VOLUME 9 NUMBER 4

REVIEW

OAK RIDGE NATIONAL LABORATORY · FALL 76

THE COVER: This motley grid, the result of the combined talents of designer Bill Clark and darkroom artist Tom Maxwell, represents a collection of old pictures of young faces. They reappear throughout the pages of this issue in full resolution, for you to puzzle out. Answers on inside of back cover.

Editor
BARBARA LYON

Staff Writer
CAROLYN KRAUSE

Consulting Editor
ALEX ZUCKER

Special Editor
H. S. POMERANCE

Historical advisors
for this issue:

S. E. BEALL, S. F. CARSON,
A. H. SNELL, E. H. TAYLOR.

Publication Staff: Graphic Design/Bill Clark; Technical Editing/R. B. Parker; Typography/Linda Jeffers, Nancy Smith; Makeup/Mary East; Reproduction/Bill West.

The *Review* is published quarterly and distributed to employees and others associated with the Oak Ridge National Laboratory. The editorial office is in Building 4500-North, Oak Ridge National Laboratory, P.O. Box X, Oak Ridge, Tennessee 37830. Telephone: 483-8611, Extension 3-6900 (FTS 850-6900).

REVIEW

VOLUME 9, NUMBER 4 **FALL 1976**

Editor's Note

In observation of the Bicentennial, this special issue of the *Review* contains a skeleton history of the Laboratory's first 25 years, interspersed with reminiscences, anecdotes, funny pictures, and a few expansions on particular aspects of importance to the Laboratory.

The "history," merely a compilation of selected high points in ORNL's progress from pile site to major research and development institution, contains the inevitable inadequacies inherent in such a hastily (three months) gathered account. Our apologies go to the thousands of gifted scientists, engineers, and support personnel who contributed to that progress, but who remain unsung here. Our history suffers from tunnel vision, having been developed from a number of sources that were necessarily limited by the length of time given to the issue's preparation. Sources of information for the history were Alvin Weinberg's "State of the Laboratory" addresses; W. E. Thompson's "History of Oak Ridge National Laboratory—1943-63"; the ORNL *News*; and Richard G. Hewlett and Francis Duncan's two-volume *A History of the United States Atomic Energy Commission*. The reviewers and critics of the early drafts, although necessarily low in quantity, are of unassailable quality. They are: John Auxier, P. S. Baker, S. E. Beall, D. S. Billington, S. Cantor, S. F. Carson, Waldo E. Cohn, Floyd L. Culler, Jack Cunningham, Doyle M. Davis, A. T. Gresky, William Gude, W. O. Harms, H. G. MacPherson, Peter Patriarca, Herbert Pomerance, A. H. Snell, John Swartout, Ellison H. Taylor, Alvin M. Weinberg, and Alexander Zucker. Perspective was the motive in keeping the time scale to 25 years.

As for the personal reminiscences, like the gentle rain from heaven, they were, for the most part, forthcoming without strain. The result is a low-control melange that has its own charm. Almost all of the material that was proffered has been included; the anecdotes not used largely described events outside the Laboratory's technical life: the mud has been well covered elsewhere.

A very select few have been singled out for special tribute. Of the living, the seven members of the National Academies of Engineering and/or Sciences, and our one Nobelist were chosen. In addition, four deceased staff members are included. Such arbitrary limits had to be observed, as there was no other equitable way to draw the line: the honors bestowed upon the Laboratory by the distinctions and achievements of its staff members are legion.

If the mix works, this issue will provide the flavor of the growing Laboratory. It is for enjoyment. It is ORNL's third nod to the Historical Year.—B. L.

OAK RIDGE NATIONAL LABORATORY

OPERATED BY UNION CARBIDE CORPORATION • FOR THE ENERGY RESEARCH AND DEVELOPMENT ADMINISTRATION

Dedication

By HERMAN POSTMA

This issue of the ORNL *Review* constitutes a retrospective look at the Laboratory—how it evolved, its history, and a selection of anecdotes and personal recollections of the original ideas and decisions that contributed to the creation of an entirely new national institution. During the nation's bicentennial year of its revolution, we should also recognize that the Oak Ridge National Laboratory was itself integral to another type of revolution: the Laboratory, too, was conceived out of necessity and has experienced similar growth pains, important changes, and uncertainties, and was a model for others to emulate.

Although national laboratories are now taken for granted, there were, at the time of their formulation, no predecessors, much less any such institutions managed for the Government by a private contractor. After the Second World War, the national laboratories were needed to fill the widening research gap between universities and industry, and their successful response to this need has resulted in the establishment of similar institutions abroad, with even a "sister laboratory" relationship between some U.S. and foreign national laboratories.

The Oak Ridge National Laboratory was established during the Second World War. Its primary attention focused on chemical reprocessing of fission products, with a growing emphasis on many of the basic sciences to support the technologies. Subsequently, work evolved for agencies other than the AEC, particularly in environmental and health areas, and ultimately developed into today's immense national mission embracing the R&D of supply and demand energy sciences and technologies. Through the many evolutionary processes and decisions depicted in part in this issue of the *Review*, the Laboratory has become a pacesetter among national laboratories as an institution in which multidisciplinary R&D intertwines with multiprograms, responding to the needs of many agencies, serving as well to reintegrate, at a common scientific level, the many fractures that have been created by the governmental budget allocation process. In addition, ORNL has supplied needed continuity to research and technologies and has stepped into the breach in those areas where industry could not be expected to be objective or where universities could not put together long-term teams of dedicated investigators.

The impact of the Laboratory's scientists and technologists on the country—indeed, on the world—has been great, and the Oak Ridge National Laboratory has, itself, become an instrument of revolution. It has created profound changes during a third of a century in many areas that have served to make ours a stronger nation and one to which our institution can be a proud contributor.

To the entire staff, past and present, who made these fundamental contributions, this issue is dedicated.

Selected Technical Highlights in ORNL's First 25 Years

Compiled by CAROLYN KRAUSE

Since 1943, Oak Ridge National Laboratory has made outstanding contributions in the life sciences, physical sciences, and engineering disciplines relative to the safe, economic production and use of energy, particularly nuclear energy. Over the years the Laboratory has had a number of missions—demonstrating the safe production and chemical recovery of plutonium as part of the effort to develop an atomic bomb, assisting the U.S. Atomic Energy Commission in its Power Reactor Demonstration Program by developing safe chemical methods for reprocessing nuclear fuels, producing radioisotopes for scientific research and medical purposes, training people to operate reactors and chemical reprocessing facilities, developing economically competitive nuclear power systems, developing advanced nuclear power systems such as breeder and fusion reactors, and supporting the U.S. Energy Research and Development Administration in its missions of ensuring sufficient energy supplies through the development of nuclear and nonnuclear energy options, including coal utilization and conservation. Following is a chronological account by years of the missions and some of the technical achievements of ORNL researchers during the first 25 years of the Laboratory's life.

1943

Originally known as Clinton Laboratories, Oak Ridge National Laboratory was established early in 1943 as the pilot plant for demonstrating the safe production of plutonium-239, a fissionable man-made element. At the time, Clinton Laboratories was a branch of the Metallurgical Project under the direction of Arthur H. Compton, head of the University of Chicago Metallurgical Laboratory. Martin D. Whitaker was the Laboratories' first director. Actually, Clinton Laboratories had its inception as early as 1942, when the U.S. Army organized a new Manhattan Engineer District under the Corps of Engineers to manage activities focusing on producing an atomic bomb. The Army engineers bought 80 square miles of Roane and Anderson Counties in Tennessee, telling residents for security reasons that the tract—selected by General L. R. Groves—was being acquired for the establishment of the "Kingston Demolition Range." The Corps of Engineers built a town and administrative buildings under the name "Clinton Engineer Works." whose mission was to produce fissionable isotopes of plutonium (at X-10) and uranium (at the Y-12 Electromagnetic Separations Plant, the S-

50 Thermal Diffusion Plant, and the K-25 Gaseous Diffusion Plant) as part of the national effort to develop an atomic bomb.

In order to supply the first gram quantities of plutonium and to test processes for chemically separating this fissionable material, ground was broken February 1, 1943, for the X-10 Graphite Reactor in the area called Bethel Valley. This event took place about two months after the first self-sustaining nuclear chain reaction was achieved (December 2, 1942) under the direction of Enrico Fermi at the west stands of the University of Chicago athletic field. Based on data from the original Fermi pile and the CP-2 (the second Chicago pile), the X-10 Pile, or Graphite Reactor, was designed as a pilot plant for large-scale plutonium production reactors at Hanford, Washington. The 1-MW air-cooled pile, the world's first operating nuclear reactor to produce sizable amounts of heat, was built in nine months. On November 4, the reactor, holding 30 tons of uranium, was started up under the supervision of Fermi, with Compton looking on. Fermi and Compton were summoned from the Oak Ridge Guest House at 5 AM, because the reactor went critical sooner than expected.

Clinton Laboratories, operated by the Metallurgical Laboratory of the University of Chicago and constructed by E. I. du Pont de Nemours & Co., Inc., in addition to its plutonium pilot-plant activities, trained 180 Du Pont operators for the Du Pont–operated Hanford facilities, produced some radioisotopes, and studied the occupational hazards of using radioactive materials. Between February 1943 and the summer of 1944, Du Pont constructed 150 buildings at the X-10 site at a cost of $13 million, including the X-10 reactor and the radiochemical pilot plant, where, within cells surrounded by concrete walls that were 5 ft thick, plutonium first was separated on a large scale from fission products.

M. T. KELLEY

Representatives of Laboratory service divisions at the third annual staff conference in January 1952. From left, E. A. Bagley, General Office Division; D. D. Cowen, Information and Reports; K. A. Fowler, Industrial Relations; T. A. Lincoln, Health; M. E. Ramsey, Operations; L. P. Riordan, Laboratory Protection; and D. W. Cardwell, Engineering and Maintenance.

The First 15 Years

By A. F. RUPP

K. A. KRAUS

Art Rupp retired in 1973 from his job as Superintendent of Laboratory Services, after 30 years of loyalty to Oak Ridge, barring one year spent at Hanford in 1944-45. His principal technical activity was in radioisotope production. Here he tells how it was different then.

The "Project" (shortened from Manhattan Project) was the unifying force that covered all the activities, thoughts, and aspirations of the technical, military, and other personnel who had some understanding of its objectives. From universities, industrial laboratories, and military units across the country an extraordinary group of scientists, engineers, and specialists converged on Project sites the spring and summer of 1943, including Site X—Clinton Labs, ancestor of ORNL.

H. SOODAK

The sense of mission on the Project transcended even the resolute dedication of the general American public in World War II. This feeling was also manifest among the support crafts and service personnel, even though they were unaware of the true objective of the Project. The vital job of transforming abstract ideas into concrete and steel could not have been entrusted to better people than the patriotic, taciturn people of East Tennessee. To them, security was a personal matter: no need for lectures and slogans. Such cautionary admonitions as: "Don't ask unnecessary questions; don't say anything to anyone; what you see here, forget it" were not needed. Tennessee Highland people knew how to keep a secret.

But they speculated among themselves. One notable guess (which came close!) by the workers putting in the 7-foot concrete walls for the graphite pile:

"Some wall—wonder what it's for?"

"Dunno, must be some kind of explosive."

"Must be a heller."

The wartime scene at X-10 was a curious military-academic blend with overtones of the chemical industry. There were many young officers and GIs with scientific or engineering backgrounds who worked in groups alongside civilians. The GIs were quartered in barracks in Oak Ridge. Rain and Tennessee red clay mud formed the background in 1943 for simultaneous, round-the-clock construction and operations.

M. J. SNOW

Loading the graphite pile was a signal event. Almost all the technical men were enlisted to help charge uranium slugs into the graphite channels, working through the night; then, the approach to criticality and the chain reaction on November 4, 1943! At that point, in a real sense, THE LABORATORY was born; its mission: produce, separate, and purify experimental amounts of plutonium; provide data for the design of the big production units at Hanford. Never again was the Laboratory's mission to be so clear, so circumscribed, so urgent.

The first hot slugs from the pile, hand-pushed out the back with long rods inserted into the front face channels, were collected in buckets in the canal, the mysterious blue glow observed for the first time with amazement. This was more radioactivity than anyone had ever seen before! Transported through the semiworks and pilot plant by chemical processes that had only been worked out on micrograms of cyclotron-produced plutonium, the first milligrams of a man-made element were separated and purified.

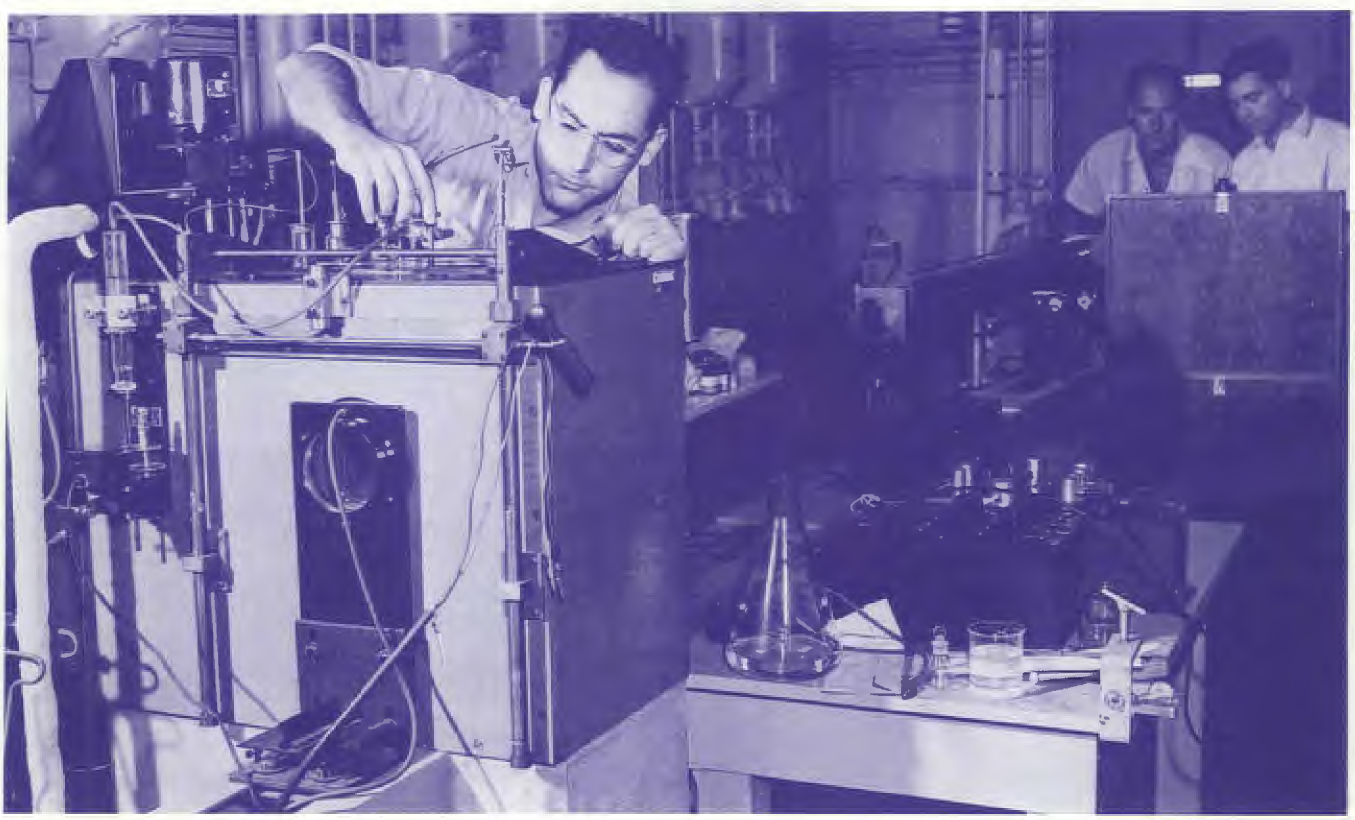
The importance of these first relatively huge amounts of plutonium would be hard to exaggerate. The fission products also were sought almost as avidly by the chemists all over the plutonium project. Frantic chemical work ensued to check, refine, and revise flow sheets, with équipment changes going directly into the fabrication shops.

The administrative control of the Laboratory was complex: the Army, overall administration; the University of Chicago Metallurgical Laboratory, science and technology; Du Pont, overall operations. The fact that it worked so well is certainly a tribute to those who directed the several parts. The temper of those times is well expressed in the words of Arthur Holly Compton in his message to Met Lab personnel January 1, 1944:

"We are beginning the third year of our intensive effort with courage and the will to win. ... Our hearts are warmed by the comradeship of our colleagues who work with us amid forgotten hardships with loyalty and devotion toward the successful completion of our common task. We rejoice to play an active part in the world's historic struggle. ... We are determined to do all in our power to bring prompt victory."

Then, VE day, Hiroshima; the war was over! Following the exhilaration and celebration, the inevitable letdown came. It was all over, the race won, the objective attained—so now, what? What was to become of the Project, THE LABORATORY? Everywhere, groups were

Fred Vaslow with equipment in Biology Division with which he could determine the purity of irradiated enzymes by electrophoresis (1949).



talking, discussing, letting down from the great effort so suddenly concluded.

Postwar changes swept over the Laboratory. Atomic energy was more than bombs—the great power of the atom should be used to help mankind produce abundant power and radioactive materials for science and medicine. Big changes surged through the Lab—Monsanto replaced Du Pont; the University of Chicago phased out; Monsanto phased out; the University of Chicago almost came back in; Union Carbide took over and ORNL was born. Dick Lyon paraphrased "Clementine":

R. N. LYON

"... you are lost and gone forever,
Dreadful sorry, Clinton Lab."

The Oak Ridge National Laboratory, born from a wartime project and suddenly orphaned, rose shakily to its feet, made suitable supplications toward its new principal, the Atomic Energy Commission, and tried to establish new goals.

The main programs decided upon were power reactor development; radioisotope production; supportive sciences—physics, chemistry, biology, and health physics. The following 15 years (1945–60) were great years of growth and achievement, during which the international reputation of ORNL was firmly established. The first new physical expansion was the building of the radioisotope area at the end of the plant site just east of the chemical area. (Of the old wood buildings, those still standing are: 706-A, chemistry; 706-D, "RaLa"; 706-C, high-radiation chemistry.) The great brick stack, an ORNL landmark, was built to serve the radioisotope area and the RaLa process, but during the years most of the radiochemical areas were attached to it. The great push to put radioisotopes into medicine, science, and industry was started. The Graphite Reactor and the radiochemical processing units furnished radioisotopes for the world, virtually as the sole supplier. Many of the other Laboratory developments were still shrouded in secrecy: the design of light-water high-flux reactors; the construction of the LITR (Low Intensity Test Reactor, forerunner of the MTR and Navy reactors); the power pile group's work; the Daniels gas-cooled pile; and the multitude of pioneering work in nuclear physics, chemistry, and biology.

T. I. ARNETTE

During this time, the ORNL isotope program (which included stable isotopes electromagnetically separated in the Y-12 calutrons) was the main contact with the "outside world"; the radioisotope missionaries spread the word about the great laboratory on the Clinch River in the Tennessee hills.

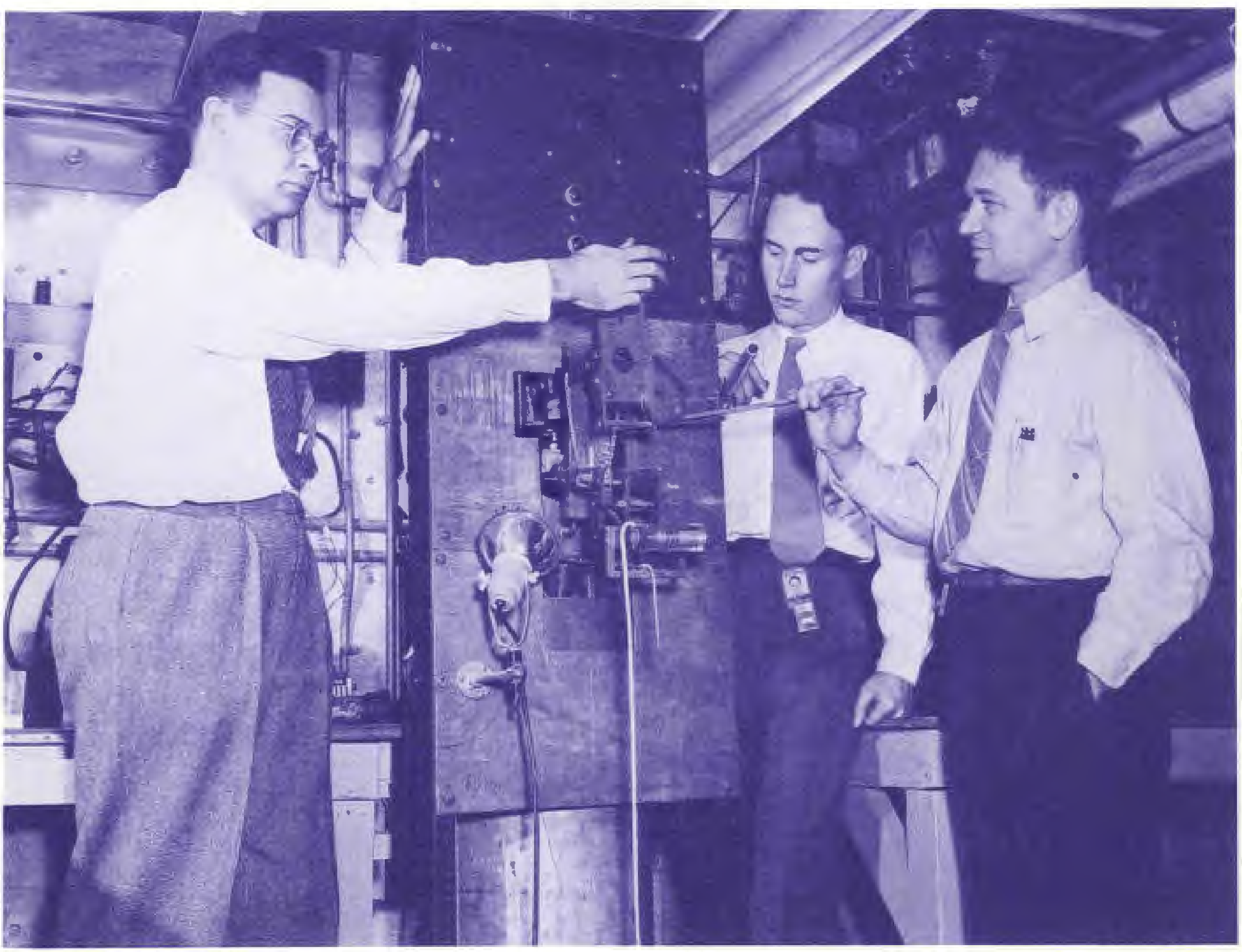
The outstanding event of this period was the first Geneva Conference of 1955. ORNL built, shipped, and reassembled an operable "swimming pool" reactor for the conference on the grounds of the old League of Nations in Geneva. At this conference, nuclear science became international, and the nuclear fraternity expanded to include fellow scientists and engineers in almost all the countries of the world. This was probably the zenith of the nuclear program from the standpoint of interest, zeal, and prestige of the scientific and engineering personnel.

What made the Laboratory different (and, in overly mellowed retrospect, seem better) in those pre-1960 years? Overall size is one important reason—total personnel ranged from about 1500 to 3500. To a great extent, everyone knew everyone else. Management contacts were comparatively direct; work moved swiftly, unencumbered by detailed reporting. Furthermore, the whole world of peaceful uses of atomic energy was largely untried and undeveloped. The great bulk of basic nuclear energy work, as well as the disillusionments and frustrations, still lay ahead.

As important as the work was the Laboratory's esprit de corps—the social fabric was strong and closely knit. At many cocktail parties, dinners, and dances, almost the entire Laboratory personnel developed and maintained a pleasant camaraderie, much of it probably a carry-over of the Project spirit. During this period many distinguished scientists, politicians, journalists, and other personalities visited the Laboratory. ORSORT, the Reactor School, and the ORINS Radioisotope School drew hundreds of scientists, engineers, and medical men to Oak Ridge, adding zest and flavor to the scene.

I hesitate to venture much beyond 1960. About then, changes began in the entire country, as well as in the nuclear program and at ORNL. The Laboratory burgeoned with new buildings, more personnel, and more budget. What happened in ORNL's second 15 years? That is a question for a younger observer to answer.

E. C. Campbell, Wilbur A. Strauser, and Wilfred M. Good, "manipulating an apparatus employed in connection with the Oak Ridge Pile for the purpose of determining the distance fission fragments travel in gases," developed by Good and E. O. Wollan (1949).



Special Tribute

Alvin Martin Weinberg, member of both the National Academies of Engineering and Sciences, was a member of the Manhattan Project and the staff of Oak Ridge National Laboratory from 1942 to 1973; he served as Director of the Laboratory for 18 years. Since then he has served in Washington, D.C., as Director of the Office of Energy Research and Development in the Federal Energy Office, which became the Federal Energy Administration. He is currently director of the Institute for Energy Analysis at Oak Ridge Associated Universities. Of him it has been said, "He has made pioneering contributions to the application of science and technology to the service of mankind. ... His vision and creativity, his manifest dedication to the public interest, and his intellectual integrity have made him a valued consultant and advisor to the highest levels of the national government. His many publications, on the complex relationships between science and government and between science and education, reveal a mind notable not only for its perspicacity and integrating power, but also for its dedication to the enhancement of the cultural and social climate of the world. He represents, at its best, what is meant by the phrase, 'statesman of science.'"

Memories of Oak Ridge

By MILTON BURTON

Milton Burton, physical chemist, was on the faculty of New York University when he was summoned to the Metallurgical Laboratory in Chicago in 1943 to provide his know-how to the Manhattan Project. From Chicago, he designed the technical aspects of Oak Ridge's chemistry laboratory construction, then moved here in 1945 for a year on the staff. His friend Ellison Taylor requested that he record an account of his memories of his contributions and experiences on the Project, an abbreviated version of which is presented here. He is today Professor Emeritus in Chemistry, University of Notre Dame, where he can be reached at the Radiation Laboratory.

I remember being charged with outlining not only the layout of Building 706A, but also with getting up a list of materials and equipment that would be needed when people started moving into that building. There were some very difficult problems; there were some amusing problems; and there were some genuinely funny incidents. The younger men, particularly, were really stimulated by the knowledge of the importance of what they were doing, and they had a tendency to plan and order everything in sight.

L. R. ZUMWALT

An example of such a tendency was given very well by Charles Coryell—as several examples of so many things are given in stories about Charles. One day before my first trip to Oak Ridge I received a call from somebody in the Du Pont headquarters at Wilmington who was really extraordinarily apologetic and fearful that I would misinterpret the intent of his call.

"Now, I don't want you to misunderstand," he started. "If you people really want this, of course you can get it, and we will give it to you. However, we were going over requests that seem to have come from Dr. Coryell, and on looking into his request for platinum foil"—I forget what thickness it was—"he indicated an amount which, if you calculate it, comes out to be about"—and here my memory isn't doing too good—"an area of something like (either several hundred or several thousand) *acres . . .*"

They wanted to know whether that was really intended. Well, you can imagine that I had my doubts, and I went to Charles immediately with the problem. He, in turn, discovered that he had quoted a numerical error. By that time I guess I had become accustomed to requests of that numerical magnitude.

I had an experience, for example, with one of the physicists when we were considering the matter of shielding for the Hanford reactor. I had to have some ideas as to the thickness of the material that I had to prescribe for. Using his data, I concluded that there was something dreadfully wrong. Actually, I thought that there was an error of a factor of 10^6. I called him up and his attitude was one of bland surprise.

L. R. DUVINSKY

"After all," said he, "for a numerical factor, what's so terrible about that?"

Of course, it turned out that it would be a matter of shielding thickness that could be anywhere from paper thin to something like the width of the United States, and that didn't bother him, it's just the way things are.

MILTON BURTON

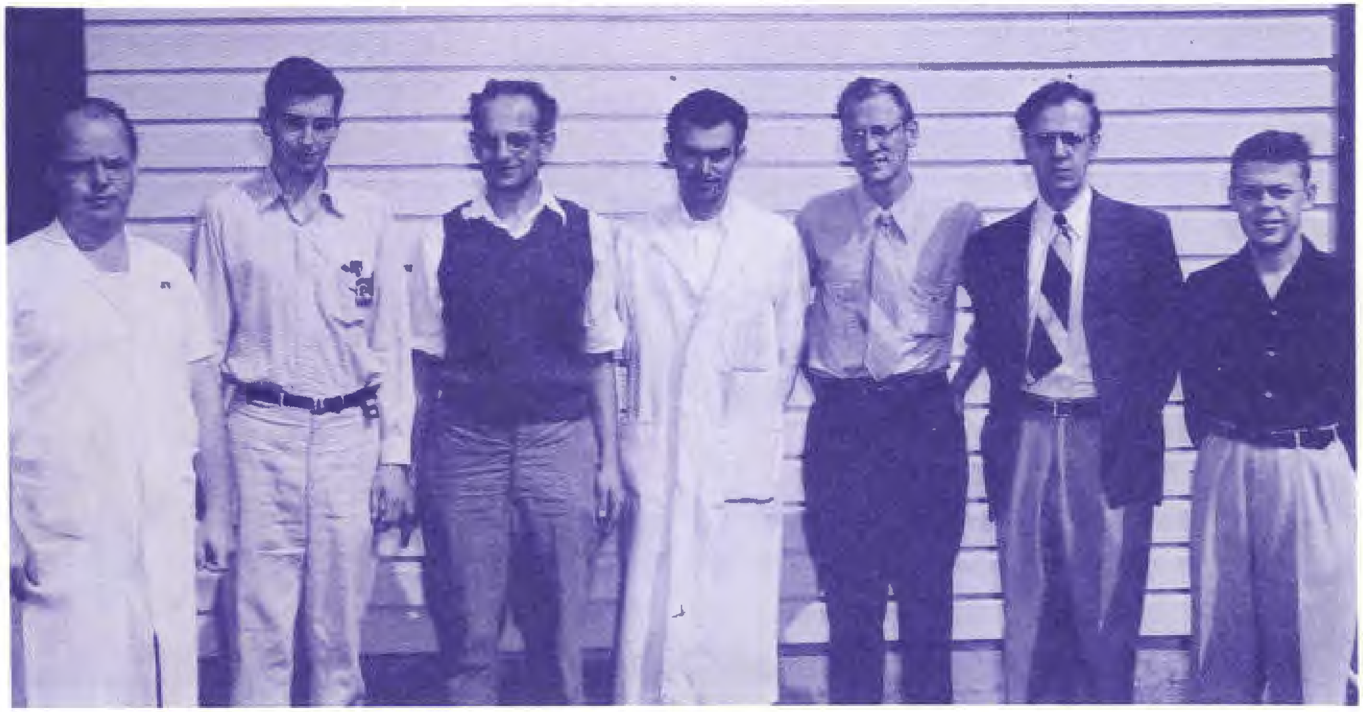
At Oak Ridge I had said I would be largely responsible for the layout there, but the fact is I never saw any of the construction until it was nearly finished in 1943, at which time I went down on July 1 to see what progress had been made. The first thing I discovered was that they had put the water still between the receiving room and the hall that connected with the rest of the building. They put it there, they told me, because it was unoccupied space. I had to tell them that that space was unoccupied because it *had* to be unoccupied and explained why. Of course, the still was ultimately moved to a less objectionable place.

While I was in Oak Ridge, one of the people suggested to me that the microanalytical laboratory seemed to be awfully large. I didn't see anything so unusually large about the microanalytical laboratory. Then I realized that it was the prefix "micro" that threw him. I said, "Look, you have the wrong idea of this microanalytical laboratory. This is for microanalysis, not for micro people. The man who is going to be in charge of the microanalysis is about 6 feet 7 inches tall. You better make this room big enough for him to move around." They got the point.

H. A. LEVY

For the record, I alone did not design 706A. I was responsible for the design. I spent time with all the section chiefs at Chicago working it out, and with many others who weren't section chiefs but who had special needs of their own that had to be answered, and the decisions had to be incorporated in the final plans. My function was to see that all these requirements could be reconciled and that the final plans would bear some sort of resemblance to my idea of a logical design. Considering that 706A was used and expanded long after my immediate interest in it had terminated, I think that we did all right.

I'd like to go back to that first July morning (in 1943) when I actually went down to Oak Ridge. I had arrived the evening before and stayed in Knoxville overnight. I then had to go to some military headquarters where I was put on a bus that went out over the dustiest



This youthful group of ORNL chemists photographed in 1949 include, from left, S. A. Reynolds, R. W. Stoughton, K. A. Kraus, F. A. Nelson, W. H. Davenport, G. E. Boyd, and W. S. Lyon.

roads I had ever been on, in what I recall as a sort of convoy. It was a miserable trip and I had no idea where I was going, but we did eventually get to this God-forsaken place, and after I saw 706A I was taken up on the hill to see the construction of the pile, which to me was a perfectly magnificent thing. It was so far in excess of anything we had had yet in Chicago that I could not help being awed by it. Then I was shown the various devices that were going to be used to take the slugs out of the reactors and to take them through these underground water channels to an area where the chemistry was to be done on them and the traces of plutonium that were to be produced were to be removed and made chemically available. I remember overhearing someone saying, "Damn it, I worked on a lot of these damned explosive plants, but I never worked on one yet where they used concrete for backfill." It was a long time before I realized that this was a very funny remark, largely because I had begun to get deathly sick from the heat, necessitating my being transported someplace where I could be sick at my leisure.

Charles Coryell was always a character, a very interesting one, and I think that any of the young men (now senior citizens) who worked with him at that time will testify that he was a very thrilling experience. When I visited at Oak Ridge I would always drop in to see him, and to my mind the most interesting thing was coming into his office when he was conducting one of his very frequent research conferences. For some reason, he could not behave in the normal way, ever. His way to hold the research conference, a really productive one, was to lie on the floor on his back with his feet in the lower drawer of his desk and the other people sitting around in whatever convenient places they could find. Charles was a character—one of the most wonderful characters there ever was. He got into all kinds of problems just because of being a character. He got into trouble once by mentioning one of the elements—I don't know which one—that ended in "ium." Something about its property struck

R. B. BRIGGS

J. A. KYGER

Jack Kyger Writes . . .

I believe the year was '46–'47 when Eugene Wigner was running the technical operations at the Lab. He dedicated a certain part of every week to a stroll though the laboratories, talking to the workers and observing the activities. These visits were anticipated with a certain sprinkling of butterflies-in-the-stomach because of his searching and often unexpected questions, backed by an encyclopedic inventory of factual and theoretical knowledge, including, it seemed, knowing all handbook information by heart, or being able to deduce it instantly from first principles.

I remember chatting with R. Beecher Briggs following a visit by Wigner to Briggs's lab. Beech said, "I've worked for smart people before, but this is the first time I've worked for someone an order of magnitude smarter than I am."

(I'm pleased to report that I still see Professor Wigner a few times a year, as that fine gentleman consults with us on a variety of topics, still asking his unsettling and insightful questions in his humble and apologetic way.)—Jack A. Kyger, Associate Laboratory Director for Engineering R & D, Argonne National Laboratory

him, and he was in a conversation on the back of a bus and, as was natural with Charles, he was overheard. This time he was overheard by somebody who was connected with security, who promptly reported him. Charles got called down to some office, and it was pointed out to him that he had been overheard talking about secret matters outside the laboratory. He asked where, and when they told him, he pointed out that it wasn't anything secret. He was then asked in the future to try to avoid talking about anything that ends with "ium." Charles thought this was great stuff; his delight in such illogic was one of the things about him that was fun.

What isn't generally known nowadays is that our conversations on the telephone were very frequently monitored, not so much to check our reliability as to check our good sense when we used the telephone. Anything secret was usually discussed in a sort of ad hoc shorthand. Charles, on any given day, could be discovered in conversation with colleagues in places far remote from the Laboratory. Usually he would be called by someone who was irritated or by someone who wanted to tell Charles something that would irritate him; either way, irritation was the center of the conversation. Charles would sit there and he would cuss out whoever was in charge in the most blistering terms, employing such language as would be an excuse for cutting off the use of the telephone. The powers that be were trying to discourage his excessive use of the telephone, so just on a sheer hunch one of these times they monitored and recorded the whole telephone conversation and had themselves a ball playing it back. When they registered their complaint to me, however, I told them, "You know, Charles has a knack of using a very powerful kind of shorthand in his conversation, making up his code as he goes along. It is altogether possible that what you had thought of as a cuss word was code for something classified."

After August 7, of course, we were all terribly concerned about the impact of what we had done. Guilt sensations began to take over; people were afraid of extended use of an atomic bomb; people were afraid that some terrible things were going to happen. There was outspoken fear that the Russians would get hold of "these secrets" and take advantage of the opportunity to take over the world. I don't think anybody in his right mind could imagine how ridiculous some of the things could be which were said or thought of then. I remember one of these council meetings in Warren Johnson's house, where we were all terribly upset because the order had been given formally to MacArthur—and he had tried to resist it, if my recollection is correct—to take the Japanese cyclotron (on the pattern of which, incidentally, the Michigan cyclotron had been built) out to sea and toss it overboard. Now you know that taking a cyclotron out to sea isn't a small job. You have to break the thing up first, and that will take a job of doing, and furthermore, why? I suppose some idiot had the idea that this was in some mysterious way to prevent the Japanese from doing some awful things. MacArthur did resist this command, but he promptly got the word that he had to obey it, and I imagine that it involved a considerable cutting up job before he could even remove the cyclotron from the laboratory, and they took it out to sea and sank it. The newspapers covered this, but I don't think the newspapers realized the significance of what had been done. What we did on this evening in Warren Johnson's house at Oak Ridge was to talk about it, and sort of by autocatalysis we finally worked ourselves into a pretty angry mood, and we arrived at a decision to make a statement to the newspapers. Well, I was the only one who really had access to a newspaper, and I got in touch with this fellow at the *New York Tribune*, but before I told him that we were all upset about this, I think he asked us

J. S. KIRBY SMITH

P. R. Bell checks the scintillation spectrometer he developed. In March 1949, this development was lauded as an "advancement of great significance in the field of radioactivity detection."

whether we would make a statement, and we said, "Yes, we were going to make a statement." He said we should call back collect when we were ready, and each one of us wrote his own statement (there must have been a half dozen of us). We were all from universities (at that time I was associated with New York University), and we all made statements and cited our university connections. We then called up White and gave him the statements, and the statements appeared in a box in the upper right-hand corner of the editorial page of the *Tribune*. Anyway, it may have done some good; it may have served to assure people that a country could get too nonsensical in its fears. I remember Warren Johnson said in his statement that the least the United States could now do was to repay or rebuild a new cyclotron for the Japanese and give it to them. We weren't thinking of reparations at all; I think what we were trying to say is that the wrong people were being punished—that it was not the Japanese scientists but scientists generally who were being punished, and for an incident to which they were not related. Maybe we began to get the message of scientists across. Not that we ever really got the message of the scientist across, even in the present day, but at least we began to let people know that scientists did have something they wanted to say and that we could become indignant as we did become at that time.

