concerns Associated with Disposition of Weapons Plutonium	1120-1140
Ron Knief	Criticality Analysis for Weapon Disassembly at the Pantex Plant – Part II: Staging	1140-1200
Lunch	Speaker: Dr. Robert Wilson (US NRC)	1200-1300

WEDNESDAY, MAY 11 (cont.)

Session 5: Criticality Software and Development

Co-chairs:
Arthur R. Forster
(LANL)
Michael Westfall
(ORNL)

Speaker	Topic	
Roger Blomquist (ANL)	VIM – Monte Carlo Neutron Transport Code	1300-1320
Dan Hollenbach (ORNL)	KENO Developments	1320-1340
Bill Lloyd* (LLNL)	COG Developments	1340-1400
Art Forster (LANL)	Recent Developments in the Los Alamos Radiation Transport Code System	1400-1420
Mark Williams (Louisiana State University)	Energy-Pointwise Discrete Ordinates Transport Methods	1420-1500

** Paper presented by Jor-Shan Choi*

Session 6: Criticality Safety Studies at Universities

Chair:
Robert Busch
(University of New Mexico)

Speaker	Topic	
Don Harris (Rensselaer Polytechnic Institute)	Critical Experiments with Mixed Oxide Fuel	1500-1520
David Hetrick (University of Arizona)	Student Research in Criticality Safety at the University of Arizona	1520-1540
Lee Dodds (University of Tennessee)	Criticality Safety Research at the University of Tennessee-Knoxville	1540-1600
Robert Busch (University of New Mexico)	Nuclear Criticality Research at the University of New Mexico	1600-1620

WEDNESDAY, MAY 11 (cont.)

Session 7:
Training

Chair:
Mayme Crowell
(ORISE)

Speaker	Topic	
Ava King Harvey (Y-12 Plant)	Training at the Y-12 Plant	1630-1640
Bart Woodruff (LANL)	Criticality Safety Training	1640-1650
Richard Taylor (Y-12 Plant)	Training of Nuclear Criticality Safety Engineers	1650-1700
Lee Dodds (University of Tennessee)	Nuclear Criticality Safety Course Descriptions	1700-1715
Richard Malenfant (LANL)	Criticality Safety Training at Los Alamos	1715-1730

THE IMPORTANCE OF EXPERIMENTAL INFORMATION IN CRITICALITY RESEARCH

H. J. Kouts
Defense Nuclear Facility Safety Board

About 30 years ago, when I was a member of the Advisory Committee on Reactor Safeguards, a frightful thought occurred to me. It was that at some distant future time there would be nuclear power plants, but there would be no people who really understood the neutron physics of chain-reacting systems. Reactors would be designed by cookbook methods, using procedures written by people who themselves had received their instruction from books written by other people. The thought was frightful because of the implications for the safety of the reactors. To be sure, neutron physics and the protection against power excursions underlie only part of the safety of nuclear plants, but that part is very important. And I am not comfortable with the thought that there might be no individuals associated with the safety of these plants who had developed the kind of insight into the behavior of chain-reacting systems that comes from taking systems of this kind to critical under a variety of situations. Likewise, as my attention has shifted in recent years to safety in the defense nuclear arena, I have the same frightening thought concerning nuclear weapons in the future.

I have faith that the era of nuclear power is not drawing to an end, that the Luddites who oppose all advances brought by high technology will in due time be defeated by reason and the reality of a world hungry for electricity produced even when the sun does not shine and the wind does not blow. And I am unable to visualize a future world without nuclear weapons, if that world contains more than a single country. I do not believe that any major nuclear-weapons power will ever place itself in the position where it would become defenseless in the face of discovery that some other country had not played by the same rules of disarmament.

So, I am convinced that it is important to make sure that there always continues to exist a cadre of research scientists who knows criticality as something more than what happens in running a reactor simulator, or what is found by solving an eigenvalue problem, or running a computer code like KENO, or a weapons design code.

What has been the origin of my personal feeling of concern regarding this matter? It is a result of a long-time background in a world in which experimental studies in criticality abounded. Such studies were carried out in numerous facilities of the Atomic Energy Commission (AEC). Simply to list them is a source of comfort in respect to a widespread diffusion of understanding of the behavior of neutron chain-reacting systems.

