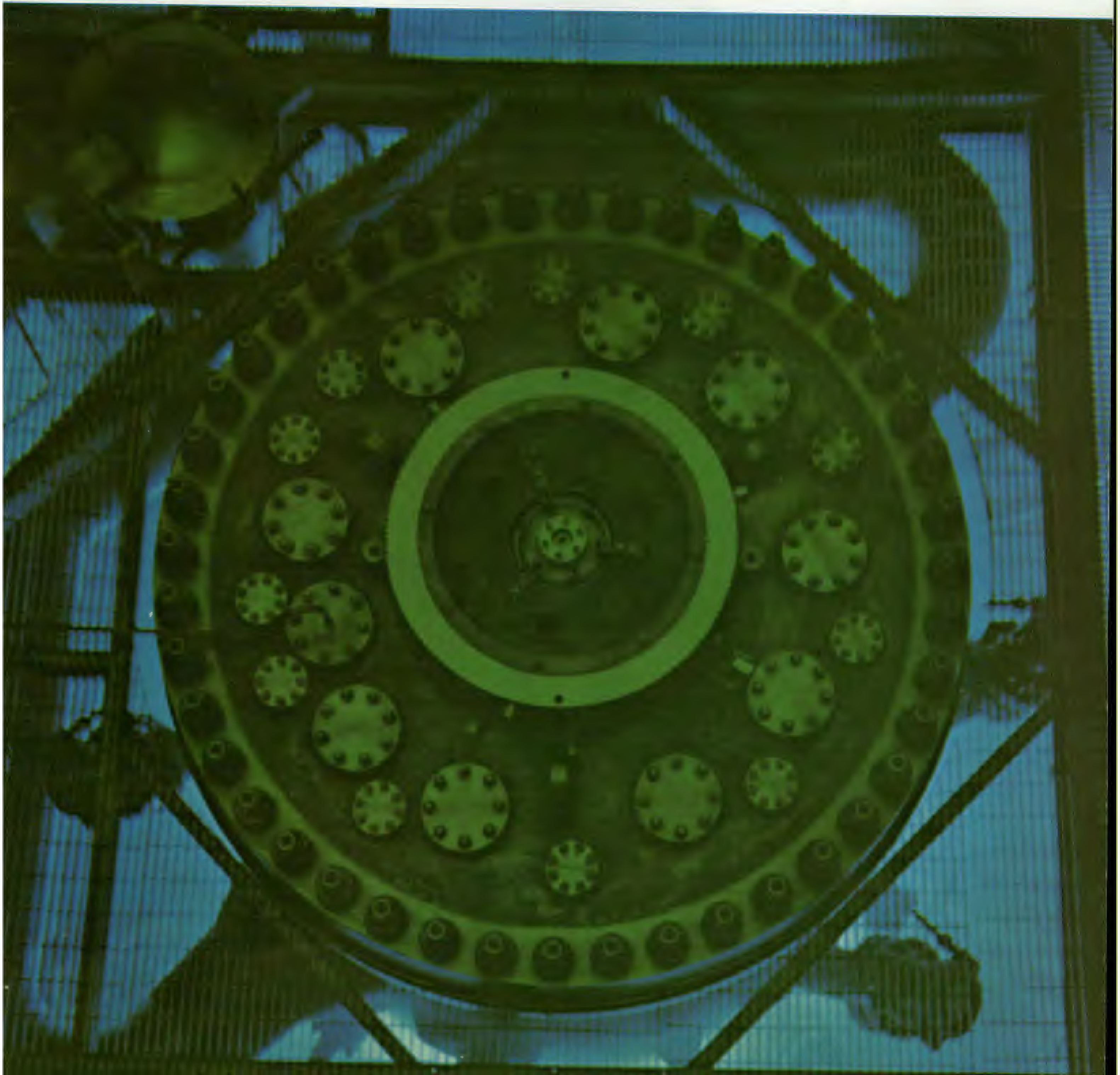
REVIEW

OAK RIDGE NATIONAL LABORATORY · SUMMER 74



REVIEW

VOLUME 7 NUMBER 2 SUMMER 1974

THE COVER: One of the most visited areas at the Laboratory is the HFIR-TRU complex in the 7000 area. It plays an important role in the nation's transuranium program, providing as it does the bulk of the manmade elements heavier than plutonium for use in research laboratories here and abroad.

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FEATURES

OAK RIDGE NATIONAL LABORATORY
OPERATED BY UNION CARBIDE CORPORATION • FOR THE U.S. ATOMIC ENERGY COMMISSION

Rube McCord has been with the Manhattan Project since, as they say, the Year One. He was at Argonne with Du Pont before moving to Tennessee to spend a year at Clinton Laboratories in 1943, where he took a hand in loading the Graphite Reactor for its initial startup. He then moved to Hanford Engineering Works in Richland, Washington, and in 1947 joined the Physics Division back in Oak Ridge. There he stayed until 1951, when he transferred to the Operations Division, where he is today. (From 1955 to 1958 he served in the Director's Division as one of the four Laboratory Shift Supervisors.) He has been the High Flux Isotope Reactor supervisor now since 1961, and in this article he puts together a picture of this noble work of ORNL as it is today.

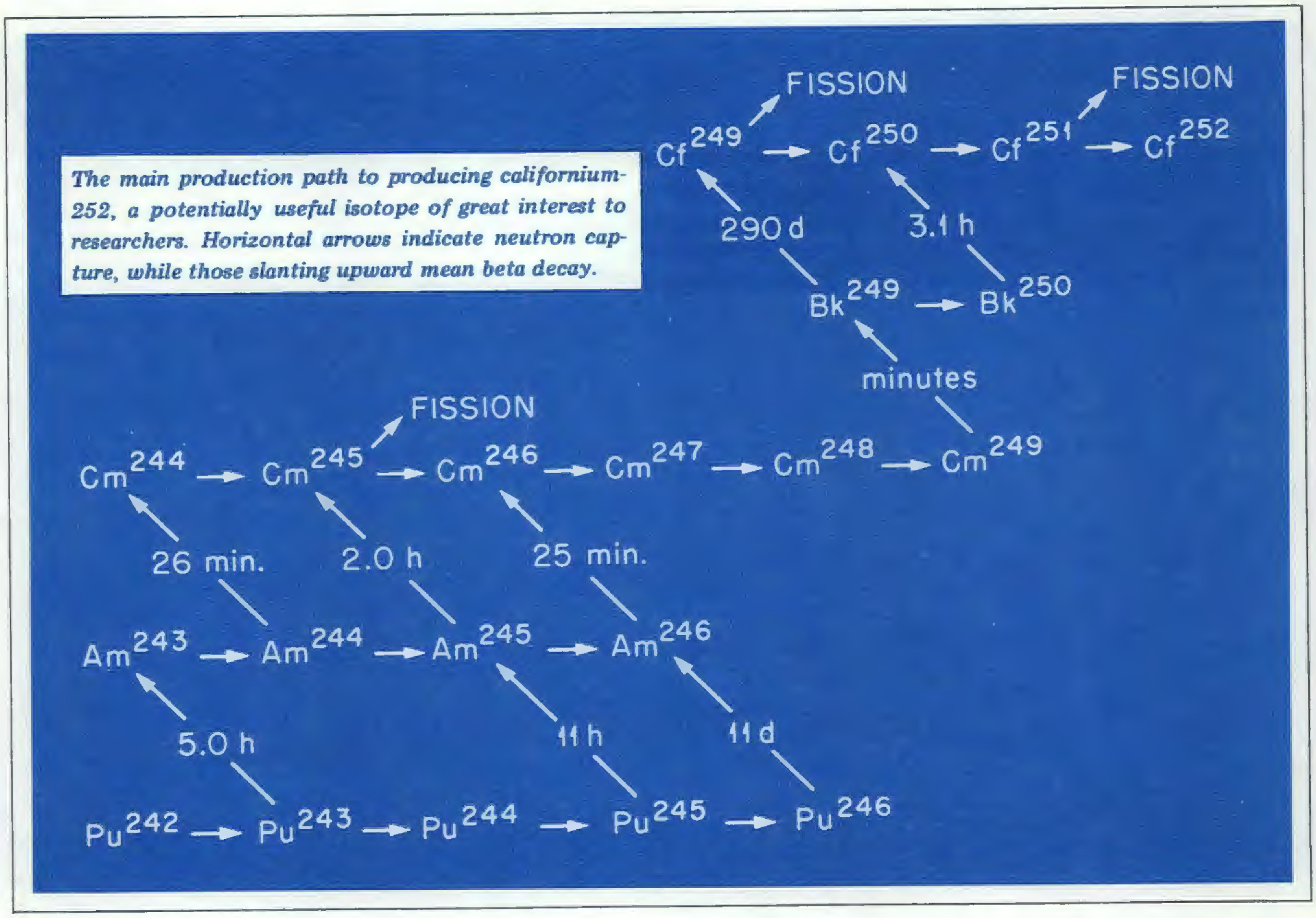
The High Flux Isotope Reactor at ORNL

R. V. McCORD

Since it attained its design power of 100 megawatts in September 1966, a little over five years from the beginning of its construction, the High Flux Isotope Reactor, or HFIR, has achieved a record of operation time unsurpassed by any other reactor in the U.S. The HFIR completed its one-hundredth fuel cycle of operation last December. A fuel cycle, the lifetime of a fuel assembly, is about 23 days. The HFIR is a beryllium-reflected, light-water-cooled and -moderated, flux-trap reactor with a design power level of 100 megawatts. It burns 93% enriched uranium-235 fuel in aluminum-clad plates. The main purpose of the reactor is to make research quantities of transplutonium elements; so it is primarily a production reactor, but it is also used directly for research.