M. M. VIAR

If my recollection is correct, I left Oak Ridge permanently in about March of 1946. Prior to leaving, however, I was asked to say a few words to a meeting of the entire scientific group at Oak Ridge. There was a great fear at that time of an exodus of the young scientists from Oak Ridge. The older scientists were bound to leave; some of them had already indicated an interest in Brookhaven, which was coming into existence. Most of them were going back to their old universities. All that would be left, essentially, would be a cadre of younger people, some brilliant younger people, most of whom wanted to be with these older people who had performed such remarkable feats during the war years. They complained about a number of things. They complained that the older people were leaving; they complained that Oak Ridge was a hell of

R. D. PRESENT

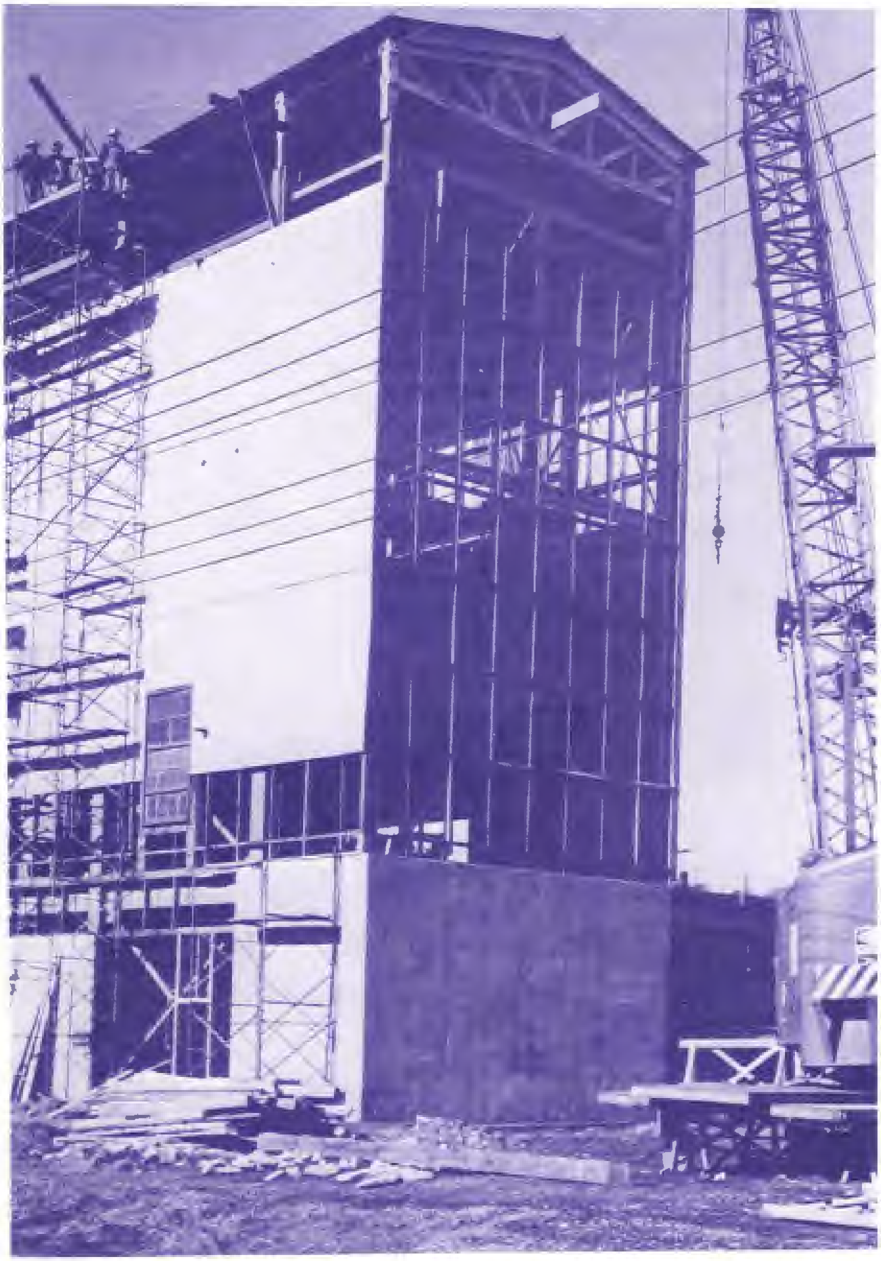
Note from Gale Young . . .

Since Clinton Engineer Works (the original name for the Oak Ridge operations) was managed by the Army during and for some time after the war, there were always troops about. After the end of the war, there was considerable changing of personnel as troops from overseas were rotated through home-base "decompression chambers" like Oak Ridge.

Thus it was that we met a certain tough young captain of artillery with a considerable record of combat service. It was apparent that Oak Ridge was not his idea of a weekend in Paris. Upon drawing his first pass, he decided to go into Knoxville to look the place over. We anxiously awaited his reaction after his return. It was not long in coming. "Hell," he said, "We liberated better places than that."—Gale Young, recently retired from Director's Division, ORNL.

a place to live in. There was nothing scenically beautiful about Oak Ridge. It was a place to endure when you had work to do; it was a place for your family to endure when they knew you were engaged in some noble enterprise. But the war was over now, and many of these young people were brilliant, and they wanted to get back to the academic environment where they felt they were doing something useful and where they felt they could bring up their families in decency. Charlie Thomas felt that I should give a talk and tell them why I was going back to university life and why I thought *they* should stay. Well, I gave the talk, and fortunately some darned good people stayed. I don't have to repeat their names; you have a record of that. Whether I had any influence in their staying down there, I don't know, but if I did it was probably one of the best things I ever did in my life.

N. KROPOFF



This 40-ft, two-story building, which originally housed wartime boilers, became the long-awaited home of ORNL's Van de Graaff generator in 1949.

A Visit from St. Nucleus

By H. W. NEWSON

Henry Newson joined the Metallurgical Laboratory when it began at the University of Chicago, which is where he had received his doctorate in physics. He came to Oak Ridge for a year to start up the pile, as described here; went to Hanford for a year; spent a year at Los Alamos; and then returned to Oak Ridge in 1946, where he stayed until 1948, when he followed Nordheim to the Physics Department at Duke University. He is there now, officiating over the Triangle Universities Nuclear Laboratory.

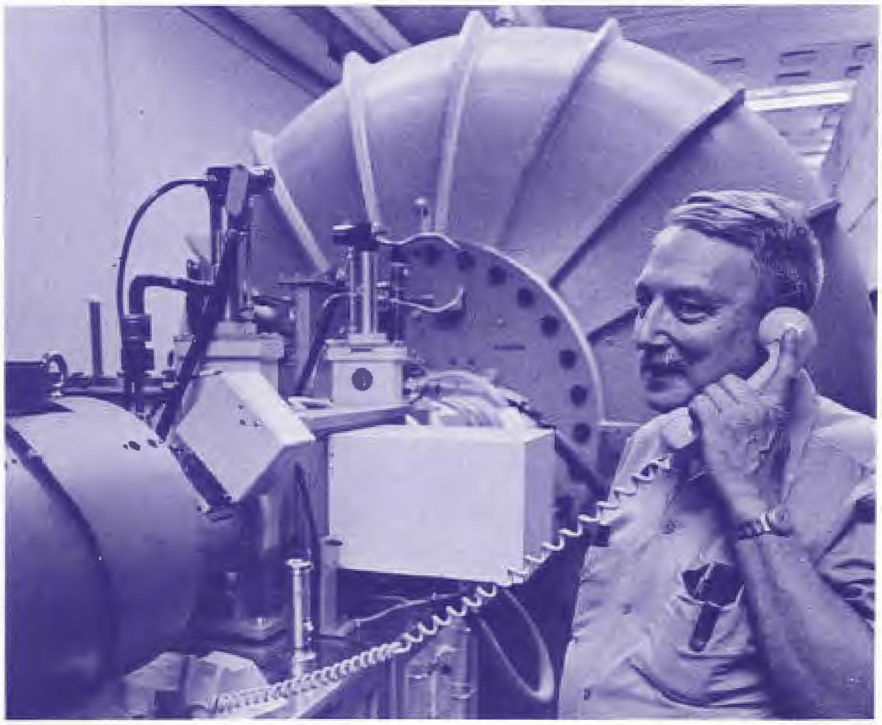
'Twas the night before critical and a dozen or so creatures (in addition to numerous field mice) were stirring about the Graphite Pile. The monitoring procedures necessary to make sure that we did not accidentally overload had started about noon with the loading itself. The safety rods and the detecting instruments, which would scram the pile in case of an unexpected critical, were of course activated, but they were not sensitive enough to give significant readings yet. Unlike the safety rods, the horizontal control rods were inserted until each scheduled batch of uranium had been loaded; then they were withdrawn and indium foils were inserted at the center of the pile, withdrawn after a few minutes, and taken to the counting room. After a few minutes and a short calculation, the count was compared with the amount of uranium metal in the pile. During all these operations a boron counter was also registering in the counting room. This counter was not accurate enough to give us quantitative readings, but it would give a warning if critical were approached much sooner than expected. Thus while watching the instruments and keeping the automatic safety systems activated, we loaded a batch of uranium, pulled out the rods, measured with the indium foils, reinserted the rods, added another batch of uranium, etc. Even the most accurate measurements (those with the indium foils) had little meaning until several tons of uranium had been added in such a way that the part of the graphite cube containing the uranium was close to a cylindrical shape.

H. W. NEWSON

However, measurements were made for what they were worth, even though our early points, while indicating roughly that things were proceeding as expected, had little significance. I remember showing an early plot to Martin Whitaker in the cafeteria at about dinner time and remarking jocularly that the pile was jumping up and down. This turned out to be a very indiscreet remark, since it was overheard by one of our managerial friends who might have taken alarm and assumed that the

K. Z. Morgan Writes . . .

It is of interest to recall that in 1943-44 some of the engineers at Clinton Labs objected to my extreme conservatism. I was using 0.1 roentgen/day as the exposure level for White Oak Lake. They insisted I should use 100 roentgens/day, because persons certainly would never drink or swim in this water. It is little comfort to me today that I held my ground and we used 0.1 roentgen/day, since my level was 7000 times the present ERDA-ALARA level of 5 millirems/year.—K. Z. Morgan, former director of Health Physics Division, retired from ORNL in 1972, currently a professor in the Nuclear Engineering Dept. at Georgia Tech.



H. W. NEWSON

"jumping" was unexpected and ominous. Whitaker declared hastily that these early data had "no earthly significance." Shortly after this incident, we did achieve a sufficiently regular shape of the loaded region so that we could start taking our measurements seriously.

Our routine now went smoothly for the next 8-hr shift; the count taken on the indium foils increased gradually as each batch was loaded. At first this increase was largely due to the fact that each batch of uranium contributed additional neutrons to those already bouncing around inside the graphite cube, but as the loading increased, the effect of the chain reaction became dominant. We could see it by observing the boron counter after each batch was loaded and the horizontal rods were withdrawn completely. The counts continued to increase for several minutes before reaching the constant counting rate which showed that the chain reaction was going on but had not reached critical. At this point the indium foils were inserted, since boron counters in those days were not accurate enough for our purpose; after each foil was counted, we could determine whether the loading of the next scheduled batch could be completed *without reaching critical*.

M. D. PETERSON

This procedure was designed to be tantalizing. We actually plotted the reciprocal of the (saturated) activity of the indium foils against a function of the amount of uranium and extrapolated from the last two points to zero reciprocal counting rate, which meant criticality. The function of the amount of uranium was chosen so that, while the plot could show that the next batch would not take us to critical, we could not tell how much more would be needed.

As midnight approached, the shift changed and the curiosity seekers went home. That left only those of us who were scheduled to stay for the graveyard shift. This was not meant to be an honor, since according to calculations originating at the University of Chicago's Met Lab, it should take all night to load just up to critical. Enrico Fermi was

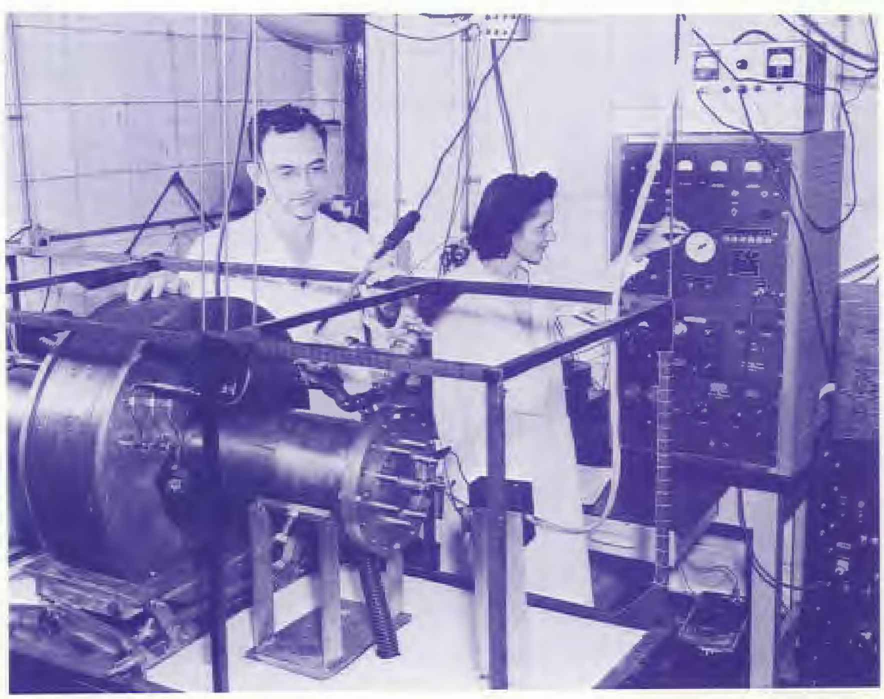
E. FERMI

scheduled to arrive from Chicago in the morning and preside over the finale.

Shortly after midnight we saw that, while the next batch would not bring criticality, the one after that *might*. Since we turned into the home stretch much sooner than expected, the operating group occupied the loading elevator and handled the uranium, which had been segregated into batches in advance. This group was composed entirely of Du Pont senior engineers under the supervision of Kent Wyatt. The counting room was manned by Haydn Jones, George Packer, and several junior Du Ponters. George Weil (the only Chicago representative present) and I stayed in the plotting room. We were authorized to start or stop the loading as necessary to bring the pile *just to critical*. In the unlikely event that it should happen before morning, we were to notify Arthur Compton, Whitaker, and a considerable number of others by messenger and wait for further instructions until they arrived at the pile. At that time there was no telephone communication between Oak Ridge houses and "Clinton Labs."

As we proceeded, the operating group became more and more proficient with the long poles they used to push the uranium slugs into the pile. Batch after batch was loaded, as each extrapolation indicated that *two* more batches would be enough. It began to look as though the predicted critical mass was correct after all, in which case this would go on all night! But finally a point was plotted which predicted that the next batch would be too much! Unfortunately, we had not told the operators to stop loading during the counting and plotting, when we thought they could only add a negligible amount. I hurried out and gave the word to stop. The operators complied grudgingly. They knew that critical was not expected for hours and thought that I had panicked and stopped the loading prematurely.

I was at least spared this embarrassment. When the operators finished loading the hole they had started, plugged it up, and removed



Investigating properties of rare earths with a beta-ray spectrometer are Bruce Ketelle and Lucille Petty (1949).

the control rods, the boron counter flashed faster and faster. That last hole was one too many! We were over critical by 3 "inhours." I had exceeded my instructions, but by a small amount compared with that needed to operate the pile under design conditions. The next day Fermi ordered enough uranium to be added to bring the activity up to more than *1000 inhours*.

1944

By January 30, the radiochemical pilot plant (later called the ORNL Pilot Plant) was processing a third of a ton per day of irradiated uranium from the adjacent Graphite Reactor by the bismuth phosphate process conceived at the Metallurgical Laboratory in Chicago and tested by chemists at Clinton Laboratories. By March 1, several grams of plutonium had been delivered for research at Los Alamos and elsewhere. Physicists and chemists at Clinton Labs studied ways of averting the reactor poisoning problem of xenon-135, which caused a temporary shutdown in the first Hanford reactor (xenon-135 is a fission product that absorbs neutrons, thus inhibiting the chain reaction). Several years later, Seymour Bernstein measured the xenon-135 cross section—a tour de force in physics. In 1944, scientists measured the heat output from irradiated uranium slugs removed from the reactor and developed methods for detecting slug-jacket failures. The Physics Division, headed by Lothar W. Nordheim, determined the Graphite Reactor's operating characteristics, temperature stability, control rod behavior, and neutron distribution throughout the pile. On April 1, the Physics Division submitted a report to Los Alamos Scientific Laboratory on the fast-neutron fission-to-capture ratios for uranium-235 and uranium-238, which showed that a large mass of natural uranium would have to be enriched by a factor of about 12 in uranium-235 to support a fast-neutron chain reaction.

Besides studying the bismuth phosphate process for isolating plutonium, Warren Johnson's Chemistry Division separated some radioisotopes from fission products (in this way several hundred curies of barium-140 were obtained for Los Alamos) and prepared other radioisotopes by neutron bombardment of substances placed in the reactor (thus obtaining the first weighable quantities of tritium and phosphorus-32 for biological studies). The chemists, guided by Charles Coryell, identified numerous radioactive fission products and characterized them as to amounts produced, chemical properties, nature of radiation, and rates of decay.

The Technical Division, headed by Miles C. Leverett, consisted of chemical engineers, mechanical engineers, and metallurgists. It examined the adequacy of shielding materials to be used in constructing the Hanford reactors (started during June 1944), worked on separation of plutonium from fission products, studied the effects of intense radiation on the rate of corrosion of the aluminum cladding and cooling-water tubes in the Hanford reactors (first operated in September 1944), and looked at problems associated with failed fuel slugs, such as radioactivity in cooling water.

The Medical Division, headed by J. E. Wirth, included health physics and biology sections. The health physics section monitored personnel, analyzed air and water samples for radioactive substances, devised instruments to measure radioactivity, and studied the biological effects of high doses of ionizing radiation on thousands of research animals. In 1946 the health physics section became the Health Physics Division, under K. Z. Morgan. The biology section, which became the Biology Division in 1946 under Alexander Hollaender, developed techniques for exposing animals to radioactivity and supplied the Metallurgical Laboratory in Chicago with pure fission products for its studies of ingested radionuclides.

By July 1944, the Graphite Reactor could operate at 4 MW (four times its original power level), owing to improved uranium distribution, better aluminum jackets for the uranium slugs, and larger fans for air cooling. By the end of the year, chemical separation of plutonium ceased, and the reactor was then operated primarily for production of radioisotopes and for physical research.

A True Fission Story

By J. X. KHYM

Joe Khym came to Oak Ridge in 1944, working first in radiochemical processing, and later in biochemical research.

Long before the word "environmentalist" made its way out of the dictionary, Clinton Laboratories, way ahead of the times, had two environmentalists on the job—George W. Parker and me.

Early in the 1940s, the higher-ups were concerned about radioactive wastes—particularly the fission products—ending up in White Oak Creek. The higher-ups deemed that White Oak Creek—at the point where it widens into a small lake or pool (near the bridge crossing toward the now Melton Hill exit)—should be checked for radioactive fish. So George and I were chosen to carry out this task. We were immediately given the title of Environmentalist-AAA. (We didn't even have to fill out any forms, be evaluated, or appraised; nor were we asked, "Do you know how to catch a fish?" You see, in those days a fellow could move up quite rapidly.)

A. D. CONGER

Feeling good about our new titles, George and I set out to catch a fish at White Oak Lake. At first, the endeavor was simply a "cane-pole operation," but we landed a "biggy"—I recall it was a largemouth carp. The "Big One" was hauled back to the lab to be checked for radioactive fission products. We did this by carbonizing some of the fish's viscera by putting the selected tissues and organs in porcelain counting planchets and heating them in an oven. When subsequently placed in the old lead counting chamber (a "pig"), the fish's baked innards jammed the

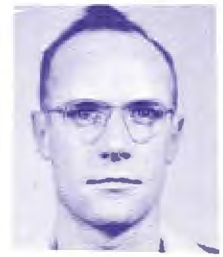
Alan Conger Remembers . . .

Old-timers in the Biology Division may remember the overhang roof facing the highway outside my second floor lab in 9207—our lunch patio where we dined on balmy days under the morning glories. At the height of the UFO scares (ca. 1951?), coming across some balloons and a helium tank left over from our first lab Open House, I filled six or so balloons with helium, tied them together with string, and attached a 6-ft strip of aluminum foil beneath as a radar target. With Kim Atwood's help, I got it out of the lab and launched from our roof patio, admiring its stately ascent as it drifted down Bear Creek Valley, rapidly transforming from a recognizable bundle of balloons and foil into an unidentifiable flying object. We then ran down the hall, calling out to Jack Von Borstel, Bill Arnold, Shelly Wolff, and others, "See the UFO!"

It caused great excitement and much speculation about what it was, its size, velocity, and height; and soon, even more excitement when it was detected by the nearby radar station on Pilot Mountain, and the fighter-interceptor squadron then stationed at Knoxville was scrambled to intercept the intruder. With planes buzzing around, and our scientist friends seriously considering the object, the situation had rapidly become so very imposing that neither Kim nor I had the guts to confess to our hoax. We kept quiet and hoped the Air Force or AEC would be unable to identify us.

A few years ago, my son, reading a book on UFOs, came across this incident as one of the case histories of UFO sightings from Air Force records. He recognized it as a hoax, and surmised that some unknown Oak Ridge scientists probably perpetrated it.—Alan D. Conger, Professor of Radiobiology, School of Medicine, Temple University

Geiger-Müller counter. When this information was relayed to the higher-ups they became, to use an early East Tennessee expression, "dithered." One of them even said, "Holy hot mackerel!" (and here it was a largemouth carp all the time). After some composure had returned and they had had several high-level secret conferences, they announced their game (and fish?) plan: "Send Khym and Parker back to the lake and have them check this thing out thoroughly." Only this time they ordered, "No cane-pole operation; put in a rush order to get Khym and Parker fish traps, fish nets, good strong poles, and a boat and motor, and anything else they need."



J. X. KHYM

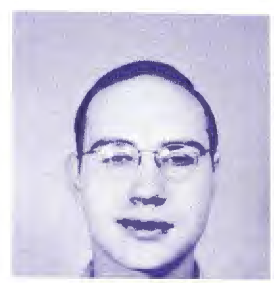
While waiting for the heavy equipment to arrive, George and I often went back to the lake to catch a few (they were always hot!) just to keep in shape. Also this was the time for reconnaissance and for laying out logistic plans for the big siege (those poor fish in the lake were as good as baked).

By this time, we had an Army chauffeur assigned to take us to the lake at about 8:30 in the morning and to pick us up at about 4:10 in the afternoon. The chauffeur even brought us hot (thermal, that is) lunches about noontime. There was the lake, and the living was easy! After we had made several such reconnoitering expeditions, bad weather forced us environmentalists indoors to engage in desk work.

At one such indoors skull session, we happened to see Waldo Cohn place a beaker containing solid yellow stuff in the oven in which the fish parts were baked. We asked Cohn what he was doing. He said he was making radioactive phosphoric acid. "Simple," said Waldo. "You put sulfur in the pile and through an (n,p) reaction you get phosphorus-32, and then by melting the sulfur in an oven and pouring it into boiling, fuming nitric acid, and subsequently cooling the preparation, you have phosphoric acid in the nitric acid, and the untreated sulfur as a block at the bottom."

Well, you guessed it, gang. During the melting process the sulfur preparation gurgles, spits, splatters, bumps, stews, sprays, and besmears; and not only that, it contaminates! Following Cohn's radioactive manipulations, the oven was found to be hotter than John Wayne's pistol; so were blank planchets following oven treatment.

Here, then, started a small cover-up. Our heavy equipment had arrived just when we weren't quite sure whether the fish in the White Oak pond were radioactive in their own right or had gotten that way upon analysis. The lid was kept tight about the oven incident. Besides, the boat and motor were there, all that equipment was there, the lake was there, and a chauffeur and a car were available. Why bust up a dream assignment until things were "checked thoroughly" as directed?



B. I. SPINRAD

With the new equipment we brought back a "ton of fish." To our disappointment the innards of these fish all proved to be way below background.

When this information was relayed to the higher-ups, at first there was jubilation, backslapping, and expressions such as "Whew! That was a close one!" or, "I thought there was something fishy!" All this gaiety stopped when suddenly the higher-ups thought of Khym and Parker.

Very soon a new directive went out. In summation it said, "Bench those two guys, take away their Environmentalist-AAA titles, all their equipment—the chauffeur included. Keep them in the lab and do not ever again allow them to go outside and mess around in the environs."

This is a true fission story.

A "Cover-up" Story

By W. E. COHN

W. E. COHN

Waldo Cohn was producing radioisotopic materials of biological interest after he joined the Manhattan Project in 1943, but later used his skills to study nucleic acids. Although he retired in 1975, he is still active on the National and International Commissions on Biochemical Nomenclature.

Anyone reading Khym's "fission" story may wonder what I was doing making phosphorus-32 in a reactor that was, in 1944, dedicated to exploring reactor technology and plutonium production and recovery. After all, I was charged, along with E. R. Tompkins, Khym, George Parker, and others with the job of exploring the radiotoxicity of the fission products, which were at that time considered nothing but nuisance waste products to be separated from the plutonium and uranium in which they were borne, and to be buried before any human being could inhale or ingest them.

But I had recalled my graduate student days at Berkeley, when I had waited, sometimes for months, for time on the cyclotron (the old 37-inch model) to make phosphorus-32 by bombarding red phosphorus with deuterons, yielding a few microcuries of phosphorus-32 in a large mass of phosphorus-31—hardly high-specific-activity material. (To make matters worse, sometimes the bombarded red phosphorus would burst into flames on being removed from the cyclotron's vacuum and be doused in the nearest vessel of water—at least once an old paint bucket filled with dirty water—which presented a nice recovery problem.)

M. B. HAWKINS

Recalling this, and being aware of the possibility of making truly carrier-free phosphorus-32 by the (n,p) reaction on sulfur in the reactor, I began to "bootlet" blocks of sulfur into the experimental holes of the reactor. After exposure for a couple of half-lives, about a month, these blocks would be withdrawn, allowed a brief cooling-off period for undesired short-lived nuisance activities to disappear, and processed.

But how does one remove, without carrier, an infinitesimal mass of phosphorus from a large (kilogram) mass of sulfur? Chemical attack on the sulfur would not simplify the task; the disparity in masses would

Betsy Ross . . .

Probably few of his friends know of his claim to immortality, but Myron B. Hawkins is the Betsy Ross of the Nuclear Age. Assigned to the Chemistry Division in 1946–48 as design engineer for the Research Facilities Department, Hawkins submitted the finally accepted design for the universally used radiation symbol: the three chartreuse blades on a magenta field. (The colors were not his; they were selected by E. E. Hawk—at the time a draftsman in the Physics Division.) Mike Hawkins left Oak Ridge in 1949 for AEC-Washington, moved later to the West Coast, where he is today, operating his own market information service, Hawkins/Mark-Tell. He modestly disclaims full authorship of the symbol, recalling dimly that it was modified from one designed originally by Nelson Garden at Berkeley. This, however, seems to be much the way Betsy Ross operated also, in her day. (Hawk, too, has moved on—she is now married to Herbert Pomerance.)—B. K. Lyon remain. Noting that sulfur melts at 120° and that fuming nitric acid boils at the same temperature—and would oxidize the phosphorus to phosphoric acid without attacking the elemental sulfur—I arrived at a simple solution: melt the sulfur, pour it into boiling, fuming nitric acid, cool the biphasic mixture (molten sulfur at the bottom, nitric acid above) to solidify the sulfur, and evaporate the nitric acid to leave the $H_3{}^{32}PO_4$. (Actually, a small amount of phosphorus-31 was necessary to inhibit loss of phosphorus-32 by reaction with the glass vessel.) Result: hundred-millicurie amounts of high-specific-activity phosphorus-32, dwarfing the cyclotron route. (If I had only had this seven years earlier!)

E. E. HAWK

What to do with the product? It couldn't in 1944 be distributed to researchers on the outside, because security wouldn't allow that. It couldn't be used on the inside, because that was outside the mission. But then a happy thought struck those who had fuller knowledge of the details of the plutonium project. The reactor was not yet producing enough plutonium for the needs of those chemists and physicists whose mission was to determine its properties (the long irradiation, cooling, and processing periods added up to a very long interval between start-up and final recovery). It was known that the cyclotron at Berkeley would make small amounts of plutonium: Alvarez, McMillan, and Seaborg had developed that route, and Seaborg was in the project and was leading the group that had the greatest need for plutonium. But the Berkeley cyclotron was heavily involved in supplying the country's medical needs for phosphorus-32; to cut that off would not only be a disaster for said needs, but might breach security by going over to uranium bombardment.

M. D. WHITAKER

It was probably Seaborg who saw the solution to this impasse: send Cohn's phosphorus-32 to Berkeley, he said, whence it could be released as if it were Berkeley's usual product, and let the Berkeley cyclotron produce plutonium for Oak Ridge.

And so began the double cover-up of bootlegged phosphorus-32 from Oak Ridge being swapped for plutonium from Berkeley, with, we hoped, no one on the outside being aware of the true source of the materials—at least until August 1945.

More from K. Z. . . .

The first director of Clinton Laboratories (now ORNL) was the late Dr. Martin D. Whitaker. Much to the dismay of the health physicists, the radiation protection rules in 1944 did not always apply to top management. One day Martin had some very distinguished visitors from Washington whom he took to see the X-10 Graphite Pile. There were signs on the west side of the Pile, "No Admittance," but these (so Martin thought) did not apply to him. He took his visitors past the signs and showed them experiments under way on the back face of the Pile. It was assumed by Martin and his visitors that they did not have to wear their film badges, so when the health physicists heard about this tour of their Director and his visitors, they could only estimate the dose at between 5 and 50 roentgens (rems were not yet invented). It seems that the water in the tank in the large hole on the back side of the Pile had been temporarily drained, with the result that no shielding was provided from the fuel slugs on the back face of the Pile. Perhaps it was a blessing in disguise, however, because after this incident health physics regulations became law.—K. Z. Morgan

W. C. JOHNSON

August 1943 to December 1945

By WARREN JOHNSON

Warren Johnson, now living in Grand Rapids and holding the emeritus professorship in chemistry and the emeritus vice-presidency of the University of Chicago, was the Laboratory's first Chemistry Division Director. He had been a member of the U of C faculty since 1927, when he interrupted his academic career to come to Oak Ridge for three years. His interest in the Laboratory and in education was evinced in 1953, when he became a member of the ORINS board of trustees, serving nearly continuously until 1968, and showing an active interest in the Institute thereafter until 1973.

My first trip to Oak Ridge occurred August 18–19, 1943. I had joined the staff of the Metallurgical Laboratory on the previous July 1 and was asked to participate as a member of a committee to review Y-12 chemistry. As I recall, W. K. Lewis and Charlie A. Kraus were the other two members of the committee, and Col. Ruhoff of the Manhattan Engineer District served as secretary. After the meeting terminated on the second day, Col. Ruhoff took me to X-10 to see the Pile under construction. From the top of the structure we took a somewhat makeshift elevator to the bottom of the Pile. That day two layers of graphite, the first two, were laid in place. Little did I realize that the pile would be in operation in a few months and that one gram of plutonium would be produced by January 1, 1944. Also, little did I realize that five weeks later, September 24, I would be a member of the Chemistry Division of Clinton Laboratories, to stay for a period of three months or so. As it turned out, my stay was about 27 months, until December 1, 1945. Those months were the most interesting ones of my life; it was a privilege rather than a wartime duty to be in Oak Ridge at that time.

During the first four months in Oak Ridge, I lived in a dormitory, M-2. In January 1944 a house became available at 102 Olney Lane. We moved the family from Chicago with a minimum of belongings; most of the furniture was rented from the Army. Our apartment in Chicago was rented to Farrington Daniels and his family. Living in Oak Ridge in the early days was a little rugged, with lots of mud, but since no one minded it much, it was sort of fun. Everyone had to cope with the same problems.

At the Laboratory, the Chemistry Division was housed in Building 706-A. It was located about one-fourth mile from the main gate, which provided entrance for everyone in the X-10 area. There were no walks, and the roads were deep in mud, especially in wet weather. I recall that walking to 706-A to begin the day's work, the girls often removed shoes and stockings (nylons at a premium) to preserve them; once their stockings were covered with mud, it was almost impossible to restore them to a usable condition. The irony of this walk was that the safety engineer, "Safety Smith," had posted a sign at the side of the road near the tank farm area that read, "Walk—Do Not Run." As if anyone could run in that sea!

C. V. CANNON

Shortly after arriving in Oak Ridge, I was assigned a government car, which I appreciated greatly. The car had to be parked outside of the West Gate at X-10. One day I received a phone call from Charlie West, who was in charge of the janitorial staffs and was a lifetime resident of the area. He informed me that a couple of Army personnel from "The Castle" (Manhattan Engineer District headquarters in Oak Ridge) were at the front gate to give me a driver's test for the operation of the government car. It was raining hard, windy and muddy, and I was in no mood to walk a quarter of a mile in that weather if I could possibly avoid it. So I said I would take the test if they would come to 706-A to administer it, but I would not go up to the gate under the circumstances.

From Howie Adler . . .

In the late 50s, the Biology Division entered a period of extremely rapid growth, and the opportunity for all the Division members to know each other reached a new low. One incident recalls to me the difficulties we were having in recognizing each other. Dr. Hollaender, then director of the Division, came to my lab one day and asked how work was progressing in the renovation of a laboratory which could only be reached by passing through mine. I offered to take him back to see the work in progress. In the other room we observed that much of the old furniture was in the process of being torn out. Still, a lone scientist stood amidst the clutter, carrying out his experiments. He was holding a frog in one hand with its legs extended, intent upon injecting some solution into a muscle—a classical biologist's pose. Dr. Hollaender made some comments about the room and then asked the gentleman what he was doing, and a short scientific discussion was held. Hollaender and I then left the room, and as we reached the corridor, he said to me, "Who was that guy?" I informed him that he was a scientific assignee who had been in the division about ten months and who would shortly be leaving. Hollaender walked off, saying, "I never saw him before in my life."

I went back to my lab and thought I could have some fun with the visiting scientist by pointing out how little he had impressed the Director in the months he had been in the Division. I returned to the room being renovated, where he was still working with his frogs. Before I could speak, he turned to me and said, "Who was that fellow with the German accent and the moustache?"

I later arranged for the two to meet each other formally.—H. I. Adler

A few minutes later Charlie West called and said that they would give me the test over the phone.

What is your name? I answered correctly.

Where do you live? Answer: 102 Olney Lane.

How old are you? Answer: 42.

West: Very good, you have answered all questions correctly and have passed the test. Your license will be coming along in a few days. (I am sure that the boys from the "Castle" were no more anxious than I was to venture out in that weather.)

M. A. BREDIG

The government car was valuable in many ways. Aside from the fact that I had some difficulty in walking, it also provided me with the opportunity to leave for work in the morning at a more appropriate time. I disliked trying to make the 7:15 bus that would get you to the gate at 7:40 at the latest, in time to march in to the tune of marching music, which was supposed to get you all pepped up for the day's work. I would leave home about 8 AM in the government car, about 45 minutes later than the bus, and would stop at the Guest House to pick up those who had missed the bus; the next bus would not be around until about 11 AM, and no one wanted to be seen coming in the front gate shortly before noon. My transportation was known as "The Guest House Special," and it arrived at the Laboratory only about a half hour late, in plenty of time to join those who were in the process of becoming settled for the day's work.

On the first day, when I visited Oak Ridge and went to X-10 to see the Pile under construction, Col. Ruhoff told me that after the two tall stacks and auxiliary equipment had been constructed and were in place, one of the engineers said that this was the craziest power plant he had ever seen, declaring, "There is no possible way that any smoke is going to get into those stacks."

O. SISMAN

In addition to working on the bismuth phosphate process, as well as some alternate processes, and the chemistry of the fission products and plutonium itself, the Chemistry Division was assigned the task of producing radioactive lanthanum (RaLa) in a very pure form to be shipped to Los Alamos. Its use was not revealed, but everyone knew what it was to be used for. Charles Coryell was in charge of the program. A new laboratory (706C) was built, and the process was designed to be carried out in all-glass equipment. Radioactive barium, separated from the fission products, was the source of RaLa. The first sample had an activity of about 100 curies; the second was a larger sample, and the third approached 300 curies. Fortunately, the equipment worked well and the glass held up, though at times there were great uncertainties as to its worthiness. After the third lot, a new laboratory was built by the engineers using metal equipment. However, Charles, having a highly sophisticated scientific mind, was anxious to obtain some valuable and interesting data from the 300-curie lot. He got permission, I have forgotten how, to have the 300-curie sample, contained in a shield, hoisted to the top of a high pole near 706A. There the shield was removed temporarily and Charles measured the radiation distribution,

Special Tribute

"In paying tribute to Alex Hollaender's 20 years at ORNL one can point to the many specific discoveries like messenger RNA or screwworm eradication or thymine dimer repair enzymes, all of which were either discovered at ORNL or whose investigation was encouraged by Hollaender during his tenure here; or the many young investigators who started their research careers at Oak Ridge and with Hollaender's encouragement became mature scientists; or the extraordinary influence he has had in using radiation biology as an instrument of international goodwill. But I believe Alex Hollaender's impact on biological research goes beyond even these. It was he who invented this new style of biological investigation: the melding of enormous, expensive mammalian experiments with basic investigations on a much smaller scale in which the principles underlying the mammalian experiments could be demonstrated and tested in the most delicate and far-reaching way. It is this unique combination of the big and the small, the mission-oriented and the discipline-oriented that is Alex Hollaender's great contribution to biomedical science. It is a contribution that has forever changed biology."—A. M. Weinberg, 1966 "State of the Laboratory" address, referring to Alex Hollaender's retirement.

including the bremsstrahlung, at several angles. As soon as the shield was removed, the radiation alarms sounded at the front gate, a quarter of a mile away. It was a good radioactive sample.

One day the head of the Manhattan Engineer District, General L. R. Groves, was scheduled to visit the Laboratory. At the time, I happened to be in the office of the Director, M. D. Whitaker, in the administration building overlooking the front gate. The General's car and entourage came to the gate and went through. After passing the gate by about 50 ft, General Groves got out of his car and walked back to the gate to give the GI guard hell for not asking for his badge. What GI would have ever asked for the General's badge?

W. G. POLLARD

In mid-September 1945, about a month following the end of the war, a conference, quite informal in character, was scheduled at the University of Tennessee. Director Whitaker, Research Director Richard L. Doan, and I attended as representatives of Clinton Laboratories. Dean Smith (which one I do not recall) and two or three others from the University were in attendance. The discussions lasted for two or three hours. The University of Tennessee was interested in becoming a partner in the development of relations between the Laboratory and the University in scientific and technical programs. Our advice was to get in touch with Washington and the Manhattan Engineer District. This meeting was probably the first one that about a year later led to the establishment of the Oak Ridge Institute of Nuclear Studies (later ORAU).

The Xenon Culprit and Other Tales

By C. D. MOAK

Charles Moak joined the Metallurgical Laboratory in 1944, moving to Oak Ridge in 1945. His interests moved from reactor physics at the end of the war to the Van de Graaff accelerator and nuclear reactions. He recently returned from a year at the Niels Bohr Institute in Copenhagen.

I was a young research assistant during World War II at Clinton Laboratories. None of the great men of those days knew me, but I knew them and all of it was very exciting. The old laboratory cafeteria had full meal service around the clock. The atmosphere of urgency in everything we did was almost unbelievable by today's standards. Mistakes were OK, but delays almost amounted to treason. Many problems were approached by parallel lines of attack without the slightest thought about wasted effort. Time was of paramount importance. A war was on. The absolutely forbidden subject of discussion was the idea that we could possibly lose the war, which meant that everybody was intensely aware that we *could* lose, and that every single one of us would do anything to help us win. We didn't know whether the Germans were working on a bomb, or the Japanese, and we didn't know whether they were ahead of us or behind us. We had no doubts at all about what they would do to us if they got their hands on such a weapon.