The principal centers for development of data on criticality were Los Alamos, where, in my time, Hugh Paxton and his coworkers developed so much understanding, particularly of metal systems, and Oak Ridge, where Dixon Callihan and his associates did so much work on uranium systems of many kinds. But there were many other places where important experimental work was carried out. At Rocky Flats, a group under Schuske generated information on criticality of plutonium metal systems, important to the safety of handling components of nuclear weapons. At Hanford, Duane Clayton's group developed wider understanding of the criticality of plutonium systems, especially solutions. Of course, at Argonne East and Argonne West there were numerous critical experiments directed to reactor design, ranging from those pertinent to the first Nautilus reactor core to the basic design experiments for the Savannah River reactors and numerous basic studies and design experiments for fast reactor cores and breeder assemblies containing oralloy and plutonium fissile elements. Other submarine design experiments were done at the Bettis and Knolls Laboratories. We should not forget the important basic water-reactor studies at Bettis under Dan Klein, and the early studies at Knolls on beryllium-moderated intermediate-neutron-energy critical systems.

We can continue at greater length: there were flexible critical experiments at Savannah River for improved understanding of the physics of heavy-water-moderated reactors and for design of production reactor loadings. At what is now the Idaho National Engineering Laboratory, there were basic studies in the RMF and the ARMF facilities, as well as critical experiments to ensure safe loading of the test reactors operated at that site. At Livermore there were basic experiments in weapon design and weapon safety that only ended after a plutonium fire shut down the experimental facility. Critical experiments were conducted at Sandia. The Air Force ran a critical experiment facility at its Plum Brook facility. Critical experiments were conducted at Hanford in the design of graphite-moderated reactors, leading up to design of the N-Reactor. And there were important reactor design facilities operated by Westinghouse at Walt Mills, by General Electric at Vallecitos, by Babcock and Wilcox near Lynchburg, and by Combustion Engineering near Hartford. I know that I have slighted some important areas that I have just not recalled or that I was not aware of.

But I do have to add to the list the richly varied array of critical experiments that were done by my talented group of experimenters at Brookhaven. And you will have to forgive me if I mention this work in somewhat more detail, as it does underlie the importance that I personally assign to the actual experience of conducting critical experiments.

At Brookhaven we did basic exponential and critical experiments with slightly enriched uranium, ^{233}U/thorium, and plutonium/uranium systems–most often with light water as the neutron moderator, all in order to produce general reactor physics data. But we also did experiments on graphite-bismuth systems in connection with the design of a liquid-fueled reactor, and we ran series of neutron physics studies for design of a number of research reactors, including the Brookhaven High-Flux Beam Reactor, the Brookhaven Medical Research Reactor, the reactor for the Aberdeen Proving Ground, and several university reactors. In designing the High-Flux Beam Reactor, we ran a very large number of critical experiments; I believe it must have been well over a thousand criticalities. Our program ended with several fast reactor critical assemblies performed with a fuel of thin uranium-aluminum alloy foil, for the purpose of evaluating heterogeneous effects in fast critical assemblies.

During this period when critical experiment facilities were abundant and very active, the community of those engaged in the experiments was large and strongly interacting. Data and techniques were widely shared. Individuals made frequent visits to each others' facilities. We at Brookhaven had a joint program with Bettis that involved use by both facilities of the same slightly enriched uranium fuel elements and permitted interchange of experimental data and analytical methods. This program established the experimental data base underlying design of the Shippingport Reactor and successor light-water reactors. We sent some of the fuel that we had used to MIT for use in exponential experiments using heavy water. We sent some to the SPERT facility in Idaho for use in the second SPERT destructive test. In some of our experiments, we used fuel made at Fernald. In others, we used fuel made at Oak Ridge, at Los Alamos, at Babcock and Wilcox, at Nuclear Metals in Cambridge, Massachusetts. We sent data to a number of places for a variety of uses: the interactive program with Bettis that I have just mentioned; to Hanford for use in ensuring safety in dissolver operation at the PUREX facility; to the California Research Corporation for use in designing the target for E. O. Lawrence's Materials Test Assembly, which was to be an accelerator used to produce plutonium; to Savannah River; and to commercial facilities for use in development of reactor design codes. These interactions are simply examples of those that I knew firsthand and that involved research at Brookhaven, which was not even one of the major sites for criticality studies. No doubt those who were engaged in programs at the major sites could relate even richer stories of accomplishment and interaction.

Of course, the abundance of research in the days whose history I am repeating was served by a much greater freedom of action than is found now. There was much less formality associated with funding, and there was much less external safety review. Again a note from the Brookhaven program to illustrate the point: in the course of our conduct of exponential experiments, we built three source reactors each having a maximum power of 100 kilowatts without the need to seek authorization or approval from the AEC. We did write safety analysis reports on each, and we subjected each to a safety review by our local safety committee, which was a very high-class group, but the review ended there. I doubt that we could have done nearly as much research if we had worked in today's climate. Tolerance for error was higher in the past, and I will have more to say about that in a few minutes.