Reactor Core

The reactor core is a series of concentric annular regions, each about 2 feet high. A 5-inch-



The main production path to producing californium-252, a potentially useful isotope of great interest to researchers. Horizontal arrows indicate neutron capture, while those slanting upward mean beta decay.

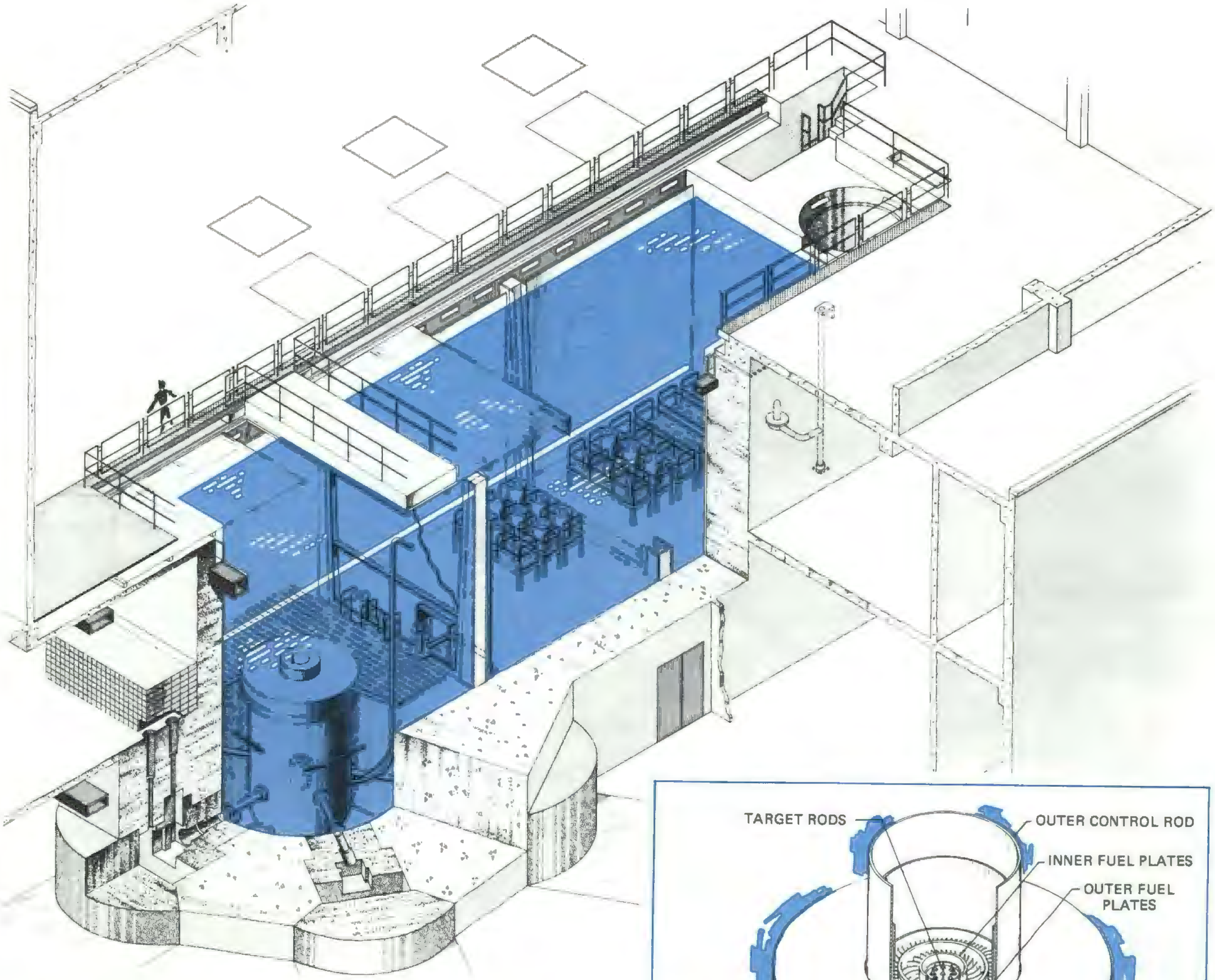
diameter hole in the center of the core is where the neutron flux is concentrated. Into this region the thermal neutrons stream at a rate higher than any other research reactor in the world. The target material, a bundle of rods containing transplutonium isotopes, is slipped down inside this hole. The surrounding fuel is in two concentric cylinders in the form of curved fuel plates. The inner one, with the inside diameter of 5 inches, has an outer diameter of 10½ inches; the outer element has inner and outer diameters of 11 and 17⅛ inches.

The fuel plates, 0.05 inch thick, are curved so as to be in exact parallel, thus providing a constant coolant-channel width. They are of complex sandwich-type construction, with the fissile material held between thin plates of aluminum. A burnable poison (boron, a neutron absorber) is included in the inner cylinder to further flatten the neutron flux and increase the shutdown margin early in the fuel cycle. A core loading holds about 9.4 kilograms of enriched uranium and 2.8 grams of boron.

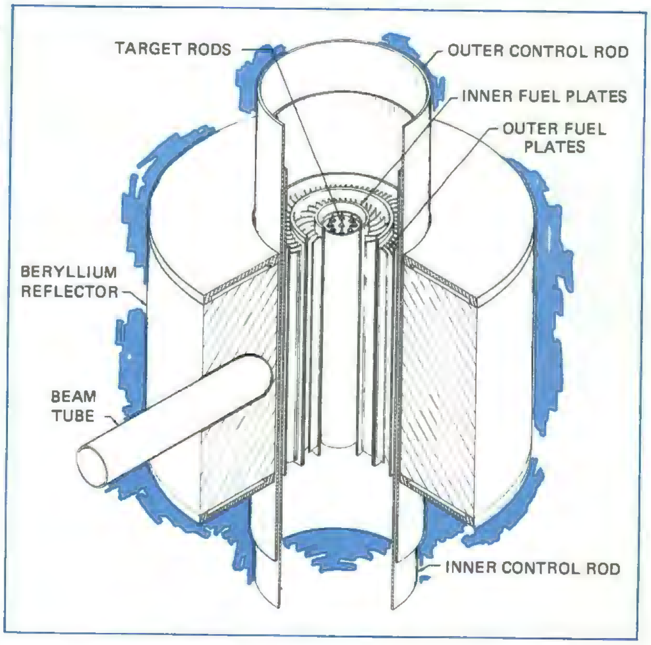
The fuel region is surrounded by a 1-foot-thick ring of beryllium reflector. The water that the whole assembly is immersed in serves as a second reflector, of effectively infinite thickness.

Although the primary purpose of the HFIR is the production of transplutonium isotopes, as many as 46 access holes are drilled in the beryllium reflector. Thirty-eight of these are vertical experiment facilities, with diameters from half an inch to nearly 3 inches. There are four 4-inch beam tubes and four slanted grooves in the outer surface of the beryllium to accommodate the engineering facility tubes. A hydraulic rabbit tube is located in the center of the flux-trap region.

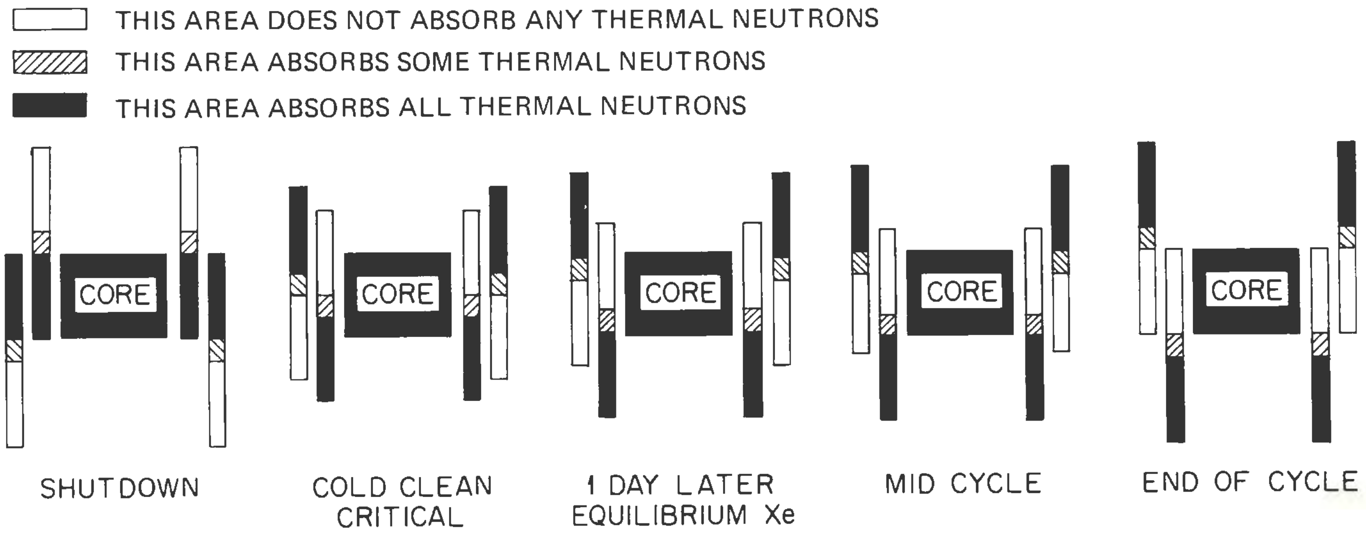
The control rods are two thin poison-bearing concentric cylinders that move vertically between the outer fuel element and the beryllium reflector, operated by drive mechanisms located under the reactor. The inner control cylinder has its poison arranged so that reactivity is increased by downward motion. This cylinder is used for shimming and regulation; it has no fast safety function. The



Even the view from the bay area of the HFIR gives only an incomplete picture of its complexity. Obscured from the visitor are such details as the access facilities for neutron experiments, the pumps, the cooling methods, and the relatively tiny fuel core, the energy source for the whole operation.