L. B. EMLET

Although security was very tight, there was almost no compartmentalization of information within the scientific groups. Thus reports from site Y (Los Alamos), site W (Hanford), and the Metallurgical Laboratory came into the Physics Library at Clinton Laboratories regularly. When the Hanford reactors went into operation, we heard almost immediately that there was big trouble, that if a reactor was shut down it wouldn't start up again. A fission product poison was suspected. Round-the-clock efforts were mounted at Chicago and at Clinton Laboratories to measure the neutron absorption cross section of xenon-135, the number one suspect, a short-half-life fission product. The stuff had to be prepared from the parent fission product iodine and split into two samples. Both samples were counted; then one was sealed up in a quartz ampule and placed in the Graphite Reactor, brought out after a suitable length of time, and recounted. Its activity had been reduced, relative to its uncooked mate, proving that the xenon had been capturing large numbers of neutrons and, in this case, deactivating itself. Now to get down to being quantitative. Samples had to be transferred to counting cells; counters must work; samples were then transferred to quartz ampules (some broke, some leaked), put into the reactor, brought out, transferred without loss to counting cells again; and again the counters must work, all without a hitch and very quickly because xenon-135 has a very short half-life. The setup was built by Ernie Wollan, Lou Pardue, and me. We worked for days at a time. I went one stretch of 40 hr, slept 4 hr on the job in a bunkhouse, and worked 16 more. Lou Pardue was a bluegrass Kentuckian, so at every crucial step we would stop while he got up bets on whether something would go wrong. Finally after dozens of tries, a series of repeating answers. Good data. Result: a staggering four-million-barn cross section. Almost simultaneously, the news from Chicago was the same. By then the Hanford reactors had started up without our help. A terrifying crisis was over. Then I heard that Eugene Wigner had predicted that such a large cross section might occur among some of the fission products. What a towering hero that man was to me then. He still is. The funny thing is that it never occurred to any of us to attack any of the problems we studied at a more leisurely pace, or worry in the least about duplication of effort.

C. D. MOAK

To go from Knoxville to work, I had to pass through four perimeter checkpoints; from Oak Ridge, three. The guards were partly soldiers, partly civilians. Some gates had Geiger counters, and sometimes we'd put a penny in the reactor and plant it on some poor sucker as he went hurrying out to catch his bus home. The day I arrived, in August 1944, John Brolley was taking a bath under the fire shower in the hall of the Physics Building, because he'd been stacking graphite all day and looked like a coal miner. He had a very fine singing voice. The halls got pretty wet.

E. P. WIGNER

At the beginning, the engineering at Clinton Laboratories was run by Du Pont, and research was under the University of Chicago. Later the Laboratories were run for a while by Monsanto. When Monsanto decided to pull out, everyone wondered who would run the Laboratory next. There was a meeting in the old Physics Building, and a number of research people urged Bill Pollard to propose that ORINS (now ORAU) operate the Laboratory. Bill said that ORINS was just getting started and was simply too small to tackle such an administrative responsibility. Thereafter, in 1948, Union Carbide agreed to manage the Laboratory. It is interesting to note that if Monsanto had remained an additional year, ORNL might today be operated by ORAU rather than by Union Carbide.

I am sure that, in those earliest days, we were very wasteful, but we worked so hard and the people I watched were so inspired and talented that a tremendous amount of work got done in an extremely short time. From start to success, the Manhattan Project cost only two billion dollars and took about three years. Several ways of reaching the goal (electromagnetic separation, gaseous diffusion, thermal diffusion, reactor-made plutonium) were tried simultaneously in that short space of time. The reactor side of things was part of ORNL's early contribution, and this set the pattern for much of ORNL's activities through the years.

B. T. FELD

Fermi's New Toy

By BERNARD T. FELD

Professor Feld, physicist at MIT, was in Oak Ridge only long enough to participate in the uranium lattice experiments that formed the basis for the water lattice work described herein by Art Snell. This is only one of many instances in this issue where we get two independent versions of related research.

Long time, indeed! Since age has dulled my memory, the best I can do in describing my very early involvement in ORNL is the following rather disjointed set of loosely connected recollections:

I first came to Oak Ridge as one of a small group of assistants brought from Chicago by Enrico Fermi, when he was asked to "oversee" the start-up of the first Oak Ridge reactor. Since the reactor was very well designed, the starting was relatively routine—if any enterprise in which Fermi was involved could be characterized as routine. In any event, as soon as it had been established that the reactor could be operated at more or less the designed level, and the calibrations of control rods, etc., had been accomplished, Fermi proceeded to play with the new toy. Since the main "new" feature in the reactor consisted in its record-high neutron densities, he characteristically set about searching for new effects associated with this feature and soon discovered the "fission product poisoning" effect. The measurements we made, then and there, on this effect were crucial for the successful design of the Hanford reactors.

Living in the bachelor dormitory, I met the late Louis Slotin, and we soon became fast friends. Louis was an imaginative scientist, and although trained as a chemist, he had quickly mastered nuclear physics and turned himself into a first-class reactor engineer. (We were, of course, all "reactor engineers" then, since we were inventing the field.) It turned out that he and I, aside from many other common interests, were both concerned at that time with the same question of physics, although for different reasons. The question was: What are the neutron multiplication properties of pure, natural uranium metal? My interest stemmed from my attempts at calculating the so-called "fast fission" multiplication factor (i.e., the contribution to the reactor multiplication factor from the fast fission of uranium-238 by fission neutrons before they emerged from the reactor fuel element into the moderator. The total multiplication factor, which is all you need to know to build a given reactor, can be measured in an "exponential pile" experiment; but if you want to design new reactors, it is useful to be able to break it up into its component parts.) Louis was interested in knowing just how close natural uranium, in pure metallic form, would come to sustaining a chain reaction, both because of an interest in the future possibilities of fast reactors and also to determine, in the most direct way possible, the minimum degree of enrichment that would be necessary in order to make a uranium bomb.

We decided to try to perform an exponential pile experiment with uranium metal, using the residue of the cylinders that were used in the reactor, and the reactor itself as the neutron source for the exponential

Manager Harry Maggart's champion 49ers. In front from left to right are James Schenck, Maggart, Garrett Tyler, Rube McCord, Ray Davis, N. H. Marney. In back are Dick Jernigan, C. E. Clifford, Francis McGowan, Bob Kernohan, Jim Leslie, and Wally Koehler (1949).

The Forty-niners Softball Team

The Physics Division softball team during the war was known as the Albedos. After the war it became the Forty-Niners. Why should 49 be so secret that it couldn't be used at the start?

Well, uranium-235 is a mouthful and plutonium-239 is a mouthful, and anyway they were words that couldn't be used openly. So the chemists and physicists used the last digit of the atomic number plus the last digit of the atomic mass to designate each nuclide. All uraniums had numbers with an initial digit 2, all plutoniums had a 4, all neptuniums had a 3. And so uranium-235 became 25, uranium-239 became 29, plutonium-239 became 49, and the pioneers in the production of 49 became the Forty-Niners.

But I am not sure why albedo was any less secret, although it is less obscure. It is a word in astronomy for the reflectivity of moon or planet, and in neutron diffusion theory it became the reflectivity of the outer wall of a reactor for neutrons that would otherwise escape.

Several of the original Forty-Niners are still associated with ORNL. The star left fielder was Francis McGowan, the star second baseman was Alvin Weinberg, and the hapless right fielder was Herbert Pomerance.—Herbert Pomerance, analytical chemist, neutron physicist, lecturer, editor, and, currently, information specialist, who has been at the Laboratory since 1943.

M. C. LEVERETT

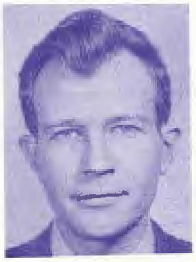
pile. Louis obtained permission to use the available stock of cylinders; I was granted permission to stay on for a few more months when the Fermi group left; and we built our uranium castle on the top of the reactor, sitting over a cadmium shutter on one of the graphite thermal columns. Louis was a great builder, and I was an experienced neutron measurer; and so the experiment went quickly and well. We got our results, wrote them up, and I went back to Chicago. (Not long afterward, Louis and I both turned up at Los Alamos in the same group—the critical assembly group—where we again worked together until the end of the war. I then left, but Louis stayed on at Los Alamos.)

For a New York-bred boy, Oak Ridge in those early days was an exciting and exotic experience. I later married a girl from the South and have come to appreciate the subtle delicacies of southern cooking, but the Oak Ridge cafeteria then was not the ideal representative of southern gastronomy. As I recall, turnip greens stewed with fatback pork was the core of a not-too-varied menu. Hence we dormitory dwellers frequently inflicted ourselves on our married-couple friends where, in exchange for occasional baby sitting, we scrounged edible meals. The Newsons, Cannons, and Shapiros were my favorite victims, the last especially, because—being a chemist—he had more direct access to the store of lab alcohol, which, mixed with canned grapefruit juice, was the favorite lubricant at parties in dry Oak Ridge.

And so, in a frenzy of work, interspersed with one or two forays into the magnificent Great Smokies and the cultivation of some friendships that were to become lifelong, I passed a brief interval in early Oak Ridge. As I look back, a great deal was crammed into a few months, but that was characteristic both of the times and of youth.

The Water Lattice Experiments

By A. H. SNELL



S. E. BEALL

Arthur Snell retired in 1973 as senior research advisor, but remains active as a consultant at the Laboratory in the physics of fission.

To introduce this story I have to go back a little to lay some groundwork. In 1942, the Cyclotron Group at the Metallurgical Laboratory in Chicago was beset by a whirlwind visit by Robert Oppenheimer from Site Y (Los Alamos). We were being asked to do an experiment involving pure uranium metal, which was just starting to appear in appreciable quantities. The experiment, which involved measuring fission-to-capture ratios in uranium-238 and uranium-235, would give information about whether natural uranium metal would chain-react if a lot of it were stacked together, and if not, then how much it would have to be enriched in uranium-235 to bring it to the point of chain reacting. The results would be of obvious importance to Site Y, and inasmuch as they had weapons implications, they were classified as "CF"—an especially restricted category. However, at the time of Oppenheimer's visit, we weren't told all about this; we had to surmise it after we had time to catch our breaths. Edward Teller, John Manley, and others were there; ideas flew thick and fast, and then Oppenheimer left and we had a job on our hands.

In due course, five tons of uranium arrived, some from Westinghouse (the purest) and some from Metal Hydrides. We assembled it against the cyclotron target, fired fast neutrons into it, and obtained the measurements. I wrote the results up in report CF-589, entitled with deliberate vagueness "Studies in a Five-ton Metal Pile." Then the uranium went into the heart of Fermi's first pile, CP-1.

B. H. DABBS

Now this was all well and good (patience, dear reader, I'm slowly getting there), but the results could not really be believed. They were good enough for a first shot (remember, nothing was known about fast-neutron cross sections or fission-neutron energy spectra; in fact, we didn't know for sure that the uranium wouldn't chain-react as we stacked it up), but our five-ton pile simply had not been big enough. The dimensions of a stack like that should be several times larger than the relaxation length associated with the neutron capture or escape from the pile; our pile had been too small.

As a consequence, the experiment was repeated at the Clinton Pile in 1944, using 35 tons of uranium in the form of Clinton Pile slugs. A hole 6 ft square had been left in the top shield of the pile for the purpose of testing a section of prototype Hanford shield. After that job was finished, the loading of the Clinton Pile was modified by building a "pedestal" on the active lattice so as to extend the reacting volume up close to the hole in the top shield, and a "thermal column" of pure graphite was installed in the shield. The uranium slugs were then stacked in a tank over the thermal column, and the Chicago experiment was repeated. The work, done by John Brolley, F. J. Byerly, Bernard Feld, Beth Olds, Dick Scalettar, Louis Slotin, and R. B. Stewart, is described in report CF-1627. The results confirmed the Chicago measurements, but now they carried conviction. Natural uranium metal would have to be enriched by a factor of about 12 in uranium-235 for a large mass of it to become critical.

Now I come to the point of my story. While all of this was going on, Alvin Weinberg had been considering the question: "What if you put some ordinary water in some kind of a lattice arrangement in the uranium?" This would be something like the Hanford lattice without the graphite. To the surprise of many, Weinberg found that you could get to critical—within the rather large errors imposed by poor knowledge of the nuclear cross sections involved.

Thus the water lattice experiments got started. Here was this thermal column left from the previous experiment. You put a 6-ft cubical water tank over it, arrange patterned tube sheets for the rods and spacings that you want to test, and lower the uranium rods into the water one by one. But be *careful*! You have a prediction that the assembly now might go critical, so with a false move you could easily kill yourself.

C. E. CLIFFORD

Measurements were taken on over 20 lattices, with variation of rod size and metal-to-water ratios, with and without air gaps, at different temperatures, and so on, until a firm understanding of the situation had been reached. The report, CP-2842 (June 30, 1945), was written by Garland Branch, Haydn Jones, Joe Rush, and Alvin Weinberg, but about 15 other people helped in the work. Some of them were Thelma Arnette, Seymour Bernstein, Charley Clifford, Ida Coveyou, Bernard Feld, Rube McCord, Henry Newson, Warren Nyer, Dick Scalettar, Louis Slotin, and Linda Watson. Quite an array!

The main result was that the best reproduction factor was $k = 0.993$, instead of 1.000 as required for the chain reaction. This meant that the uranium-235 would have to be enriched from 0.71% only to 0.75% in order to reach critical, but a more comfortable margin would be needed to overcome the xenon poisoning and bring the pile down to a useful size.

From here it was a natural step for Alvin Weinberg, Bob Christy, and others to move to the concept of the pressurized-water reactor. High enrichment was not required, so the cheapest and most natural of coolants would do. In fact, the information was solid enough for the engineers to take over, as they did at General Electric and Westinghouse, giving us the nuclear power reactors that we have today.

Chemistry and Philosophy

By JOHN SWARTOUT

JOHN SWARTOUT

John Swartout is one of the "local boys who made good." He showed up in the hills of Tennessee in 1943 from the Met Lab in Chicago, spent a year getting Hanford cranked up, and then returned to Oak Ridge to rise eventually to Assistant Director and Director of the Chemistry Division, Deputy Director of the Laboratory, and on out the top to Director of Technology for Union Carbide Corporation and Finally Vice President of the Corporation. He is now retired and living at Hilton Head, S.C., but keeping in touch with his many friends in Oak Ridge.

Most of our group, which had been working for several months under George Boyd's direction on ion exchange processes for separating plutonium, made the train trip from Chicago to Knoxville together via L&N, arriving on a Sunday afternoon in the fall of 1943—October 2, as I recall. The booted traffic and red clay in the Andrew Johnson lobby were foretastes of what lay ahead. After arrival at the X-10 site the following morning, a brief check-in, and assignment of dormitory space in East Village, we explored the new laboratory assigned to us in Building 706A—seven or eight of us in one room, not a great improvement over the makeshift laboratories we had just vacated under the stands of Stagg Field. Although workmen were still installing electrical wiring and completing other parts of the building, we located and assembled our equipment and supplies, ordered in Chicago, finding time to visit the nearly completed Graphite Pile, the fuel and graphite fabrication building, and the chemical pilot plant, then about half finished.

Much has been told of the tribulations of those first days, and there were many: the mud alternating with dust which drifted into our experiments, the strange food in the old cafeteria, the "cattle car" transportation over disappearing roads to "Townsite." However, in looking back through the intervening years, other events appear far more distinct:

- The successful start-up of the Graphite Pile on November 4, 1943, followed shortly by full-power operation. From this point in history, when ten years seems to be accepted for the design and construction of a power reactor, the design, construction, and attainment of full power in less than one year of this first reactor to produce heat, relatively simple though it was, appear little short of miraculous.

- Crowding into Lou Werner's laboratory to see with naked eye the first macroamount of plutonium. The impression which this made on us is better understood if one remembers that for months we had worked with such minute amounts; the quantity allocated to our entire group from the St. Louis irradiations had been about 1 microgram.
- The thrill of finding that our ion exchange processes would not only separate plutonium but would also fractionate the fission elements.
- The identification of element 61 by L. E. Glendenin and J. A. Marinsky.
- The first proof that the $BiPO_4/LaF_3$ separations process, developed with tracer amounts in the laboratory, worked in the pilot plant.

W. H. SULLIVAN

At this point, memories of the first inhabitants of 706A and of the conditions under which we worked may be of interest. In retrospect, we were crowded by current standards, although the simplicity of our equipment alleviated that. In addition to the Chemistry Division, which then included Analytical and Cas Borkowski's Instrument Group, the predecessor of Chemical Technology and a semiworks team were also housed in the building. We worked six days a week, buoyed by the excitement of rapid advances and urgency—with air conditioning limited to the counting rooms. We were a heterogeneous group, ranging from first- and second-year graduate students to veterans from Du Pont and other companies, with the relatively high-paid veterans often serving under a young graduate-school group leader subsisting on slightly more than assistantship salary. At some time in 1944 or early 1945, Warren Johnson made a survey of those in the Chemistry Division and found the average age of the chemists to be 26, about 96% of whom had received Phi Beta Kappa or equivalent recognition as undergraduates.

By the summer of 1944, the success of the bismuth phosphate precipitation process seemed assured, and our ion exchange section was assigned to specific phases of the adopted process; following the decontamination of specific fission products fell to me and was made possible by the increasingly detailed identification and characterization of the fission isotopes by the section under Charles Coryell. As a result of this involvement, I was asked to join the group being assembled under W. H Sullivan to assist with the start-up of the Hanford chemical plant. After a cross-country trip in October 1944, at the wartime speed limit of 35 mph, I joined those collected from various parts of the Manhattan Project in setting up yet another laboratory, and in adapting to the change from red clay to sand. As start-up of the production piles and of the chemical plant began, we were in frequent contact with those in Chicago and Oak Ridge—as the xenon crisis arose in the pile start-up and minor quirks appeared in the chemical process. Here we first confirmed the anticipated, but yet unencountered, limitation on decontamination factors set by the complex chemistry of ruthenium, which was to plague later developers of new extraction processes. I recall the satisfaction, upon monitoring the first container of "purified" plutonium slated for Los Alamos, in finding that the fission product contamination agreed with prediction. There is also the memory of conducting with Truman Kohman an experiment with about a gram of plutonium, in the then "standard" laboratory hood.

C. J. BORKOWSKI

G. E. BOYD

J. A. SWARTOUT

L. T. NEWMAN

Upon returning to Oak Ridge in November 1945, after the war's end and a brief detour to test industrial research again with Du Pont, I found the Clinton Laboratories in a state of flux and uncertainty. Monsanto had replaced the University of Chicago as operating contractor, many of the senior men had returned to prewar university positions, graduate students were leaving to complete interrupted studies, new men were arriving from other parts of the Manhattan Project and from the "radar" project. Several of the Du Pont assignees decided to remain and others were returning, as I was, gambling that research on atomic energy would be continued. A new director, Jerry Coe, replaced Warren Johnson as head of the Chemistry Division, with Ellison Taylor as assistant. The goal of the Manhattan Project had been attained, and the Corps of Engineers maintained a holding action. The future of Clinton Laboratories and its sister labs was unknown. Many of the Laboratory staff entered the political arena to push for civilian control of atomic energy.

In view of the conditions, it is surprising that anything useful was accomplished. On the contrary, the Laboratory and its sister laboratories took initiatives and made decisions which have shaped the development of nuclear energy to this day.

- Although research centered on the graphite piles as the primary research tool, it was realized that a much better neutron source would be required. The Chemistry Division initiated research to explore the feasibility of an aqueous homogeneous research reactor, work which led to the Homogeneous Reactor Project within a few years, with thermal breeding as the goal and, indirectly, at least, to the Molten-Salt Reactor.
- With the same goal of a better neutron source, the physicists initiated work on a solid-fuel, water-cooled reactor which resulted in the Materials Testing Reactor, the prototype of the submarine reactors and of all the water-cooled power reactors of today.
- As the MTR Project progressed and realization appeared possible, the need arose for a chemical process tailored to the enriched fuel. A few of us in Chemistry joined with a few in Chemical Technology to oppose the decision to adapt the wartime precipitation process, favoring the development of a solvent extraction process, and we won. The process then developed at ORNL and proved in the Idaho plant set the pattern for all subsequent reprocessing.
- The production of radioisotopes was begun on a very small scale, and in the Chemistry Division, ion exchange processes were refined by the groups under George Boyd and Waldo Cohn to effect separations of rare earths and the other fission products.
- A reactor training school, the "Clinch College of Nuclear Knowledge," was started with faculty and lecturers drawn from all parts of the Laboratory and with students who were later to take key roles in nuclear energy development.
- The Laboratory joined with Argonne in the Daniels Power Project, the first, although premature, concerted effort to produce nuclear power.
- Among other discoveries and programs which have had similar long-term impacts was the discovery of the low cross section of zirconium by Herb Pomerance, then in the Chemistry Division (as I recall), and the subsequent development of chemical processes for

J. G. STANGBY

M. J. STANGBY

removing hafnium, initiation of the enrichment of stable isotopes of lighter elements at Y-12, initiation of research to improve the extraction of uranium from its ores, and on and on.

During these first transition years, the Laboratory was truly master of its own destiny, a situation which necessarily changed as areas of emphasis became matters for national decision. The independence of the laboratories and lack of "paper work" are almost inconceivable today. I recall stopping by the Chemistry Division office in late 1947 or '48 and listening to Ellison Taylor and James Stangby as they attempted to work out the Division's first budget request. With no cost experience, in fact no accounting system, as basis, they decided to make a stab at what the cost of supporting a chemist might be and guess at the likely number of chemists for the year following. If there was any text to accompany the page of figures, I did not see it.

Amidst the excitements of an unfolding technology there were difficult, sometimes dark periods.

- The decision by the newly created AEC to concentrate reactor research at Argonne, which left many of us pondering the question of moving to Argonne or elsewhere, and many did.
- The initiation of security clearance investigations which, in my case, resulted in service on the Oak Ridge Security Review Board as scientists' representative and, later, to my denouncing many of the procedures and principles.
- The prolonged and agonizing search for a research director of the Laboratory after Union Carbide became contractor, with the eventual and most fortunate selection of in-house talent in the person of Alvin Weinberg.

In the first postwar years the inadequacies of the temporary Laboratory structures were recognized. Eugene Wigner appointed a building committee composed of representatives from the major divisions, with a Monsanto architect as chairman. After thorough studies, including visits to the newest laboratory structures in the country, the committee produced a grand design of one large research building to house most of the Laboratory's research and to be located at the then rear side of site, east of the reactor hill. Although this grand design was never realized, its successors, which incorporated many of its features, were constructed in the early 50s, after which 706A and its companion wooden structures were depopulated and vanished piece by piece.

More from K.Z. . . .

In the early days, Clinton Laboratories issued employees knee boots as part of our essential equipment. One day as we left the cafeteria (50 yards east of the present west gate), we heard a plaintive cry, and looking around we saw Frances Bishop, a health physics technician, into mud above her knees. We got boards and made our way out to her. By pulling from the front end and pushing from the rear we finally extracted her, but her boots were left buried knee-deep where they remain to this day—now of course under the hard-surface main Laboratory road.—K. Z. Morgan

M. LEVERETT

Juice at the X-10 Pile, and Other Stories

By MILES LEVERETT

Miles Leverett was the first director of the Technical—ultimately Reactor—Division. He was at the Laboratory for the most part of 16 years, and then joined General Electric's Nuclear Division, from which he recently retired. He keeps in touch from California.

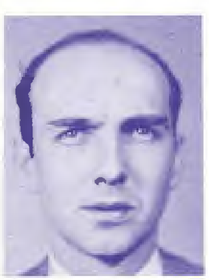
I still recall the dismay on Garrett Tyler's face. In 1942 he was at the Kankakee Ordnance Works (KOW), and as that plant neared completion, he was assigned his final job there. It was to dispose of—to surplus—all the leftover materials from the construction job. Fencing, pipe, fittings, wire, sheet metal, tools, hardware—all had to go and the name of the game was "Get it out of KOW—we don't care where you send it, just get rid of it." Garrett, always energetic and resourceful, did just that. Some of it went to various other plants, but a great deal was sent to the Clinton Engineer Works (CEW), because—who knew?—they might need it there in whatever it was they were doing. His job at last complete and he at last free of the carloads of stuff which day after day he had been dispatching to CEW, Garrett found that he too was going to CEW. There, at the Clinton Laboratories he coped for years with the mountain of ill-assorted material which he himself had surplused to CEW at KOW. Crime does not pay.

• • •

We were about to fill in the excavated dirt around the radwaste tanks located down the hill from the chemical separation building, and the Army wanted to make some last-minute seismographic tests on the tanks. Thus, on a cold, rainy, November evening, just as dark was falling, here came a Jeep full of equipment driven by a cocky soldier. "Don't drive onto that loose earth," we shouted to him, "or else you'll get stuck." "Hell, nuthin' stops a Jeep," was his reply. We worked until midnight, with cranes, shovels, and bulldozers, getting him out. The test results were entirely trivial and negative.

Not too many people know that the first generation of electricity from nuclear fission was not at Arco, Idaho, as is widely recorded, but was at Oak Ridge in the X-10 Pile in, I think, 1945. That pile, now the Graphite Reactor, was a very stable, well-behaved reactor, so there often was little for the night crews of operators and engineers to do. To pass the time (and prove a principle) they made an aluminum thimble, inserted it in one of the side holes of the reactor, connected it up to a water supply, and put one of the aluminum-canned fuel slugs in it. The resultant steam was led to a toy steam engine which turned a small generator and lighted an electric lamp bulb. This feat is reported to have vastly annoyed top brass in the Project and in the Army, and so far as I know has not yet been generally publicized.

• • •



G. W. TYLER

The atmosphere of urgency about the labs in those days was all but unbelievable. I was assigned (in October 1944, I think) the task of designing a process to produce, in very pure form, hundreds of curies of barium-140 (the radioactive lanthanum, or RaLa Project, Building 706D). Since this material was *urgently* needed in the weapons programs, the traditional chemical engineering approach through test-tube experiments, semiworks scale, pilot-plant testing, and final plant construction was compressed beyond recognition. Nor was that all. Whole carloads of stainless steel pipe and plate were commandeered from shipments destined for and actually en route to Hanford. As for craftsmen, anybody who had ever struck an arc was put to work welding. Reinforced concrete walls, floors, and roofs were designed one week and poured the next. Pipefitters were working 12 to 16 hours a day, their time being limited only by their loss of endurance. Some would quit only when their feet became so swollen that they couldn't get their shoes on. In the midst of this frenzy one day came word from the Castle (Oak Ridge) that "Major somebody wants to have a review of the RaLa Project." The message came during one of many brief planning and expediting sessions. There was a dead silence. Then one of the construction foremen, who probably hadn't slept in a bed for two weeks, said, "Tell the major that if he comes on this plant he's got to fit pipe." There was no review. And, incidentally, we made our first shipment of barium-140 in May 1945 and continued to make ever larger shipments thereafter whenever needed.

E. G. BOHLMANN

• • •

One of the most interesting projects of the early postwar period was the work which led eventually to the Materials Testing Reactor. The MTR was to be—and was—a really superior experimental tool, and much thought went into designing it for maximum value to the groups of scientists who were to set up experiments in or around it. Some of the most trying sessions, from the viewpoint of the engineer who wanted to "get the show on the road," were those in which the biologists decided what their experimental requirements were. We discussed putting in holes for the irradiation of mice (mouseholes), or rats (ratholes), or rabbits (rabbit holes). Then the question of larger animals came up. Goats? Swine? The discussion went on endlessly about the proper size of the largest of all experimental holes in the shield. Finally, in desperation the engineer whose job it was to do the design said, "I have decided. We'll make it big enough for a donkey and call it by its right name."

• • •

In some ways those early years at Clinton Labs were the most satisfying I have known. Our mission was clear. We were doing our best to accomplish it, and everyone up and down the line knew that. We were finding in ourselves unsuspected abilities to learn and to do. We had maximum support and minimum second guessing. It is no longer possible to operate in that way, nor would it be desirable. But it is great to look back upon it.

From Orlo Myers . . .

The most exciting thing that happened to me in 1944–45 was when we were asked to obtain a few liters of "dissolver" from the top of Building 205 (as I remember). This was "superhot nitric acid solution of slugs from the Pile." My roommate, Orville Hill, said he wouldn't touch it with a 10-foot pole, so we got a 20-foot pole. The pole broke.

We were sent home (radiation), and a 15-minute (each) crew chipped on the building walls, dug the ground from underneath, and leaded our water-carrier truck so we could use it again.—Orlo Myers, Aerospace Division, General Dynamics Corp.

LOTHAR NORDHEIM

G. P. NORDHEIM

Old Times and New Horizons

By LOTHAR W. NORDHEIM

Lothar Nordheim was the first Physics Division Director at the Laboratories. He and his talented and vivacious physicist wife Gertrude came to Oak Ridge from Duke University just long enough to give birth to their son Ricky, then returned to the Duke faculty in 1947. Later, Lothar joined the General Atomic Division of General Dynamics, from which he is now retired. He lives in La Jolla, California.

One memorable day in the summer of 1943, Martin Whitaker, a close associate of Arthur H. Compton, visited me at Duke University and opened our conversation with the magic word "uranium." At that time I was busy on a defense project of my own on "Thermal Effects of Propellant Gases in Guns." Though this venture proved to be eminently successful and answered all questions which could be asked, it appeared to be somewhat outside the mainstream of the war effort. Like others, I had a vague notion about the potentialities of nuclear fission, but because of the tight security clamps, I had no knowledge of the vast effort then under way in the U.S. Thus I did not hesitate to accept the position as chief theorist at the Clinton Laboratories then being built at X-10.

Oak Ridge was not yet operational, so I was directed first to the Met Lab in Chicago to learn about the marvelous developments that had been going on there under the aegis of Enrico Fermi and Eugene Wigner. We drove to Chicago by way of Oak Ridge for a first glimpse of our new home for the next few years. We got our due initiation to military security when the guards found a half-empty bottle of whiskey among our belongings and promptly confiscated it. We were not alert enough to demand that its contents be poured out immediately. Inside the reservation we were duly impressed by the enormous amount of construction going on.

In Chicago I received my first initiation to the intricacies of nuclear chain reactions and the properties of plutonium. I asked Wigner what would happen if one assembled a large amount of pure plutonium and he answered, "So?" and shrugged his shoulders. That was enough for me; this subject was never mentioned at X-10 during wartime.

When housing became available in January 1944, we took up residence at Oak Ridge and lived through the usual experiences of all the early inhabitants. Your feet were apt to stick in the mud while your head choked in the dust. You had to learn to shovel coal and to bank the furnace for the night. You felt quite isolated, even within the reservation with the tight security barriers between the various sites.

From a long-range point of view, this isolation was not a real disadvantage, for we each had to learn to solve our own problems. Also we developed a spirit of comradeship and helped one another. Close, enduring friendships were made easily. We were all in the same boat.

The long working days were not made any shorter by the bumpy rides to the distant lab. But they were not without their amusing incidents. One day on the bus, my wife, then visibly pregnant, engaged in a spirited discussion with Charles Coryell. She mentioned the element rhenium, and the guard thought she had said "uranium." Due to her delicate condition she got way with a mild reprimand, but not until after she had been given a lengthy exhortation in my presence.

During 1944, there was great pressure on X-10 for its service as a pilot plant for Hanford in physics, chemistry, and biological effects of radiation. One memorable incident was the observation of the xenon effect. The Oak Ridge reactor showed after each start-up a slow downdrift of reactivity. Nobody paid any attention to this effect, since it was well within the operating range. Then came the news from Hanford that their first reactor had shut itself down at a trial run, and shortly afterward that the fission product xenon-135 appeared to be the likely culprit. Within days we set up an experiment involving a series of planned shutdowns and power runs, and by the end of a week we had the full confirmation of all details. Too bad that we had not worried about the reactivity drift beforehand. Later, Seymour Bernstein was the first to measure the energy dependence of the xenon cross section and to determine its resonance parameters. Other work in this period concerned the prediction of polonium production for neutron sources. The production was carried out by two massive stringers of bismuth in the pile. Its analysis led to Dick Scalettar's and my theory of eccentric control rods. Another experiment of great interest was Art Snell's measurement of neutron propagation in a massive block of natural uranium.

S. BERNSTEIN

By the end of 1944, Hanford had started its operation and the pressure on X-10 diminished, so the promises of the nucleus for peaceful purposes became our foremost preoccupation. Among the prime topics of research was the utilization of neutrons: in beam applications for cross-section measurements, and in diffraction for solid-state studies, isotope production, and concepts for future reactors. At the time of Hiroshima and Nagasaki and the release of the Smyth report (telling the public about the once-secret Manhattan Project), we were on vacation in the Carolina mountains, and so I did not observe the direct impact at the Laboratory. However, after the end of the war, life at Oak Ridge became more normal and the barriers between the various sites, X-10, Y-12, and K-25, were relaxed. That added such items to our program as the production of stable isotopes with the calutrons, and criticality experiments for safety in handling and processing enriched nuclear materials.

L. W. NORDHEIM

The Army looked favorably at Oak Ridge probably because it was still a military reservation. Thus expansion was encouraged. Many people returning from defense activities were at loose ends, and we recruited at Los Alamos, the Rad Lab at MIT, and elsewhere. Many of these new staff members contributed greatly to the future development of the Laboratory.

After the dramatic end of the war as a result of the actual use of atomic bombs, there was turmoil in the minds of the technical people connected with the Manhattan Project. We realized that we were the only persons with factual knowledge of the far-reaching implications of nuclear energy for war and for peace. Politically, we pushed for more freedom in communications, for a greater voice of the scientists in atomic policies, and for civilian control of nuclear energy. We founded ORES, the Oak Ridge Association of Engineers and Scientists, which shortly joined with similar organizations that had sprung up all over the United States to form the Federation of Atomic Scientists (FAS). The fight for civilian authority was successful; FAS was instrumental in the defeat of the May-Johnson bill, which would have left the military in charge, and in the success of the McMahon bill, which created the Atomic Energy Commission. We also sent speakers around the country to help generate public opinion in step with the changing times.

In addition to these exciting political activities, there was also much technical and organizational work to do. Clinton Laboratories was developing into one of the major national laboratories. It became clear that the number of people with knowledge in the nuclear energy field formed too small a basis for sound development. Because rigid security classification persisted, universities and institutes of technology could not provide the proper education. So it was decided, on Wigner's initiative and under his direction, to run a one-year (1946-47) training course on reactor physics and engineering. This school, affectionately dubbed by its participants as the "Klinch Kollege of Knuclear Knowledge," was highly successful, with Captain—later Admiral—Rickover as its best known student.

H. G. RICKOVER

During this period, Clinton Lab researchers studied new reactor designs. The most successful Oak Ridge product was the high-flux reactor designed and engineered in 1946. It consisted of assemblies of aluminum-clad thin plates of enriched uranium through which ordinary water was circulated at high velocities. The water serves both as a moderator and coolant. This compact configuration and efficient cooling permitted a large step up in power density and is the true conceptional forerunner of the light-water reactors now used so widely for naval propulsion and electric power generation. The AEC, however, was at that time reluctant to place such a new and powerful reactor at Oak Ridge. It was built later (1952) at Idaho Falls as the Materials Testing Reactor. It proved highly successful.

E. R. TOMPKINS

I found myself becoming more and more involved in administrative functions, so I decided to go back to Duke University in the fall of 1947 to return to physics and academic pursuits. This is nearly the end of my story. At Christmas 1947 we visited Oak Ridge to celebrate it with our many friends there. Everything appeared to be rosy. The former Clinton Laboratories was to become the Oak Ridge National Laboratory to be administered by the University of Chicago with Professor Warren Johnson, the former highly regarded Director of the X-10 Chemistry Division, as its new director. Then just before New Year's came Black Friday, when the General Advisory Committee of the AEC descended on Oak Ridge and informed us that all reactor research should be concentrated at Argonne and that Oak Ridge should devote itself to applied work only. Everything looked bleak. However, ORNL rose like a phoenix from the ashes. This was due to the dedicated effort and technical excellence of the people at the Lab, in particular Alvin Weinberg, who refused to be daunted. Also, the new contractor, Union Carbide, turned out to be able and understanding.

The early years at Oak Ridge will remain in the memory of its participants an unforgettable experience. Los Alamos was certainly a still more peculiar place, but Oak Ridge suffered to some extent from the same isolation and turmoil of wartime vagaries. In addition, we felt that we held the key to the extraordinary promises for peacetime applications of the atom. There was the grim purpose during the war years and a sharing of hardship combined with the recognition of the revolutionary importance of the work. There was the development of a community from scratch to a model of enlightenment in a backward region. Above all, it was the people who molded this enterprise through dark days and bright, and made this city and laboratory into what is now a shining example of what man can achieve when there is a will.

1945

In April, J. A. Marinsky and L. E. Glendenin, under the leadership of Charles Coryell, identified and produced element 61, bringing it into existence for the first time. They used the ion exchange chromatography technique pioneered by George Boyd, Waldo Cohn, and associates. The name approved for the element by the International Union of Chemistry—promethium—was the one suggested by Coryell's wife, Gracemary.

On July 1, the Manhattan Engineer District contracted with Monsanto Chemical Company of St. Louis to operate Clinton Laboratories, which had 1000 employees. The radioisotopes production program was expanded, and work began on designing a high-flux heterogeneous experimental reactor (in which the fuel and moderator are separate) and an aqueous homogeneous reactor (in which the fuel and moderator are combined). The latter concept, which was to become ORNL's largest single project for the next 15 years, involved pumping a liquid fuel of enriched uranium (or plutonium) salt dissolved in water through the reactor and heat exchanger.

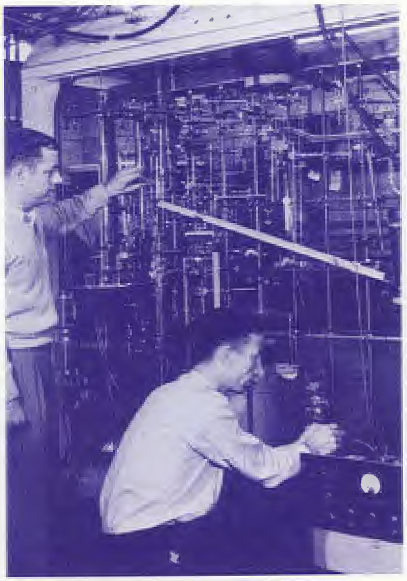
On August 6 (following the successful Alamogordo, New Mexico, test on July 16), the United States dropped the first atomic bomb of World War II on Hiroshima, Japan, thus hastening the conclusion of the war. The bomb used enriched uranium from Y-12. The second bomb, dropped on Nagasaki on August 9, was fueled by plutonium from Hanford. Clinton Laboratories' wartime effort now shifted completely to peacetime research, including basic studies of the structure of matter. By using neutron beams from the Graphite Reactor, E. O. Wollan developed and demonstrated neutron diffraction techniques for determining how atoms are arranged in crystals. These techniques became particularly useful for studies of the structure of hydrogen-containing crystals and have been important in studies of the magnetic properties of crystal lattices (as in later rare-earth studies by Wollan, W. C. Koehler, and M. K. Wilkinson).

A Memory of Jerry Coe

By E. H. TAYLOR

Ellison Taylor, Director of the Chemistry Division at ORNL for 25 years, recently stepped down and returned to the bench, where he is performing research in catalysis.

Jerry Coe showed up one day in the fall or early winter of 1945 as the new director of the Chemistry Division. I suppose he'd been interviewed for the job, and maybe he'd even talked to some of the section chiefs, but for me, he just "appeared." He went away after only about two years and never really came back, but the Division still bears his imprint, and all of its accomplishments owe something to the two years he gave us.



Glen Jenks and Fred Sweeton with calorimeter they developed for studying radioactive decay characteristics (1949).