But first, I want to relate some stories from the past that illustrate the importance of good understanding of the physics of neutron chain-reacting systems. Most of these stories are not written down anywhere that I know of. Some may even be apocryphal to some extent. Some are slight in content, and some are amusing to a degree. But all are related to experimental experience in criticality.

The first story concerns the first post-war production reactor built at Hanford, which, I believe, was the H-Reactor. I simply repeat what I was told afterwards by someone who had access to information on the event. This reactor was designed by a new crew–Fermi ,Wigner, Weisskopf, and coworkers having long departed that scene. The new crew decided that they would use better reactor theory than had been used in design of the earlier reactors and better neutron data that had been developed in the interim. They calculated the expected number of channels for operation and fortunately, following the example that had been set by Fermi prior to his discovery of xenon poisoning, built into the graphite structure and the cooling capability a generous excess of channels above the number estimated as necessary. It developed that the extra channels were needed. I was also told that this was the first of the production reactors that used commercial steel plate for the forms containing the poured concrete shielding. For this purpose, the earlier reactors used excess steel armor plate that had been set aside for battleships that had been sunk at Pearl Harbor. This armor plate came in odd shapes and had numerous holes for fasteners. It had been necessary to make design drawings showing how the plate was to be reshaped and the holes filled for the new purpose. The shield for the new reactor started from these very drawings. The commercial plate was cut to fit the original shapes as shown in the drawings, and the holes were drilled to match. They were then recut as the drawings showed, the holes were filled, and the forms were erected and the shield was poured. I have no firsthand knowledge of these early Hanford stories, and they may be apocryphal to some extent, but I only relate to you anecdotal information as I received it.

My second story is better established because I did hear it from the principal. It illustrates the triumph of insight over bad theory. Irving Kaplan, of whom I am sure you have all heard and some know, had left Brookhaven, where he had been the physics designer of the Brookhaven Graphite Research Reactor (GRR), which was the first nuclear reactor built for purely peaceful research. Irving went to MIT to join the faculty being assembled by Manson Benedict in nuclear engineering. On a visit about a year later he told me that one of the problems he had assigned his class was to calculate the k_∞ of the Brookhaven GRR. Now at the time, the reactor was fueled with natural uranium slugs identical to those used in the eight Hanford production reactors: 1.1 inches in diameter and about four inches long. These were placed end-to-end in aluminum cladding, so that each fuel channel contained two composite elements about 12 feet long. Irving expected some straightforward use of the four-factor formula.

One of the students brought in his results in the form of a thick sheaf of calculations. Irving went directly to the bottom line of the calculation, where the result was stated as something like "$k_\infty = 9$." Irving said simply, "No." The student was outraged: "What do you mean? You haven't even looked at my calculation. See, I used multigroup theory, and it's all laid out." Irving said "No" again. "But I used a computer, and it can't be wrong," the student replied. This is certainly a straightforward example of the value of *insight* into the neutron physics of a system, and how it can defeat bad science. It illustrates what I call a "sanity check," which is a simple practice of asking whether an answer makes sense in the context of all that is known about the subject of the question.

A third story involves another operation at Hanford in the early 1950s. This was also told to me by an individual who knew it firsthand. As I said earlier, some of the Brookhaven water-lattice criticality data was used at Hanford in connection with dissolver safety. It was also used for ensuring criticality safety of irradiated fuel slugs stored in buckets underwater in spent-fuel storage pools. Both of these uses were highly conservative because the criticality data were based on the assumption of regular arrays of fuel in a water moderator, whereas the storage was under conditions far from optimum for achieving criticality. A Hanford visitor told me that the conservatism had been a source of vexation to one of the technicians involved in storage. He had heard that it should really be possible to place many more spent-fuel slugs in a bucket than the rules allowed. So he was found testing that point, moving into an already fully loaded bucket additional slugs from a neighboring bucket. The technician was fired, but the maximum loading in buckets was increased to reflect his experimental finding. Another triumph of experiment over idealized theory.

Now a story about Rocky Flats. Schuske's group conducted experiments to establish the safety of handling and storage of plutonium in process and after the formation into weapons components manufactured at the Flats. At one point, during the intensive buildup of the weapons stockpile during the Cold War, the vaults were becoming rather full of plutonium components, and the question was raised as to whether safety of storage might be compromised by neutron moderation in the bodies of individuals working in the vaults. So an experiment was run. The count rate was measured with the vault empty of people, with one person in the vault, with two people in the vault, etc. The results were plotted as an inverse multiplication curve, as in an ordinary approach to critical.