A schematic of the HFIR core.



Control plate positions during a cycle

outer control cylinder has four separate sections, each with its own drive and safety-release mechanism. Reactivity is increased as the outer plates are raised.

The whole reactor assembly is contained inside an 8-foot-diameter pressure vessel, which is located in an 18-foot-diameter cylinder filled with water. The top of the pressure vessel is 17 feet below the surface, with the reactor centerline 10½ feet farther below. Adjacent to, and connected with, the reactor pool is a storage pool, and together they form a tank about the size of a very deep swimming pool.

Reactor Control and Instrumentation

The instrumentation system in the control room reflects the emphasis that is placed on the importance of the reactor's continuity of operation. Three independent safety channels, each one a shutdown device activated by any of seven parameters, are arranged in a coincidence system that requires agreement of two of the three for safety shutdowns. This feature is complemented by an extensive "on-line" testing system which permits the safety functions of any one channel to be tested at any time during operation. In addition to the independent safety systems, three independent automatic control channels assure that failure of any one of the channels will not significantly disturb operation. As a result, the HFIR's number of unscheduled shutdowns during routine operation has been impressively low.

Reactor Coolant

The coolant, demineralized light water, flows through the reactor primary system at about 17,000 gallons a minute, impelled by three vertical centrifugal pumps. The nominal system pressure of 650 pounds per square inch (gage) is provided by a nine-stage horizontal centrifugal pump. The coolant enters the primary system at 120°F and exits at 162°F. The heat removed is dissipated to the atmosphere by a conventional cooling tower.

Approximately 200 gallons per minute of primary coolant is taken from the exit of the heat exchangers and circulated through a low-pressure cleanup system. This consists of a degasifier, prefilter, demineralizer, and a 150-micron afterfilter. From here the clean water is sent to a head tank, from which the pressurizer pump takes its suction.

Operating History

Operation of the reactor since attaining full power has been generally smooth. The percent on-stream time for the last 5 years has averaged 91.5, with a maximum of 94.0 attained in 1973.

This is an unusually high record and has been attributed to a number of intentionally instituted factors. In addition to its sound basic design, some of them are:

— A thorough bench testing of components before final acceptance of the design.

The neutron reflector which surrounds the fuel assembly and control cylinders is one of the largest beryllium components ever machined.

- A shakedown period to debug all systems and components.
- A computerized, programmed system for handling routine maintenance of essential equipment.
- A two-out-of-three coincidence safety system that allows on-stream testing and maintenance of individual channels. This safety system has considerably reduced the number of accidental shutdowns. Its reliability can be attributed almost entirely to the high quality of the instrumentation and controls designed and developed by the Reactor Controls Department of the Laboratory's Instrumentation and Controls Division.

Hydraulic Tube

A hydraulic tube runs down the center of the flux-trap region of the HFIR core. Small samples enclosed in thumb-sized cylinders called rabbits can be inserted into and removed from the core by means of a loading system in the pool at any time during normal reactor operation. The in-vessel section of the tube penetrates the vessel through the quick-opening hatch and runs straight down into the center target position of the flux-trap region.

The rabbit is made of the same material as the target rods, with fins that space it from the tube wall. Nine rabbits can be irradiated simultaneously.

Rabbits placed in the loading station can be moved into the reactor core by water flow from the high-pressure area of the core. This water also provides the normal coolant flow for the in-core rabbits. Valve manipulation can then drive the rabbits out of the core and return them to the loading station. The valves are all at the loading station.

Target Rods

As many as 30 target rods are irradiated simultaneously in the flux-trap region of the reactor. Each one contains about 10 grams of actinide oxides and remains in the neutron flux from 1 to 1½ years. After removal, usually in bundles of ten, the targets are loaded, under water, into a specially designed shielded shipping cask and carried next door to the Transuranium Processing Plant (TRU) for chemical separation of the transplutonium isotopes and refabrication into new target rods for the HFIR.

A technician uses an optical device to get a close-up view, through the 54-inch-thick window, of the cell interior.

The ORNL Transuranium Element Program

By JOHN BIGELOW and LESTER KING

The Transuranium Processing Plant (TRU) is the production, storage, and distribution center for the USAEC heavy element research program. It functions integrally with the researchers, rather than merely as a supplier. At TRU, target rods irradiated in the High Flux Isotope Reactor (HFIR) are processed for the separation, recovery, and purification of the heavy actinide elements, that is, the elements plutonium through fermium. These elements are then distributed to laboratories throughout the country for research work, usually in a highly purified state. They can be, and frequently are, sent in special chemical forms and even in special devices if so requested by the experimenter.

A total of 645 shipments of transuranium elements have been made from TRU to researchers throughout the world. Totals of about 2 kilograms of curium, 200 milligrams of berkelium-249, 1.7 grams of californium-252, 6 milligrams of einsteinium-253, and 5 picograms of fermium-257 have been processed.

In addition to the normal production activities at TRU, various programs are undertaken there in cooperative ventures with other research laboratories to assist them in their work. Special isotopes may be provided at their request, and hot-cell space is made available for experimental programs that may involve large quantities of material, when such an approach is feasible. All target materials to be irradiated in the HFIR flux trap are prepared in TRU. Such materials include recycled americium and curium in HFIR targets and heavy isotopes in many special forms, including the rabbits that are used for short-term irradiations.

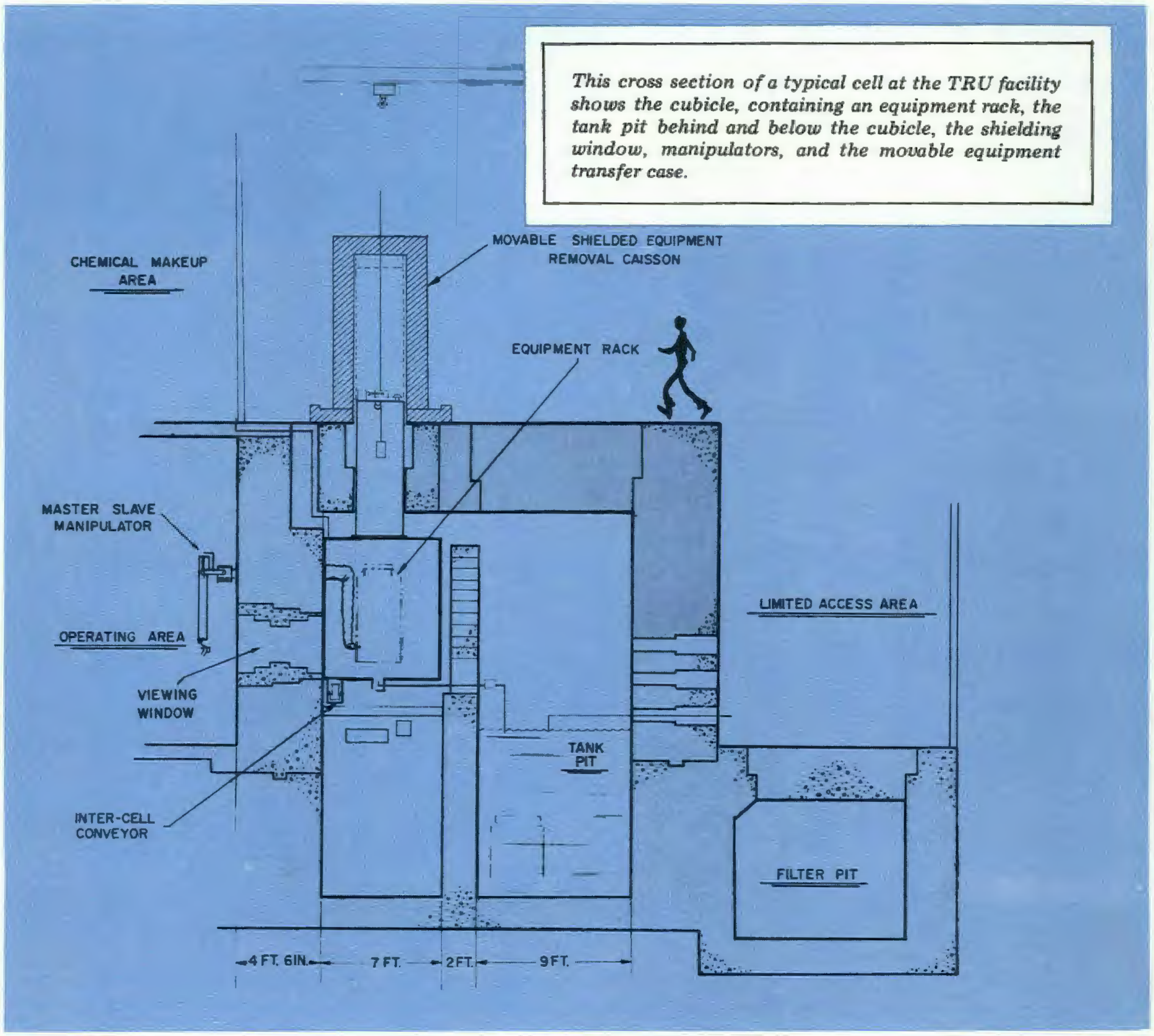
The isotope californium-252, with its half-life of a little over $2\frac{1}{2}$ years, is itself valuable as a source of neutrons. Over fifty such research tools have been fabricated during TRU's lifetime for loan to scientific investigators throughout the