His full name was James Robert Coe, and his wife called him "Robbie." "Jerry" presumably arose through "Jericho" as a pun on "J. R. Coe." He was a little above average height, rather thin, and very slightly stooped. He had dark hair, an aquiline, very mobile face, and a habit of wearing his dark-rimmed glasses either on his forehead or pushed down on his nose. When he talked sitting down he stared at you over the top of his glasses like an owl. I can remember only his summer clothes—then he wore a seersucker suit from Brooks Brothers. He didn't seem to have the kind of money that Brooks Brothers implies, but he must have grown up with it, since all his instincts were for quality. He didn't have a fancy car, to be sure, but his Ford roadster (I think, but can't be sure, that it had a rumble seat) was what only a few undergraduates were driving when he was in college. I think his record player was a Garrard, but the amplifier was homemade. His toolbox (still in a Cemesto attic in Oak Ridge) contained the highest grade of hand tools, all scrupulously oiled, and he had a frightening machete for cutting weeds. He had been in technical liaison for the District Headquarters, carried secret documents back and forth to Washington, and had a nickel-plated automatic that he oiled more often than necessary.

I can't remember his taking any regular vacations, but he took a day off now and then to work in the garden of his A house. He had a "dirt mine" down in the woods, and brought up enough black soil to build flower beds almost all around the base of the house. Fleta, his wife, was the real gardener, but he did the heavy work.

Conversation with Jerry, and particularly with Fleta and Jerry together, was an exercise in the oblique construction. Presumably this came from their Washington years (he had been a chemist at the Bureau of Standards), but one could believe that they had been the ones to introduce it. When obliquity went so far as sarcasm, Jerry could be a real trial to the innocent. One day a big billboard appeared at the gates, extolling the virtues of Monsanto (the new operating contractor). Jerry's complaint against "that green and yellow Monsanto monstrosity out front" was heard clear to St. Louis—within a week the same message appeared in black and white. One morning the mail included the Nth version of some chart or memo of no foreseeable use which he'd already rejected $N - 1$ times—to no avail. "Why in hell," he asked his empty office, "don't they put it out in three colors?" About five minutes later his new secretary came in with a distressing message. "If you want it in color, they say it'll take another week."

Obliquity, an eye for quality in tools and suits, a background of second-layer, New-Deal Washington—these don't quite add up to a research director. But Jerry Coe did—at the level I knew him—to the best I've ever seen. I've tried to figure out why, and I can list some obvious qualities, but it misses the full picture. He did have a very quick understanding, he listened well (if impatiently), but what it really was was style. He just *was* a leader—naturally, instinctively, intellectually.

In my third spring in Oak Ridge, after we'd weathered the shock of New Year's 1948 (Deck the Pile with Garlands Dreary), Jerry's cough was diagnosed as tuberculosis. For a while we could visit him in the new west wing of the old hospital, and we talked of his returning in the fall, but his kind of TB resisted the newer medicines, and he was shipped off to Trudeau in Upstate New York for the sanitarium treatment. A year there (or was it two?) and a further period living in Saranac as an outpatient didn't quite effect a cure. On a friend's advice he entered a sanitarium in New Jersey. This finally put him back on his feet, but his long-awaited return to work was blocked by the AEC, which suddenly refused to reinstate his security clearance. His hearing was in the "Castle" in Oak Ridge, where most of the participants for the AEC were old Manhattan District hands who'd known him during the war. I went with him for the hearing—it seemed to go all right, but the verdict was to deny him clearance. He appealed, engaged legal help, and won in Washington.

But neither he nor Fleta ever came back. Sometime in the year of his clearance problems she went away to the hospital to die quickly of cancer. He was pretty much out of touch for several years (like Dick Diver at the end of "Tender Is the Night"), but we knew he was moving around, having relapses, and not in any state to return to work. He went west for a while, met someone who appreciated him, got married, went through a brief flurry of looking for a job he could handle physically, and died fairly suddenly but not surprisingly while on a trip back east. I think it was something like pneumonia. He was buried in the rain near Waterbury, Connecticut, where he grew up. His father had been (I think) chief engineer for Waterbury Brass, which explains MIT, Brooks Brothers, and the box of tools. I could look up the dates, and a lot of the other items I'm not sure about (or never knew), but it doesn't really matter. The part that counts is the two years in Oak Ridge, back when we were all starting out together.

J. R. COE

The Carbon-14 Factory

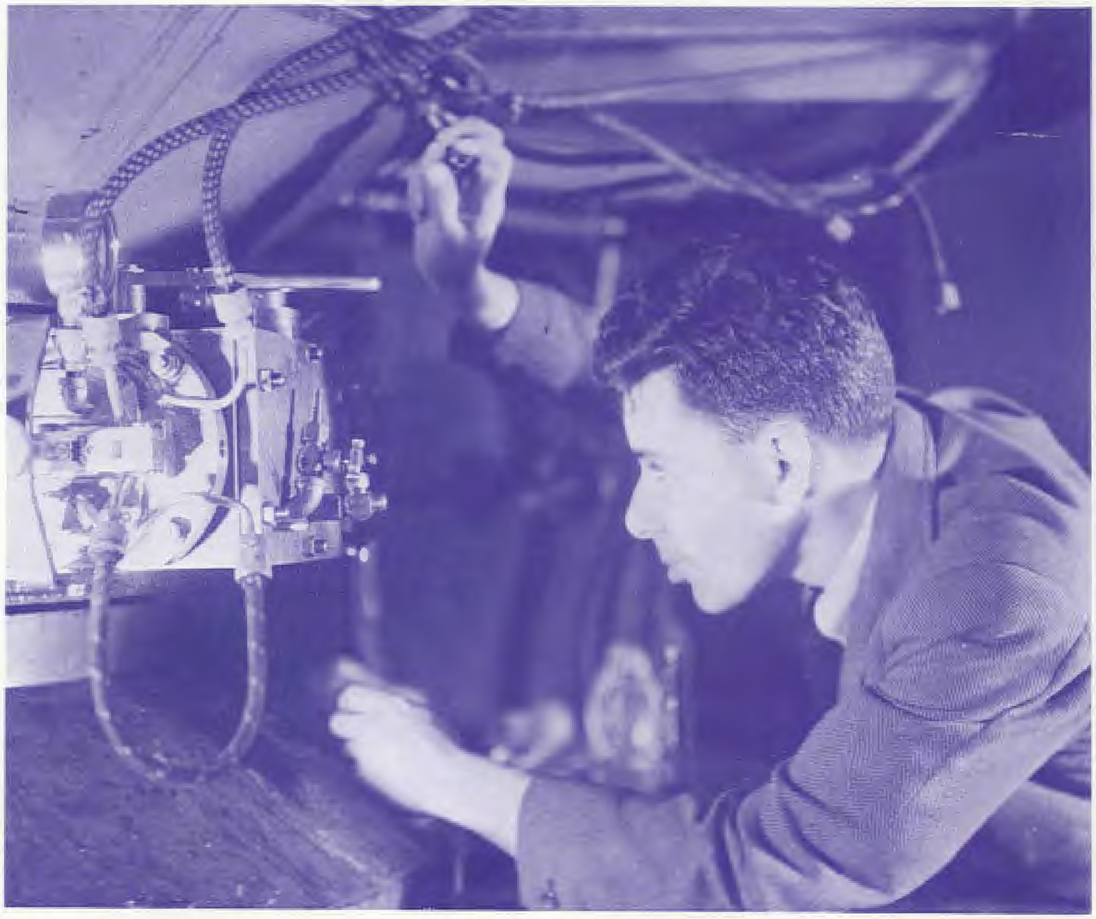
By A. H. SNELL

Carbon-14 was discovered in 1940 by Ruben and Kamen at Berkeley. It was made by neutron irradiation of nitrogen, $^{14}N(n,p)^{14}C$, a reaction which goes well with thermal neutrons. Ruben and Kamen used neutrons from the cyclotron, so when reactors became available during the war, with their tremendous abundance of slow neutrons, it was obvious that carbon-14 could be made in quantities previously far beyond contemplation.

The excitement and interest arose at the time because, of all radioactive tracers, carbon-14 seemed to be most important in the light of its implications in biological research. Its half-life was known to be long (estimates at the time ranged from 4000 to 20,000 years), and this made it hard to get even a microcurie of it from a cyclotron. It was natural, therefore, in 1944 for people like Louis Slotin, Dewey Norris, Ed Meiners, and me to take advantage of the Clinton Pile (as the Graphite Reactor was then called) and do something about the situation. (Waldo Cohn was in there agitating, too.) A carbon-14 factory would involve the insertion of a lot of nitrogen into the pile, together with some simple means of extracting the carbon-14 following irradiation.

M. E. ROSE

As a quick way to get going, we decided upon a system that used a solution of ammonium nitrate circulating through the pile, with an external extraction system for the carbon-14. Ammonium nitrate is highly water-soluble, and in itself would be free of unwanted radioactivity. We surmised that the carbon-14 would come off as $^{14}CO_2$ and could be trapped simply by bubbling the off-gas from the solution through a solution of barium hydroxide.



Louis Slotin, one of the Nuclear Age immortals, shows some of the intensity and enthusiasm that characterized him among his colleagues. This photo was taken by Art Snell at a newly installed target plate on the cyclotron at the University of Chicago in 1940.

So Dewey set up the carbon-14 factory. Early in 1945 an aluminum U-tube was inserted through the shield on the north side of the pile; it extended across the graphite region to the inside of the shield on the south side and contained 55 liters of concentrated ammonium nitrate solution. Outside of the shield on the north side were the circulation pump, the bubblers, and a shielded reservoir for cooling the solution and for collecting off-gas.

Soon Dewey was producing carbon-14 by the tens of millicuries. Inasmuch as the formal isotope production program of the AEC was not yet functioning, he shipped his material informally to users all over the country. In fact, during its period of operation, the factory produced a total of 440 millicuries and saturated the market. Dewey had some of the material analyzed mass-spectroscopically, thereby determining the half-life as 5100 years (modern values group around 5700 years—an important quantity in radiocarbon dating). Then, drawing upon my deep knowledge of high school chemistry, I suggested that some carbon-14 must be coming off as ^{14}CO and that, if he first extracted the $^{14}CO_2$ and then oxidized the remaining gas, he might get $^{14}CO_2$ of high specific activity, inasmuch as he would then be free of ordinary CO_2 that came from the air. He did this, and produced some carbon-14 of 40% isotopic purity.

This served to settle one of the questions of the time. There was a theoretical puzzle as to why the half-life of carbon-14 was so long. One suggestion was that carbon-14 might have an anomalously high spin that inhibited its beta decay back to nitrogen-14. Although everybody *thought* that the spin would be zero as in all the other even-even nuclides that had been looked at, nevertheless the spin had not been *measured*. So Dewey sent a sample of his 40% material to the late F. A. Jenkins at Berkeley—a well-known spectroscopist who was interested in this kind of thing. Jenkins ran the $^{14}C^{14}C$ band spectrum and found that the spin was indeed zero, so the theoreticians had to look elsewhere for the reason for the long half-life.

But the carbon-14 factory was also an infernal nuisance. Bubbles or irregularities in its circulation system made the pile operation unsteady, and after some weeks the solution would build up impurities that gave a disturbing radiation level around the pump, and the solution had to be periodically replaced. So, in November 1946, the factory was pulled out of the pile with long ropes, and the pile operators (stalwarts like Logan Emlet, Mansell Ramsey, Charlie Cagle, and Wells Stanley) breathed a deep sigh of relief. The AEC isotope program got started; the production was switched first to calcium nitrate slugs in the Oak Ridge pile and later to beryllium nitride slugs at Hanford.

The factory had served its timely purpose, and it was a long time before the more sophisticated production methods again reached the isotopic concentration of 40%.

The Discovery of Promethium

By L. E. GLENDENIN

In the story of the elements, Larry Glendenin, with his colleague Jack Marinsky, shares certain immortality, being destined to be listed forevermore as one of the two men who discovered promethium. Also at Oak Ridge, he devised a theory of the initial nuclide for the several fission product chains. He has been a member of the staff of Argonne National Laboratory since 1949.

L. E. GLENDENIN

In the spring of 1942, a section of the Metallurgical Laboratory at the University of Chicago was organized under the direction of the late Charles D. Coryell to carry out radiochemical research on nuclear fission. I joined the section in July and was informed by Coryell of the nature of the project. He said that our group would have to become "instant nuclear chemists" to carry out the task of separating and identifying some 30 or more fission product elements and characterizing their many radioisotopes. In late summer of 1943, the section itself underwent slightly asymmetric fission, with the smaller fragment proceeding to the newly constructed Clinton Laboratories at Oak Ridge with Coryell, and the rest of the section remaining at the Metallurgical Laboratory under the direction of Nathan Sugarman. For the benefit of those of us who were seriously considering giving up the bright lights of Chicago for the wilds of Tennessee, Coryell made a special trip to scout Oak Ridge and returned to describe it in glowing terms and, in fact, pronounced it "a scientific utopia."

I remember arriving in Oak Ridge in September during a rainy spell to find what looked like a frontier movie town in a morass of mud and boardwalks. It certainly bore no resemblance to the expected scientific utopia. For several weeks Coryell was obliged to defend his opinion on many occasions. He was, of course, absolutely right, and the next three years at Oak Ridge were destined to be the most challenging, fruitful, and happy period of my life.

M. BURTON

The graphite-moderated reactor was soon in operation, and we were busily engaged in making neutron irradiations of uranium and investigating new radioisotopes of the fission-product elements. The most difficult of these to separate, identify, and characterize were the rare earths because of their great chemical similarity, in particular the "praseodymium group" (the sequence Pr, Nd, and element 61). The last was one of the "missing elements" of the periodic table (i.e., not existing in a stable form in nature with any degree of certainty) and so was of considerable interest. In early 1943, one of the members of our section, Nathan Ballou, had discovered in this rare-earth region a low-energy, beta-emitting fission product with a half-life of 3 to 4 years. Subsequent work utilizing tedious qualitative fractionations by oxidative fusion with $NaNO_3$ and fusion with KOH showed that this activity and another rare-earth fission product with a half-life of 11 days, discovered by Harrison Davies in 1944, were not associated with lanthanum or praseodymium and must therefore be isotopes of neodymium or element 61.

At this juncture, Jack Marinsky and I decided to attempt the separation and identification of these radionuclides, using the new method of ion exchange chromatography pioneered by George Boyd and his co-workers, and shown by Waldo Cohn and co-workers to effect remarkable separations of the rare earths. Tests with radioisotopes of La, Ce, Pr, Sm, and Eu demonstrated that the rare-earth elements are eluted from a cation exchange column in reverse order of atomic number and that even adjacent elements are effectively separated. It was then clear that the unknown atomic number of a radioisotope of a rare-earth element could be positively identified by the order of elution in ion exchange chromatography. This we proceeded to do with the rare-earth fission products and with the radionuclides formed in neutron irradiation of neodymium. We were able to identify the three-year activity as an isotope (mass 147) of element 61, and the 11-day activity as an isotope (also mass 147) of neodymium. We also identified 1.7-hour neodymium-149 and a 47-hour isotope (mass 149) of element 61. This experiment was the first carried out to separate radioisotopes of element 61 from those of neighboring elements.

O. H. KLEPPER

In the summer of 1946, Marinsky and I left Oak Ridge for graduate work under Coryell, who had accepted a professorship at MIT. In the following year we researched the literature on the various claims to discovery of element 61 in nature and in cyclotron bombardments of praseodymium and neodymium. We decided to enter a claim as the rightful discoverers and began to consider an appropriate name for the element. Some of the possibilities to be discarded were phoenicium (phoenix rising from the ashes of nuclear fission) and clintonium (after Clinton Laboratories). The final choice of "promethium" was suggested by Charles Coryell's wife Gracemary, who died in 1965. The name refers to Prometheus, the Titan in Greek mythology, who stole fire from heaven for the use of mankind. It not only symbolizes the dramatic way in which the element is produced as a result of the harnessing of the energy of nuclear fission, but also warns of the danger of punishment by the vulture of war.

We announced our discovery of element 61 in September 1947 at the American Chemical Society Meeting in New York. With characteristic modesty, Coryell refused authorship on the discovery paper, insisting that his contribution had not been sufficient and that his name would detract attention from us. The name promethium was confirmed by the International Union of Chemistry on September 5, 1949, at the Amsterdam conference.

Memories of Charles Coryell

By JACOB A. MARINSKY

J. A. MARINSKY

In staffing the wartime Project, the Army selected draftees of the highest technical and intellectual caliber, so that when they were all together at Oak Ridge—GIs and officers—it was discovered that the average IQ of the enlisted men was ten points above that of the administrative officers. One of the drafted geniuses was Jack Marinsky, who moved with his bride into Milton Burton's house to care for the motherless two-year-old Burton in exchange for (otherwise-prohibited) conjugal billeting. He is now on the faculty of SUNY Buffalo.

Late in March 1944, a small contingent of soldier students and I were, upon dissolution of the Army Specialized Training Program, transferred from Purdue University, where we had studied chemistry, chemical engineering, electrical engineering, etc., to Oak Ridge. On our arrival we joined a number of other soldiers in a pool of the Special Engineering Detachment (SED) that had been established by the military as one answer to the manpower shortage that prevailed at that time. After being interviewed by Drs. C. D. Coryell and W. H Sullivan, I, with other soldiers and a few civilians, was assigned to X-10 on April 4, 1944. I remember clearly L. Riordan's startling revelation of the military objectives of the science-fiction-like establishment that I had somehow joined. Professor Coryell led us into the laboratory area, his shirttails flying. As soon as we entered the high-security area he pointed to a black, barnlike building on a hill. Charles did not restrain the excitement he felt in telling us of the nuclear reactor which was to become the focal point of our research activities.

There were seminars and lectures by the superstars of science (Wheeler, Teller, Nordheim, Wigner). I reflect many times on this most important period of my life. As a teacher, I know from this experience that the most effective study of science comes from close interaction of the student with his teachers. I have tried to achieve this effect in my development of students at the University at Buffalo.

Eventually I was assigned the problem of identifying a several-day gamma emitter in the rare-earth fission product region. With the cooperation of my more experienced colleagues, especially L. E. Glendenin, progress in the solution of this problem was quite rapid. Classical fusion methods were first employed to narrow the range of assignment. At this point the potential for chromatographic separation of rare earths by ion exchange was deduced from the excellent pioneer work of Paul Tompkins, George Boyd, Ed Tompkins, and Waldo Cohn.

J. A. MARINSKY

The sequence of rare-earth elution with ammonium citrate was then examined with J. X. Khym. It was shown that this sequence was the inverse of the atomic number of the rare-earth element. With this chemical basis for element identification, the definitive experiment which led to the discovery of element 61, promethium, was available.

At this point, much to my dismay, I was shifted to George Boyd's section to assist in the improvement of the process for isolation of Ba-^{140}La from the fission products. Improved yield of barium-140, essential as the trigger of the atomic bomb, was being sought and took precedence.

After several months, an interval that felt much longer, I was able to return to the rare-earth identification problem. The column experiment that had been postponed was successful. Element 61 was discovered.

At this point I must emphasize the importance of Professor Charles Dubois Coryell to me and to all members of Clinton Laboratories. Never had I met such an inspiring, exciting, energetic individual. Charles Coryell was special in every sense of the word: as a teacher, a scientist, a friend. Not one individual who has met him has not benefited from this experience.

He was a giant: as a scientist, he overshadowed his colleagues, and as a person he was beyond comparison. He loved people and gave himself and of himself to them. He was as well the most honest, the bravest, and the kindest of persons: the most impressive man I have ever known.

On the Manhattan Project, his peers looked to him for technical and supportive guidance. He set an unmatched example of honesty and integrity for them with respect to credits for individual research efforts.

When L. E. Glendenin and I suggested that he be listed as a codiscoverer of element 61, promethium, he refused. He insisted that his contribution had not been sufficient for this. He explained, as well, that his name would detract attention from us. Remember that Professor Coryell has received a good deal of recognition for the outstanding research in nuclear chemistry that evolved from the Manhattan Project. Note also that this recognition is the bare minimum because of his values. No one on the Project was less self-serving.

G. W. Parker and P. M. Lantz in Building 706C isolate rare-earth elements from fission products. Technetium and promethium were both produced and identified here (1946).

1946

On August 2, the first radioisotope shipment to the private sector was made from Clinton Laboratories, as one millicurie of carbon-14 produced in the Graphite Reactor was delivered to the Barnard Free Skin and Cancer Hospital in St. Louis, Missouri. For many years, A. F. Rupp played an important role in the safe production of radioisotopes for peaceful uses. The stable-isotopes program began under C. P. Keim, Leon Love, C. E. Normand, and others as ORNL operated some of Y-12's wartime calutrons (used for the separation of uranium isotopes for the Hiroshima bomb) for stable-isotope production. Enriched copper-63 and -65 were the first stable isotopes shipped from ORNL in 1946. It was also in this year that G. W. Parker and W. J. Martin isolated technetium (element 43) in measurable quantities from fission wastes.

The Biology, Metallurgy, Health Physics, and Power Pile Divisions were established. The Biology Division, which later became ORNL's largest division, searched not only for radiation injuries in exposed animals but also for the effects, at the cellular level, of beta, gamma, and neutron irradiation on living organisms.

Although new divisions were being formed, 90% of the research and development work was being carried out by the Chemistry, Physics, and Technical Divisions. Efforts included development of new methods (such as the Redox Process) of recovering plutonium, uranium, and other elements such as ruthenium-106 from laboratory wastes and irradiated reactor fuels by ion exchange and solvent extraction; the development and calibration of a crystal spectrometer for neutron diffraction and cross-section measurements; and partial completion of a design of a high-flux reactor.

Eugene Wigner, who led the group that developed the theory of nuclear reactors at the Metallurgical Laboratory, came to Oak Ridge on leave from Princeton University to become research director of Clinton Laboratories, succeeding Richard L. Doan, who had served under Director Martin D. Whitaker. Wigner and James H. Lum became codirectors, replacing Whitaker. Fred Seitz also arrived that year to assume the directorship of the newly formed Clinton Laboratories Training School.

In another educational development, the Oak Ridge Institute of Nuclear Studies was formed. William G. Pollard became its executive director. ORINS, representing 19 southern universities, established mutually beneficial arrangements between the Laboratory and member institutions in scientific areas. As a result, Laboratory staff members collaborated with faculty members and Ph.D. students in basic research projects.

On August 23, Wigner and Alvin Weinberg, a reactor physicist who had worked with Wigner in Chicago, decided to change the original high-flux reactor design to a simpler, more compact design using cooling with ordinary water instead of heavy water. Such a design promised an increased power density and a much higher flux of fast neutrons. The decision led to the initial development of efficient, aluminum-clad, water-cooled fuel elements. Physicists working with Weinberg in Oak Ridge demonstrated that, if ordinary water and natural uranium were arranged in an appropriate mutual lattice, the assembly would become marginally critical in large size. The result from this water lattice experiment showed that, if the uranium were enriched by only a few percent in uranium-235, compact power reactors could be built that used forced-water cooling.

ORNL's oldest information center, the Nuclear Data Project, began in the fall of 1946 when a group of physicists and chemists at Clinton Laboratories formed a committee called the Nuclear Data Group. Katharine Way, currently at Duke University, was chairman of the group, which was formed to keep abreast of the increasing amount of data obtained by nuclear research. The group issued monthly lists of the experimental results on nuclear properties reported in project reports and scientific journals during the previous months. Included were data on nuclear reactions, cross sections, masses, abundances, moments, and radioactivity. In the late 1940s the Nuclear Data Project moved to Washington, returning to ORNL in 1964 at the invitation of Alvin Weinberg. The project continues to prepare and publish a journal devoted to compilations and evaluations of experimental and theoretical results in nuclear physics.

Special Tribute

Samuel Colville Lind's international renown in the field of physical chemistry was established long before he chose to return to his native Tennessee and honor Oak Ridge with his final work. He was well past the traditional retirement age when he moved to Oak Ridge to consult for Union Carbide Nuclear Division, and had long since been singled out for worldwide recognition for his books on radiation chemistry, making his name also as President of the American Chemical Society and editor of numerous technical journals. But because, in his short time at the Laboratory—during which he was, for three years, Acting Director of the Chemistry Division—he reflected such distinction on ORNL by his presence, and with his death left such a large coterie of friends and admirers, we have chosen to include him for special tribute in this issue.

The Origin of Shielding Research at ORNL

By LORRAINE S. ABBOTT

Lorraine Abbott has been involved in technical writing at the Laboratory since her arrival in 1948. She is currently an editor and handbook compiler and an administrative aide in the Neutron Physics Division.

Robert J. Gray and Robert S. Crouse prepare to make photomicrographs of metal specimens—early work here in metallography.

One of the oldest continuous programs at ORNL is the radiation shielding program of the Neutron Physics Division. As would be expected, it had its origin at the X-10 Pile soon after that reactor went critical on November 4, 1943. Although the X-10 Pile (also called the Clinton Pile) was built as a pilot plant for the larger plutonium-producing Hanford Reactor already under construction, several features of the two reactors were quite different, one being the difference in their shields. The X-10 Pile was surrounded with 5 feet of barytes-haydite concrete between two 1-foot thicknesses of ordinary concrete. But since surrounding the higher-powered Hanford Reactor with concrete would have required a thickness too large to be practical, a laminated steel and Masonite shield was designed instead. A test of its adequacy comprised the first major experiment at the X-10 reactor.

C. E. Clifford, a Du Pont engineer at the time, participated in that first experiment and has engaged in shielding research ever since. The experiment was performed on top of the X-10 Pile, with a test section of the Hanford shield installed in a large hole extending through the pile shield down to the graphite moderator. Measurements of the attenuation of radiation by the Hanford sample were gratifying: the shield would be more than adequate. As it turned out, however, shortly after the reactor at Hanford had been in operation, the Masonite suffered severe radiation damage and decomposed, and a search began for alternative shield materials for a second Hanford Reactor.

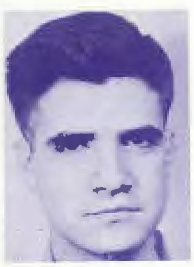
About that time, circa 1946, the Navy began feasibility studies for a nuclear-powered submarine and the Air Force began a similar study for a nuclear-powered aircraft. That same year, E. P. Blizard, a physicist employed by the Navy, was sent to Oak Ridge by Captain Hyman Rickover to attend the Clinton Laboratories Training School, which Rickover had had a hand in organizing to instruct selected individuals on matters nuclear. Blizard obtained permission to prolong his tenure at Oak Ridge to complete research for a doctoral thesis; however, in April 1947 he received a truncated telephone order from Rickover to start doing research at Oak Ridge on reactor shielding. In a letter to Professor John R. Dunning, his thesis advisor at Columbia University, Blizard wrote: "My plans have changed considerably during the last week due to circumstances beyond my control. . . . I have hopes that I will be able to find something suitable to the University of Tennessee since they have little or no objection to a classified thesis." But Blizard never obtained his degree. Instead, as his friend Herbert Goldstein later eulogized, "he became the father of reactor shielding."

E. P. BLIZARD

Blizard consulted with Clifford, who at that time was working with Ted Rockwell on the development of high-density concretes which Rockwell hoped would be used as reactor shields. The three men collaborated to test samples of the concretes in a 2-foot-square "core hole" through the back face of the X-10 Pile, which by then had been named the Graphite Reactor. The fast-neutron flux incident on the hole was increased by moving some of the fuel slugs in the core to a position immediately adjacent to the hole.

M. M. MANN

Use of the Core Hole Facility began in July 1947, and the several experiments performed during the next year included measurements on shield samples provided by Brookhaven National Laboratory and NEPA (Nuclear Energy for Propulsion of Aircraft), the samples for NEPA being tungsten-bearing materials considered for shields on nuclear-powered aircraft. From these experiments, several facts became apparent, one of which was the inadequacy of the Core Hole Facility due to cramped quarters and to the streaming of neutrons around the samples through the less-efficient reactor shield. Another was that secondary gamma rays produced by neutron interactions within the samples would be an important factor in shield design. Contemplating how to design a better shield facility, Blizard wrote to a friend in April 1948: "We expect to try a lid experiment. ... An alternative will be to drill another hole in the top. This, of course, would be a large, serious, and dangerous undertaking, but there are few things that cannot be done with sufficient perspiration and perspicacity."



C. FELDMAN

The lid experiment was eventually chosen, the "lid" consisting of a thin disk of enriched uranium placed on the outside of the original core hole. Thermal neutrons from the pile induced fissions in the disk, providing a local source of fission neutrons for experiments. Clifford suggested that a large tank of water be positioned adjacent to the fission plate so that the shield materials and radiation detectors could be submerged, reducing the radiation background. The resulting Lid Tank Shielding Facility began operation in mid-1949.

The LTSF proved to be an ideal facility in many respects. It could accommodate "full-scale" mock-ups with respect to shield thickness, and just as Rickover had planned, a shielding facility was available in time to test the shields designed for the nation's first nuclear-powered submarines. Other tests aided the design of shields for several stationary reactors. In its later days, during the last several years before the Graphite Reactor was shut down in 1963, the LTSF yielded a large quantity of fundamental data on individual shield materials which tested theories advanced by those engaged in analytical shielding.

L. S. ABBOTT

To some, perhaps the main contribution of the LTSF was that it dispelled forever the idea that reactor shielding was just a matter of deciding how much material to pile around a reactor. Realizing that shield physics should be pursued as vigorously as core physics, Blizard joined the ORNL staff in 1949. He soon headed a shielding program that expanded into a large effort for the abortive nuclear-powered aircraft program and prompted the construction of two larger shielding facilities—the Bulk Shielding Facility, the famous swimming pool reactor; and the Tower Shielding Facility. On cancellation of the aircraft program, the BSF was converted to other purposes, and the TSF was adapted to shielding research for nuclear weapons shielding, for SNAP (Systems for Nuclear Auxiliary Power) shielding, and, today, for stationary reactor shielding. But the experiments performed at the TSF nowadays are seldom of the mock-up type. Instead, the mock-ups are "constructed" on computers to a scale never envisaged by the experimentalists. The current role of the experimentalists is to devise and perform experiments that will test the calculational techniques. Blizard forecast this turn of events and saw its beginnings before his death in 1966, but those of us still with the program are sure that even he would be surprised at how completely the computers have taken over the reactor shielding program he began.

1947

On January 1, the nation's atomic energy activities were transferred from the Manhattan Engineer District to the newly established United States Atomic Energy Commission as called for by the Atomic Energy Act of 1946. The name of Clinton Laboratories was changed to Clinton National Laboratory. The Laboratory's major efforts at this time were radioisotope production; chemical reprocessing; basic research in physics, chemistry, and biology; and design work on a high-flux experimental reactor under Miles C. Leverett. Kellex Corporation was under contract to do design work and construction drawings on the reactor.

A program of neutron absorption cross-section measurements with a "pile oscillator" gave less than 5% error for the 69 elements which could be obtained in concentrated form. Using this device, Herbert Pomerance discovered that pure zirconium could be a useful reactor material because of its low absorption cross section.

E. P. Blizard, C. E. Clifford, and Ted Rockwell collaborated on tests of high-density concrete samples to determine if they could be used for reactor shields. This shield testing was done at the Core Hole Facility at the Graphite Reactor in 1947. In 1949 the Lid Tank Shielding Facility was built at the Graphite Reactor to test various shield designs, including those planned for use in the nation's first nuclear-powered submarines. Blizard, who has been called the father of reactor shielding, headed a shielding program in 1949 that expanded with the construction of two larger shielding facilities to test shields for the proposed nuclear-powered aircraft—the Bulk Shielding Test Reactor and the Tower Shielding Facility.

Researchers prepared two ruthenium-106 sources, one plated on metal and the other on plastic, for research use by two medical doctors at the ORINS hospital for cancer studies.

ORNL biologists—particularly Norman Giles, W. K. Baker, Alexander Hollaender, George Stapleton, R. F. Kimball, and Elizabeth von Halle—began studies on the oxygen effect, discovered in England in 1947. The British evidence showed that the presence of oxygen increases the likelihood of chromosome breaks in living cells exposed to radiation. Later it was found by ORNL biologists that the oxygen effect can be counteracted by a variety of chemicals and that most of these chemicals seem to protect against radiation damage by removing oxygen.

Wigner, who predicted in 1943 that radiation damage to reactor materials could pose a serious problem to design and contruction of postwar power reactors, instigated a program of research in which basic and applied aspects of radiation damage were studied by metallurgists and physicists. The theory of radiation damage was first developed by Fred Seitz. Sid Siegal and D. S. Billington were the principal investigators in this research program, part of which involved the construction of a hot cell in the Graphite Reactor building that would permit the first remote manipulations and measurements of highly radioactive metals and alloys. Some early measurements made in the hot cell were on prototype fuel elements (for the high-flux reactor) that had been irradiated in the Hanford reactors. These measurements confirmed that radiation damage was severe but not sufficient to nullify the design parameters established by the Technical Division. (Subsequent studies by these investigators revealed that order-disorder reactions and precipitation from solid solution were significantly affected by neutron irradiation. These observations led ultimately to the concept of radiation-enhanced diffusion. Electrical measurements of irradiated silicon and germanium were initiated under this program. Later, in-pile measurements at low temperature by J. H. Crawford, J. W. Cleland, and J. C. Pigg were to provide valuable information for the design and protection of space probe instrumentation.)

Wigner's leave from Princeton ended in August, and Monsanto Chemical Company remained only as an interim contractor for operating the Laboratory. Thus, Clinton National Laboratory plunged into a period of administrative uncertainty between August 1947 and March 1948. This situation was aggravated further near the end of the year by a series of AEC decisions. In November the AEC ordered Kellex Corporation to halt its design work on the high-flux reactor. And on December 27 ("Black Christmas"), Clinton National Laboratory's aspiration to be the AEC's lead laboratory in reactor development was nipped in the bud when the AEC announced plans to:

- consolidate reactor development activities at Argonne National Laboratory near Chicago;
- retain Clinton National Laboratory as the AEC's center for nuclear-related chemical engineering research and for radioisotope production;
- build experimental reactors at another site, later chosen to be Arco, Idaho.

1948

A number of drastic changes took place in a year in which the future of the Laboratory seemed in doubt. Following the AEC's announcement on consolidation of reactor development activities came the transfer of many Clinton employees to Argonne National Laboratory and to the Navy reactor effort. The AEC decided to locate the ORNL-designed high-flux experimental reactor—to be called the Materials Testing Reactor (MTR)—at Arco, Idaho, now designated the National Reactor Testing Station. The AEC authorized funds for the construction of an MTR mockup at the Laboratory; the mockup later was modified into a research reactor called first the Low-Intensity Training Reactor (because it was used to train operators for the MTR at Arco) and, later, the Low-Intensity Test Reactor (LITR).

The Metallurgy Division, which was established by Wigner in 1946 and headed by W. A. Johnson, began work that led to two important contributions to the successful operation of the MTR in the 1950s: the development of new fuel elements and the fabrication of a beryllium reflector (to scatter neutrons back into the

More from Kyger . . .

In the course of developing fuel elements for the MTR, we arrived finally at the point of casting the first ingot of U-Al alloy where the uranium was fully enriched. (Uranium-235 had a much more respected aura in those days, when it was a rare and treasured material, than it does today.) Charlie Smith was in charge of the "rolling mill building" and in charge of the operation in question. The auspiciousness of this occasion called for the presence of a group of Washington dignitaries and security agents. When the starting ingredients were melted but before the ingot was poured, Smitty reflected his steel mill background by walking over to the melt, examining it with satisfaction, and flipping his cigarette butt into it! This led to some disorderly behavior on the part of the visitors, and for weeks or months I had a parade of FBI agents coming through my office inquiring about the reliability and loyalty of one Charles Dominic Smith.—Jack A. Kyger core). The MTR design drawn up at ORNL called for a type of fuel element that had never been made before—curved plates of highly enriched uranium-aluminum alloy clad with aluminum. Metallurgists developed successful methods of fabricating these fuel elements and found a way to can the uranium in aluminum jackets so as to reduce the rupture problem that plagued fuel elements in the Graphite Reactor. ORNL researchers supervised the production of MTR beryllium pieces by powder-metallurgy techniques worked out elsewhere and developed methods of fabricating massive beryllium metal pieces to close tolerances. To provide for cooling of the beryllium reflector (the first ever to be used in a reactor), metallurgists adapted a rifle-drilling technique to boring long, small-diameter holes in the beryllium.

Researchers in Oak Ridge, including Warren Grimes, Charles Barton, Glenn Clewett, and William Leaders, developed a method for separating zirconium from hafnium. Hafnium-free zirconium with its low neutron absorption was a promising structural material for the proposed homogeneous reactor and for fuel cladding material in Navy reactors being developed for the nuclear submarine program headed by Navy Captain Hyman Rickover, who first began learning reactor technology at Oak Ridge in 1946. The Navy reactors used a pressurized version of the water-moderated MTR (with the zirconium replacing aluminum as structural material) as a result of discussions between Rickover and Oak Ridge researchers led by Weinberg. Thus the pressurized water reactor—widely used in American nuclear power plants today—had its origins at ORNL.

In chemical reprocessing developments, ORNL chemical engineers proceeded with semiworks and pilot-plant demonstrations of the Redox and Hexone-25 Processes in conjunction with related programs at the Hanford, Argonne, and Knolls Laboratories.

Other changes at Clinton National Laboratory included a new operating contractor, a new name, and a new research director. On March 1, Carbide and Carbon Chemicals Company (predecessor here for Union Carbide's Nuclear Division) assumed responsibility for operating Clinton National Laboratory, whose name was changed to Oak Ridge National Laboratory. (Carbide had already been operating two other Oak Ridge installations—the Y-12 Electromagnetic Separations Plant and the K-25 Gaseous Diffusion Plant.) Also on March 1, Alvin Weinberg, Director of the Physics Division, who came to Oak Ridge from Chicago in 1945, was named Research Director, a post that had been vacant for many months. Laboratory management and researchers began to see ORNL as the AEC did: a center for nuclear-related chemical engineering and chemical research, for production and processing of stable and radioactive isotopes, for training programs (some in cooperation with the Oak Ridge Institute of Nuclear Studies), and for basic research programs in physics, health physics, metallurgy, and biology. A biology program involving experiments with mice was initiated to establish an estimate of the radiation-induced mutation rate in mammals.

1948—Survival and Purpose

By ALVIN M. WEINBERG

Alvin Weinberg, Director of ORNL for 18 years, is now head of the Institute for Energy Analysis, a branch of Oak Ridge Associated Universities.

Oak Ridge National Laboratory really had two, somewhat separate, beginnings. Most of the histories tell about Clinton Laboratories as a pilot plant for production of the first grams of plutonium; of the X-10 Graphite Pile, the first nuclear reactor that produced sizable amounts of heat and was cooled by forced convection; of the imposing, almost grim, concrete-shielded radiochemical plant where large-scale separation of

R. S. LIVINGSTON

plutonium from fission products first was achieved; as the place where man first dealt with radioactivity on a large scale. This was the time when, under the guidance of Director Martin D. Whitaker and Research Director Richard L. Doan, Clinton Labs acquired its unique flavor: a mixture of science, technology, industry, academia, and government. The Laboratory was operated by the University of Chicago; was built by Du Pont and served as a training ground for "Du Ponters" who were headed for Hanford; and was paid for, and supervised—rather gently, I would say—by the Manhattan Engineer District.

Clinton Labs was at that time part of the Chicago-based Metallurgical Project directed by Arthur H. Compton. There was very close liaison between the Met Lab and the people at Clinton. The chemistry of the fission products and of plutonium was developed both at Chicago and at Oak Ridge, and much of the early design on the X-10 reactor originated in Eugene Wigner's theoretical group at Chicago. It was therefore natural that when Wigner turned his attention to the future of nuclear energy, he considered Clinton as the site for both a large laboratory devoted to the development of nuclear power, and a school where industrial and university scientists and engineers could study the nuclear technology that had been developed during the war. He promoted the idea among the people immediately around him—Kay Way, Gale Young, Fred Seitz, and me—and by early 1945 we were making active plans to move to Oak Ridge.