This story was somewhat ruined by new information on this experiment that I received not long ago from Tom McLaughlin when I mentioned the experiment to him. It turned out that not only did he already know about it, he had a copy of the original report. In fact, the count rate in the vault was reduced as additional people entered. The neutron density was affected more by the neutron *absorption* of the additional bodies than by the neutron *moderation*. What is the moral of this story? I guess it is that even the best insight from long experience with criticality needs testing experimentally. I wonder what theoretical calculations would have predicted?

And a final story in this sequence. This concerns experiments at Livermore during the period before the fire shut down the critical experiment facility at that site. Again, the story was told to me by a participant. Concern arose as to effects of heightened neutron reflection during handling of one particular plutonium assembly by experimental personnel. Someone in the experimental group established that reflection by a human hand could be reproduced by the use of a pork chop. So, in this case, a reciprocal multiplication curve was plotted as a function of the number of pork chops piled on the assembly. In this case, I was told that the familiar form of an approach to critical was seen, though I never saw the curve myself. Nor was I told of the subsequent fate of the pork chops, and whether they formed the basis of someone's dinner.

These are a few examples of the importance of experimental information on criticality, where theory, or even intuition, has not been adequate. Of course, the examples refer back to times when theory was not as advanced as it is now, when neutron data were not as well established, and when powerful digital computers were not available to take advantage of detail in calculational methods. But the advance in capability over the years is being matched by growth in difficulty of problems, as attention shifts from design of simply connected reactor cores of elements in regular arrays and relatively simple geometries encountered in weapons designs to complicated problems of arrays of storage regular containers generated during cleanup of facilities that are now surplus from defense activities.

I said earlier that I would say some things about how the formality of research in criticality has changed over the years. Why can't we still build small source reactors without long processes to get programmatic and financial approval, and environmental impact studies and prolonged safety review?

Part of the reason, of course, is that bureaucracy grows with time, and administrative arteriosclerosis sets in. Things naturally become more difficult as time passes. I remember going to see Dixie Lee Ray a few months after she had left the AEC and had become the First Assistant Secretary of State for Oceans, Atmosphere, and Environment. She told me that shortly after she joined the State Department she had needed to take on a certain former high official of the AEC as an advisor. She managed to get a consultant contract through for him in a week's intense effort. Whereupon, she said, the State Department formed a committee to find out how she had done that so quickly, and to close down that process so it could never be done again. So bureaucracy is part of the problem. But it is not the full problem.

I am afraid that there are two diametrically opposed sets of arguments that can be made regarding the need for formality–and here I narrow the coverage of the discussion to mean safety in critical experiments. One argument is from the standpoint of logic and reason. The other is a societal one that pays attention more to effect and popular reaction.

First, the logical argument. A very large number of critical experiments have been done in the United States. There have been critical experiment accidents, a fair number, in fact–perhaps one or two dozen, depending on the definition of a critical experiment accident. But following the accidents that occurred during the Manhattan Project, there have been no fatalities in these accidents. The reason is that the experiments have been done with care and under conditions such that if an undesired excursion did take place, injury to nearby individuals would be very unlikely. The design of experimental facilities takes advantage of distance and shielding.

This realization has caused some to feel that the level of safety that seeks no accidents at all may be too stringent. Some years ago, one well-known practitioner in critical experiments said to me that if you never have an accident you are probably being too careful. I shall not say who said this to me. He meant that the balance between programmatic and safety needs was probably not optimized right in such a case. There can be some truth in this view, strictly from the standpoint of logic.

But the institutional arguments on the other side will clearly win. We live in a world now where the slightest departure from the normal in nuclear matters is a cause for hysterical reaction. Such stories receive the widest possible circulation, in the most lurid prose, and the readers, who know so little about nuclear matters, are frightened by even the inconsequential. Accidental criticality would be regarded by most people as equivalent to the detonation of a nuclear weapon.

The prevalent requirement for formality in critical experiments recognizes this, and seeks to avoid accidents of whatever magnitude and consequence.

I am afraid that the free and easy days will not return.

But it is necessary to maintain active programs in critical experiments under the prevailing rules, to maintain as respectable the number of individuals who understand from firsthand experience the physics of chainreacting neutron systems.

I am deeply pleased to have made the opening remarks at a meeting dedicated to answering this.