John Bigelow was one of the early benefits from the felicitous relationship that has long been enjoyed by ORNL and the Massachusetts Institute of Technology. A member of the MIT Practice School in 1951, he joined the staff as a summer employee the next year after earning his master's degree in chemical engineering. With his doctorate in 1956, he became a member of the Chemical Technology Division, where today he serves as transuranium materials coordinator, planning the TRU facility production to meet the needs of the scientists that use its products as research tools. Co-author of this article is Les King, another veteran ORNL staff member, who has been in the Chem Tech Division since 1954, coming here, after a military stint, with a master's in chemical engineering from the University of Rhode Island. He was active in developing the Thorex Pilot Plant and other projects in Unit Operations, becoming an expert on automated data logging and analysis. He has been with TRU since 1966, and became manager of the facility in 1972. He and Bigelow, shown here (right and left) in King's office, have together supplied this updated account of the Transuranium Processing Plant and its role in the nations's heavy element program.

country. Other aspects of the program include the development of remote chemical separations processes and of methods of fabricating HFIR targets.

From 1966 to 1973, TRU had been recovering transuranium elements as rapidly as they could be produced in the HFIR and fabricating targets as fast as targets were being processed. Production rates were limited because of the low-quality feed materials (plutonium-242 and 94% curium-244), which required long irradiation periods (up to 18 months) to build up appreciable amounts of the heavier elements. In the period from 1970 through 1973, a great amount of material was processed that had been irradiated as part of the Californium-I Program at the Savannah River Plant. This program was designed to evaluate the commercial market for californium-252. As a result, the availability of the heavy isotopes increased sharply, not only because of berkelium and einsteinium recovered during the processing, but also because some of the curium recovered from the Californium-I material could then be used as feed for the HFIR targets. Berkelium, californium, einsteinium, and fermium are produced much more rapidly from this curium because it is rich in the heavier isotopes (curium-246-248) and can be transmuted in shorter irradiation periods (up to eight months).

The experience gained at TRU in handling actinide elements in quantities two orders of magnitude larger than those handled by other facilities is of direct use to the AEC's nuclear power program. It is anticipated that in the next 20 years nuclear power reactors will generate tens of tons of americium and tons of curium. These hazardous alpha emitters will eventually accumulate in the high-level wastes from reprocessing plants. One suggested method to decrease the long-term hazard, which extends beyond 500 years, is to separate the actinides and transmute them by exposing them to neutrons of a specific

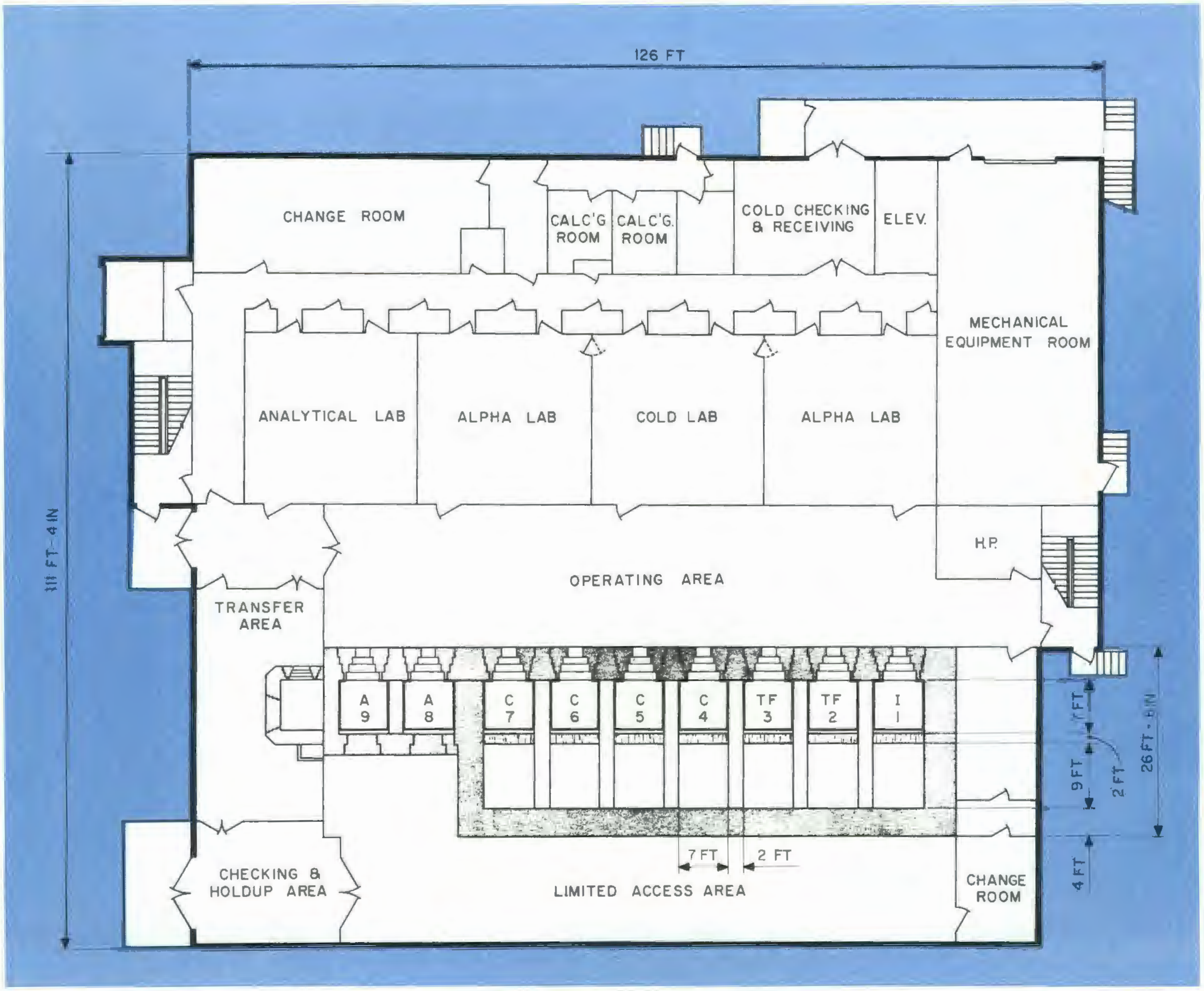


This cross section of a typical cell at the TRU facility shows the cubicle, containing an equipment rack, the tank pit behind and below the cubicle, the shielding window, manipulators, and the movable equipment transfer case.

energy, to shorter-lived fission products. The radioactive materials remaining in the wastes would then decay in a much shorter time period. Personnel previously associated with TRU are now applying their experience to determine the feasibility of this concept and to develop methods to remove actinides from commercial high-level wastes.

The Plant

The heart of TRU is a battery of nine heavily shielded process cells housed in a two-story building. Each cell, with its 54-inch-thick walls of a special high-density concrete, has enough shielding to stop the neutrons and gamma radiation from 1 gram of californium-252 and associated fission products. Four cells contain chemical processing equipment for dissolution, solvent extraction, ion exchange, and precipitation. Three contain equipment for the preparation and inspection of HFIR targets, and two cells are used for analytical chemistry operations. In addition, there are eight laboratories used for process development, for part of the process-control analyses, and for product finishing operations.



The two-story TRU facility is divided into a laboratory wing and a process area, which includes the shielded cell bank. Remote target fabrication is performed in cells 1, 2, and 3. Chemical separations processes are done in cells 4—7, while 8 and 9 are for analytical chemistry work.

The building was designed for absolute containment of the highly radioactive materials handled in the cells and laboratory glove boxes. The vessels that carry the materials, the cubicles within the cells, and the cells themselves constitute three successive containment envelopes, each separately ventilated; ventilation exhausts are doubly filtered before the air is discharged to the stack. To further ensure that all materials are contained within the building, all operating areas and laboratories are maintained slightly below atmospheric pressure by the building ventilation system.

All operations and maintenance are performed remotely with master-slave manipulators and viewed through specially designed windows. Each window consists of several thick panes of lead glass alternating with compartments filled with mineral oil. The oil helps shield against neutrons, while the glass controls the gamma radiation.

Smaller items of chemical processing equipment, such as valves, pumps, and ion exchange columns, are mounted on racks in the cells. This equipment can be installed or removed remotely by using manipulators and air-operated impact wrenches to open or close the disconnect clamps on the many process lines. Process vessels in pits behind the cubicles are serviced and maintained with a combination of remote and underwater maintenance techniques.

Contaminated equipment can be removed from the cell bank either through an inter-cell conveyor to a shielded carrier at a loading station at one end of the cell bank or through the top of the cell to a shielded caisson designed to maintain shielding and contamination control during the transfer.