I was the first of Wigner's group to arrive, in May 1945. Wigner came in 1946 as Research Director, along with Fred Seitz, as Director of the Clinton Laboratories Training School, Gale Young, and Kay Way. Clinton Laboratories, under Eugene Wigner's leadership in 1946–47, began a new life.

There were no blueprints for the new institution. Our experience was shaped by the Metallurgical Laboratory and by the wartime Clinton Laboratories. These institutions had had an immense advantage: everyone in each laboratory had understood and believed in its underlying purpose. By contrast, during those days immediately after the war, everything was ambiguous: the role of nuclear power, the relative priority to be given to reactors for power and reactors for military propulsion, the role of basic nuclear research, the responsibility of the Laboratory to the scientific educational community of the Southeast. Then there were many practical questions: who would operate the Laboratory, who would be its permanent director; indeed, would the Laboratory survive?

F. M. HAILEY

Wigner saw many of these issues with great clarity. I remember his outline for a permanent Laboratory staff of about 3500, which seemed at the time impossibly ambitious since, at the end of the war, Clinton Laboratories had some 1000 employees. All the present scientific and engineering disciplines were represented in Wigner's plan for the future. Recognizing the importance of the biological aspects of nuclear technology, he persuaded Alex Hollaender to join the Laboratory and expand the small biology group into what eventually became the largest division of ORNL.

Some purposes of Clinton Labs were never in doubt—in particular, the production of radioisotopes. If at some time a heavenly angel should ask what the Laboratory in the hills of East Tennessee did to enlarge man's life and make it better, I daresay the production of radioisotopes for scientific research and medical treatment will surely rate as a candidate for the very first place. The Laboratory deserves great credit for forging radioisotopes into a major technique that now pervades large parts of the physical, biological, even social sciences.

None doubted that the X-10 reactor was important and had to be kept in operation. But what about nuclear power? Three main possibilities claimed attention. In 1946–47, Captain Rickover and his busy young men pushed for nuclear submarines—a possibility that had been discussed rather widely at the Metallurgical Laboratory during the war. The chemists at Clinton wanted a super water boiler that could be used to produce large quantities of radioisotopes. This idea merged with the one for a homogeneous breeder reactor and in turn was displaced by a high-flux reactor, when we realized that before anything as ambitious as a homogeneous breeder could be tried, we would have to test materials in a high-radiation environment. Thus was born the Materials Testing Reactor. And from the MTR emerged the first submarine prototype — essentially a pressurized version of the MTR. But there was also Farrington Daniels, who insisted that what was needed was a demonstration of electric power from nuclear energy as quickly as possible. His Daniels pile—a gas-cooled power reactor not too different in principle from the modern HTGR (though Daniels originally proposed beryllium oxide, not graphite, as the moderator)—received considerable support from industry as well as from the Manhattan District.

A. M. WEINBERG

These threads of reactor development—the breeder, which required the MTR; the pressurized-water Navy reactor; and the Daniels pile—gave purpose to Clinton Labs in those days, at least as perceived by many of the staff. But the situation was complicated: various members of the Laboratory saw its purpose differently. For those associated with chemical pilot plant operations, continued pilot planting of new separation processes at Hanford was central. For those attending the Reactor School, Clinton meant their introduction to what could become an extremely important new power source. For the biologists, the Laboratory was a home of basic biological research. For those working with radioisotopes, purpose enough was to be found in their production and distribution. And for the basic physical scientists, there was always plenty to do in exploiting that remarkable new scientific tool, the X-10 Graphite Reactor.

I have spoken of purpose, because, in final analysis, purpose determines most of the rest. Where the purpose is clear, the means follow; where the purpose is unclear, the means are somewhat irrelevant. But even purpose is secondary to survival. I recall in later years a little game Norris Bradbury, former Director of Los Alamos, used to play at the Laboratory Directors' meetings. He asked, "Suppose your laboratory were obliterated by some act of God; would the authorities reestablish it?" This was a particularly frightening thought, since the survival of Clinton Labs had hung in dramatic balance at Christmas 1947. The Lab had been without a research director for many months, and the quest for a new director had been fruitless; Monsanto, then the operating contractor, was barely holding on until the University of Chicago took over again; the General Advisory Committee had recommended that all reactor work be consolidated at Argonne, and that the MTR *not* be built at Oak Ridge. Pity Jim Fisk, an excellent and gracious fellow, who as the Commission's Director of Research, was the harbinger of the bad news: no MTR, and a new and unknown contractor,

M. C. EDLUND

A. ZUCKER

Union Carbide, to replace Monsanto and the University of Chicago. How he withstood the abuse we heaped upon him at those well-lubricated Christmas parties I shall never understand. (Jim later served for many years as president of Bell Labs, so he apparently survived very well.)

When Clark Center, then General Manager of Carbide and Carbon Chemicals Division at Oak Ridge, asked me to become Research Director early in 1948, I readily accepted, because I saw many elements of great strength that may have been less apparent to those in Washington and elsewhere who were not as confident of Oak Ridge's future: the unique position of ORNL vis-a-vis the educational institutions of the South; the strength that Carbide would bring to the Laboratory in dealing with the AEC; the perception, still dim but forming in my mind, that nuclear energy, and particularly the breeder, really was important; but most of all, the imagination, the expertise, and the capacity for sheer hard work of the many people at ORNL.

As things turned out, ORNL survived admirably, much better than some around the Commission in 1947 had expected. It is a curious coincidence that as I write these words more than a quarter of a century after Black Christmas, the entire nuclear enterprise should be facing a crisis of survival. No matter what the fate of the various nuclear moratoria bills, these great debates will affect the whole nuclear enterprise, and therefore ORNL. Come what may, ORNL will of course survive, just as it did some 30 years ago. Parts of ORNL will forge new purposes. But for both of these, survival and purpose, ORNL has a strong tradition upon which to draw. Those qualities that stood ORNL in good stead then—confidence, intelligent hard work, and vision—ORNL possesses in abundance now. The future should be exciting, rewarding, and—above all—purposeful.

George Parker recalls . . .

Some of the most memorable periods in the early history of the Chemistry Division Hot Laboratory for highly radioactive operations were the occasions when it was necessary to use difficult or at least very expensive hot-cell operation.

Out of many such procedures, one in particular stands out as a reminder of the past. This involved the technique of removing the aluminum jacket from a highly radioactive uranium "slug" following its normal activation cycle in the Oak Ridge Graphite Reactor.

Since the jackets were not bonded, a simple mechanical operation was all that was required, and this was supplied with the help of a hot-cell rotary crew of whoever happened to be available, including some old ORNL hands like Henri Levy, Ed Nicholson, and me as well as others who have long since left the Laboratory: Ed Brady, Louis Stang, Ed Tompkins, etc.

The touch-and-go procedure we used was to allow each individual only 5 to 10 seconds participation before he was recalled and the next individual was put to work to continue the process. The initial step was to lift the slug from the lead cask with a pair of suitable tongs and to clamp the slug in a bench vise with the welded head upward. The second step required the cutting of the thin aluminum with a pair of side-cutters and to start pulling a spiral strip of aluminum off the end. The third step took the spiral halfway down the length of the slug, and the fourth step required releasing the bench vise and inverting the slug so the remainder of the cladding could be pulled off in one last spiral. The last step was to place the slug back in the cask. This procedure never required more than 60 total seconds, and no one ever got more than a fraction of his permissible exposure, since he was always glad to get the job done as quickly as possible.—G. W. Parker, former member of the Chemistry Division when it began in 1943 and currently in the Chemical Technology Division, working on transient release in the LMFBR programs.

Special Tribute

Everitt Pinell Blizard served on the Laboratory staff from 1946, as a protégé of Captain Hyman Rickover, until his death in 1966, at which time he was the first director of the newly formed Neutron Physics Division. In that time he earned the name of "father of reactor shielding." Of the many words said in tribute to him are these: "Everitt Blizard has had a greater effect on the development of the science and technology of radiation shielding than any other single person. Almost from the inception of the nuclear programs for peaceful purposes, he assumed a position of leadership in the field. ... Radiation shielding of all types, throughout the world, has felt his touch as experimenter, teacher, administrator, editor, and advisor."

From Ed Brady . . .

T. Harrison Davies, a group leader in Charles Coryell's section, was frequently exasperated by the tendency of some of the chemists in his group to ignore the fume hoods, sometimes sending clouds of acid fumes into the air. One afternoon after some particularly strong remarks from him, a few of us composed the following poem, drawing heavily upon Robert W. Service for inspiration. The only reason Nate Ballou was chosen as the victim was because the meter and rhyme of "The Strangling of Nate Ballou" fit much better with the "Shooting of Dan McGrew" than would "The Strangling of Larry Glendenin."—E. L. Brady, now Associate Director for Information Programs, National Bureau of Standards.

THE STRANGLING OF NATE BALLOU

L. E. GLENDENIN AND E. L. BRADY

1944

A bunch of us boys were fuming it up,
Ignoring the nearby hoods.
The kid that handles the centrifuge
Was swirling the usual cruds.
And back of the bench with a belching tube,
His profile hidden from view,
Stood a stalwart form in the midst of the storm,
The chemist that's known as Ballou.

When out of the hall that was clear and cool
And into the din and the smoke,
There staggered a group leader, gasping for breath,
And he looked about to choke.
His face was mottled with fiery red,
And the anger within him burned.
As he cast his eye about the room,
Everyone coughed and squirmed.

His eyes went rubbering 'round the lab,
And he seemed in a kind of daze
'Til at last that spewing test tube fell
In the way of his wandering gaze.
He trembled and shook, had an awful look,
But began as meek as a lamb.

"Now, boys," said he, "You all know me,
And none of you gives a damn,
But I want to state, and my words are straight
And I'm mighty sure they're true,
That one of you is a hound of hell,
And that one is Nate Ballou."

But Nate just shook the tube some more,
And fresh clouds billowed out.
This was too much for T.H.D.
And he roared, "You stupid lout!"
And he lunged at Nate with a cry of hate
And gripped him by the throat.
Nate tried to speak, but soon grew weak,
And his face began to bloat.

The end was near, we were sick with fear
As he fell out in the hall.
And we all stood 'round without a sound
And watched him pitch and fall.
His eyes were red and his nose had bled,
And his face was of purplish hue.
And there in the hall a corpse was all
That was left of Nate Ballou.

1949

The AEC approved a research and development program at ORNL that would lead to the construction of an aqueous homogeneous reactor using a solution of enriched uranium sulfate in heavy water as fuel, moderator, and coolant, and demonstrating that a circulating liquid-fuel system could be used for producing electrical power. Weinberg had pressed hard for such a reactor approach because it avoided the high cost of fabricating fuel elements and offered the possibility of continuous chemical processing of the fuel. Tests were made on hafnium-free zirconium to see whether it could withstand the high temperatures required in a homogeneous reactor.

During the first half of 1949, development work began on the use of tributyl phosphate as the active extractant for uranium and plutonium in the Metal Recovery Process. The similar Purex Process, which has become the worldwide method of recovering uranium and plutonium from spent reactor fuels, was developed during the latter half of 1949. Modifications of the Purex and Metal Recovery Processes were eventually employed in feed materials plants such as the one at Fernald, Ohio.

In September, the AEC asked ORNL to set up an Aircraft Nuclear Propulsion (ANP) Program to conduct research for the United States Air Force on building an airplane powered by nuclear energy. The program was set up in 1949 under Alvin Weinberg. Since 1946, ORNL had done research on shielding and radiation damage for the Air Force's NEPA Program (Nuclear Energy for Propulsion of Aircraft) at the S-50 Thermal Diffusion Plant near the K-25 Gaseous Diffusion Plant.

ORNL achievements in 1949 included production of cobalt-60 to replace more costly radium for cancer therapy and to aid the U.S. Weather Bureau in predicting water yield from snow; development under P. R. Bell of a scintillation spectrometer for mapping the distribution of radioactive tracer agents in human organs and for locating uranium ore deposits; preparation of carbon-14-labeled vitamin K for studies of the mechanism by which vitamin K aids blood clotting; development of an ionization chamber for monitoring power levels in reactors; use of ion exchange separation to isolate radioactive rare earths in pure form from uranium fission products, and subsequent spectrometer studies of these elements' radioactive disintegration schemes; development of a thermistor adapted for use on weather balloons for recording upper air temperatures; installation of ORNL's first Van de Graaff accelerator; and establishment of ORNL's first cancer research program (under Jacob Furth) to study cancer and other pathological changes induced by radiation.

A youthful research director breaks ground ceremonially for the "new Research Building." Among the witnesses to this 1950 event were S. C. Barnett, F. C. VonderLage, and D. W. Cardwell.

1950

ORNL's second reactor, the Low-Intensity Test Reactor (LITR), went into operation, offering the highest neutron flux available for research. The first photograph of a reactor core in operation was taken, showing the blue glow of Cerenkov radiation emanating from a reactor core submerged in water. This research reactor was used to test effects of radiation on possible construction materials for reactors and to produce rare radioisotopes such as chromium-51.

ORNL's chemical engineering research included initial work on the Amex and Dapex solvent-extraction processes for recovery of uranium from ores (carried out under the direction of K. B. Brown). The processes, developed over a five-year period, were used extensively in domestic and foreign uranium mills. Later, the processes were adapted to the recovery of a variety of other metals and were modified for recovery of vanadium, molybdenum, thorium, and strontium-90 used in the Systems for Nuclear Auxiliary Power Program of the 1960s.

The Oak Ridge School of Reactor Technology opened in March, continuing the type of educational program pioneered in the Clinton Laboratories Training School that operated in 1946–47. ORSORT trained industrial and university scientists, engineers, and fellowship students in reactor operations, chemical reprocessing, supervision, and design evaluation. The school closed in 1965, when it became apparent that universities could provide training in reactor technology.

Clarence E. Larson, named ORNL Director February 1, presided over the Laboratory's $20 million expansion program, as nine separate buildings were completed, four buildings underwent large-scale modification, and additional space was found in buildings at Y-12. Among the technical achievements in 1950 were: attainment by John Dabbs and L. D. Roberts of the lowest temperature ever for that time (0.04°K above absolute zero); development of ways to use neutron activation analysis for detecting trace amounts of impurities in materials; use of a photographic adaptation of the scintillation spectrometer (based on the observation that certain crystals emit light flashes when irradiated) for accurately measuring energies of radiation emitted by radioisotopes with short half-lives; characterization by E. O. Wollan and C. G. Shull of the antiferromagnetic state of matter by neutron diffraction; separation of stable mercury isotopes to measure the isotopic effect in superconductivity; and the development of a process for recovery of the radioisotope cesium-137 from ORNL's radiochemical wastes. Between 1943 and 1950, ORNL had made nearly 20,000 shipments of radioisotopes, as applications expanded to include treating cancer, detecting childhood diseases, battling insect pests, gauging eggshell thicknesses, and aiding in the identification and diversion of oil and gasoline products flowing through common-carrier pipelines.

Waldo Cohn and C. E. Carter were the first to employ the technique of ion exchange chromatography, originally developed for separating fission products, to separate and identify the constituents of nucleic acids, which are polymers approximating the size of proteins. Use of this technique led to the concept that one kind of nucleic acid (ribonucleic acid, or RNA) has the same general structure as the other (deoxyribonucleic acid, or DNA). This achievement had a significant impact on biochemistry worldwide.

ORNL's 86-inch cyclotron, which was first operated November 11, produced the world's most intense proton beam. Assembled under the

And K. Z. again . . .

We developed a new meter to monitor alpha radiation. With it we could get down on the floor and into the dark places of the Laboratory to find alpha contamination. It is not surprising then that we called it Pluto, for Disney's bloodhound. A few months later, security made us change the name; it sounded too much like plutonium. Thus the origin of "Snoopy."—K. Z. Morgan direction of Robert Livingston, it was used in studying the basic physics of proton-induced nuclear reactions, in inducing radiation damage in materials under study, in supplying a source of fast neutrons for biological research, and in producing isotopes such as gallium-67 that cannot be produced in nuclear reactors. The cyclotron was designed initially for the production of curie quantities of polonium-208.

A technical advisory board of outstanding U.S. scientists met at ORNL to evaluate various aircraft reactor designs and recommended that an experimental aircraft reactor be built at ORNL, inasmuch as the Laboratory had made progress in reducing shield weight.

Weinberg was looking for ways to bring reactor development back into Oak Ridge National Laboratory. His hopes were buoyed by the participation of ORNL with Argonne in designing the Materials Testing Reactor and by the promise of a homogeneous reactor experiment and an aircraft reactor project. These projects came to fruition in the early 1950s. According to Richard G. Hewlett and Francis Duncan in their *A History of the United States Atomic Energy Commission: Atomic Shield, 1947-52*: "The scope and variety of reactor development at Oak Ridge was a tribute to Weinberg's efforts to make the Laboratory a national reactor center."

Radiation Biology at ORNL

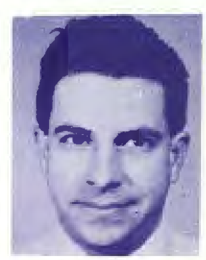
By JACOB FURTH

This historical account of early radiation biology, by the Distinguished Emeritus Professor of Pathology of Columbia University, forms part of an "Autobiographical Essay" that appeared in Cancer Research, Vol. 36, No. 3. It is with that journal's and Dr. Furth's kind permission that we include it as a most appropriate reminiscence in an issue such as this.

Late in 1949, a cordial invitation came from Alexander Hollaender to join Oak Ridge National Laboratory, which I accepted following consultation with Shields Warren, a trusted statesman in the American scientific world.

J. W. T. DABBS

The invitation was a bonus received for the research on biological effects of radiations prior to learning about the consequences of the atomic bomb. The research began with induction of leukemia by x rays (1929–1932), followed by the discovery of ovarian and other tumors as a late effect, and attempts to cure leukemia by external gamma radiation. The latter led to the conclusion that external radiation cannot destroy all leukemic cells without killing their hosts. Rescue of animals from lethal irradiation by bone-marrow transplant was a later development. I keep dreaming of a project to establish deep-freeze storage banks of bone marrows of normal people for possible use later in their lives or, better yet, to try to grow their marrow stem cells and freeze these away.



R. H. WARD

While at Cornell, I accidentally fell into the area of radiation neoplasia. I was unaware of the gigantic developments in radiation physics which had brought Nobel prizes to a succession of scientists (of the 22 Nobel laureates until 1948, 11 were radiation physicists) and had remained ignorant of these while teaching radiation pathology at Cornell. In Hollaender's splendid Biology Division, I was brought up to date by taking a course in radiation physics, and I learned about the many-sided problems it presented to radiation biologists.

Aided and guided by members of other sections of the Biology Division, I was able to make a thorough study of several problems and to analyze the hormonal consequences of the ovarian tumors, which I carried with me from Cornell. (The granulosa tumors produce hypervolemia with anemia; the luteomas cause hypervolemia with erythremia. Both were quantitated by isotopic techniques.) A few of our observations remained unpublished (e.g., the "impossibility" of killing mice with carbon-14). Others, notably the role of hormones in neoplasia, opened avenues of research that have been keeping me busy ever since. The breakthrough in this direction came from an analysis of Gorbman's reports that iodine-131 induces tumorous changes in the pituitary, which he attributed to "stress" or radiation. Our studies led to an incrimination of a derangement of the thyroid-pituitary axis, demonstrating how this can be manipulated to produce at will either thyroid or thyrotropic pituitary tumors. J. Dent performed a critical experiment, induction of thyrotropic pituitary tumors in mice by surgical thyroidectomy done under a binocular microscope, leaving behind a parathyroid.

J. FURTH

Having learned about thyroid-thyrotrope homeostasis, we raised the question: What is the nature of the pituitary tumors that have been induced independently by several outstanding investigators? Transplantation experiments indicated that they were invariably mammotropic hormone (prolactin) secreting. Nowadays in characterizing a tumor, we tend to rely on rapid test-tube reactions that are often incomplete or of dubious validity. When puzzled, I turn to the isologous animal host to tell me the character of a given functional neoplasm.

L. D. ROBERTS

The pathogenesis of the acute radiation death with anemia also puzzled me, since RBC are radioresistant. The appearance of lymph nodes loaded with hemosiderin cells gave a clue. The lymph ducts, which are normally free of erythrocytes, were full of them. Diversion of erythrocytes into the lymph stream, a one-way track, solved this puzzle. (This was missed in the otherwise classical treatise describing the pathology of acute death among the Hiroshima casualties.) But why the diversion? Thrombocytopenia was known to be associated with radiation anemia (E. Cronkite). Within minutes after platelet perfusion, the bloody lymph became clear. An editor of one prestigious journal, prejudiced by some work of his own on permeability of cutaneous lymphatics, rejected our paper, but another journal, *Blood,* accepted it. This was an exceptional experience. Conditional acceptance of a hastily submitted paper was not rare in my life. (Having been on the editorial boards of several journals as endocrinologist, microbiologist, and oncologist, I learned to appreciate the time-consuming job of reviewers—time given freely and anonymously.)

H. C. McCURDY

After mastering the preparation of platelet transfusion, I applied it to a patient given supralethal doses of radiation for leukemia. Fair recovery followed, but death from leukemia was not prevented. Our efforts to preserve platelets as we had preserved living normal cells by freezing failed. Plateletpheresis is now effectively used to aid patients with platelet deficiency disorders.

When Shields Warren (founder and Chairman of the Division of Biology and Medicine of the Atomic Energy Commission) asked me to take part in a gigantic interdisciplinary study of an experimental atomic bomb explosion, known as Operation Greenhouse, he gave me ample extra support and permission to do anything else in which I was interested, with the budget that had been appropriated to my section of experimental physiology and pathology.

In studies of radiation-induced leukemias of various types and the relative biological efficiency of diverse types of radiations, my senior associate, A. Upton, did a "lion's share" of the work. Research on radiation-induced cataracts was so precisely done by two expert practicing ophthalmologists as to serve as a biological dosimeter in animals subjected to various types of ionizing radiations. In these and other areas of radiation biology, I was mostly a manager or catalyst. So shielded, I was able to expand research in novel directions related to hormonal "politics."

F. KERZE, JR.

The well-financed, newly funded radiation biology laboratories of the Atomic Energy Commission created tough competition in almost every area of radiation research. In our Biology Division, Hollaender wisely opened a new subdivision devoted to radiation protection in mammals. This and other scientifically attractive and practical radiation-related problems became extremely competitive ("bandwagon") areas. I yearned to return to the free university life. So I welcomed the invitation of Sidney Farber to join his "Jimmy Fund" (Children's Cancer Research Foundation) as his Associate Director and Chairman of the Experimental Pathology Section, with no restrictions on investigation of any area of experimental medicine.

My five years at Oak Ridge National Laboratory were among the most productive of my career. The knowledge gained there came with me and from time to time has been helpful in research unrelated to radiation.

M. K. HULLINGS

Dispensing Nuclear Knowledge

By BARBARA LYON

Review *editor Barbara Lyon is the wife of veteran nuclear engineer R. N. Lyon, recently retired after 34 years in the business. She has been at the Laboratory, in a succession of writing and editing jobs, since 1966.*

In the mid-40s, a popular radio program was known as Kay Kyser's Kollege of Musical Knowledge, and so, when a training program was set up at Clinton Laboratories in 1946, it seemed natural to dub it the Klinch Kollege of Knuclear Knowledge. Eventually, the spelling relaxed to Clinch College. Frederick Seitz, Professor of Physics at Carnegie Institute of Technology, was summoned by R&D Director Wigner to direct the school. An enrollment of 35 technical men, chosen from universities, private industry, and government agencies, came to Oak Ridge for the academic year 1946–47 to take courses in nuclear technology. Faculty members were Harry Soodak, Alvin Weinberg, Gale Young, A. O. Allen, C. L. Critchfield, Eugene Greuling, Lothar Nordheim, Dick Present, D. G. Rose, Kay Way, Eugene Wigner, Sergio De Benedetti, Henry Newson, Art Snell, Ernie Wollan, George Boyd, Henri Levy, Kurt Kraus, Ray Stoughton, and Waldo Cohn. Sitting in on many of the classes was a group of selectees from the Navy that included Capt. Hyman G. Rickover.

F. SEITZ

Since the information imparted in the courses was largely classified, the students had to have full clearance to attend. They were not permitted to take any written material away with them, except as it could be transmitted through security channels. Some stayed on, to be members of the Laboratory staff, but most of the class returned to their former jobs, where each became the local nuclear expert.

The degree offered by President Seitz of Clinch College was Doctor of Pile Engineering, enabling the graduates to place the initials D.O.P.E. thenceforth after their names. Dr. Wigner's commencement address, distinguished, among other virtues, for being but four minutes in duration, closed with the optimistic remark that, as a result of the knowledge dispensed by the Clinch College, he "expected to see piles springing up all over the country."

Vic Masket, one of the graduates, wrote a 16-stanza poem for the occasion, which contained such gems as:

FREDERICK SEITZ

I spent an academic year
Apprenticed to these scholars
The money they so wisely spent
Were tax-collected dollars.

I sat around a pile each day
Absorbing neutron flux;
Many an atom was transformed.
In matter, it's the crux.

Now these converted nuclei
Were subject to a test
And down at the dispensary
I gave them on request.

. . . . .

Well, that was all I had to know
I had the secret sure.
But Oh! the disillusionment
When Seitz made things secure.

He took away my written notes
And told me that instead
Of ever telling what I know
To keep it in my head.

But that is all behind me now
Let no one say 'twas bitter.
I made my contribution there
As Nordheim's baby sitter. ...

So successful was the training course given this class that, although similar schools were being held at other institutions, it was Oak Ridge National Laboratory that was chosen by the AEC in 1950, when a more permanent school was needed, to supply trained personnel for the Commission's reactor development program. A member of the original class, Fred C. von der Lage, was designated Director of the new Oak Ridge School of Reactor Technology, and in March of 1950 a pilot group of 18 trainees began a preliminary course at the Laboratory. The following September the school began in earnest, with a class of about

50 men, chosen from universities, industry, and the military. In that class, sent down from the Bureau of Ships, was a young man who would later become a member of Rickover's senior staff. His name was Milton Shaw.

The faculty of the first ORSORT course in 1950-51 included: Elda Anderson, E. C. Campbell, Milt Edlund, George Evans, E. D. Klema, Neal Lansing, Lewis Nelson, and Herbert Pomerance. Ernie Wollan was faculty consultant, and text consultants were Dr. and Mrs. Sam Glasstone. Rebecca Rickman was the school secretary.

ORSORT continued to answer AEC's "urgent needs," providing training that "cannot be given in universities because of its classified nature." At the Commission's request, the school expanded its enrollment, and in 1952, ORNL built an ORSORT Laboratory (Building 3017) to accommodate the increase. As the universities began to include nuclear science and engineering in their curriculums, the rationale of the Reactor School began to change subtly, and the catalogue announcing the courses explained that the school taught the "latest advances in reactor technology not yet a part of the general fund of technical knowledge because of pace and intensity of the reactor development effort, the large backlog of project-generated knowledge," and less importantly, the security restrictions. The course of study, seen to be analogous to the in-house training offered by industry, was offered in support of the academic programs.

Finally, in 1957-58, with Lewis Nelson as the new Director, the school began operating in cooperation with six institutions of learning: Carnegie Tech, Case Institute, Northwestern, Union College (Schenectady), UCLA, and the University of Florida. It operated in this mode, combining the elsewhere-unavailable latest advances in nuclear technology that ORSORT could offer with the standard curriculums of basic knowledge at the universities, for two years.

In 1959 the reactor school accepted its first international enrollment, stressing two courses in particular: a nine-month course in Nuclear Reactor Operations Supervision, and a 12-month course in Nuclear Reactor Hazards Evaluation, available with or without the standard ORSORT courses. Applications from noncitizens, submitted through the embassies, were handled by the Commission's Division of International Affairs. The expanded ORSORT service reflected the "USA interest in the orderly, safe, and rapid development of nuclear power by all free people." By 1961, both courses were offered for a full year: nine months of academic work, three of practice. It was soon referred to formally as ORNL's International Programs in Reactor Technology at the Reactor School, but kept its shortened name.

In September 1965 the school shut its doors, having been rendered unnecessary by the proliferation of nuclear science courses offered in the academic world. Of the 986 students who were formally enrolled in "ORSORT" during its 15 years of operation, only ten failed to complete the full course of study.

There follows a warm reminiscence, tendered jointly by Director Seitz, who is now president of another institution, Rockefeller University in Manhattan; and Research Director Wigner, who is Emeritus Professor of Physics at Princeton.


D. K. TRUBEY


M. SHAW


J. W. CLARK

G. A. CRISTY

E. G. SILVER

A. V. MASKET

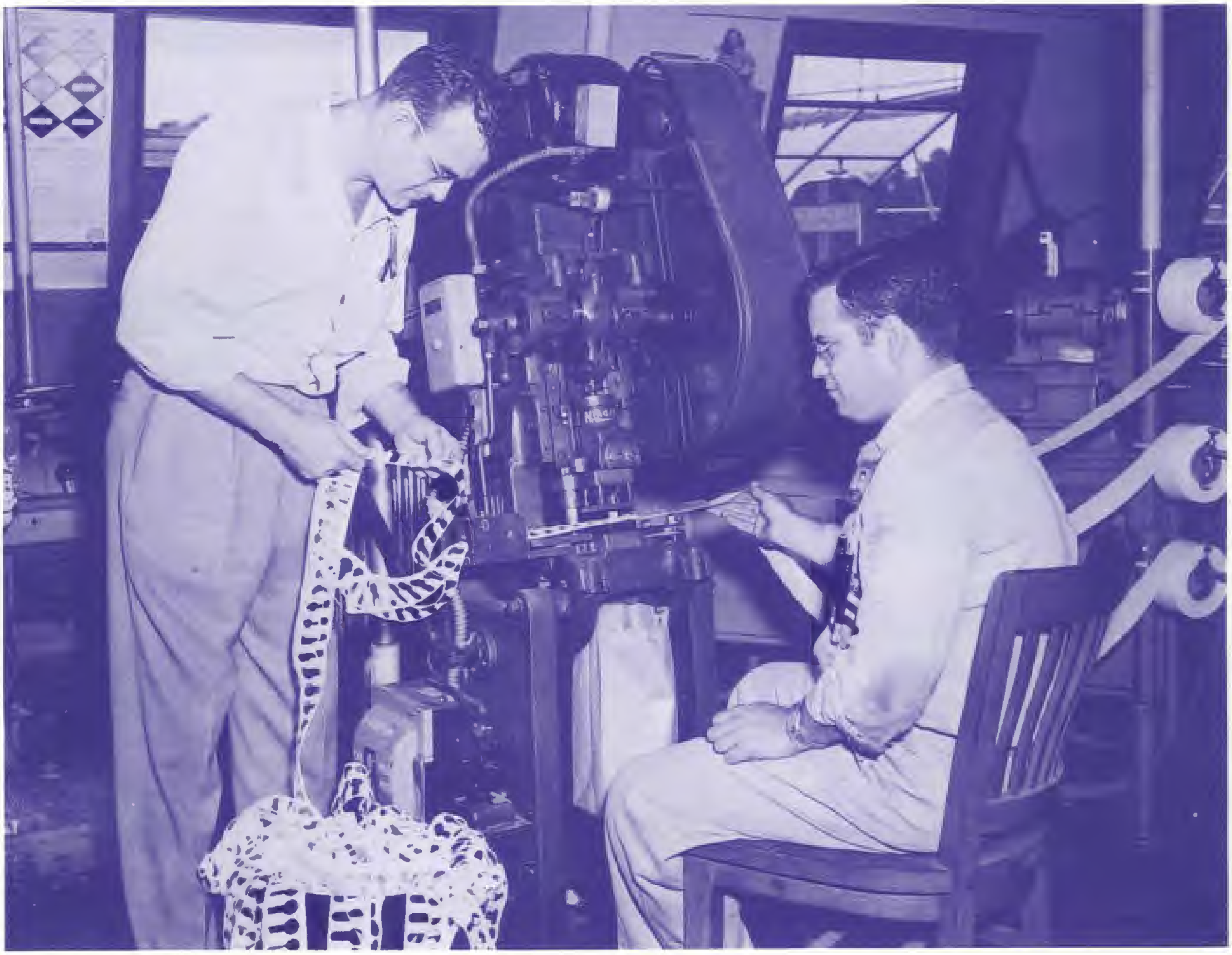
Recollections of Clinch College

By E. P. WIGNER and F. SEITZ

The spirit of our Laboratory was very different in the fall of 1946, when we came here for a year, from its present one. Surely, we all understood the two or three technological objectives which the Laboratory was intended to strive for: to assist in the defense of the country, to render the use of nuclear fuels for power production practical and economically attractive, and to produce auxiliary substances which help research in other areas. These objectives, and the love of fundamental research, have not changed in the 30 years that have passed since; but the spirit of the Laboratory and the relations of its members to each other have undergone great changes. In 1946 we all had a feeling of dilettantism and uneasiness to be members of such a large and important laboratory. We also had a large degree of independence from each other, and intense collaborations were less frequent. We were also more dependent on the will, and often whim, of the administrators, who felt a much greater responsibility to direct the attention and the objectives of the scientists than they do now. Today there is a greater appreciation that any increase of our knowledge may become useful.

The two of us were actually in close accord with the basic objectives of the founders and administrators of the Laboratory. One of us (Seitz) took responsibility for organizing the Clinton Laboratories Training School. This appeared to us as a very important undertaking—after all, the engineers were in charge of designing the reactors, and they had to learn about neutrons, chain reactions, radiation, and many other subjects. Most of them were, at that time, very unfamiliar with these subjects. Wigner recalls, with some amusement, the first design of a radiation shield which was presented to us; it was a cage, with bars close enough to each other so that goats or sheep could not have escaped, but hares or hens could have quite easily. Of course, this picture has changed fundamentally, chiefly as a result of the nuclear engineering courses at many universities, but the Clinton Labs School was a very useful beginning.

Wigner directed most of his attention to the design of nuclear reactors—in particular, ^{233}U-Th breeders—and to fundamental research. He recalls, with amusement, the difficulty he had when supporting Art Snell's work on the neutron's half-life. Some of the authorities (not the Director) were opposing this work: if the neutrons were found to decay, the reactors might prove to become barren. However, apart from a few such conflicts, the relations with the higher administration were pleasant. We both recall with pleasure the time we spent in Oak Ridge, the weekly meeting with the division directors which we initiated, the time we could spend on independent research, or contacts with colleagues; and last but not least, the beauty of the environment, which we greatly enjoyed.



Early health physics. Paul E. Galyon, left, holds filter paper strip from which are stamped smear tabs used "to collect samples of radioactivity" (1950).

1951

A number of buildings were completed and equipment was installed under ORNL's new master plan of construction called the "Permanent Facilities Plan." The Health Physics Waste Research Building was dedicated to removal of radioactive materials from water and to study public health aspects of radioactive waste disposal. A 5-MeV Van de Graaff generator was installed for pure physics research and shielding studies.

Arthur Snell and Frances Pleasonton measured the half-life of a free neutron in an experiment that confirmed that the neutron turns into a proton through the process of radioactive beta decay. The technique used in this experiment gave rise to a new field of physics—charge spectrometry.

Don Reid, N. G. Rigstad, and others at the ORNL Pilot Plant recovered several kilograms of plutonium-239 from Chalk River and Hanford fuel rods. The Chemical Technology Division helped in training Du

Pont personnel for the reprocessing facilities at the Savannah River Plant.

ORNL biologists participating in Operation Greenhouse studied the late effects of ionizing radiation on 4000 mice which were placed with other animals on ships at various distances from the Pacific Ocean's Eniwetok Island, over which the first experimental thermonuclear device was detonated in April. The mice were brought to the Biology Division, where they were observed until death and examined postmortem by Jacob Furth, Arthur Upton, and others Among the effects triggered by instantaneous exposure was the depigmentation of hair (changing from a tan color to varying degrees of gray and white), suggesting that mouse depigmentation could be used as a biological radiation dosimeter.

At graduation ceremonies of the first Reactor School class in June 1950, participants in one of the skits were, left to right, Lisso Mims, Larry Widdoes, Fred Grisak, and Milton Shaw. They billed themselves as the The Four Fission Fragments.

1952

The Bulk Shielding Test Reactor (later called the Bulk Shielding Facility) went into operation as ORNL's third reactor and first "swimming pool" reactor. This low-power reactor, built to study bulk materials for use in improved radiation shields, was a much simplified Materials Testing Reactor design. The reactor core was submerged in an open "swimming pool," which provided shielding, cooling, and neutron moderation. This swimming-pool design was employed in a number of low-cost reactors later built for universities and other nations.

A process for the recovery of krypton-85 and xenon isotopes in the nitrogen oxide off-gases from the nitric acid dissolution of irradiated uranium was developed and tested in the ORNL Pilot Plant, under the direction of Kirk Jackson.

The completion of two major facilities at the AEC's National Reactor Testing Station at Arco, Idaho, was a source of pride to ORNL staff members because the research and design work for both facilities had been done at ORNL. The MTR reached criticality, making available high neutron fluxes for testing reactor construction materials in a high radiation field. Operated by the Phillips Petroleum Company, the MTR had several reactor cores fabricated at ORNL's Rolling Mill Facility. The Idaho Chemical Processing Plant, designed under the direction of Floyd Culler, was also completed that year. Frank Bruce, Don Ferguson, and others developed the solvent extraction process for uranium-235 recovery in the plant; the construction and initial operation were supervised by several ORNL staff members.

The Solid State Division was formed, growing out of the Physics of Solids Institute established in 1950. The Physics of Solids Institute hot cells (Building 3025), designed and built in 1952 under the guidance of S. E. Dismuke, were used for radiation-damage research. Using these cells for performing physical, chemical, and metallurgical measurements remotely on irradiated materials, researchers learned that energetic neutron bombardment affects the structure-sensitive properties of all classes of solids. These results enabled ORNL researchers to increase their knowledge of which materials are least damaged by irradiation and to develop alloys that could better withstand such exposure. Building-3025 hot cells were later augmented by construction of the more elaborate High-Radiation-Level Examination Laboratory, which went into operation in 1964. D. S. Billington, who headed the Physics of Solids Institute, became the first director of the new Solid State Division. In its first year, the Division could boast of several accomplishments. J. C. Wilson observed enhanced creep of alloys during irradiation in a reactor; and Oscar Sisman and C. D. Bopp compiled a handbook on the irradiation effects in polymers, plastics, and other organic materials.

Another facility completed in 1952 was the Homogeneous Reactor Experiment, which went critical April 15. The HRE was ORNL's fourth nuclear reactor.

The 63-inch cyclotron began operation as the first cyclotron to accelerate heavy ions—particles heavier than helium. Alexander Zucker and associates observed 20 new nuclear reactions in this cyclotron, as they demonstrated the fusion of nitrogen atoms (nitrogen bombarding nitrogen) and bombarded targets of oxygen and carbon with beams of nitrogen nuclei to form the heavier nuclei of fluorine, sodium, and aluminum. Physicists showed that nitrogen—nitrogen nuclear reactions were infrequent enough to allay fears that an explosion of a hydrogen bomb might set the earth's atmosphere on fire. They also saw for the first time the richness of the phenomena produced when complex nuclei collide.

ORNL conducted neutron activation analyses for the AEC to measure accurately micro amounts of impurities in foods, drugs, metals, lubricants, plastics, and fertilizers. ORNL began selling polonium-210, the first reactor-produced alpha emitter to be sold. And ORNL chemists studied the radioactive decay scheme of gallium-67 in connection with the ORINS Medical Division's research on bone cancer. Development of the acid-deficient Thorex Process and the Interim-23 Process for the recovery and decontamination of uranium-233 from irradiated thorium was begun during 1952 by A. T. Gresky.

1953

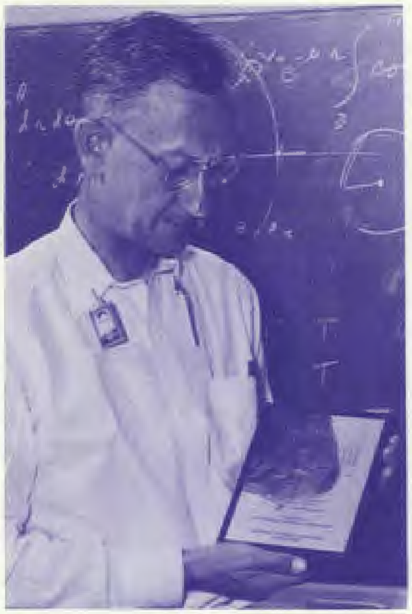
On February 24, the Homogeneous Reactor Experiment, under the direction of John Swartout, operated at a power level of 150 kW. This was the second demonstration by an AEC laboratory of electric power production from nuclear energy, with the first occurring at Argonne National Laboratory's EBR-I (Experimental Breeder Reactor) in Idaho in December 1951. Since 1953 marked the end of ORNL's first decade, this event occurred at an appropriate time, as it underscored ORNL's changing mission—from contributing to the nation's defense to helping achieve a peacetime national goal of economically competitive nuclear power.

The Tower Shielding Facility—designed by a group led by C. E. Clifford to allow shielding studies free of ground scattering—was completed for the Aircraft Nuclear Propulsion Program. It could suspend a shielded cabin and an MTR-type reactor 200 ft above ground from cables between two hairpin towers 315 ft high. Operated remotely from an underground control room, the TSF was used in the development of aircraft reactor shields, which were to be as light and compact as possible. (Tower Shielding Facility-2 was completed in 1959.)

P. R. Bell and his colleagues designed and built a multichannel pulse analyzer for determining the energy distribution of signals from scintillation spectrometers. Such an instrument made possible detailed studies of short-lived fission products.

K. L. Vander Sluis determined the nuclear spin of uranium-233 and found that the magnetic moment of the nucleus is in the opposite direction to that of uranium-235. J. R. McNally found that the shape of the uranium-235 nucleus appears to deviate about 10% from sphericity. These discoveries contributed to developments in the theory of fission.

Researchers completed development of the TBP-25 solvent extraction process (using highly diluted tributyl phosphate), later to be used internationally for recovery of spent alloyed fuels from various research reactors.



Alston Householder holds a bronze plaque presented to ORNL by Argonne National Laboratory after the ORACLE's demonstration at the Symposium on Digital Computers held in Chicago in 1953.

The Mathematics Panel and the ORACLE

By A. H. HOUSEHOLDER

Alston Householder directed the mathematical aspects of the Laboratory from 1946 until 1969, when he retired. He is now on the West Coast, remaining active as a consultant to the University of California.

Henry Garabedian and I arrived at Oak Ridge in the fall of 1946 to join the Physics Division of what was then the Clinton Laboratories. There was only one Ph.D. mathematician, Forrest Murray, but Bob Coveyou was in charge of a small group operating Fridens and Marchants. About a year later, Alvin Weinberg asked me to take charge of a Mathematics and Computing Section of the Physics Division, and about a year after that, since customers came from all over the Laboratory, this group was given divisional status. But since it was very small, it seemed inappropriate to call it a division, so it was called the Mathematics Panel—at whose suggestion I can no longer remember.

In January 1947, Howard Aiken unveiled his Mark I computer at Harvard and celebrated the event with a symposium on high-speed digital computers. At that time the only truly high-speed, electronic digital computer was the ENIAC, then at the University of Pennsylvania, where it had been built by Mauchly and Eckert. It was later moved to Aberdeen. Speakers at this symposium included Aiken; Mauchly and Eckert; Jay Forrester, who was building the Whirlwind at MIT; Sam Alexander, who would build the SEAC at the Bureau of Standards in Washington; Harry Huskey, who would build the SWAC, also for the Bureau of Standards but in Los Angeles on the UCLA campus; and von Neumann and Goldstine, who would design a machine at the Institute for Advanced Studies that would have many modified copies, including the ILIAC at Urbana, Illinois, the SILIAC at Sydney, the MANIAC at Los Alamos Scientific Laboratory, the AVIDAC at Argonne National Laboratory, and the ORACLE at ORNL. Aiken, of course, would go on to produce a series of Marks, while Mauchly and Eckert would organize their own company and produce many UNIVACs and later be taken over by Sperry Rand.

J. O. KOLB

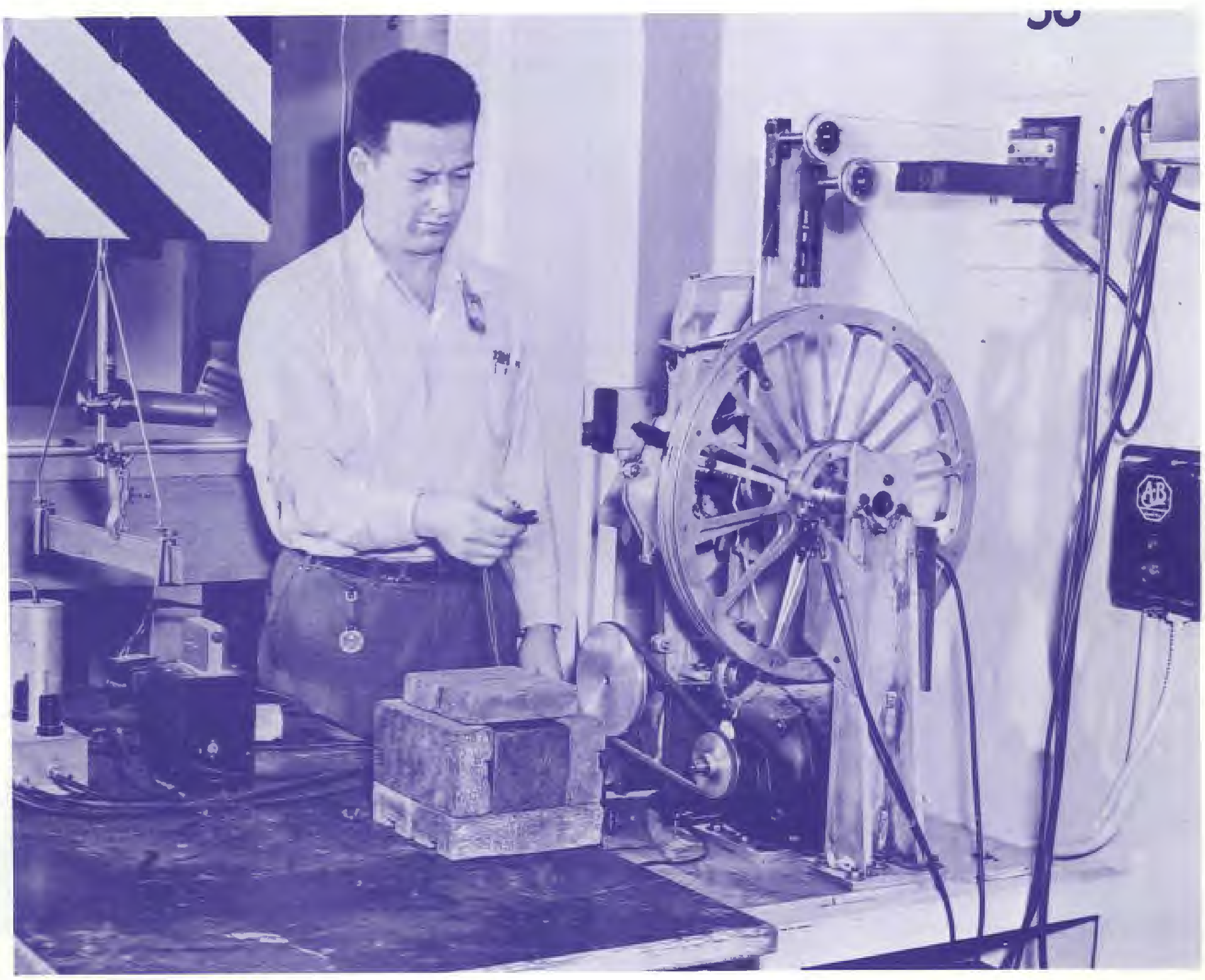
At that time, much of the talk seemed quite grandiose, and it hardly seemed possible that the entire country could saturate more than, say, half a dozen machines computing at such high rates. Nevertheless, I began inquiring and checking, and eventually ORNL management approved taking steps to acquire for ORNL an electronic digital computer.

L. C. EMERSON

Mauchly and Eckert were offering to construct UNIVACs for $175,000 (as I recall), though the price was to rise before the first delivery, which would be in 1951 to the Census Bureau. But this was not merely to be a scientific computer, and we felt the versatility would be of little interest to us. Raytheon was asking $350,000 for a strictly scientific computer, but we thought we could do better than that. We considered a copy of the Mark IV to be made by G.E. But several copies of John von Neumann's IAS machine were being made, including the AVIDAC at Argonne. Eventually an agreement was reached between ORNL and ANL whereby some Oak Ridge engineers would go to Argonne to spend six months studying the design of the IAS machine and making improvements, thereafter to undertake the construction of a machine for ORNL. Chuan Chu was the Argonne electronic engineer in charge. The machine was put into limited operation at ANL in the summer of 1953, then shipped to ORNL the following winter. For a short while it was the most advanced machine in existence, in terms of speed and storage in particular.

During this period, finding names (mostly acronyms) was a pleasant pastime. The Oak Ridge Automatic Computer was a natural, but this name had been preempted by a very special-purpose machine at NEPA, so we added "and Logical Engine," at the suggestion of Wallace Givens. The AVIDAC was Argonne's version of the Institute's Digital Automatic Computer. Los Alamos had the Mathematical and Numerical Integrator and Computer (MANIAC). Some of the early computers, not all digital, were named after atmospheric disturbances, like the Whirlwind at MIT. So a very modest machine at Vienna was called the Mailufterl. A proposal was made at Brookhaven to name a machine, being considered there, Willie Higinbotham's Old Reliable Engine.

Herbert Pomerance at the pile oscillator, which moves material into Hole 56 for irradiation and subsequent neutron capture cross-section measurement. Equipment was formerly the working mechanism of a Maytag washing machine (1950).



Fred Dewey, of the Information and Reports Division, puts the finishing touches on a poster destined for a safety display (1952).

While the ORACLE was being built, computing needs came to exceed the capabilities of Marchants and Fridens. There was plenty of card-punch equipment at both K-25 and Y-12, so Mathematics Panel personnel were stationed at both places to do ORNL computations on their equipment. IBM was quite slow in deciding to make electronic machines, but they finally came out with the 701, which bore some resemblance to the IAS machines (they had hired von Neumann as a consultant). At one point we were pressured to discontinue work on the ORACLE (one argument was that it would never get finished) and to have all ORNL computing done on a 701 to be installed at Y-12. But the ORACLE, once completed, performed beautifully until its inevitable obsolescence.

As with all the IAS machines, the ORACLE operated with a 40-bit word length in fixed-point binary arithmetic. But storage capacity was 10^{11} words, twice that of the others. At that time the standard storage devices were the mercury delay line and the Williams tube (cathode ray). The early IAS machines all used the Williams tube. Magnetic cores were just being discussed as the ORACLE came into operation. The ORACLE tapes were wider and permitted more rapid access than any others elsewhere in use. The drive was designed by Rudolph Klein, an ORNL engineer.

With developments proceeding at such a rapid rate, the ORACLE's preeminence did not continue for long, and the day of the homemade machine went out with it. An era came to an end.

One of the earliest major programs run on the ORACLE was the Givens eigenvalue routine. Givens was then a consultant at ORNL, and the programming was done by Virginia Klema, under his direction. This became, and remains, the standard method for computing eigenvalues of symmetric matrices. It was for a time partially superseded by what came to be called the "Householder method," which, however, was only a modification of the Givens method, and, indeed, has more recently been largely superseded itself by a modification that is closer to the original Givens method. Givens also introduced, in connection with his original program and independently of J. H. Wilkinson of Teddington, the technique of backward error analysis, which revolutionized the study of this extremely important problem.

1954

The Aircraft Reactor Experiment (ARE) began operating in October at a design power of 2 MW(t) for 100 hr. It used a circulating molten fuel consisting of fluorides of uranium, sodium, and zirconium. Like the Homogeneous Reactor Experiment (HRE), the ARE demonstrated the advantageous features of fluid-fuel reactors, including nuclear stability, strong coupling between power demand and power level, and ease of operation and controllability. But the ARE could operate at a much higher temperature (as high as 860°C) and much lower pressure than did the HRE. The ARE research effort brought forth a molten-salt fuel mixture that was a radiation-stable fluid over the proper range of temperature and which exhibited satisfactory heat transfer properties.

The ORACLE (Oak Ridge Automatic Computer and Logical Engine), an advanced type of digital computer constructed at Argonne National Laboratory, was installed at ORNL. For a short time this vacuum-tube device was probably the fastest machine in existence. Following installation, the ORACLE was improved to increase its versatility, as storage was doubled to 2048 words of 40 bits each, and several devices were added. The ORACLE was

Special Tribute

Dr. Raymond C. Briant, a nationally renowned mathematician and chemical engineer, joined the Laboratory's Aircraft Nuclear Propulsion (ANP) Project in 1949, to become Director of the ANP Division in September 1950 after a summer with an advisory panel on nuclear propulsion of aircraft. He served in this capacity until his death in April 1954. He championed molten fluoride mixtures as nuclear aircraft power plant fuels and was quickly convinced that such fuels were readily adaptable to other nuclear applications. His inspirational leadership mobilized and motivated the teams who successfully designed and, after his death, operated the Aircraft Reactor Experiment. It was, moreover, his farsighted and enthusiastic support of basic researches on molten fluorides that set the stage for the concept of molten-salt reactors as civilian power producers and uranium-233 breeders.

based on a design by John von Neumann, renowned mathematician and member of the AEC. (In the late 1950s and 1960s, researchers began using computers run by transistors, with minicomputers coming on the ORNL scene in the mid-1960s. Microcomputers appeared at ORNL in the 1970s.)

ORNL researchers under A. L. Boch completed a design for the Army Package Power Reactor to be built for the U.S. Army Corps of Engineers at Ft. Belvoir, Virginia. With the 1-MW(e), 3.5-MW(t) reactor, which began operation in 1957, ways were studied for using nuclear power for application in remote military stations.

The first Homogeneous Reactor Experiment was dismantled as HRE-2 (or HRT) was being built. The development work on the HRE-2 sparked ORNL innovations in methods of maintaining and repairing highly radioactive systems.

After two-and-a-half years of research, ORNL scientists learned how to separate cesium-137 in large quantities from spent reactor fuel. The isolated cesium-137, a long-lived beta- and gamma-ray-emitting isotope, was compressed into pellets for use in cancer research and therapy.

The acid-deficient Thorex Process was successfully demonstrated in a pilot plant. This solvent extraction method, using tributyl phosphate, separates protactinium, uranium-233, and thorium from each other and from fission products.

The use of molecular beams in studying the mechanisms of chemical reactions was demonstrated successfully by Ellison Taylor and Sheldon Datz in an experiment using beams of potassium and hydrogen bromide. These researchers recognized that much could be learned about the details of gaseous chemical reactions if the reactants could be brought together as crossed molecular beams. Such studies have been carried out at ORNL since 1948.

The Alignment of Nuclei

By J. W. T. DABBS

John Dabbs has been at the Laboratory since 1946, engaged in research physics. He has recently taken an active interest in local spin-off industries.

The late M. E. Rose, one of ORNL's most prominent theoretical physicists, developed, in May 1948, the idea of using the coupling between the magnetic moments of paramagnetic electrons and the nuclear magnetic moment (only 5×10^{-4} as large) to cause nuclei to line up, or polarize, in an external magnetic field. Rose also proposed various experiments in which the spins of states in compound nuclei, formed by neutron capture, could be measured using polarized neutrons with these polarized nuclei. (These proposals became the basis of a long sequence of fundamental nuclear physics studies which ended in 1974 at ORNL in a million-dollar experiment in collaboration with Los Alamos Scientific Laboratory.) Later in 1948, Prof. C. J. Gorter, then and now head of the famous Kamerlingh Onnes low-temperature laboratory in Leiden, Holland, developed an almost identical set of ideas and published them in November. Rose published in January 1949. The effect was called the "Rose-Gorter" or the "Gorter-Rose" effect, depending on which side of the Atlantic you lived. The denouement came when Gorter finally saw copies of old reports on the subject written by Rose early in 1948. He then wrote to Rose and apologized for having believed for several years that Rose had plagiarized his ideas!

H. C. Hoy measures the distance between the big Ds of the 63-in cyclotron, shiny and new in 1952.

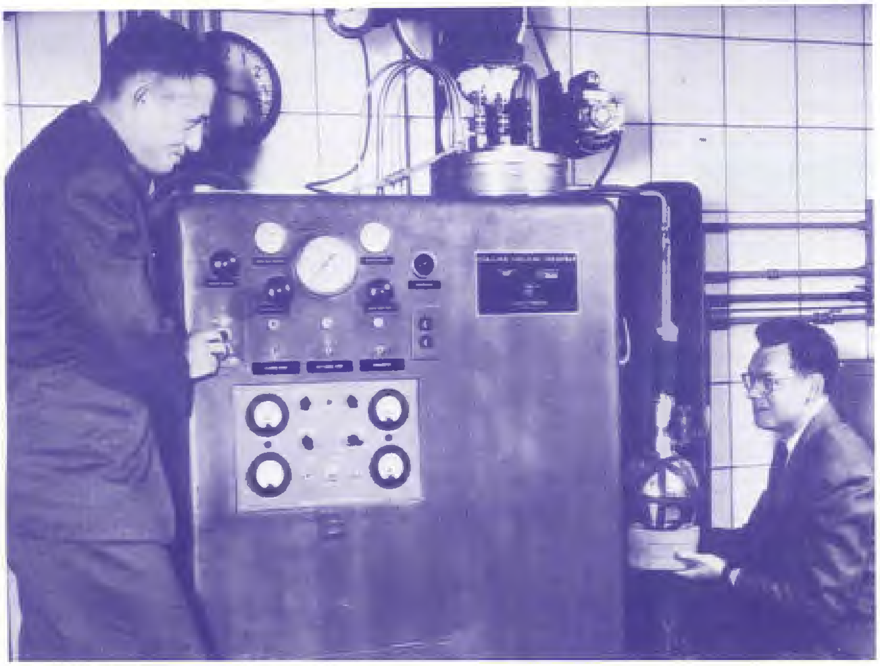
Louis D. Roberts, since 1968 Professor of Physics at UNC (Chapel Hill, North Carolina), and I spent some 12 years, beginning in 1948, following the ideas that Rose initiated. In 1954 my thesis problem involved so-called "brute force" nuclear polarization of indium metal—it was really pretty gentle, since it involved only 11,000 gauss at a temperature of 0.04° above absolute zero and gave a polarization of only 2.1%! But that was enough to convince the great Nicolas Kürti of Oxford University that ORNL was going to try for the world's low-temperature record by magnetic cooling of those nuclei. Only two years later, after a heroic effort, including a brilliant shortcut, he and his student Spohr reached a few millionths of a degree above absolute zero.

The work with Roberts took an important turn (based on other work at Oxford by B. Bleaney) toward the discovery that alpha particles were preferentially emitted from the tips of cigar-shaped nuclei. The first results were seen in 1955, but it required much real detective work, culminating in a classic 1960 experiment by Steve Hanauer (now with NRC in Washington) to prove that the chemical bonding in the compounds used was so subtle that the direction of the effect was in fact reversed from the original theoretical ideas. Here was a case of a nuclear effect acting as a very useful tool to detect chemical bonding details. In all this work, chemist G. W. Parker played an indispensable role. George probably still remembers the time a summer heat wave caused a rise in room temperature (no air conditioner) to a point that caused three months of crystal growing in Building 4508 to be wiped out overnight.

Nuclear orientation played another important role with the Nobel-recognized "parity" experiments in 1957 that proved that the universe doesn't have mirror (or reflection) symmetry, and which stood the theoretical physics world on its ear. Roberts and I were intrigued by the prediction by Lee and Yang that such an effect could be seen in a polarization experiment on cobalt-60. This interest became stronger when several famous physicists contacted us and said, "Why don't you fellows do that experiment?" (One of them said, "Why don't *we* do that experiment?") We consulted our local theoretical oracles, who said (to a man) that it was a 1000-to-1 shot. Then we learned that our colleagues at the Bureau of Standards, Ernest Ambler and Ralph Hudson, were working with C. S. Wu and others. We concluded that we wouldn't try, since their equipment was slightly better adapted for the experiment, and because the aforementioned mystery of the alpha particles was so unique and fascinating.

Another exciting achievement was the first real understanding in 1959 of the inner workings of solid-state particle detectors, which have led to revolutionary strides in nuclear, fission, and heavy-ion physics. F. J. Walter, whose thesis developed these ideas, is now head of Oak Ridge Technical Enterprises Corporation's Physical Sciences Division in Oak Ridge and has been responsible for many important and practical developments in nuclear instrumentation. Hanauer's experiment, mentioned above, used a solid-state detector at 0.2° Kelvin, and our group was also the first to use a solid-state detector in a nuclear physics experiment, and the first to observe fission fragments with a solid-state detector.

Neutron time-of-flight spectroscopy found a leader at ORNL in Jack Harvey, now Codirector of the Oak Ridge Electron Linear Accelerator, who built a 180-meter, evacuated flight path at the Oak Ridge Research Reactor. In the little house at the end of this 600-foot tube, we had an exciting month in 1963 when we watched neutrons fall about 6 inches under the influence of gravity. The experiment was about 25 times as good as had ever been done, gave an answer accurate to ±0.3%, and is only now being improved upon.



L. D. Roberts, l., and John Dabbs of the Physics Division "milk" a Collins Helium Cryostat machine to obtain liquid helium for their low temperature project. Early in 1950 they attained a temperature within 1/25 of one degree of absolute zero. It was the lowest temperature attained in the United States at the time.

The Nuclear Airplane Engine

By E. S. BETTIS

Edward S. Bettis, now a consultant in thermonuclear engineering, worked at Y-12 from 1943 to 1945, left for one year with DuMont Laboratories, where he made the first remote telecast of a baseball game (Brooklyn), then joined the NEPA Project in Oak Ridge in 1946. He has been engaged in reactor development since then until his retirement in 1973.

Coach John Loy

Times have certainly changed since the Laboratory designed, built, and operated the Aircraft Reactor Experiment (ARE) in the early 1950s. With the current red tape, restrictions, and regulations, we would never be able to duplicate the project. We would never get beyond the milestones, millstones, environmental impact statements, quality assurance, etc., etc., etc. Yes, times have changed—drastically!

This is not to imply that the ARE was worth doing—it was a crackpot idea, but it did develop some technology which *could* have been useful. I shall always be convinced that the whole idea of a nuclear-powered airplane came into being because the Navy had—or was developing—the nuclear submarine. The Air Force had to have a part of the nuclear scene.

Fairchild Engine and Aircraft Corporation had initiated the Aircraft Nuclear Propulsion (ANP) program, and after a five-year study, General Electric Company took over the main project, but ORNL was given the job of designing, building, and operating an experimental reactor. Ray Briant succeeded Ed Shipley as head of ORNL's ANP program, of which the ARE was only a small part.

There was a great esprit de corps in the group working on the ARE project. I was project engineer, and I think Alvin Weinberg, as ORNL Research Director, was a little nervous at having such an unconventional person in charge of a reactor project. Briant helped a lot to calm Alvin's nerves. (I'm not sure yet how this reflects on Ray's judgment.)

Larry Meem was put in charge of the project as far as all nuclear aspects were concerned. Ray Mann served as father confessor and general advisor. Warren Grimes was in charge of the chemical operations—fuel production and all molten-salt operations. The design had many inputs from numerous persons. I'm afraid to try to name the leaders here, although Bob Schroeder and George Cristy played leading roles. Approval of design was exceedingly informal. Bill Manly was responsible for the materials, and Ray Hously was in charge of the welding. Al Yundell with Bill Cobb and others in Wes Savage's pump group "manned the pumps."

The major decisions were generally made by a caucus of those carrying primary responsibilities in the various fields. If we couldn't come to a conclusion, I would make a decision—frequently on a hunch. Bruce Webster was the general foreman in charge of the installation, and I'm sure I gave him some difficult moments. As we neared completion of the installation, the shift supervisors, Bob Affel, Grady Whitman, and Bill Cottrell came to the ARE.

At the shutdown of the Aircraft Nuclear Experiment are, from left, E. R. Mann, Sylvan Cromer, Ed Bettis, USAF Col. Clyde Gasser, and Larry Meem.

The fuel salt was enriched with uranium by a very primitive operation. Warren Grimes and his crew literally loaded the fuel by hand. The enriched salt was contained in small metal cylinders which we called "penguins." A small tube from the penguin was attached to a tube extending from the top of the pump bowl. The penguin was placed in a furnace to melt the salt, and welding torches were played on the "umbilicus" to permit the salt to be pressurized into the system. At one point the tube near the pump bowl was overheated and a pinhole was made. This did not interfere with completing the job, but was the cause of serious difficulty later.

The fueling operation involved many people, and the urgency to get on with the job caused considerable confusion, to say the least. There were several janitors around trying to keep a path open for the crew. As we neared the end of the fuel enriching operation, it was discovered that one of the penguins of uranium was missing. A frenzied search failed to locate it. After some time, it was noticed that the trash cans had been emptied, and the trash had already been taken to the burial ground. It was already pitch-dark by this time, but a crew was dispatched to search the burial ground. Word had gotten out that we had lost a significant quantity of uranium, and Clarence Larson, ORNL Director then, joined the search crew as they combed the burial ground with flashlights.

It was a great piece of luck that someone, and I'm sorry I can't remember who, spotted the tube from the penguin sticking a few inches above the ground where the uranium-containing penguin had already been buried. If it had not been found, there would have been holy hell to pay.

After the fuel loading was completed, the moment of truth came about 11:00 one Sunday morning in 1954 when we entombed the entire red-hot complex system with the placing of the final concrete shield block. We cautiously started to bring the reactor up to power. At about the 10-kW level (as I remember it), the radiation alarm in the basement sounded and kept ringing. We found that the pinpoint leak in the fuel loading line was releasing fission product gas, which was leaking out of the pits into the building. By dint of some ingenious retrofitting with some 2-inch pipe, we managed to divert the leaking gas to a less hazardous containment before it could do any harm.

W. R. GRIMES

Since the ARE was supposed to be a forerunner for a reactor that could power a jet engine, it was not expected to operate a long time. It did have to operate at a high temperature. In fact, I think that no reactor since has operated at a higher coolant temperature. We planned to operate it at power for only 100 hours. It ran so well that several people wanted to extend its operation, but as soon as the proposed operating time was reached, we shut it down. Five hours after shutdown, the overheated pipe that had leaked a little fission gas opened completely, so we had to evacuate the building. The shutdown timing was most fortunate.

Considering its very brief life, the ARE attracted some rather distinguished vistors, including Gen. Jimmy Doolittle; Gen. Vogel, then Chairman of TVA; Adm. Lewis Strauss, then AEC Chairman; and Capt. Hyman Rickover.

As I mentioned before, there was a great spirit of dedication on the part of every member of the ARE crew. It is unfortunate that each one cannot be mentioned by name. I'm sure I have forgotten even some of the key persons. After the final shield blocks were put in place, there were several of us who never went home until the reactor was shut down in November of 1954. We cooked our meals on hot plates, slept on desk tops, and made do. When there was an opportunity, we would occasionally go to the dispensary and sleep a few hours on the EKG table.

There was a group feeling of dedication that I'm afraid cannot be engendered again. Everything now, it seems to me, is so encumbered by regulations and red tape that the particular job is lost sight of. I know it is corny to refer to the "good old days," but it is somewhat sobering and frustrating to realize that probably never again will we be able to take on a project like the ARE, in which we could achieve, without restraint, the technical objective.

1955

What Mrs. Enrico Fermi called "the prettiest reactor ever built" was designed, constructed, and tested by ORNL researchers (led by C. E. Winters) and was exhibited at the First United Nations International Conference for Peaceful Uses of Atomic Energy, held August 8–20 in Geneva, Switzerland. (T. E. Cole suggested the idea of building a reactor for exhibit.) In a span of five months, the research reactor of the "swimming pool" variety (but with low-enrichment uranium) was designed, built, tested, taken apart, shipped by air from Knoxville to Geneva, reassembled, tested, and operated. President Eisenhower was among the thousands of

In 1950, one of the Research Director's duties was to crown the Queen of Hearts at the Girls' Club annual Valentine dance. From left, Helen Keener, Official Weinberg, Queen Neva Patrick, Margaret Fuller, and Boots Stout.

visitors who observed the now familiar blue glow of the irradiated fuel elements in the reactor, which typified the research reactors designed for radioisotope production, shielding studies, and neutron cross-section measurements. This was the first reactor to use uranium dioxide fuel. ORNL researchers delivered 28 scientific papers at the conference; one of the papers was given by Liane B. Russell, the only woman in the official U.S. delegation. (Mrs. Russell's studies of the effects of radiation on mouse embryos at different stages of development resulted in a change in medical practice as doctors became more aware of the potential hazards of administering x rays to pregnant women.)

Small-scale research and development activities started at ORNL on Project Sherwood, the major effort being carried out by five U.S. research installations to control thermonuclear reactions. Two years before, ORNL had undertaken a small theoretical study of some possible approaches to the achievement of controlled thermonuclear reactions. Its entry into the fusion field seemed justified by its expertise and experience in the motion of ions in electric and magnetic fields, as in electromagnetic separations and high-current cyclotrons.

Alvin Weinberg succeeded Clarence E. Larson as Director of ORNL on October 1. Larson was appointed Vice President in Charge of Research for National Carbon Company, a division of Union Carbide Corporation. Weinberg, in his first "State of the Laboratory" address as ORNL Director, hailed ORNL as an institution with an international reputation and advocated that national laboratories continue to work on heavy reactor projects despite competition from private industry in nuclear power development. This competition between industry and national laboratories for people, money, and facilities was increased by AEC's establishment of a Power Demonstration Reactor Program designed to open the way for American and foreign industry to develop, construct, and operate experimental nuclear power reactors. Thus the AEC's role changed from being the principal user of its own developments to that of coordinator, supporter, and regulator of a burgeoning nuclear industry.

The first major field experiment to measure the radiations from nuclear bombs was conducted at the Nevada Test Site by the Health Physics Division. The results led to a comprehensive and successful program (under John A. Auxier) to obtain the radiation doses to survivors of the atomic bombings of Hiroshima and Nagasaki.

In the field of radioisotopes, a new facility for processing large quantities of iodine-131 went into operation at ORNL. Iodine-131, an isotope with a half-life of eight days, useful primarily for diagnosis and treatment of thyroid disorders, was in great demand by industry and by medical and clinical researchers.

Application of a tributyl phosphate-nitric acid solvent extraction process developed at ORNL for separation of rare earths produced a small quantity of europium, an extemely rare element with potential for use in reactor control rods.

Out-of-pile meltdown experiments were started under G. W. Parker to answer reactor safety questions on the release of fission products from irradiated fuels melted under various conditions. The problem was studied concerning which fission products would escape from a reactor if the fuel core melted down because of loss of coolant in a reactor, using simulated loss-of-coolant accidents.

A nondestructive testing (NDT) program was begun under R. B. Oliver for developing and applying new techniques to detect welding flaws and other materials problems. ORNL has one of the most sophisticated NDT development laboratories in the world, utilizing penetrating-radiation, ultrasonic, and phase-sensitive eddy-current techniques. Robert McClung, who had headed the program since 1960, has played a leading role in development of new NDT techniques.

Neutron Resonance Spectroscopy at ORNL

By J. A. HARVEY

Jack Harvey came to ORNL in 1955 from Brookhaven to build a time-of-flight neutron spectrometer. He is now Director of the Oak Ridge Electron Linear Accelerator and is still measuring neutron cross sections.

In the early 1950s, neutron cross-section measurers exploited the game of resolution-man-ship, that is, the art of comparing your latest and best data to your competitor's earlier, published (and naturally inferior) data. This era was the golden age of "fast chopper" neutron spectrometers, which usually won the resolution race over the pulsed accelerator spectrometers. ORNL, which had already won the competition for measuring the world's largest cross section, namely, the 3.2 × 10^6-barn cross section of the 9-hr isotope, xenon-135, also entered the high-resolution game in the mid-1950s. Art Snell (then Director of the Physics Division) and Joe Fowler (Associate Director), with enthusiastic backing from Alvin Weinberg, persuaded Bob Block and Grimes Slaughter from Duke University and me to set up the "best" fast chopper at the "best" high-flux research reactor in the world, the Oak Ridge Research Reactor (ORR), which was under construction in 1955.

Z. B. DWORKIN

With the excellent cooperation of engineers from various divisions—Layton Howell for the mechanical design, Nat Hill and Jack Davidson for designing and building the first 2048-channel time analyzer, and C. R. Rickard for supervising the mechanical construction—the equipment was completed in 1957, a year before the ORR was available for research. In the interim, the spectrometer was installed at the LITR (Low-Intensity Test Reactor), and a flight path was built to the second floor of an extension to the Swimming Pool Reactor building. Shortly after we started in business, Ernie Silver installed a small Cockcroft-Walton neutron source directly underneath our bank of BF_3 counters at the end of the flight path and produced more counts in our detectors than did the chopped beam from the LITR. This required gating off the detectors whenever this accelerator was operating.

In 1958, the fast chopper was moved to beam hole 6 at the ORR. Since a 150-kg fast chopper rotating at 12,000 rpm represents a large amount of kinetic energy, it was contained in a 20-ton, 6-in.-thick armor-plate and concrete shield. A safety committee considered the possibility that the fast chopper might rupture and that fragments might travel down the collimator to the reactor core. Although minor incidents have occurred with almost all fast choppers, none has been more than an exciting experience for those in the vicinity. Our incident occurred at a low speed of ~600 rpm. As we attempted to displace the rotating chopper it made contact with the inside of the housing, which had eight portholes around it. The housing went into violent convulsions, so Grimes Slaughter instinctively threw himself over the housing to try to hold it down. After the rotor stopped spinning, we gathered up from the floor dozens of small appendages which had been shaken loose; fortunately none belonged to Grimes.

J. A. HARVEY

To achieve high resolution, an evacuated flight path, 180 meters long and 48 in. in diameter, was constructed so as to pass over the pump house of the ORR to our detector station. Using this long flight path, Toyo Fuketa, a visitor from Japan, made transmission measurements on all ten isotopes of tin, with a time-of-flight resolution of 11 nanoseconds/meter, almost an order of magnitude better than was obtained at earlier facilities. Measurements were made on several radioactive fission product isotopes which had never been previously measured, such as ^{99}Tc, ^{147}Pm, ^{151}Sm supplied by George Parker and his co-workers; enriched isotopes such as ^{180}Ta, ^{138}La, ^{176}Lu produced from the calutrons with the cooperation of the Stable Isotopes Separations Group; and highly enriched ^{233}U produced from decay of separated ^{233}Pa (90 megacuries).

During the late 1950s, a pulsed Van de Graaff neutron spectrometer was developed by Bill Good, John Neiler, and their co-workers for measurements in the keV energy region. Using this facility, Dick Macklin and Jack Gibbons made capture cross-section measurements on many isotopes relating to the slow and fast processes in nucleosynthesis.

In the early 1960s, Frank Firk from the Harwell linear accelerator neutron project spent a year at ORNL and convinced us of the potential of an electron linear accelerator for neutron spectroscopy. After several years of effort, we obtained funding for the project late in 1965. With the completion of ORELA in August 1969, we have the best facility in the world for neutron cross-section measurements (~500 times better resolution than was obtained with the fast chopper) and we can cover the energy region from 0.004 eV to 40 MeV—nine decades in neutron energy.

As for the old "fast chopper," it is alive and well at a reactor in Taiwan. (It was shipped by slow boat.) The 180-meter flight path still stands at the ORR and is available to any imaginative experimenter. It was used by John Dabbs and me to measure the gravitational constant for neutrons. It turned out to be exactly the right length for such a measurement, since the drop of a neutron in centimeters in traveling this distance just happens to be exactly equal to the square of the wavelength (in angstroms) of the neutron [drop (in cm) = λ^2 (in Å)].

1956

A milestone in molecular biology was reached when L. Astrachan and Elliot Volkin, working with *E. coli,* a bacterium, discovered messenger RNA, which directs the order of assembly of amino acids into proteins.

Takashi Makinodan successfully replaced the blood of experimental mice with rats' blood in an experiment to learn if bone marrow transplants could be achieved in man to alleviate injury to the blood-forming cells from ionizing radiation. By administering an almost fatal dose of x rays, Makinodan destroyed the mice's blood and capacity to reject foreign tissue. He then injected the mice with rats' bone marrow, which produced 100% rats' blood in the mice.

Health physicists G. S. Hurst and Rufus H. Ritchie made the first of a series of ORNL visits to Japan to correlate ORNL test information with data developed by Japan's Atomic Bomb Casualty Commission on persons exposed to radiation during the Hiroshima and Nagasaki bombings. At this time, health physicists at ORNL were developing new techniques of neutron dosimetry that have been used throughout the world.

An Ecology Science Section was formed under Stanley Auerbach in the Health Physics Division as more emphasis was being given to ecological studies, begun in 1954, to examine the effects of radioactivity (from buried radioactive wastes, for example) on bacteria, fungi, mites, and insects in soil and fresh water. (The Ecology Section evolved into the Environmental Sciences Division in 1970.)

Metallurgists and solid-state physicists studied the effects of irradiation on elastic and plastic properties of polymers, ceramics, and alloys, and determined the extent of gamma-ray damage in germanium. In-reactor irradiation studies at 4°K (using liquid helium to preserve damage in irradiated samples) were achieved for the first time at the Graphite Reactor by R. R. Coltman, C. E. Klabunde, T. H. Blewitt, and J. T. Howe.

Liane B. Russell performs some of her early studies on mammalian genetics and their response to ionizing radiation (1950).

The International Biology Division

By VIRGINIA P. WHITE

Virginia White was an administrative officer in the Biology Division from 1955 to 1967. Since then she has held executive positions in the Salk Institute and the Woodrow Wilson International Center for Scholars. She is currently on leave from her position as Director of the Office of Sponsored Research at the Graduate School and University Center of the City University of New York to write her second book. Her first, "Grants," is a compendium of information about research money and how to get it.

The Oak Ridge National Laboratory was purposely located in an out-of-the-way place, as everyone knows, where it would be difficult for the world to beat a path to its door. That the world did, indeed, find its way to that beautiful isolated spot looking out on both the Cumberland and Great Smoky Mountains is a tribute to the quality of the scientists who came there after the end of World War II, especially those who were

Sheldon Wolff, Richard D. Brock, CSIRO Australia, Sir Julian and Lady Huxley.

dedicated to turning the wartime installation into a laboratory where research concerning the peaceful uses of atomic energy could be carried on.

When I came to the Biology Division in 1955, I had recently lived in three of the world's most sophisticated and important cities, Tokyo, Manila, and New York; and in moving to Oak Ridge, I resigned myself to settling into a "backwoods" community where contemporary social and cultural developments would take years to penetrate. The extent to which I was wrong about this—the extent to which the world did beat a path to the ORNL door—can be well illustrated by one of my earliest experiences.