Heavy Element Separation

When an irradiated target from HFIR arrives at TRU, it enters the main-line process, which consists of:

- dejacketing the target by dissolving the aluminum in caustic-nitrate solution;
- dissolution of the actinide oxide particles in nitric or hydrochloric acid;
- separating the residual plutonium by batch solvent extraction into di(2-ethylhexyl)phosphoric acid (HDEHP) in aromatic diluent;
- feed conditioning and general cleanup by batch extraction of the actinides (and lanthanides) from a low-acid salt solution using HDEHP in aliphatic diluent;
- further decontamination of the actinides from fission products in either a solvent extraction or an anion exchange process using concentrated lithium chloride solution;
- final decontamination from fission products and separation of americium and curium from the transcurium actinides by lithium chloride-based anion exchange; and
- isolation of the berkelium, californium, einsteinium, and fermium by chromatographic elution from a cation exchange resin column.

Final purification of the berkelium, californium, einsteinium, and fermium is accomplished by repeated application of cation exchange processes. Berkelium is also purified by a batch solvent extraction technique using HDEHP in dodecane. Once free of californium, the other products receive their final purification in glove boxes.

The americium-curium fraction is recovered from the lithium chloride solution by batch solvent extraction using tertiary amines. Final purification is done by successive oxalate precipitations. Actinide oxide particles are produced for recycle to the target fabrication process by loading the purified americium-curium solution onto cation resin beads and burning the loaded resin to decompose the organic matrix and drive off the volatile constituents. The resulting particles have ideal properties for HFIR target use.

The chemical processes are complicated by the very corrosive nature of the solutions and by the effects of radiolysis; in addition to beta and gamma radiation from fission products, energetic alpha particles from the heavy elements contribute to the breakdown of solution components. A particular problem is the destruction of acid, the concentration of which must be held within narrow limits if the separation process is to work. The process lines and vessels must be made of such special corrosion-resistant materials as Hastelloy C, tantalum, and Zircaloy-2.

The TRU operation is an exotic one, complicated as it is by the extreme radioactivity of the materials involved. As a result, many problems heretofore not encountered have had to be dealt with. One of them is that solutions left standing have a tendency to evaporate rapidly to dryness because of the high temperatures generated by the radioactive decay of the unstable isotopes. The result has been the development of special vigilance and skill on the part of the operators to prevent this from happening.

Preparing the Target Rods

Three of the Transuranium Processing Plant's nine hot cells have equipment for the preparation of recycle target rods. The first cubicle is used for preparation of the pellets, the second for loading and welding the tube, and the third for inspections.

A blend, about 18% of whose volume is actinide oxide powder and the rest aluminum powder, is cold-pressed at 15,000 pounds per square inch into a thin-walled aluminum sleeve capped on both ends with aluminum powder. Thirty-five of the resulting $\frac{1}{4}$-inch-diameter, $\frac{1}{2}$-inch-long pellets, with a density approximately 80% of theoretical, are then loaded into a finned aluminum tube. This tube is sealed by tungsten—inert-gas welding and is hydrostatically collapsed onto the pellets at about 10 tons per square inch.

The target rod is then subjected to a rigorous inspection procedure which includes helium leak check, radiography of the end-closure welds, dimensional inspection, and a contamination check.

Insertion of the target rods into the HFIR completes the processing cycle.

BOOKS

By JOHN CATHCART

COME IN, BARNARD'S STAR

Communication with Extraterrestrial Intelligence. Proceedings of the CETI Conference held in September 1971, in Yerevan, capital of the Armenian SST, in sight of Mt. Ararat. Edited by Carl Sagan, MIT Press, Cambridge, 428 pages, $10 (1973).

If you ask a dedicated reader of science fiction whether or not it would be worth while trying to communicate with beings in outer space, he would say, "Certainly!" knowing full well that the galaxy is populated by all sorts of interesting and exotic creatures. Ask the same question of a practicing scientist, and he will undoubtedly try to give you some estimate of the probability of success for such a venture.

That is exactly what happened during a conference held in September 1971 to consider just that question. The proceedings, edited by Carl Sagan, are now available and make fascinating reading. The meeting, held at the Byurakan Astrophysical Observatory in Russia, was organized by a group of Soviet and American scientists and was attended by an elite group of 54 men drawn from both the physical and the life sciences. The participants were mainly Russian and American, with a sprinkling of representatives from Europe and Canada.

The conference began with the presentation of a deceptively simple formula. The number, N, of technical civilizations in our galaxy that are at least as advanced as we are is given by

$$N = R_* f_p n_e f_l f_i f_c L \; , \qquad (1)$$

where R_* is the rate of star formation in the galaxy; f_p, the fraction of stars possessing planets; n_e, the number of planets per "solar system" capable of sustaining life; f_l, the fraction of such planets on which life actually originates; f_i, the fraction of planets where life not only originates but develops intelligence; f_c, the fraction of planets on which intelligent beings develop to a "communicative phase"; and, finally and ominously enough, L, the mean lifetime of technical civilizations.

At first glance it would appear totally impossible to assign meaningful values to the factors in Eq. (1), and I must say that that could also be the final conclusion of the skeptical reader even at the end of the book. But then astronomers, especially, rarely draw back from an "educated guess" or, to use the more elegant language of the conference, the use of "subjective probability statements"! Even so, the results were both entertaining and illuminating, not to mention exciting!

The rate of star formation, R_*, was apparently an easy one: 10 stars per year and no arguments. Even f_p, the fraction of stars with planets, and n_e, the number of planets capable of sustaining life, aren't too difficult to estimate. Although we will probably never be able to detect directly the planet of another star with an earth-based telescope, a detailed study of the motion of Barnard's star (a nearly red dwarf) strongly suggests the presence of at least three planets circling the star. Furthermore, the otherwise strange variation of angular momentum with stellar mass can be understood if one assumes that stars slightly less and slightly more massive than the sun possess planetary systems. Thus, literally billions of stars in the galaxy can be expected to have planets revolving around them, and Bode's law would indicate that in any planetary system at least one planet would have such a mass and would be at such a distance from its "sun" that life as we know it might exist.

The life scientists took over in evaluating f_l, the fraction of planets on which life evolves. All we really know is that life did develop on earth and in a relatively short time, no more than a billion years after the formation of the solar system. We also know that an electrical discharge in a mixture of methane, ammonia, carbon dioxide, and water (the constituents of the atmosphere of the primordial earth) leads to the formation of amino acids and proteins, the building blocks of living creatures. These same organic compounds have also been found in meteorites. Thus one can infer the existence of at least primitive life in very many places in the galaxy.

And what of f_i and f_c, the possibility of the evolution of intelligence and of technical civilizations? Here the discussion, of necessity, becomes somewhat murky and anthropomorphic. The latter characteristic was much inveighed against, but what else can one do when he knows of only the example of Earth and man? There was much talk of the number of nerve cells required for the development of intelligence (10^{10} to 10^{11} in the case of man) and the number of memory bits needed to make an "intelligent" computer. There seemed to be a consensus that the development of a complex nervous system almost certainly leads to learning and the evolution of intelligence. A technical civilization, however, is a long time in coming — more than 3.5 billion years on earth.

For the last factor in Eq. (1), L, the average lifetime of technological civilizations, we cannot offer even one example from human experience. Estimates ranged from 50 to 1,000,000 years and higher, and most of the discussion was philosophical or psychological in tone. Suggestions concerning the future of technological civilizations ranged all the way from the existence of computer-derived logical abiological life to a situation in which the very creature comforts provided by technology would dull the creative drive of such societies.

At this point in the conference, Carl Sagan attempted to give an estimate of N, the number of advanced galactic civilizations. He took R_* as 10 stars per year, and he assumed f_p and n_e, the fraction of stars with planets and the number of "habitable" planets per solar system, to both be about $\frac{1}{2}$, that is, on the order of unity. The product of $f_l f_i$ and f_c (the probability of the sequential emergence of life, intelligence, and advanced technology) he conceded to be subject to wildly varying estimates, but a value of 10^{-2} seemed to be the consensus of the conference. Thus he could say immediately that $N = 0.1L$ — which means that in the Galaxy one advanced civilization is formed every ten years!

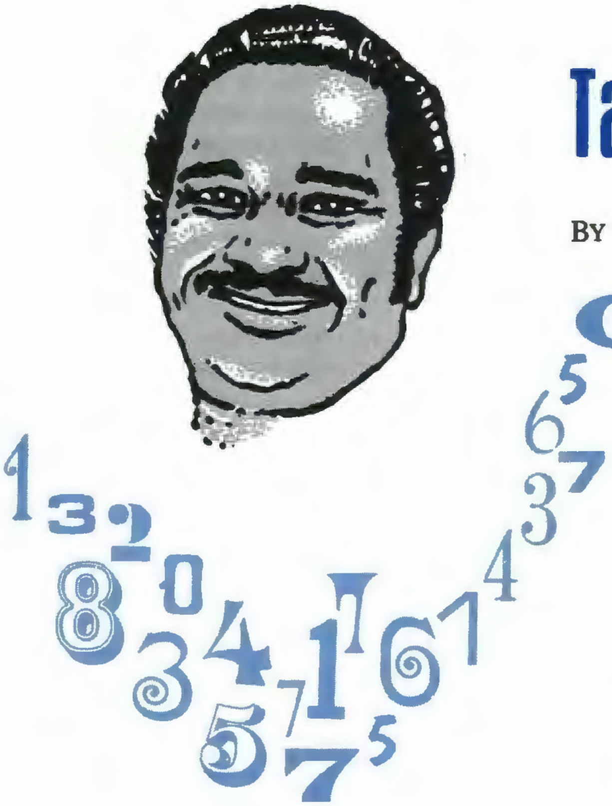
What value to take for L? The pessimists insisted on times like 10 or 20 years, in which case we might be the only advanced civilization currently extant in the Galaxy. The optimists, on the other hand, argued that perhaps 1% of evolving civilizations survived their "Time of Troubles" and lived for a very long time — 10,000,000 years. On this basis, there would be a million technical civilizations in the Galaxy, and the distance to the nearest one would be only a few hundred light years.

The balance of the conference was devoted to a discussion of various aspects of communicating with extraterrestrial intelligence: what kinds of spaceships might be built, how to receive or send a radio message, what the message should say, what the characteristics of a really advanced civilization might be, and what the consequences for us would be if we did make contact. Some of the suggestions were farfetched, but all were fascinating!

One topic is notable by its absence in the program of the conference: UFO's. They are referred to in passing in three places and then frequently disparagingly. One paleontologist from Arizona considered belief in extraterrestrial guidance to be "a monument to gullibility"! Perhaps so, but at the moment it strikes me that UFO's represent the only terrestrial hint we have of extraterrestrial intelligence!

Take A Number.....

BY V. R. R. UPPULURI



PERFECT SQUARES

It is well known that, for all values of a and b, $a^2 + 2ab + b^2$ is a perfect square. How about $a^2 + 3ab + b^2$? Clearly, $a^2 + 3ab + b^2$ cannot be a perfect square for all values of a and b. But for $a = 3$ and $b = 7$, $a^2 + 3ab + b^2 = 3^2 + 63 + 7^2 = 121$, which is a perfect square $(=11^2)$. Similarly, $a = 2 \times 3$ and $b = 2 \times 7$, $a = 3 \times 3$ and $b = 3 \times 7$, etc., make $a^2 + 3ab + b^2$ a perfect square. Is there a systematic way of finding other pairs (a, b)? How about $a^2 + 4ab + b^2$, etc.?

ROOM SQUARES

There are several examples known which show that certain statements are true for small values of n and false for larger values. There are not too many known, however, that are false for small values of n and true for larger values. The existence of Room Squares falls into this latter category.

In 1955 the Australian mathematician T. G. Room proposed an interesting combinatorial problem. The directors of duplicate bridge tournaments have grappled with virtually the same problem in arranging and pairing of opposing teams and the assignment of prearranged deals to tables. In a 7 by 7 square grid we are required to insert the eight individuals A, B, C, D, E, F, G, H in accordance with the following rules: Each of the 7^2 (=49) squares either is to be left empty or is to contain two of the individuals; every possible pair of individuals should occur exactly once; and each row and column should contain each of the individuals exactly once. The accompanying figure indicates a way of accomplishing this and establishes the existence of a Room Square of order 7. It is interesting to note that on a 3 by 3 square, we cannot insert four individuals A, B, C, D with the above rules. Similarly on a 5 by 5 square, we cannot insert six individuals A, B, C, D, E, F with the above rules.

Mathematicians have strived to prove the existence of Room Squares of all odd orders larger than or equal to 7. For some time, 257 by 257 squares gave trouble. In 1973, another Australian mathematician, W. A. Wallis, settled this problem.

A B	–	E F	G H	–	–	C D
F H	A C	–	–	–	B D	E G
–	F G	A D	B C	–	E H	–
–	D H	–	A E	C G	–	B F
D G	B E	C H	–	A F	–	–
C E	–	–	D F	B H	A G	–
–	–	B G	–	D E	C F	A H

The accompanying article is the result of a taped conversation with Hal Schweinler, subsequently augmented, edited, emended, and censored by his colleagues in the Health Physics Division, Ephraim Klots and Ray Garrett. They have all participated about equally in the work they describe here. Schweinler is a member of the increasingly exclusive group at ORNL who joined the Manhattan Project in the forties, their graduate education half completed, and then left to pick up their doctorates when the war was over. Schweinler was at the University of Chicago Met Lab with a master's degree from Carnegie Tech from 1944 to 1945, when he came to Oak Ridge. In 1948 he left for MIT, receiving his Ph.D. there under J. C. Slater and staying on to serve on the faculty. In 1954 he rejoined Oak Ridge National Laboratory. Here he is a physicist in the Health Physics molecular theory group, putting in time also teaching at the University of Tennessee, where he has been named Ford Foundation Professor of Physics. This description of the mobilization of scientists at ORNL to study the recently discovered electricity-conducting organic crystals stands as an account of how rapidly an institution such as the Laboratory can mount a multidisciplinary study in response to an unusual scientific disclosure.

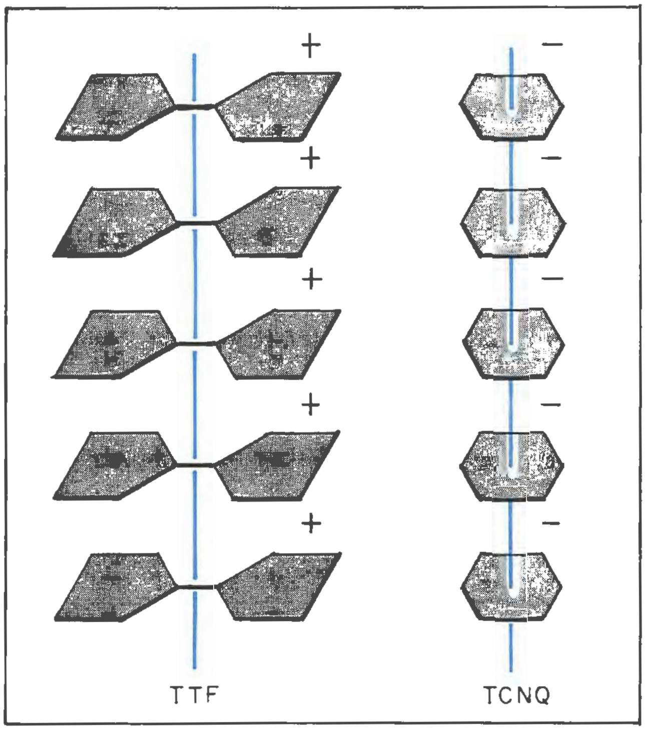
Checking Out the High-Conducting Organic Salts

By HAROLD SCHWEINLER

At the San Diego meeting of the American Physical Society in March 1973, a post-deadline invited paper by Professors A. J. Heeger and A. F. Garito and their co-workers at the University of Pennsylvania reported a startling discovery. The organic salt called TTF-TCNQ has, at about liquid nitrogen temperature, an electric conductivity close to that of copper at room temperature. This was 100 to 1000 times as high as any previously reported conductivity for organic salts. Incipient superconductivity was invoked in explanation.

The news spread rapidly at ORNL after the meeting. Harold Davis of the Solid State Division, in particular, passed the news around, and he and I and others organized a luncheon meeting in the cafeteria dining room to discuss it. If the report proved to be true, the payoff was large enough to make it well worth checking out.

We might comment here that the authors themselves, in presenting the paper in San Diego, offered this as a possible superconductor, although many other scientists at the meeting had doubts about this interpretation of the high conductivity. The excitement arose because the conductivity followed the same law $[(T - T_c)^{-3/2}]$ as is found in the electrical conductivity of Type II superconductors above the critical temperature. Nevertheless, the properties themselves were very interesting and unusual and offered a sufficient return just from studying high conductivity, whether the crystals turned out to be superconductors or not.



Good organic conductors seem to fulfill two requirements: unpaired electrons on the individual moieties, following charge transfer; and crystallization in one-dimensional homologous stacks with the planes of like molecules parallel.

Our first problem was to get materials on which to make measurements. In the TTF-TCNQ compound, the TTF synthesis was difficult, and the TCNQ relatively less so. Vernon Raaen volunteered to synthesize the TTF and in a few weeks had 5 grams of this valuable substance; he also purified the TCNQ and then grew from solution some of the large single crystals of the charged transfer compound TTF-TCNQ. Our crystals, at 0.6 centimeter, were larger than those reported from any of the other groups active in the field — Bell Laboratories, IBM San Jose and IBM Yorktown, and Du Pont, as well as Heeger's group.

Once the crystals were in hand, there were many people at ORNL who were interested in making measurements on them because they have properties that are interesting from the physics point of view, and this sparked interest on the part of the people in the Solid State Division, the Instrumentation and Controls Division, and the Health Physics Division. A commentary on the versatility of ORNL lies in the list of properties that were of interest to people in other divisions in the Lab and the measurements they made on the crystals.

At the first ORNL meeting, Carl Koch, of Metals and Ceramics Division, told us that he had phoned a friend from Brookhaven who had been at San Diego and had been told that the Meissner effect had not been established in TTF-TCNQ. This effect — rejection of the magnetic flux upon cooling in a magnetic field — is widely accepted as the crucial test for superconductivity, and it clearly had to be checked. Stan Sekula, of Solid State Division, modified some of his existing equipment to make a quick test; he found no Meissner effect.

The first measurements that had to be repeated to check the Pennsylvania group were those of electrical conductivity. The solid goes through a wide range of conductivity as the temperature is varied; for that check, a device built by Jim Blankenship in Instrumentation and Controls for measuring the Hall effect and electrical conductivity of germanium and silicon proved ideal. It had a high input impedance, which was needed for measuring the wide range of conductivity of the TTF-TCNQ. Blankenship and Bruce Warmack, a graduate student from the University of Tennessee physics department, devised holders for the single crystals, selected good single crystals, mounted them, and measured the electrical conductivity. The main problem in accomplishing this was attaching four wires to a crystal about 6 millimeters long and knowing the spacing accurately. Making contact is a difficult task when the crystals are so tiny, and, of course, nothing can be soldered to an organic crystal.