R. LIVINGSTON

I had been invited for a weekend of camping and hiking in the Smoky Mountains with a group of biologists of whom I knew only one or two. I did know a little about hiking and camping in the mountains, however, and I knew that it was a very down-to-earth operation that soon peels away the pretenses and facades with which most of us protect ourselves in our daily lives. On Friday, Dr. Alexander Hollaender, the Biology Division Director, came into my office and said that he was expecting Dr. Julian Huxley and his wife to visit the Laboratory the following week and that he had just heard from them that they would be arriving that evening instead of Sunday as planned, because they would like very much to go camping in the mountains for the weekend. He asked me to arrange to include them in our party. I spluttered something about being only an invited participant myself and not one of the organizers, adding that it was a very informal "blue jeans and sneakers affair" and nothing like an elegant "safari" that the Huxleys might expect. He assured me that, whatever it was, the Huxleys wanted to do it, and as everyone who worked with Dr. Hollaender in those days understood, that meant the Huxleys were in. I passed the word along to the group with some apprehension, but to my surprise no one turned a hair. They just wanted to make sure that I passed the word along that we would be "roughing" it, and there would be no frills. We need not have worried. Not only did the Huxleys not put a damper on the weekend, but they turned out to be the most delightful of companions in the woods, around a campfire, on the trail, or wherever we found ourselves. Lady Huxley is an amateur botanist, in the sense of "amateur" that exists only, I believe, in England; that is, she knows more botany than many professionals, and we learned a lot from her. She did worry us a bit, however, by insisting on plucking specimens from trees or shrubs, and we were terrified that the park rangers might arrest her. Sir Julian loved to play word games, and at meal time or on the drive to and from Gatlinburg he kept us on our toes with spirited and stimulating games.

C. M. HAALAND

And I learned why the biologists took in stride the announcement that the Huxleys would be joining them; they were quite accustomed to having scientists from all over the world turn up unexpectedly and request to be included in a hiking or camping outing. On that particular trip, the Huxleys may have been the "stars," but the party also included a prominent Brazilian scientist, Dr. Crodowaldo Pavan, who later spent a year in Oak Ridge, and Dr. Richard Brock of CSIRO in Canberra. As a matter of fact, hiking in the mountains with scientists from abroad was a common experience. Dr. Hollaender's Sunday morning hikes in the Cumberlands rarely took place without at least one foreign visitor who was seeing the Tennessee mountains and collecting prehistoric fossils for the first time. The Division had visitors from every country of any substantial scientific development and from several countries where the scientific activity was just beginning.

Under Dr. Hollaender's leadership and inspiration, the Biology Division was one of the first places in this country to seize the opportunity provided by the State Department's Exchange Visitors Program to invite investigators from laboratories in other countries to spend varying periods of time working with Division scientists who had similar or overlapping research interests. In 1956 the Division welcomed the first scientists from abroad, a young husband and wife from France: Michel A. Sicard from the Institut National Agronomique in Paris, and Nicole A. Sicard from Saclay. Michel and Nicole, who are now at Toulouse, have maintained close ties with Oak Ridge and still visit there from time to time. The Sicards were soon joined by other visitors: J. C. Tohá from the University of Chile; Miguel Mota from Sacavem, Portugal; Sang Chil Shim from Seoul National University, Korea; M. A. Patetta-Queirolo from Montevideo; and Kamla Kant Pandey from India came in 1957. Between 1956 and 1967, a total of 160 scientists from 34 countries carried on research as visiting investigators in the Division, and during 1967 the Laboratory hosted 42 visiting scientists from 17 countries.

J. T. MIHALCZO

My own role in this program ranged from participating in the process of selection of applicants to helping the visitors acquire visas, housing, pots and pans and blankets, baby cribs, and the like, to set up housekeeping when they arrived. The visa problems were very nearly insurmountable in some cases, due partly no doubt to our precipitously leaping into the program before the State Department had ironed out all the details and formulated workable guidelines. I like to think we prodded them into establishing procedures earlier than they might have without our impatience to get people into the Laboratory from other countries.

One of the most dramatic visa problems I had to deal with occurred in the early 1960s. It involved a Vietnamese scientist, a member of a politically prominent family with members in high position in both Saigon and Hanoi. Immediately after the assassination of Diem, the visitor was ordered to return home. This was too frightening for the scientist to face, and we spent many days— and some nights—on the telephone with officials in the State Department, who finally resolved the problem just a few hours before the departure order would have had to be complied with.

Another visitor I particularly remember was a woman biologist who came from Moscow to participate in a one-day seminar. Since permission for her visit to the United States had been received from the authorities barely 24 hours before she had to leave Moscow in order to arrive for the meeting, she had received all the necessary immunizations at one time, just before her departure. The reaction began soon after she arrived, and her entire visit to Oak Ridge was spent lying in her room at the Alexander Hotel, talking with me and Dr. Tom Lincoln, who came to see her and give her some relieving medication. She assured me that she would return to the Soviet Union with kind memories of the United States and a good impression of American medicine, anyway.

It is not unusual for individuals to acquire nicknames; in fact, it happens to nearly everybody at some time, often beginning in early childhood. But for an institution to be dubbed with a nickname is not so common. Sometime around the late 50s or early 60s, the Biology Division began to be referred to in various parts of the world, at home and abroad, as "The International Biology Division," and is still lovingly referred to that way in many laboratories throughout the world today.

1957

HRE-2, the aqueous homogeneous reactor in which the enriched uranium fuel is dissolved in heavy water that serves as moderator and coolant, went critical December 27 (and reached full power in March 1958, producing 5 MW of heat and 250 kW of electricity). The HRE-2 was the seventh power reactor, either experimental or full scale, to be placed in operation in the U.S. in 1957 (the first commercial power reactor began operating at Shippingport, Pa.).

In the Aircraft Nuclear Propulsion Program, ORNL researchers completed design of the Aircraft Reactor Test, incorporating the benefits of experience with operation of the ARE in 1954. But the ART design was shelved. The national nuclear airplane program was attacked in Congress because of its high cost and because changing military priorities made the achievement of a nuclear airplane less important as a national goal. The AEC curtailed the aircraft reactor development program, and the national ANP program was terminated June 30, 1961.

In solid-state physics, R. H. Silsbee postulated the concept of correlated collisions in solids, an idea that was to be most useful in subsequent radiation damage theory.

K. Z. Morgan, Director of ORNL's Health Physics Division and the father of the science of health physics, was becoming influential in the setting of national and international radiation protection standards. As Chairman of the International Commission on Radiological Protection Subcommittee on Permissible Doses for Internal Radiation, Morgan made recommendations based on ORNL studies on what should be done to ensure the safe application of x rays and radioactive materials for medical diagnosis and treatment. Morgan was a leader for many years in setting standards on permissible doses. He also urged concern about the exposure of uranium miners to radon gas.

In the year that ORNL's Thermonuclear Experimental Division was formed in support of Project Sherwood, a "breakthrough" was achieved in the observation that a high-current vacuum carbon arc would efficiently dissociate deuterium molecular ions so that the resulting atomic hydrogen ions would be effectively trapped inside the magnetic field. The Direct-Current Experiment (DCX) was constructed for detailed studies of phenomena occurring in such a device, the objective being to see if a dense, long-lasting plasma could be achieved at these high temperatures so that controlled fusion reactions might take place. Operation of the DCX was achieved in July, and successful trapping of the accelerated beam was accomplished August 21.

Two full-scale operating models of DCX devices were designed and constructed at ORNL for exhibition at the Second International Conference for Peaceful Uses of Atomic Energy in Geneva, Switzerland, in 1958. During actual operation of these devices, the trapped ring of ions was made visible to visitors by dusting carbon particles into it from above.

A. W. Tell, designer of manipulators; C. R. Ferrell, machinist; and Stewart Dismuke, physicist, are seen at the Physics of Solids Building completed in 1952.

V. P. WHITE

Special Tribute

William Archibald Arnold, physiologist, physicist, and a biologist at Oak Ridge National Laboratory since 1946, retired in 1970, and since then has continued to pursue his research into the wonders of photosynthesis as a consultant to the Laboratory's Biology Division. When he received the American Society of Plant Physiologists' Kettering Award, he was cited, in part, for: "... his application of the rigorous principles of physics to a biological phenomenon; his intrepid manipulation of biological materials; and his ability to grasp a problem, frame it in experimental terms, and to follow its resolution full course."

Thermonuclear Beginnings

By JULIAN DUNLAP

Julian Dunlap came to the Thermonuclear Division in 1959 to direct the first Direct-Current Experiment (DCX-1). He describes here the Laboratory's fusion research up to the concentration on toroidal confinement that resulted in ORMAK.

Our research in fusion plasma here at ORNL is now centered on toroidal geometry, with concentration on tokamak devices (like ORMAK) and a smaller effort on the ELMO Bumpy Torus. This was not the case when I joined the Laboratory in 1959; the theme then was high-energy, molecular-ion injection into magnetic mirrors, and the prime experiment was the DCX-1 (Direct Current Experiment).

Ed Shipley and John Luce were the first of our staff to become very much involved with CTR (controlled thermonuclear research). They were engrossed in its problems as early as 1952 and were soon joined by others including Al Simon, Rodger Neidigh, and Mozelle Rankin (now Mrs. P. R. Bell). Luce advanced the idea of using energetic molecular ions to create a hot atomic ion plasma of hydrogen in 1955. He suggested injecting the molecular beam transversely into a magnetic field and using an arc discharge operating along the field to efficiently dissociate these particles. This dissociation would yield a change in charge-to-mass ratio that would result in trapped orbits for the fast atomic ions ($H_2^+ \rightarrow H^+ + H^0$). Luce and his co-workers also discovered a high-vacuum carbon arc that made an efficient dissociator.

W. D. BURCH

In this scheme, the field, beam, and arc could be operated in steady-state, in contrast to the pulsed approaches elsewhere, so ours became known as the DCX-1, the first experimental facility that would allow study of the trapped ions themselves. It came on-line in mid-1957, with 600-keV injection into a simple magnetic mirror. Consideration had been given to other magnetic geometries, but mirrors were most convenient for tackling the experimental problems involving injection and trapping. With these solved, the questions of confinement limitations in mirrors could be addressed, and perhaps the techniques applied to other geometries.

A few months after my arrival I settled in to work with DCX-1. Ed Shipley and P. R. Bell were directing the fusion research then. Luce and several co-workers were involved in a number of exploratory studies, Al Simon was in charge of the theoretical effort, Bill Gauster headed a group specializing in magnetics, George Kelley was in charge of ion source development, Ed Bettis directed an engineering section, C. F. Barnett was responsible for operation of DCX-1, and Ray Dandl was responsible for diagnostics on it.

It seemed that nothing was fundamentally wrong with the approach. We had not obtained "burnout," the predicted rapid decrease of charge-exchange losses through ionization of background gas by the trapped ions, with a marked increase in trapped density. But it was probably a matter of technological improvements—more injected beam current, better vacuum, and increased dissociation.

G. L. GOODWIN

The DCX-1 experiments in 1959 showed that the carbon arc was an important target for charge exchange of trapped ions at low background pressure and that these pressures were accompanied by a spreading of the ions to significantly larger volume—both effects that reduced the chances for burnout. Planning toward a successor to DCX-1, one that would allow a larger plasma volume, had already been under way for some time. The results with the arc in DCX-1, along with other factors, led to planning around a "multiple pass" concept in which a long constant field region between the two mirrors would allow the molecular beam to pass repeatedly through the dissociative arc instead of only once. Less dense and more completely ionized arcs, say, of hydrogen or perhaps lithium, should then provide nearly total dissociation of the incoming molecular beam, and perhaps enough molecular orbits could be provided to yield high densities by trapping on background gas alone. Out of these considerations was to come the DCX-2, whose principals were P. R. Bell, George Kelley, Norm Lazar, and Bob Mackin.

A wide variety of dissociative arc configurations (carbon and gas) were developed by the Division in the early 1960s. Our experimental program on DCX-1 involved examining a number of these, with continuing improvements in vacuum, injected current, and diagnostics. Arcs limited accumulation of trapped ion density through charge-exchange losses, and the trapped plasma was spread in both energy and volume. Plasma instability was observed in the quieter environment of gas dissociation, and the spreads became significant there with the improvements in vacuum and current. Arcs in DCX-1 were gradually de-emphasized in favor of the instability studies. This trend was essentially completed in 1963 with modifications to use Lorentz trapping (the molecular dissociation due to $v \times B$ forces). By then, Dandl had moved on to creating plasmas by electron cyclotron heating with microwave power; his present ELMO Bumpy Torus is the latest of that series of experiments. Barnett had returned to full time on cross sections and diagnostic development. Herman Postma had joined the group shortly after I did, and he and I were in charge of the experiment.

The first Direct-Current Experiment (DCX-1) group (1961). Left to right: J. A. Ray, R. G. Reinhardt, R. S. Edwards, R. M. Warner, Herman Postma, L. A. Massengill, G. R. Haste, J. L. Dunlap, C. F. Barnett, Dave Price (Y-12 foreman), W. J. Schill, and E. R. Wells.

1965 was a year of crisis for the DCX approach. The DCX-1 experiments were coming to an end. The dominant instability had been identified as negative mass, and with Lorentz trapping, it limited the density to $\sim 10^{8}$ cm^{-3}. (By way of comparision, densities with arc dissociation had approached 10^{10} cm^{-3}.) DCX-2 had come on line in 1962. It was dominated by instability and was still in a period of internal diagnostics. Maximum densities there were $\sim 10^{10}$ cm^{-3}, and hopes for improvement by orders of magnitude were fading.

Also, that was the year that Ken Fowler was able to collate theoretical insights (a significant amount developed here) into a prescription for plasma stability in mirror geometry. Large plasma dimensions and a complex field structure were called for—criteria whose tests at our high injection energies would require experiments of staggering size and cost.

The next few years were uneasy ones. DCX-2 continued. For many of the rest of us, the injection of lower-energy ($\sim$15-keV) H^0, possibly into an electron-cyclotron-heated (ECH) target plasma, seemed the way to go. For a while after 1967, these elements were to merge into a Target Plasma Program centered around injection into ECH plasmas in the facilities INTEREM and IMP.

DCX-2 ceased operation in 1969, after its limiting instabilities had been identified. Like DCX-1, it had fulfilled its mandate either to succeed or to illuminate the reasons for its failure.

One era in thermonuclear research at the Laboratory ended with the passing of DCX-2, and another began. The apparent successes of the Russian tokamak approach were announced at that time, and these fired the imaginations of George Kelley and John Clarke (of the DCX-2 Group) and Mike Roberts. Their enthusiasm was transmitted to Herman Postma, who had succeeded Art Snell as Division Director in 1967; and so, also in 1969, began the ORMAK program.

1958

The Oak Ridge Research Reactor (ORR) achieved criticality March 21 and reached its design power of 20 MW on May 29. The $5 million reactor, moderated and cooled with water, provided the highest neutron flux in the world and replaced the Graphite Reactor as the chief source of radioisotopes. Using the ORR, researchers developed the field of neutron spectroscopy. The first experiment completed in the ORR involved the determining of the direction of emission of neutrinos, which accompany beta particles from decaying helium-6 produced in the reactor by bombarding beryllium-9 with fast neutrons. The experiment, carried out by Cleland Johnson, Frances Pleasonton, and Arthur Snell, employed the technique of recoil spectrometry (an Oak Ridge invention), which measured the recoil momentum of the residual lithium-6 nucleus, thus allowing researchers to infer the direction of neutrino emission.

W. R. Gambill and N. D. Greene found that forcing cooling water to swirl (in vortex flow) around hot metal surfaces significantly improves the boiling heat transfer rate. The vortex apparently prevents steam from blanketing and insulating heat transfer surfaces—a situation (burnout) which could reduce the heat transfer rate in a reactor enough to cause melting of the reactor tank walls or fuel cladding.

Ecological studies of the slightly radioactive lake bed left when White Oak Lake was drained in 1955 revealed that strontium-90 was effectively transferred from soil to the natural vegetation; in some vegetation species, the strontium-90 concentration

Frances Pleasonton and Art Snell check the performance of the coincidence circuits used in their 1951 experiment for detecting the radioactive decay of the neutron. The coincidence counters were temporarily built into a pile of lead bricks into which Miss Pleasonton inserts a radioactive source used for testing.

was found to be as high as 50% of the soil concentration.

A revolutionary finding in radiation genetics was made at ORNL when William L. Russell, Liane B. Russell, and Elizabeth M. Kelly discovered that the mutation rate in mice is lower at low dose rates of radiation than at high dose rates, for the same total dose of radiation. It was also shown that the mutation rate, in response to chronic irradiation, was less than one-fourth that for acute radiation, again for the same total dose.

ORNL's largest single project, the aqueous homogeneous reactor (HRE-2), operated at high power (5 MW) on March 29, three months after going critical. The reactor, however, encountered problems with corrosion and solution stability.

Henry Inouye, T. K. Roche, and W. D. Manly developed INOR-8 (currently called Hastelloy N), a nickel-molybdenum alloy (later turned over to industry) that is resistant to oxidation and to corrosive attack of molten uranium and thorium-bearing fluorides at high temperatures up to 720°C. ORNL researchers also established that plutonium can be burned in a molten-salt reactor by showing that PuF_3 is soluble to the extent of 1% in molten lithium fluoride–beryllium fluoride solutions.

ORNL began development work on gas-cooled power reactors moderated by graphite. A design was completed in March for a graphite-moderated, helium-cooled reactor with an output of 252 MW(e). The reactor was to use uranium oxide (UO_2) fuel elements, which were emerging as the standard fuel for solid-fueled power reactors.

Clyde Watson, R. E. Blanco, and others began four years of development work on the Chop-Leach Process for preparing spent fuels for reprocessing, now in worldwide use.

The Fission Products Pilot Plant went into operation for separating and purifying fission products on a large scale to increase the supply of radioactive sources for industrial, medical, argicultural, and research uses. Later it was called the Fission Products Development Laboratory.

Special Tribute

Richard Burton Setlow was a biophysicist at Oak Ridge National Laboratory from 1961 until he transferred to Brookhaven National Laboratory in 1974. The work that earned him membership in the National Academy of Sciences has been cited as follows: "Setlow and his associates at Oak Ridge showed that the thymine dimers in DNA made by the ultraviolet irradiation of cells affected the cells' biological activity. They showed that many cells contained repair systems that eliminated most of the deleterious effects of ultraviolet irradiation. This repair system, called 'excision,' removed the damaged structure and replaced it with new, undamaged material. It was a type of error correction."

R. A. DANDL

Reminiscences on Critical Experiments

By A. DIXON CALLIHAN

Dixon Callihan began his nuclear career as research physicist for the Manhattan District's Division of War Research at Columbia University in 1941. He retired from Union Carbide in 1973, after 28 years in criticality experiments on fissile material. He is editor of Nuclear Science and Engineering *and serves on the Atomic Safety Licensing Board for the Nuclear Regulatory Commission.*

Early 1946 marked the establishment in Oak Ridge of a small research organization which made significant contributions to the advancement of the science and technology of the nuclear community. The task assigned to this group was the determination of the neutronic characteristics and the dimensions of fissile and other materials arranged to duplicate or closely approximate the design of a nuclear reactor or of the equipment for processing, storing, or transporting the fissile material. Further, the collection of materials usually sustained a nuclear chain reaction. In the parlance of the trade, these investigations came to be called "critical experiments."

During its many years of operation, the responsible group investigated many novel designs and conditions and made valuable scientific and technological advances. In one of its first assignments, the minimum chain-reacting, or "critical," mass of uranium hexafluoride (UF_6) and of its solid decomposition products was evaluated. The result showed that the quantity of UF_6—the process material in the gaseous diffusion process for the separation of the uranium isotopes—which could be inadvertently accumulated in the interior of the plant was less than that required to produce a "nuclear excursion" or "nuclear accident." This was true, even when all of the uranium of the UF_6 was the fissile isotope of mass 235 (uranium-235).

Up to this time, the product of the gaseous diffusion plant had been limited to UF_6 in which the uranium was only 30% uranium-235. Processing to the greater isotopic purity then required was accomplished by electromagnetic separation, a batch operation much more costly than the continuous-stream gaseous diffusion. Accordingly, the production of enriched uranium by the diffusion process was increased, and the electromagnetic operation was abandoned.

In another interesting study, the limiting thermomechanical properties of the core material of a fast-pulse reactor were investigated. A fast-pulse reactor is one designed to produce sharp, intense bursts of radiation arising in fission. Such a device is composed primarily of a cylinder of uranium highly enriched in uranium-235 which can be made super-prompt critical. This uncontrollable neutronic reaction is dramatic, and the forces applied to the metal are severe. The thermal expansion of the core terminates the reaction in a few tens of microseconds. The radiation emitted is of value for radiation-damage and dosimetric studies. To avoid mechanical damage, such devices had, in the past, been operated within the limits imposed by very conservative estimates of the physical strength of the core. In the instance cited below, the yield, and hence the temperature and the mechanical forces in the core, was increased until damage truly did occur. It was possible, therefore, to establish acceptably higher operating parameters and to identify weaknesses in the mechanical design.


T. ROCKWELL

On June 16, 1958, the following incident at Oak Ridge occurred as a result of an unfortunate, unintentional production of a nuclear chain reaction in a quantity of an enriched-uranium salt in aqueous solution. During what was, no doubt, considered a routine operation, water was to be drained from some piping, in which a nuclear reaction could not be established because of its dimensions, into an ordinary 55-gallon metal drum, which was not protected against criticality by its dimensions. The operation would not have been untoward except that, in actuality, the water contained uranium, possibly because of a leaky valve upstream. As a result a chain reaction occurred, complete with a "blue glow." Several nearby workmen, in whom safety practices had been instilled, left the scene promptly after the occurrence when the evacuation alarm sounded. It is to be emphasized that no dosimetric equipment was attached to these individuals.

Ultimate termination of the nuclear reaction came about through dilution of the uranium by the water, which was supposed to have been the sole constituent of the drainings. In due course the area was entered, and a sample of the liquid was obtained from the drum. From a radiochemical analysis of that sample, the energy released by the nuclear incident in the drum was determined. In the ensuing hours, a quandary developed. From the measured energy release, a severe radiation exposure to the workmen could be postulated; yet from their appearance, behavior, and certain intensive observations, such as the radioactivity of the individuals' blood sodium, no severe exposure was evident. Even so, the medics and the radiation biologists were preparing for bone marrow transplants and other drastic treatments. So much for a stage setting. Now to the promised recital of the experience.

The Critical Experiments Laboratory essentially duplicated the dimensions and the concentration of the uranium solution in which the event occurred. This was possible from an observation of the liquid surface at the time of the "blue glow" reported by one individual and knowledge of the uranium content of the drum. The resources of several groups in Oak Ridge with expertise in the measurement of neutron and gamma radiation were brought together. The star of the performance was a donkey on loan from the University of Tennessee Laboratory Experimental Station. The critical experiment was operated until the radioactivity of the blood sodium of the donkey was essentially that of the exposed workmen. Analysis of the observed radiant energy emitted while the donkey was being irradiated disclosed that the exposure of the personnel was in the range of a few hundred millirems, consistent with their physical condition, instead of the much larger value derived from the energy measurement.

This verifying experiment was conceived, designed, instrumented, tested, performed, analyzed, and reported in the local press within slightly more than 24 hours, no doubt some kind of a record.

As an epilogue, it should be recounted that several days following the incident, a recording from a radiation monitor placed 1400 ft from the scene was identified as describing the time pattern of the radiation emission. It showed that the energy was released in a succession of pulses over a period of some 20 minutes. These pulses represent the achievement of criticality a number of times, separated by intervals of subcriticality caused by the formation of bubbles of radiolytic gases and of vapor. Obviously, the well-trained workmen left the area posthaste upon hearing the signal probably initiated by the first of the pulses.

Jim Lankford explains the workings of his new socket-wrench set to Harriet Madden, as Dot McCarter signs for her safety award in 1953.

I. SPIEWAK

1959

ORNL experiments proved the feasibility of breeding in reactors with the thorium–uranium-233 cycle. The ORNL studies revealed the reason for discrepancies in British experiments, which raised questions about the ratio of neutrons produced to neutrons absorbed in the fuel material itself (uranium-233). A thermal-neutron breeding study group under H. G. MacPherson suggested that liquid fuel systems (aqueous and molten-salt reactors) have advantages over solid fuel systems by simplifying the fuel cycle so that no fuel refabrication is necessary. The aqueous homogeneous reactor experiment (HRE-2) operated uninterrupted for 105 days. In other reactor-related developments, researchers devised an optical system for outside viewing of the interior of a reactor core and used an ultrasonic probe to determine the extent of corrosion in a zirconium reactor tank. The Oak Ridge Research Reactor operated as long as 24 hr at power levels up to 30 MW with a maximum neutron flux approaching 4×10^{14} neutrons $cm^{-2} sec^{-1}$.

Chemical reprocessing developments in 1959 included the acid Thorex Process by R. H. Rainey and others for the solvent extraction reprocessing of highly irradiated and short-decayed thorium fuels; this process is expected to be used as part of the high-temperature gas-cooled reactor technology. It was also during this year that researchers prepared the first liter-scale quantities of di(*sec*-butyl) phenylphosphonate and developed a process for using it instead of tributyl phosphate for separating uranium-233 from irradiated thorium. (This new organic extractant has been used in the 1970s at the ORNL Pilot Plant for reprocessing many hundreds of kilograms of uranium-233 for the Light-Water Breeder Reactor Program, the work being done under a contract with the Bettis Atomic Power Laboratory.)

In thermonuclear fusion studies, ORNL researchers learned how to establish a trapped, circulating current of H^+ ions in the DCX.

And from Orlo Myers . . .

The day I finally got inside the restricted area inside the restricted area inside the restricted area, in March of 1944, may have been the same day the first visible quantity of "49," alias copper, processed as the fluoride, was displayed throughout Building 706C (if I remember) in a 5-milliliter test tube which was headed for the Metallurgical Laboratory of the University of Chicago by messenger via L&N.

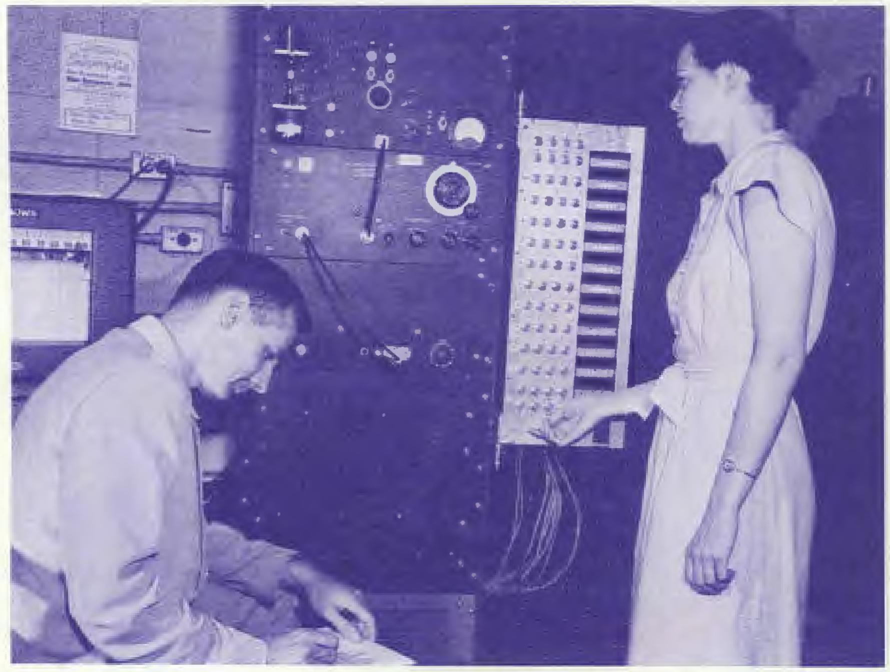
Later on, in Dayton, I met the man who spilled it.

At that time, he told me, he sincerely believed that they were going to cut off his hands to recover the plutonium.

This gentleman, who at last word is an eminent practicing radiologist, Bernie Brodie, had been working with the material for 36 hours without sleep.—Orlo Myers, Staff Scientist with Convair Arrospace Div., General Dynamics Corp, San Diego, Cal.

Special Tribute

Eugene Wigner, ORNL's only Nobelist, is adjudged by Alvin Weinberg to be, simply, "the world's greatest living physical scientist." His gigantic mind and gentle philosophy set the standards for Laboratory engineering and research in the formative days when he served as its Research Director. His vast store of knowledge and broad scientific understanding across the disciplines surely serve to fulfill any definition of genius that has yet been propounded. In a tribute to him recently, Ralph Lapp had this to say: "Wigner cannot be judged as a physicist alone because he is also a skilled engineer, chemist, mathematician, and philosopher. And any assessment of his worth must take into account the triple role he played in the U.S. atomic project: his initiative in proposing that such a project be started; his engineering skill in the design of water-cooled reactors; and a great many of his other scientific contributions that shortened the period of time it took to develop the first atomic bomb. Although Wigner might appear to be a model ivory tower scientist, he has occupied himself with the realities of the Cold War. One senses that it is his deep regard for human dignity that prompts him to speak out against all oppression and the governments that foster the regimentation of people." Oak Ridge National Laboratory has been made greater for his contributions to it.



George Kelley and Judy Cassidy at multichannel, pulse-height analyzer developed by Kelley. "It is believed the instrument will greatly facilitate research with rare earths, some of which are minutely radioactive and therefore difficult to study" (1950).

1960

In his "State of the Laboratory" address, Weinberg hailed the progress made by ORNL in the field of instrumentation. Among the most noteworthy developments were the germanium-barrier counter (which accurately measures energies of alpha particles) and a new personnel radiation monitor (the "pocket screamer" that chirps and flashes in response to rising gamma radiation levels). The use of the pocket screamer, developed by R. Dilworth and C. J. Borkowski, was one of several measures taken by ORNL officials to reduce the possibility of potentially serious radiation accidents.

In the field of radioactive waste disposal, ORNL developed the techniques of pot calcination and burial by hydrofracture. The first technique involves the reducing of high-level liquid wastes to immobilized radioactive ceramic materials by heating the wastes to 900°C in a stainless steel pot. The second technique (developed under E. G. Struxness and W. de Laguna) entails the drilling of a hole for a steel-cased well 1000 ft deep and pumping down liquid cement grout and radioactive waste under high pressure. The injected grout causes cracks in the shale and spreads into thin sheets between the strata, where the radioactive waste solidifies.

ORNL made several advances in reactor technology. The hole patching job on the aqueous homogeneous reactor (HRE-2) proved ORNL's ingenuity in remote maintenance and inspection, a technology since perfected at Oak Ridge. In addition, ORNL successfully operated its first aqueous slurry loop, which contained oxides of thorium and uranium-235.

In thermonuclear fusion research, ORNL researchers gained important information from DCX-1, including the observation that protons in a trapped ring go into a lumped distribution above certain densities. Experiments done under R. A. Dandl on radio-frequency heating of electrons in a magnetic mirror demonstrated that a plasma with hot electrons can be suprisingly stable.

ORNL received the funds to draw up plans for a 5- to 10-MW Molten-Salt Reactor Experiment (MSRE), whose purpose was to show whether it is feasible to operate and maintain a high-temperature molten-salt reactor for long time periods. The project was guided at first by H. G. MacPherson. One of its first achievements was to find a solution to the UO_2 precipitation problem. In the area of gas-cooled reactors, ORNL researchers took on the challenge of making a uranium-fuel carbon cladding that would retain fission products so as to prevent contamination of the gas coolant.

Concerned that it was a gamble for ORNL to focus on just a few big nuclear reactor projects, Weinberg organized Advanced Technology Seminars with senior researchers to explore possible nonnuclear missions in the national interest appropriate to the unique capabilities of a large, multidisciplinary research institution. These areas included water desalination, space technology, and large-scale biology.

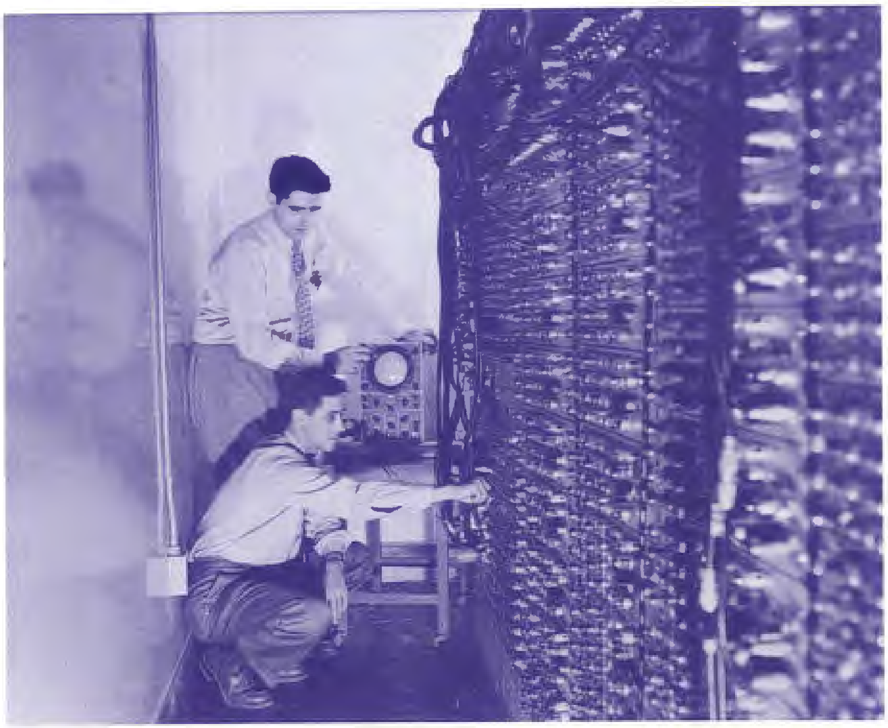
Samuel C. Lind, left, was honored at a testimonial dinner at the Oak Terrace in May 1952. On his left are Mrs. E. H. Taylor, C. J. Borkowski, and Mrs. C. E. Center.

More from Kyger . . .

In the Engineering Materials Section, we also worked on shielding materials. Ted Rockwell headed this work, and he noted in the literature references to a magnesium oxychloride concrete which contained much beneficial hydrogen in the form of water of crystallization. Ted decided to make a batch of this stuff and run some experiments on it. For reasons that I no longer remember, the mixing had to be done some distance from where the pouring was to be done. I remember this distance as being several hundred feet. Anyway it was decided to run it through a large-diameter pipe from the mixing site to the pouring site. Predictably, in retrospect, the magnesium oxychloride concrete set up faster and harder than expected and we ended up with a long steel-encased rod of the stuff. That's why $MgOCl_2 \cdot xH_2O$ is not used for reactor shielding today.—Jack A. Kyger

Special Tribute

Floyd Leroy Culler has spent his whole professional life in Oak Ridge, first during World War II at the Y-12 plant in electromagnetic separations, then since 1947 at the Laboratory. For 12 of his 18 years in the Chemical Technology Division he was its Director. He participated in or directed the development of almost all the major radiochemical processes now in use in the U.S., both as Division Director, and later as Assistant Laboratory Director. Since 1970 he has been Deputy Director or Acting Director of the Laboratory. The Ernest Orlando Lawrence Memorial Award in the field of Reactor Fuel Technology was given to him in 1965 "For meritorious contributions to the development of processes for the recovery of irradiated fuels." On election to the National Academy of Engineering in 1974 he was cited for his "contributions to the development of successful nuclear power."



Les Oakes, standing, and Jimmy Stone check out an early digital computer that originated in the NEPA Project and was later reactivated for ORNL. The ORACLE came later (1953).

1961

This year marked a turning point for ORNL as major nuclear programs were phased out, resulting in staff reductions, and new projects, including one small nonnuclear effort, were initiated. The nonnuclear effort–a water desalination research project that used ORNL's experience and expertise in the physical chemistry of dilute aqueous solutions—signaled the beginning of ORNL's evolution from an exclusively nuclear energy laboratory to one that is responsive to national needs in other areas.

The ORNL projects stopped by the AEC were the Aircraft Nuclear Propulsion (ANP) Program (the national program on which $1 billion had been spent since the late 1940s was also terminated), the Homogeneous Reactor Project, and the proposed power reactor fuel processing pilot plant (the AEC said that the aqueous-based processes developed at ORNL would be tested at AEC's Idaho Chemical Processing Plant). The aqueous homogeneous reactor (HRE-2), which had been operating with a patched-up Zircaloy core tank at powers up to 5 MW for 2300 hr, was shut down finally on April 28. Weinberg said in his "State of the Laboratory" address that the years of effort in the ANP Program and Homogeneous Reactor Project could be justified because the experience would be useful for developing the molten-salt reactor and thorium breeder.

New nuclear projects established by the AEC at ORNL were a pure materials center, a center for production of transuranium elements, a thorium utilization program to study ways of making and recycling thorium oxide for use in breeding uranium-233, and the space nuclear power program, which developed SNAP (Systems for Nuclear Auxiliary Power) isotope heat sources for use in space, remote meteorological stations, and underseas transmitters.

The tandem Van de Graaff accelerator—with its capability of accelerating protons up to 11 MeV and heavy ions to higher energies—was placed in service.

Solid-state studies at ORNL continued to grow as researchers studied the Mössbauer effect and developed a gold-mercury alloy by transmutation. Roger Boom and L. D. Roberts fashioned a superconducting magnet from 545 m (1800 ft) of niobium-zirconium wire (the Nb-Zr alloy had been used in the homogeneous reactor because of its resistance to attack by uranyl sulfate). M. T. Robinson, D. K.

Holmes, and O. S. Oen predicted theoretically the channeling of energetic ions traversing crystals.

The DCX-2 (a larger high-energy injection mirror machine for fusion research) was completed and was undergoing shakedown tests. Further studies were done on the stable hot-electron (but cold-ion) plasma made by electron cyclotron heating in a magnetic mirror device.

The Isotope Target Laboratory was established to prepare samples containing specific stable or radioactive nuclei in known atom density and in appropriate physical and chemical form so that nuclear interactions could be studied.

At the newly acquired Multilith machines in ORNL's Reproduction Department are Hall McLean and Charles Taylor in the back, and Brena Stevens in the foreground (1952).

1962

This year marked the beginning of diversification, when ORNL began to take on research projects for other Federal agencies; these projects represented only a small fraction of the total Laboratory budget but were of broad interest to the AEC. They included: (1) work for the National Aeronautical and Space Administration in designing a radiation shield for Apollo and other manned rockets, and (2) basic studies (under Kurt Kraus) of the physical chemistry of seawater for the U.S. Department of Interior's Office of Saline Water. ORNL performed related space and desalination work for the AEC. The Isotopes Division prepared curium sources to provide power for Surveyor and other satellites as part of the AEC's Systems for Nuclear Auxiliary Power Program. And the AEC supported an ORNL desalination project under R. P. Hammond, a Los Alamos scientist on leave who made a case for the economics of scaling up nuclear reactors. With feasible scaleup, Hammond argued, such nuclear plants could offer power at prices competitive with coal-fired stations. Adding to this the use of waste heat for distilling seawater, Hammond conceived of a complex for nations with arid lands that could be farmed if desalinated water for irrigation were made available. Hammond's ideas, modified by Floyd Culler and promoted by Weinberg, received international attention.

ORNL was acquiring a reputation as a home for "big science," Weinberg's term for teams of scientists and engineers attacking a problem with scientific machines costing $3 million or more. Among the ORNL machines being designed and built for the early 1960s at this time were the Oak Ridge Isochronous Cyclotron

Special Tribute

Charles Dubois Coryell was at the Metallurgical Laboratory for a year and at Clinton Laboratories for three years, specializing in the chemistry of the fission products of uranium and in nuclear chemistry in general. He was always teaching, and there was always excitement around him. When two young chemists under his direction isolated a new element, promethium, he let them announce it to the world. He was wont to say that his greatest value to the Manhattan Project was his prodigious memory for reports by number and for process solutions by molarity and for nuclear level schemes. The tribute to him at his death in 1971, written by colleague and former student Glen E. Gordon, said in part: "... in retrospect, most students found they had come away from their contact with him bearing far more insight than they realized at the time. If it is possible for one to teach 'creativity,' Charles Coryell did so."

D. R. McKay shows the small size of the 300-curie cobalt-60 source loaded in 1951 for use in a new cancer therapy unit to be tested at the ORINS Medical Division. He is standing next to the huge steel-sheathed lead container and lid used for processing the capsule prior to loading into the head of the unit.

(first operated March 18, 1962), the High Flux Isotope Reactor, and the Transuranium Processing Facility for processing targets and recovering the californium-252 that was to be produced in the HFIR.

A biology program on low-level radiation of mice was started to clear up the uncertainties about the somatic effects of low-level doses. In 1966, the Low-Level Experiment began, as 30,000 mice were exposed to doses as low as 10 rads to determine whether small doses of ionizing radiation increased the incidence of leukemia and cancer and shortened the life span of mice. The experiment, started under Arthur Upton, is now being wrapped up under the direction of John Storer.

But there were also achievements in "little science" in 1962, as H. A. Levy and M. D. Danford used x rays to determine the structure of liquid water, and H. W. Schmitt, J. H. Neiler, F. J. Walter, and A. Chetham-Strode measured the mass and kinetic energy distribution of fission fragments formed in three-particle thermal-neutron-induced fission. Biologists R. B. Setlow and Jane Setlow clearly demonstrated that thymine dimers—two fused adjacent thymine molecules in the DNA chain—are formed when a cell is exposed to ultraviolet light, which explains one mechanism of much ultraviolet radiation injury. In addition, the Setlows showed that enzymes can repair the damage.

In the field of reactor development, chemical technologists developed fuel elements containing uranium carbide particles, coated with pyrolytic carbon, that could retain more than 99% of generated inert fission gases and shorter-lived fission products. Success was also achieved in using the fluoride volatility method, first proposed in 1942, to recover uranium-233 bred in a thorium blanket and to recover unburned uranium from solid fuel elements as well as from molten-salt reactor fuels. ORNL continued to focus much of its reactor research efforts on molten-salt reactors (the basis for a thorium breeder) in a year in which the AEC issued a report to the President urging aggressive development of breeder reactors.

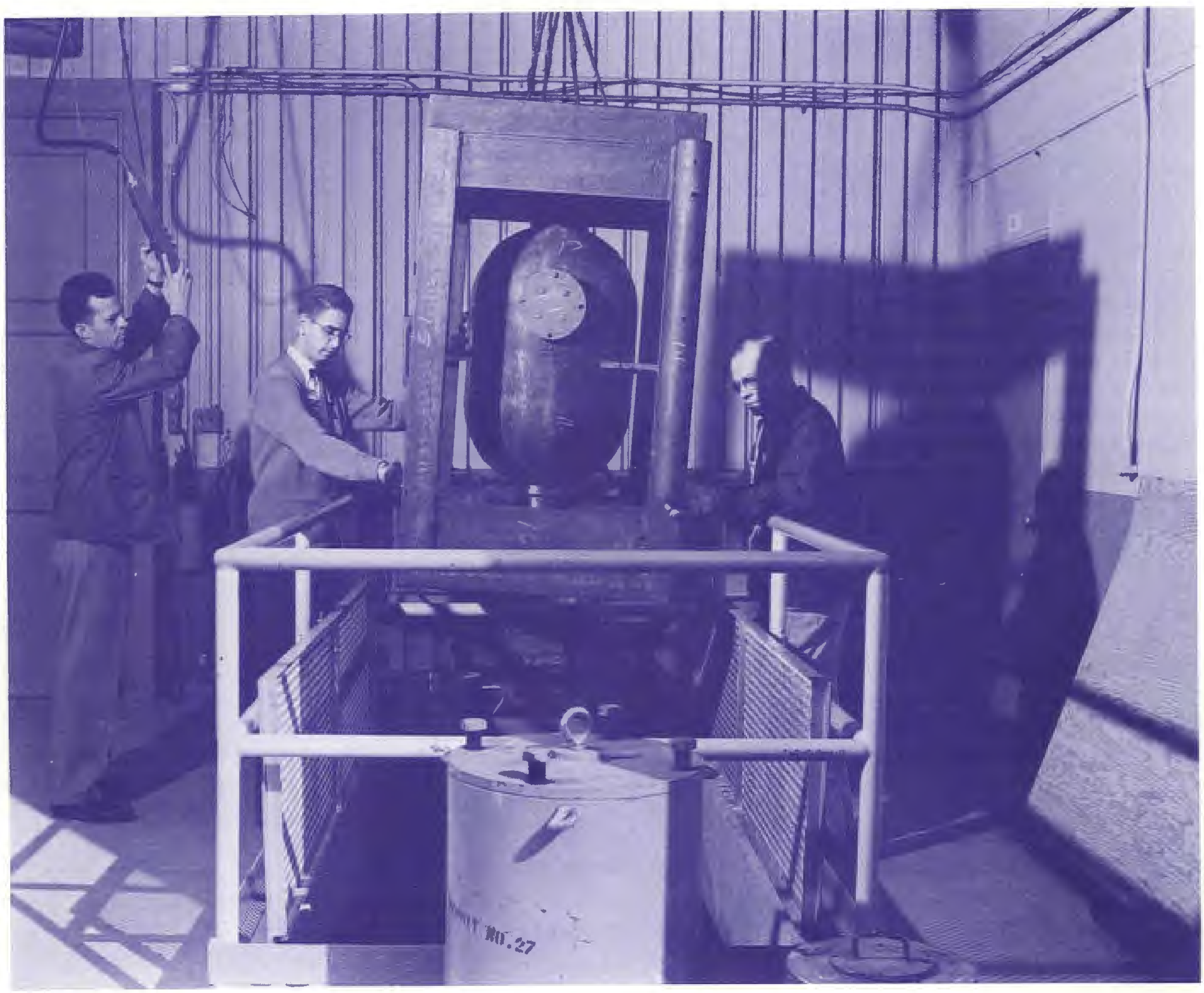
The Health Physics Research Reactor (HPRR) was constructed and was used for the first time on a 1527-ft-high tower in Nevada. The use of the HPRR on the special tower permitted the first detailed measurements of the air-ground interface effect and other important parameters. In 1963 the HPRR was shipped from Nevada and installed in its permanent home at the DOSAR (Dosimetry Applications Research) facility at ORNL. It has been used ever since for radiobiological experiments, dosimetry research, and genetics studies.

Since, in 1962, there was mounting concern about the proliferation of information, Weinberg noted that ORNL was aiding in organizing scientific information in a concise, meaningful way by being a home for several technical journals and by establishing seven specialized information centers.

The journals were *Nuclear Science and Engineering* (E. P. Blizard), *Journal of Applied Physics* (J. C. Crawford), *Health Physics* (K. Z. Morgan), and the AEC technical progress review journal, *Nuclear Safety* (W. B. Cottrell). The specialized information centers, set up to condense, review, and interpret literature for the rest of the scientific community (and, in some cases, make new scientific generalizations based on available data), covered the following areas: research materials, internal dose, nuclear safety, radionuclides, radiation shielding, nuclear reactions of charged particles, and isotopes. In 1963, the Isotopes Information Center started the publication of another AEC technical review journal, *Isotopes and Radiation Technology* (P. S. Baker).

Six years of work began on developing the Tramex and Cleanex Processes for recovering transuranium elements from spent fuels.

The largest cobalt-60 source ever produced in the U.S. by 1952 for treatment of cancer in humans was prepared for shipment here. Henry Grimoc, left, W. R. Casto, and G. L. Neely juggle the 3000-lb container on a crane in preparation for the decontamination necessary before loading and shipping the source to the Los Angeles Tumor Institute.



Special Tribute

William Lawson Russell organized—and remained for 28 years the director of—the world's largest study of the genetic effects of radiation in a higher mammal, a project involving thousands of mouse generations. His "Distinguished Achievement Award" from the Health Physics Society in 1976 cited "the work of a man who has contributed enormously and almost equally to fundamental radiobiology and the derivations therefrom of importance to radiation protection philosophy and standards." In 1973 he was elected to the National Academy of Sciences; in the same year he and his wife Liane shared the International Roentgen Medal for "outstanding contributions to the progress of research and applied science based on Roentgen's discovery." He has recently become the only member of the Nuclear Division to be named Senior Research Fellow of Union Carbide Corporation.

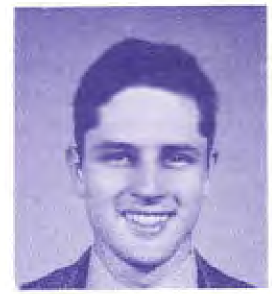
1963

One of ORNL's most important contributions to reactor technology was the successful demonstration of the sol-gel process, which is used for fabricating new fuel elements from material recovered from spent fuel or from virgin ore. The sol-gel process, largely the invention of O. C. Dean and Don Ferguson, converts thorium and uranium nitrate from the solvent extraction plant into dense thoria–urania particles, which are fashioned into fuel elements by vibratory compaction (developed by A. L. Lotts, D. A. Douglas, and their associates). Using this sol-gel process and its kilorod facility, headed by R. E. Brooksbank, ORNL produced nearly 700 Zircaloy-clad fuel rods containing thoria-$^{233}UO_2$. ORNL also did research on making graphite-clad fuel elements for a German gas-cooled pebble bed reactor called the AVR.

In nuclear safety research under W. E. Browning, charcoal-bed adsorbers were developed for removing 99% of radioiodine-131, a common fission product from gas. The Nuclear Safety Pilot Plant was being completed so that ORNL researchers could continue their studies, begun in 1955, of simulated loss-of-coolant accidents and experimental melting of irradiated fuel elements for measurements of release of fission products. In biological studies related to nuclear safety, the Russells' studies of the effect of low dose rates of radiation on germ cells of mice suggested that fallout from bombs and maximum credible accidents (should they occur) may be less of a genetic hazard than once feared.

In another biological achievement, Norman Anderson and his colleagues, using a zonal centrifuge, extracted virus-like particles from human leukemic plasma. Anderson had been adapting the high-speed centrifuges, developed during the war for uranium separation, to large-scale separation of cellular constituents. In other basic science advances, G. M. Watson, R. B. Evans, and J. Truitt developed a model (based on studies of gas diffusion through a porous medium) to predict release of fission gases in vented graphite fuel elements for gas-cooled reactors; and a study of "channeling" by Mark Robinson and O. S. Oen showed that energetic ions aimed down channels in crystal lattices undergo less energy loss than those that enter in random directions—a phenomenon that could be used both for a study of the stopping process and for obtaining new information about the crystals themselves.

The Oak Ridge Isochronous Cyclotron continued to operate successfully; its beams of protons, deuterons, helium-3, and helium-4 were being used to characterize nuclear energy levels and to quantify nuclear models. This work was carried out in close collaboration with theorists led by Ray Satchler. An oxygen isotope separation plant was completed for yielding water enriched in the rare isotope, oxygen-17 (used by chemists studying reaction



R. S. CARLSMITH

M. FELDMAN

mechanisms of oxygen-containing compounds). In another isotope development, as part of the Systems for Nuclear Auxiliary Power Program, ORNL-produced strontium-90 was used at the North and South Poles as a heat source to generate electricity for broadcasting meteorological data from unmanned stations. ORNL also supplied isotopes for auxiliary space power plants; other space work included studying the physical and biological aspects of the radiation hazard in space, developing high-temperature materials for space rockets, and designing a small boiling-potassium-cooled fast reactor.

In work-for-others programs, the water research project under Kurt Kraus began work on hyperfiltration, a technique of desalting water by passing it through dynamically porous membranes; the AEC and National Institutes of Health started a cocarcinogenesis program at ORNL to study synergistic effects of radiation and chemicals in causing cancer; and the seeds of an ORNL civil defense research program were sowed, as Eugene Wigner headed up Project Harbor at Woods Hole, Massachusetts, to study civil defense technology questions.

The Graphite Reactor was shut down on its 20th birthday. (ORNL's first reactor was designated a Registered National Historic Landmark in 1966.)

When the homogeneous reactor experiment began producing 150 kW of electricity, enough to light the reactor building and feed a substantial amount back into the Laboratory's power system, Sam Beall showed John Swartout, Charlie Winters, and Alvin Weinberg (l. to r.) how to put the building's electrical system on reactor power. (1953)

Richard Stephenson, left, and Charles Cagle check out a model of the Graphite Reactor to be used as a special exhibit in 1951. Cagle holds the thermal column built into the top of the reactor.

1964

ORNL's mission-oriented programs were focusing on what Weinberg considered the five central problems facing American society: need for cheap and abundant energy; need for cheap and abundant water; the chemical and physical assaults on the biological environment, including cancer-causing agents; need for civil defense; and need for education in deprived areas. Since the AEC in its 1963 report to the President had identified development of breeder reactors as a significant national goal, ORNL continued to commit much of its reactor efforts into development of the molten-salt reactor, its main entry in the "breeder sweepstakes," as Weinberg once put it. In the area of fission product recovery from fuel salts, M. J. Kelly demonstrated that rare-earth fluorides, which are less volatile than fluorides of fuel salts, can be separated from the latter in a fluoride volatility plant when the fuel salt is vacuum distilled. Another advance made to ensure reliable operation of the circulating-fluoride reactor was the finding that formation of elemental fluorine can be averted if irradiated molten salts are prevented from solidifying by keeping the temperature above 150°C. ORNL reactor researchers were especially encouraged by the realization that economic nuclear energy had arrived, an event recognized at the Third Geneva Conference on the Peaceful Uses of Atomic Energy and signaled by the contract between General Electric Company and a New Jersey utility to build a 515-MW(e) boiling-water reactor at Oyster Creek, New Jersey. R. P. Hammond, J. W. Ullmann, and others had argued that the larger a nuclear power reactor complex, the more economic a source of energy it is for producing electricity and heat, which could be used for desalting water and supplying thermal energy to other agricultural and industrial processes. Hammond's ideas received international attention, as the U.S., the Soviet Union, Israel, and Mexico began planning for nuclear desalting plants. The U.S. Office of Saline Water and the AEC supported a growing program at ORNL on developing large reactors and evaporators and on basic water research.

ORNL biologists were now studying the carcinogenic, genetic, and pathological effects of chemical agents (gasoline fumes, pesticides, tobacco, drugs) as well as of radiation. The interdisciplinary reputation of the Biology Division was enhanced as chemical technologists led by A. D. Kelmers joined biochemists led by David Novelli in developing techniques for extracting from bacteria the large quantities of transfer-RNAs needed to study how amino acids are assembled into proteins. Physical scientists were also getting into the biology game, as H. A. Levy and G. M. Brown used neutron diffraction studies to learn the precise structure of organic molecules such as sucrose, and physicists conducted various studies of possible correlations between the biological and carcinogenic activities of chemicals and their electronic properties.

Eugene Wigner returned to ORNL in September to lead ORNL's study of civil defense technology (designing fallout and blast shelters, for example). A new departure for ORNL was the hiring of three social scientists to aid in the study. Wigner and his colleagues concluded that civil defense was technically feasible and offered an incentive for international disarmament to reduce likelihood of a thermonuclear war.

1965

The Molten-Salt Reactor Experiment went critical June 1, and the High Flux Isotope Reactor, which was designed to produce elements heavier than plutonium, achieved criticality August 25. Success with the MSRE was considered crucial if ORNL was to convince the Government of the feasibility of molten-salt breeders, which would have a graphite core structure, a uranium-bearing fuel salt, and a thorium-bearing blanket salt. These reactors would have a low breeding ratio, but Weinberg argued that their advantage lay in their low fissile inventory, meaning that they would require less raw uranium for fuel. The HFIR, which has the world's highest thermal-neutron flux, produced californium-252 and other isotopes by bombarding with neutrons such target materials as americium, curium, and plutonium. Still under construction was the Transuranium Processing (TRU) Facility, which would use processes (developed by Rex Leuze and others) combining solvent extraction and ion exchange to separate HFIR-produced californium from other transuranium elements such as americium, curium, einsteinium, and fermium. W. D. Burch was in charge of the design of the heavily shielded TRU facility.

As for radioactive waste research, ORNL deposited its first batch of radioactive fuel elements in the Carey Salt Mine in Lyons, Kansas (under pressure from the state of Kansas, the AEC asked ORNL to stop later waste deposits in 1972, after it was discovered that oil wells and dry holes were near the salt mine).

The University of Tennessee-Oak Ridge Graduate School of Biomedical Sciences was established. UT and ORNL already had a fruitful relationship in which ORNL scientists taught at UT and UT scientists did research at ORNL. In 1963, UT received a Ford Foundation grant to study ways of setting up a graduate science education program involving UT and ORNL; the Graduate School of Biomedical Sciences, which accepted its first class of seven students in 1967, was one outcome of this study and marked an advance in ORNL's mission to improve scientific education in the Southeast. Completion of work for 142 Ph.D. theses at ORNL from 1943 to 1965 illustrates the Laboratory's contribution to education.

In civil defense studies under J. C. Bresee, ORNL designed a tunnel-grid system of blast shelters, including urban dual-use shelters that could also be used in peacetime for underground streets and parking lots. In thermonuclear fusion, researchers who had achieved relatively stable plasmas in previous years grappled with the problems of internal instabilities associated with the appearance of electric fields within the plasmas of the DCX-1. Herman Postma, Julian Dunlap, and R. A. Dory ascribed the losses in DCX-1 to negative-mass instability. ORNL, however, was credited with leadership in such areas as electron cyclotron heating and electron beam heating of hydrogen and deuterium plasmas.

In a year in which Gemini-7 orbited the earth, ORNL continued its space-related research for the AEC and NASA. This work, coordinated by A. J. Miller, included the development of auxiliary power sources for the telemetering of information from outer space via television pictures and the development of high-temperature materials for rockets, which must be able to withstand the searing heat of high-speed reentry into the earth's atmosphere. ORNL produced curium-244 sources for a small power plant in the 5-kW range and worked on the design of an isotopic heat source using plutonium or curium for a 5-kW(e) power plant for space vehicles. Work was also under way by A. P. Fraas and associates on a Medium-Power Reactor Experiment that would use liquid potassium as a coolant and potassium vapor to drive a turbine. ORNL developed refractory metals, such as niobium alloys, to withstand high temperatures. ORNL researchers led by J. R. Weir also discovered a possible mechanism to explain why metals subjected to high temperatures and low doses of slow neutrons become brittle. An electron microscope study showed that helium, produced by reactions of neutrons and residual boron in nickel- or iron-based alloys, migrates to grain boundaries and loosens the grain.

Special Tribute

Clarence Edward Larson received his doctorate in chemistry from the University of California in Berkeley. From Section Chief for Analytical Chemistry at the Radiation Laboratory in Berkeley he rose to head of the technical staff at the Y-12 Electromagnatic Plant, to Director of Oak Ridge National Laboratory, to Vice-President and President of the Union Carbide Nuclear Divison, which operates the Oak Ridge facilities for ERDA (then the AEC), to a Commissioner of the Atomic Energy Commission. His election to the National Academy of Engineering in 1973 was for "the development of processes for recovery and purification of uranium and leadership in nuclear plant design."

Officers of the ORNL Girls Club elected in 1952 were, front row, left to right, Margaret Albritton, publicity chairman; Luella O'Neill, sports chairman; Pat Sarvella, vice-president; Grace McCammon, historian. Back row: Lovelle Thompson, secretary; Bryant Humphreys, social chairman; LaWanda Estes, president; Wilma Stair, membership chairman; Bonnie Farmer, treasurer.

1966

In terms of solid achievements, this was one of ORNL's best years. The High Flux Isotope Reactor began power operation at 20 MW on January 29 and reached full design power of 100 MW on September 9 (its only problem was poisoning of the reactor fuel by the fission product samarium-149). The Molten-Salt Reactor Experiment operated well at power levels of 7.5 MW despite problems with the off-gas system and penetration of the graphite with a fission product, molybdenum. The Transuranium Processing Facility produced 130 micrograms of californium-252 and 30 micrograms of berkelium from plutonium-242 irradiated at the Savannah River reactor in South Carolina. Norman Anderson's zonal centrifuges produced highly purified flu vaccines that can be administered with safety to high-risk individuals. In a collaborative effort of three divisions in basic research, Sheldon Datz, H. O. Lutz, C. D. Moak, and T. S. Noggle made further elegant studies of the "channeling" of ions in very thin gold crystals. Researchers collaborating in five divisions assembled a 25-kilocurie curium-242 thermoelectric generator that produced a kilowatt of heat in a chamber simulating lunar conditions. In the nuclear desalination effort, ORNL developed and experimented with improved heat transfer surfaces for evaporator tubes (assembly of a vertical-tube pilot plant was under way). ORNL researchers, led by G. G. Kelley and later by O. B. Morgan, began developing the technology of neutral beams of hydrogen atoms for heating plasmas—a technology that was to produce significant results ten years later, when ion temperatures up to 15 million degrees were achieved in the ORMAK, a fusion research machine modeled after the tokamak, the Russian toroidal confinement device. The Thermonuclear Division also made plans for building DCX-3, using a "magnetic well" arrangement with superconducting magnetic coils.

In promoting the Laboratory's voluntary chest x-ray program in 1952, Health Division nurse Alice Spicer greets Mary Evans, Mary LaMaster, Ellen Klotz, Norma Miller, Nell Tuck, Yvonne Lovely, Zella Bonner, and Lou Hubbard.

Alexander Hollaender retired as director of the Biology Division, a post he had held for 20 years. Under his leadership, the Biology Division became ORNL's largest division and developed its own style of investigation, characterized by an emphasis on basic research, an interdisciplinary approach, and massive experiments, such as those involving thousands of mice. The research under way in the Division in 1966 included the molecular-level approach to understanding radiation repair systems, and aging studies under Takashi Makinodan, showing that the immune mechanism in mice becomes less efficient with age and that transplanted spleen cells from young mice rejuvenated the immune systems of old mice and lengthened their life span. William A. Arnold made the discovery of "delayed light" (a delayed chlorophyll fluorescence), which has been shown to be closely connected to the process of photosynthesis and is one of only a few ways of studying initial photosynthesis steps. Arnold and his associates have made internationally recognized fundamental discoveries in photosynthesis over the years, including classical evidence for the purely electronic nature of the first step in photosynthesis.

Development work was completed on the Low-Level Waste Process used for removing low-level radioactivity from waste water.

Members of the Oak Ridge Symphony Orchestra celebrate the opening of the tenth concert season (1953). Left to right: Herbert Pomerance, June Adamson, Ann Savolainen, Donald Ward, Irving Spiewak, John Chilton, Mildred McDuffee, Bud Perry, Barbara Marable, Conductor Waldo Cohn, James Marable, Ray Blanco, Paul Stelson, Henry Rosenstock, and Herman Krieger.

1967

On October 7, the Molten-Salt Reactor Experiment achieved 6000 equivalent full-power hours (required amount of operation in order for the AEC to consider the system for full-scale development into a molten-salt breeder). In his "State of the Laboratory" address, Weinberg said that this successful operation bolstered hopes that an MSBR could operate economically, despite such problems as graphite deformation during intense neutron bombardment at elevated temperatures. Another materials problem under study led to the observation at ORNL (and also in England) that stainless steel deteriorates under exposure to fast neutrons and loses its ductility when also subjected to high temperatures. J. O. Stiegler, Everett Bloom, and others found that stainless steel irradiated in the EBR-II in Idaho develops innumerable small voids and that its volume increases by 10 to 15%. The findings cast doubt on whether there existed reactor materials that could withstand the elevated temperatures and high neutron fluxes of liquid-metal fast breeder reactors (the AEC's preferred breeder concept).

In nuclear safety, an ORNL task force headed by W. K. Ergen concluded that the engineered safeguards, such as emergency core cooling systems, must be reliable, since only they, not the containment vessel, can prevent the calamity of fission products melting through the containment, as would result from loss of primary coolant, however unlikely. ORNL expanded its nuclear safety work to include such activities as developing methods to detect flaws in reactor vessels, determining ways to make reactors earthquake-proof, and characterizing heavy steel sections used in fabricating reactor pressure vessels (in 1974, researchers found that deliberately flawed test vessels burst only after being subjected to water pressures at least 2.2 times the design pressure).

In nuclear desalting research, ORNL's pilot vertical-tube evaporator at Wrightsville Beach, North Carolina, demonstrated that heat transfer surfaces developed at ORNL and elsewhere transfer heat up to four times as efficiently as ordinary surfaces. This technology was later transferred to industry. ORNL pushed such concepts as food factories (farms irrigated with desalted water) and nuclear-powered agro-industrial complexes (Nuplexes) for producing low-cost water and fertilizers. Senator Howard Baker (Tennessee) sponsored a resolution, passed by the Senate, calling for exploration of the possibility of building such complexes in the Middle East.

In thermonuclear fusion, ORNL pursued the design of the complex superconducting magnet system for the Laboratory's Injection into a Microwave Plasma (IMP) Experiment (a renamed modification of DCX-3). An entry into the realm of toroidal plasma confinement devices was made with the "levitated multipole"—a magnetic configuration in which two current-carrying hoops were suspended for a fraction of a second within a vacuum space in an overall magnetic field. This elegant experiment was performed by Igor Alexeff, Michael Roberts, and William Halchin.

The first experiments with transuranics were carried out in the new Transuranium Research Laboratory directed by O. L. Keller. (Outstanding transuranic research later included confirmation of elements 104 and 105 in 1973–74.) In the year marking the 25th anniversary of the first sustained nuclear chain reaction, ORNL continued its studies of the neutron, participating in work resulting in precise measurements setting an extremely small upper limit to the strength of its possible electric dipole moment. Biology research achievements included the discovery of "minicells" (highly specialized fragments of bacteria) by Howard Adler and the accumulation of evidence by Stanfield Rogers for the incorporation of genetic information from a passenger virus into the human genome (set of chromosomes and genes), leading to speculation that it might be feasible to cure metabolic diseases by supplying missing genetic information. The ultrapure flu vaccine, Zonomune, was commercially produced by use of ORNL-developed zonal liquid centrifuges.

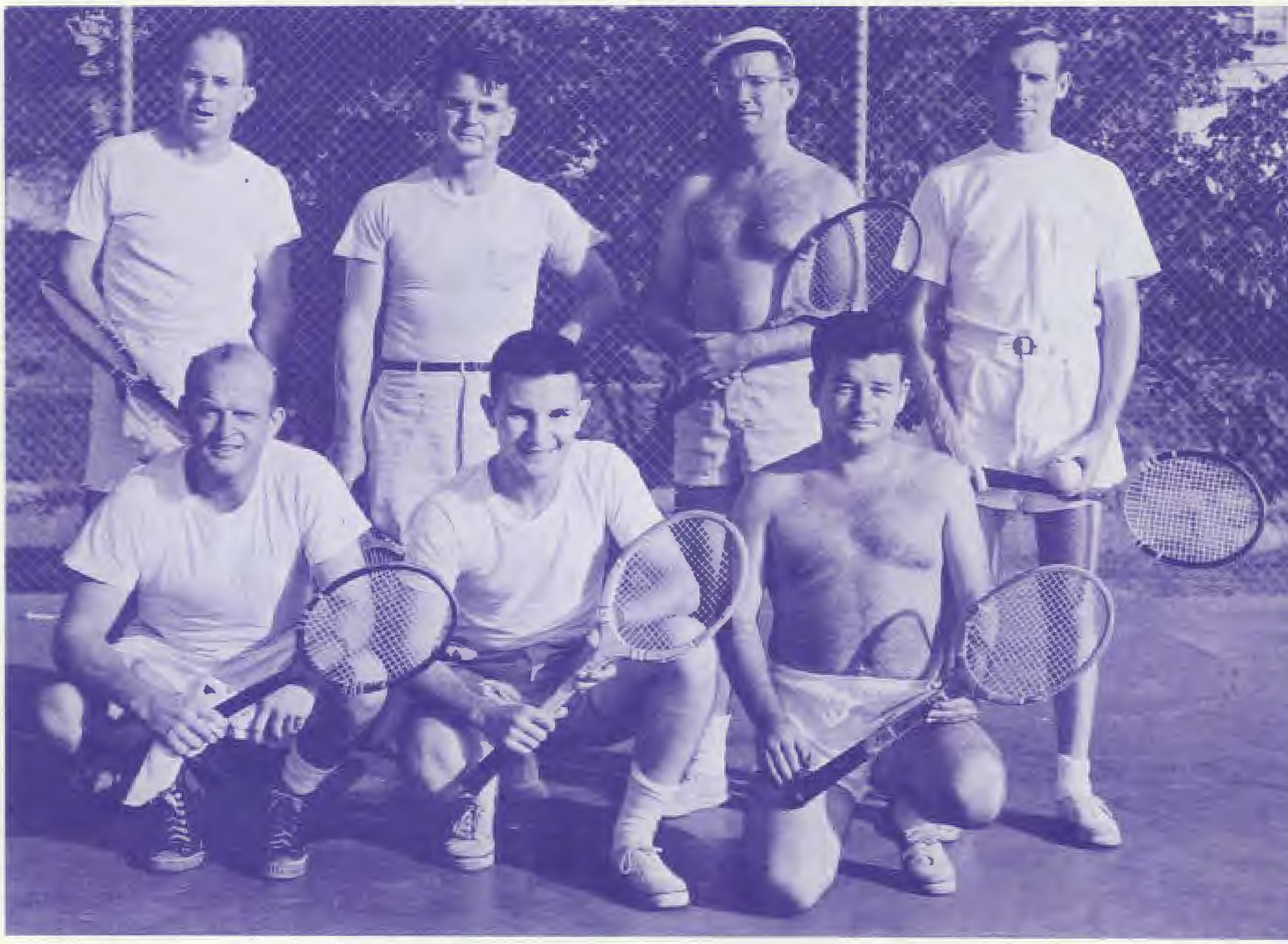
Work began under T. A. Welton to develop an electron microscope with 1-Å resolution for "seeing" individual atoms.

Dosimetry at the Pile . . .

C. E. Clifford, a Du Pont engineer who, after assisting in the reactor start-up in 1943, was working in mapping the neutron flux in the X-10 Pile for Henry Newson and Louis Slotin, tells the story about how a guard in the counter room of the pile building demonstrated more health physics know-how (or possibly just plain good sense) than did the dedicated scientific team. In the technique used by the team, the neutron fluxes throughout the pile were to be deduced from radioactivity given off by copper foils activated through exposure to the reactor neutron flux and later counted in the counter room. For the first measurements the copper foils were attached at regular intervals along a thin aluminum strip 30 feet long and about 3 inches wide, and the strips were inserted into the reactor via a hole in one of the shield faces. After a 10- or 20-minute run, the team withdrew the stips and with their bare hands rolled them up sufficiently to throw them over their shoulders. They then proceeded to the heavily shielded counter room which could be entered only through a labyrinth. As the team approached the end of the labyrinth, the sensitive ears of a guard stationed just inside the room detected a sudden change in the sound of all five of the nearby heavily shielded Geiger counters. The counts increased to pandemonium when the men stepped inside the room. For a brief moment the team stood mildly shocked and with growing concern at the increasingly mad tempo of the counters. They finally reacted by throwing down the highly active aluminum and turned to warn the guard, but the guard had already promptly disappeared back into the labyrinth. They, too, quickly departed from the counter room, but they never caught up with the guard, who was not seen by the team again that day.—Lorraine S. Abbott

1968

An interdisciplinary, interdivisional project called the MAN (Molecular Anatomy) Program was established by ORNL. The program—in which biologists, physicists, chemists, and engineers worked together under the leadership of Norman Anderson—was based on the assumption that subtle metabolic deficiencies underlie many human diseases and that detection of small metabolic anomalies through separation of cellular constituents will aid in understanding and controlling these deficiencies. The engineered products of the MAN Program included *zonal centrifuges* adapted for commercial use in preparing purified flu vaccines, experimental batches of clean rabies vaccine, and purified viruses that are specifically pathogenic for the tussock moth, which infests fir tree forests; *body fluid analyzers,* high-pressure ion exchange columns used to separate chemical constituents of blood and urine; and the *GeMSAEC fast analyzer,* one of ORNL's most famous inventions. The GeMSAEC analyzer is a fast, automatic, centrifugal machine connected to a spectrophotometer and computer. It



The ORNL Y tennis team, City League champions. Standing, from left; Russell Baldock, Leland Mann, Fred Hurst, Roy Towns; front row: Phil Baker, Bob Zeitlin, Frank Blacksher.

is designed to analyze the constituents of fluids (such as blood) simultaneously, quickly, and cheaply. This analyzer has revolutionized medical testing of body fluids in hospitals and clinics.

In nuclear desalting, researchers studied the applicability of the nuclear-powered agro-industrial complex (Nuplex) to the Middle East. They concluded in a 1969 report that the concept showed the most promise near the Gaza Strip. The desalting program began to decline and almost totally evaporated in the 1970s, as the nation turned to the more critical problems created by the energy crisis.

O. L. Miller and Barbara R. Beatty clearly visualized functioning genes (from nucleoli of amphibian oocytes) in the electron microscope. They published the first photographs of genes coding for ribosomal RNA precursor molecules (for synthesizing RNA).

ORNL played a part in the discovery at Oak Ridge Associated Universities (formerly ORINS) that gallium-67, previously studied since 1952 for its localization in bone, shows surprisingly good localization in soft tissue tumors. The gallium-67 used in the ORAU studies was produced in ORNL's 86-inch cyclotron from enriched zinc-66.

ORNL ecologists were asked to participate in the International Biological Program and coordinate related projects at universities and government stations concerned with the interactions of air, water, land, animals, and plants in the Eastern Deciduous Forest Biome. (Ecologists became involved in another big project beginning in 1971, when a federal court decision required the preparation of environmental impact statements for all U.S. nuclear power plants.)

The High Flux Isotope Reactor was being used for neutron diffraction research such as studies of inelastic scattering of neutrons from crystals and of magnetic interactions of neutrons with paramagnetic materials. G. G. Kelley discovered in DCX-2 a generalized form of the negative-mass instability, a stumbling block in achieving control of the plasma (J. F. Clarke supplied the numerical evaluation).

The Molten-Salt Reactor Experiment, now fueled with uranium-233, was brought to power by AEC Chairman Glenn Seaborg on October 8, with R. W. Stoughton, codiscoverer of uranium-233, looking on. The MSRE was the world's first reactor to operate on uranium-233 fuel. A conceptual design of a one-fluid, 1000-MW(e) molten-salt breeder reactor experiment was completed, but there was some question whether the project would be funded because of the pressures of financing the war in Vietnam. (The Molten-Salt Reactor Program was halted, but was later reactivated in 1973. In 1976, it was finally terminated, ending a project that began as an aircraft propulsion concept in the early 1950s.)

The Oak Ridge Electron Linear Accelerator was completed. This high-current, short-pulsed neutron source (neutrons are produced by bombarding tantalum targets with accelerated electron beams) is used for cross-section measurements—particularly in connection with the Liquid-Metal Fast Breeder Reactor (LMFBR) Program.

The LMFBR Program, coordinated by W. O. Harms, was established at ORNL in 1968 to support the AEC's top-priority reactor project—development of a liquid-metal fast breeder reactor for production of both electricity and nuclear fuel. Thirty percent of ORNL's research was LMFBR-related, with much of it focused on chemical reprocessing (including the development of aqueous processing for the recyle of stainless-steel-clad uranium–plutonium oxide fuels). Probably the outstanding accomplishment in 1968 in this program was the discovery by metallurgists J. R. Weir and W. R. Martin that adding small amounts of dispersed titanium to stainless steel will significantly reduce fast-neutron embrittlement. (In 1975, it was discovered by Everett Bloom, J. O. Stiegler, and others that adding silicon as well as titanium to type 316 stainless steel made the alloy even more resistant to neutron-induced swelling, thus allowing a tighter LMFBR core design and less frequent fuel element loadings, potentially saving a future commercial breeder industry billions of dollars.)

And so, after 25 years, Oak Ridge National Laboratory, which used its first reactor to demonstrate the safe production of plutonium for development of the atomic bomb, was devoting its major reactor-related effort to developing safe, power-generating plutonium breeders.

D. E. REICHLE

L. P. RIORDAN

The Bomb in Edmonton

One night at 2 a.m. in December, 1951, I was aroused from bed by a phone call that I shall not forget. On the other end of the line was the excited voice of an AEC official. "We've had an international incident," he said. "You didn't properly safeguard highly enriched fuel and it's now impounded in Canada." It was a frightening moment for me as I wondered how fuel elements fabricated at ORNL and sent by air to Idaho could end up in Canada. Gradually the story—which hitherto has not been made public—unfolded. We in ORNL's Metallurgy Division had been asked by Phillips Petroleum Company to ship the remaining fuel elements for the Materials Testing Reactor (at the AEC's National Reactor Testing Station near Arco, Idaho) as fast as possible. That ruled out the planned mode of transportation for the nuclear fuel, since shipping it from Oak Ridge to Idaho by train would take a week and a half. So we loaded four tightly sealed shipping containers—each holding six MTR fuel elements—onto a commercial airplane in Knoxville. The attached shipping instructions described the contents as highly enriched uranium, with each fuel element containing 140 g of U-235. The fuel elements were flown to Chicago, then Salt Lake City. They were then loaded on a flight destined for Idaho Falls, but the pilot decided not to attempt a landing because of heavy snow. He proceeded to Butte, Montana, which was also snowbound. Finally, he found a safe place to land—Edmonton, Canada. You can imagine what a stir was created when Canadian customs officials found among the plane's cargo four boxes of bomb-grade material. Shortly thereafter, the equivalent of Canada's state department called the U.S. State Department to protest. Distressed, State Department officials sought an explanation from the AEC. Then I was asked to account for the incident. After several phone calls, everything was straightened out, and several days later the impounded fuel was shipped to Idaho Falls and later loaded into the MTR, which went critical in the spring of 1952.

The development of the MTR fuel elements is a story in itself. Originally, we had made the elements out of 18 flat plates—each plate consisting of enriched uranium sandwiched between aluminum cladding—held together by two side plates of aluminum. Between the plates water would flow and carry away the heat. A problem encountered in pre-operational testing was that differential stresses on the plate caused bowing of the plates in either direction, raising the possibility that the coolant flow could be restricted and that undesired hot spots could develop. Circa 1947 Research Director Eugene Wigner quietly listened as researchers described the problem to him. Then he went away, apparently to contemplate the matter. Later he returned and said, "I propose that you change the design of the fuel element by curving the plates." We tried it and liked it: the curved plates could bow in only one direction, thus averting the possibility of restricted coolant passages. It was one of those good ideas for which a research director could truly take credit.—Jack Cunningham, Associate Director, Metals and Ceramics Division.

Index of Personal Reminiscences

The "first annual senior research staff seminar," the original "State of the Laboratory" address, was held at the Oak Terrace in December 1951. Both Laboratory Director Larson and Research Director Weinberg spoke of the accomplishments of the Laboratory over the previous year.

74	66	28	26	3	85						
65	54	66	29	26	54						
36	38	88	6	36	54	48	85	100	111	10	122
4	53	58	35	6	4	33	70	94	39	111	23
36	69	59	67	14	15	74	97	82	12	4	32
9	9	16	89	17	23	59	69	60	91	21	122
14	98	58	13	12	67	65	33	92	70	22	89