In the Health Physics Division, Eph Klots and Bob Compton immediately began measurements of the ionization potentials of these constituents and of the electron affinity of the TCNQ. Oak Ridge National Laboratory is unusual in its ability to study such a problem because so many different techniques are available for different aspects of the problem — from molecular properties of the constituents to x-ray diffraction studies, to the electrical measurements themselves. All these efforts were voluntary, sparked by the interests of those in the different divisions.

It is very evident that the materials problem is one of the biggest problems. Vernon Raaen supervised a summer student from the University of Michigan, Becky Wenzel, who made a TTF-chloranil compound suggested by some reading she had done in this field. Raaen made heptafulvalene, whose synthesis was much easier than that of TTF;

Crystals of TTF-TCNQ grown at ORNL. At the time, they were the largest to be synthesized. Their glossy black color is characteristic of good organic conductors.

Hückel calculations made by Iran Thomas, then of Chemical Technology Division, suggested it might be the approximate equivalent to TTF. In the Biology Division, Dave Doherty made diphenyl TTF and tetraphenyl TTF. In Chemistry, Raaen also grew crystals of hexamethylbenzene-TCNQ, and Carroll Johnson determined its crystal structure by neutron diffraction. These efforts represent only the first two months or so of the Laboratory's involvement, yet two fields of inquiry were broached: the crystal structure determination using neutron diffraction by Johnson and the electron affinity determination by Compton and Klots. It is in this approach that the Laboratory is, if not unique, at least one of only a few laboratories in the world in which such relatively sophisticated techniques can be applied to such a problem, especially in such a short time. A little later Herb Mook, of the Solid State Division, determined the phonon spectrum of TTF-TCNQ with incoherent inelastic scattering of neutrons, and this, too, is a technique that few laboratories have available today.

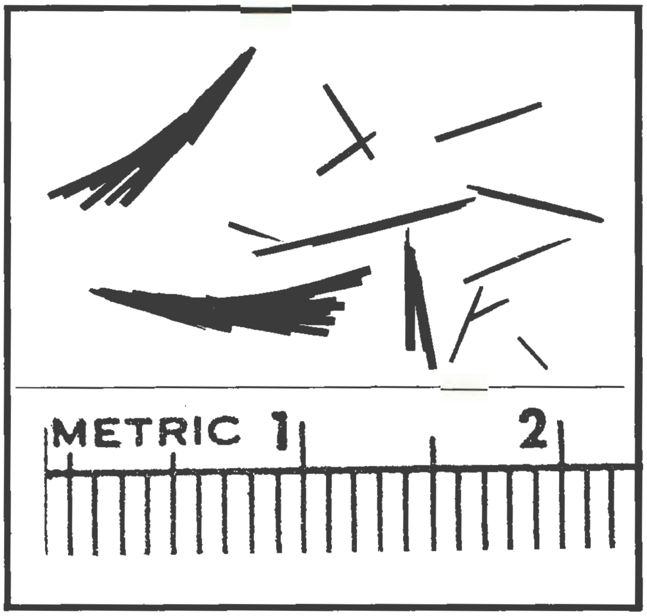
In addition to the capabilities of ORNL just mentioned — neutron diffraction, x-ray diffraction, and so forth — there are other facilities which individually are not unique to the Laboratory but whose availability at one location and on very short notice offer a tremendous research advantage. Let me give some examples:

— Crystal growing is a very specialized art, and the Laboratory has acknowledged expertise in this field.

— Orientation of crystals was also studied because we needed to know what the crystal axes of the TTF-TCNQ were. George Brown of the Chemistry Division helped us here.

— Another kind of measurement that was easy to do here was that of the optical properties of the crystals; the Laboratory already had the essential setup in the Health Physics Division.

— Molecular orbital calculations were very important to know for the selection of molecules which might have properties similar to those of TTF-TCNQ, and, again in this case, Iran Thomas had done a number of such calculations for several molecules.



— Another area of inquiry was optical absorption measurements in the vapor phase and solutions. Jack Young of Analytical Chemistry helped with these measurements.

— Measurements of Raman spectra of the molecules were made by George Begun of the Chemistry Division and by John Burns of the Solid State Division.

— And to mention one more, the crystals underwent mass spectrometry for chemical analysis. High-temperature mass spectrometry is a tool not widely available, but Bill Rainey at Y-12 was set up to help us with this.

All these capabilities are available right here at ORNL; moreover, there were scientists who were interested in the problem and wanted to make these measurements. There aren't very many places where this could have been done in such a short time. By late fall, barely half a year after the San Diego meeting, no fewer than six divisions had entered the arena.

Christmas and vacations intervened for a while, but a little extra support came through in January, making it possible to hire a postdoctorate organic chemist, Charlie Watson, from the University of Tennessee, to help us get on with the preparation of other compounds. Several other compounds had been made and tested for such things as their electronic energy, but so far no compound has been found that can compete with TTF-TCNQ. Some of the compounds formed were insulating, and some were conducting at optical frequencies: They had the same black glossy look of the TTF-TCNQ and were formed in needles of roughly the same habit, yet they did not have the electrical conductivity characteristic of this unusual salt.

Three papers have come out of this work: one by Malcolm Skove and his postdoctoral student, Jim Cook, of Clemson University, on the thermal expansion and electrical conductivity of materials prepared by Raaen; one by Klots, Compton, and Raaen on the electron affinity of TCNQ and related compounds; and the most recent on the electrical conductivity of our specimens of TTF-TCNQ by Warmack, Callcott, and me. The crystal is highly anisotropic — the ratio of conductivity being 200 to 2 to 1 roughly. Professor Tom Callcott, of the University of Tennessee; his graduate student, Bruce Warmack; and I were able to reproduce the potential and current measurements of the Bell Laboratories group, but not those of Heeger's group. The temperature dependence of Heeger's group differed from that of the other two groups.

Let me bring out one point here in the theoretical analysis that the last-mentioned work is concerned with. A number of people have suggested that Heeger's result was, in fact, possible because of a mistake in the placement of the electrical contacts on the small crystals. In the analysis just referred to, there is evidence against a mistake on the part of Heeger and his group because, in doing the theoretical modeling of the crystal, we could not reproduce his result with a standard crystal simply by misaligning the wires. Thus we can conclude that Heeger's crystal had different electrical properties and that his results were not produced by a mistake. And, indeed, at an APS meeting in March, the Pennsylvania people were holding fast to their claims. This evidence provides additional incentive to continue the research.

Scientific inquiry so far hasn't been confined to just the original compounds that the Pennsylvania people worked on; in fact, we've tried quite a few combinations of things. None of them have worked nearly as well as the original Pennsylvania combination, and we are able to hazard guesses at the reasons. The measurements on the individual molecular species of ionization potentials and electron affinity, and on the crystals, such as the x-ray and neutron diffraction, give the clues. In some cases the new combinations have crystallized in the wrong spatial arrangement; in other cases their constituent molecules don't have a low enough ionization potential or don't have a high enough second electron affinity. So we're learning from failures because, when things don't work, we pretty much know why they don't work. Knowing this leads us to believe that there's a good chance that someone will come up with some compounds and combinations of compounds that will work, maybe even better than the original combination of TTF-TCNQ that the Pennsylvania people discovered.

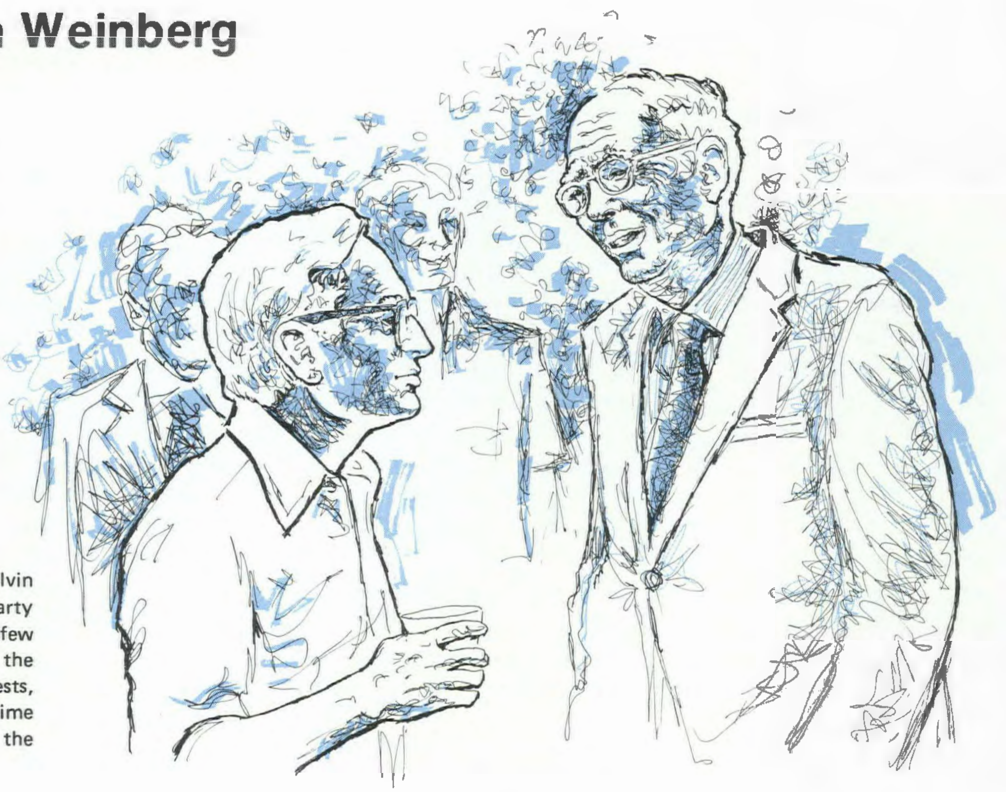
One of the nice features about organic molecules is that there are lots of them. One doesn't quickly run out of possibilities. So the fact that a reasonable number of different molecular species have been tried so far and none of them has been as good as the TTF-TCNQ isn't really discouraging. Indeed, the IBM people have produced a selenium analog of TTF which may be even better than TTF itself. TTF and its selenium analog have the highest conductivity of any organic crystals known, and, any time you have a chance to examine the most extreme example of any class of properties, it's sure to be good hunting grounds; consequently, it remains a very interesting crystal, particularly as a bridge between ionic crystals and molecular crystals.

I must say that it's a great pleasure to work with the professionals at Oak Ridge National Laboratory who, when they see something that needs doing, just move in and do it, and a week later I find that it's been done. This is a far cry from working with students, who often must repeat their work, being learners, besides requiring a good deal of guidance into what work must be done. So, for me, this has been a great experience. It has taught me some of the scope of research possible at the Laboratory, not to mention giving me the chance to get better acquainted with people who know how to do what they are doing.

It's probably only because of this capability that we are in any position at all to compete with Du Pont, IBM, and Bell Labs. These places are all knocking themselves out, pouring fantastic sums of money into all this; whereas the ORNL operation is being done on a shoestring and more or less on a volunteer basis. I don't know what it is, but we certainly must have something going for us!

(*This article was prepared by the editor from a taped conversation among Eprhaim Klots, Ray Garrett, and me; we all contributed, through editing and phraseology, to its final form. — H.C.S.*)

Words for Alvin Weinberg



A couple of hundred friends of Alvin Weinberg's surprised him with a party last month, presenting him with a few gifts and their good wishes. Voicing the deeper feelings of each of the guests, Ellison Taylor turned to his longtime friend and delivered, without notes, the following encomium:

Ladies and Gentlemen, Friends and Colleagues of Alvin Weinberg:

We've all been enjoying ourselves at a splendid party, but that's not enough these days. Even a party has to be relevant. Well, the relevance of this party is that it's a Surprise Party for Alvin Weinberg.

There was some discussion about what kind of party to call it. Some suggested a Farewell Party, but that didn't make sense because Alvin wasn't going away from Oak Ridge but, in fact, was coming back for the party. So, someone said, "Call it a Welcome Party," but someone else said that would be thirty years too late. In the end, there was nothing left to call it but a Surprise Party. I said I thought someone working in Washington these days didn't need any more surprises, but they said just one more surprise wouldn't hurt. So, here we are at a Surprise Party for Alvin Weinberg.

Now, let me adopt an unfamiliar pose for a few minutes and speak seriously.

Alvin, each of us has a reason (or several reasons) to want to thank you for things you have done: opportunities you have provided, stirring words, grand concepts, little acts of friendship. But it's hard to expess these things face to face, one on one — the words don't come out right; the moment slips away; and it's not our style in this century, anyway. So, let me try to say once, briefly, for all of us, something that can stand in for all the words that don't get said person to person.

We are all members, or sometime members, of the Oak Ridge National Laboratory — an association that has been the most important aspect of our professional lives. And that Laboratory is what you, with some help, to be sure, from our colleagues and ourselves, have made it. You knew enough to save it on that winter morning a quarter of a century ago when the rest of us would have walked out on Jim Fisk. You found us major goals. You insisted that we develop a scholarly tradition in our technologies. And you foresaw the changing roles of science and technology in our society. But, most of all — by far the most — by your example, by your leadership, by what you are — you made our Laboratory an institution to which we could give our loyalty — something that was part of ourselves but greater than all of us. None of us need regret that we were not born in more auspicious times — that we were not at the Cavendish, or at Los Alamos in the old days. We have all been part of a great and remarkable institution during its first period of greatness. And you put it all together; you made it all happen. We thank you very much.

Dave Braski is a metallurgical engineer with a bachelor's from Rensselaer Polytechnic Institute (1959) and a master's degree from Virginia Polytech (1967). In the years between schools, he dispatched his military obligation in the USAF, serving as a weather officer. Joining the ORNL staff in the Isotopes Division in 1967, he worked in Ed Kobisk's accelerator target development program (see Summer '69 ORNL *Review*), from which he moved naturally to the two NASA Skylab experiments described here. After this work, he transferred to the Metals and Ceramics Division, where he conducted iridium alloy fabrication studies connected to the Space Nuclear Systems. Right now he is in the microstructural analysis group in M & C, engaged in research on materials for the reactivated Molten Salt Reactor program. Here (r.) he confers with Kobisk and Hal Adair, Isotopes Division, with whom he worked on the NASA welding experiments.

Isotopes in Space

By DAVE BRASKI

During the past year and a half, I have enjoyed working on two NASA Skylab metallurgical experiments. The first one, M-552, studies the feasibility and various theoretical aspects of brazing in space; the second, M-558, deals with the ways in which liquid metals diffuse in the absence of gravity. The project started in the summer of 1972 when the Isotope Target Group, headed by E. H. Kobisk, was asked by NASA's Marshall Space Flight Center (MSFC) in Huntsville, Alabama, to provide radioactive isotopes and some of the flight hardware for the two experiments. The radioisotopes were to be used as tracers on M-552 and M-558; so it was our job to install them in specimens compatible with NASA's flight hardware and later to determine their movement or redistribution.

M-552 — Exothermic Brazing

The exothermic brazing experiment was aimed at providing practical answers to questions about brazing in space. These questions range from the feasibility of joining metals by brazing in a zero-gravity environment to the basic properties of liquid metals on metal surfaces. The braze assembly used in the M-552 experiment was designed by MSFC engineers in Huntsville. It consisted of a nickel tube and a nickel sleeve having two annular grooves for holding the braze rings of the alloy 71.8% silver, 28% copper, and 0.2% lithium. The assembly was placed in a canister containing a special exothermic powder which, when ignited, created temperatures over 1000°C along the tube and sleeve. The heat was enough to make the braze alloy melt and flow into the annulus between the tube and sleeve. Upon cooling, the braze alloy solidified and welded the two nickel parts together. So basically, the experiment consisted of brazing nickel assemblies both on earth and on Skylab-II and comparing the results. Our part in this was to determine the flow characteristics of the liquid braze alloy by using a radioisotope of silver as a tracer. This we did by getting small pellets of the braze alloy irradiated in ORNL's Bulk Shielding Reactor. Their exposure to the BSR's neutron flux produced about 50 microcuries of silver-110 in each pellet.

On December 19, 1972, two gleaming white cars with the NASA emblems on the doors rolled through the ORNL portals with a team of NASA engineers and technicians, headed by Jim Williams, the principal investigator for M-552. It was the day to load the flight assemblies with the silver-110 tracer pellets. With everyone in white gloves and lab coats and a portion of Building 3038 decked out like an operating room, we proceeded to load the pellets into four braze assemblies. As I placed the tiny pellets in the small ring openings, I had a feeling that most of the people involved in this country's highly successful space program must have had at one time or another. You could almost hear "10-9-8-7. . . " in the background. Late that afternoon the NASA team left with the M-552 assemblies that were later to be subjected to a myriad of tests and inspections before delivery to Cape Kennedy.

We returned to analysis of braze assemblies that had been similarly loaded with silver-110 tracer a month previously and ignited at the MSFC laboratories. The first of these to be analyzed was sectioned into thin rings with an abrasive cutoff wheel; Bill Leslie in the Metals and Ceramics Division did the sectioning. With Maurice Allan, he also made photomicrographs and autoradiographs of the braze areas around the rings to show where the silver-110 was located. Based on this metallographic work, my colleague, Hal Adair, and I decided to try using radioactive counting techniques to determine the quantitative silver-110 distribution around the sectioned braze rings. So we took sections from a second sample which had been cut on a lathe by Paul Swaggerty at the special ma-

Claude Benge, craftsman in Building 2525 (the "green door" shop), watches while the electrical discharge machine (EDM) performs the specialized sectioning of the postflight sample of copper-silver braze that was tagged with the silver-110 isotope.

terials machine shop, and placed them in a semiautomated counting apparatus. The braze ring to be counted was mounted on a special turntable mechanism designed by Jim Gibson and built in the ORNL General Shop B. This mechanism rotated the braze ring under a small solid state detector, which was then used to analyze the area around the circumference of the braze joint. Areas containing more silver-110 isotope in the solidified braze alloy produced higher radioactive intensity readings than areas with less silver-110. Intensity readings taken every 20 degrees around a ring were subsequently plotted on a two-dimensional map to show where the silver-110 isotope ended up after the exothermic braze process. We called this plot a "radiation intensity map" and used it as the base for comparing braze flow in space with that on earth.

On May 14, 1973, Skylab-II was launched with astronauts Joe Kerwin, Charles Conrad, and Paul Weitz. The M-552 package was returned to earth on June 22, 1973. The two braze assemblies containing silver-110 isotope were delivered by courier to us for radioactive tracer analysis several weeks later. Because sections cut by either an abrasive cut wheel or lathe tended to produce nonparallel and cold-worked faces, we called upon Plant and Equipment Division personnel to come up with a better sectioning technique. In response to our request, "Rick" Richard, Murl Brown, and Luther Leavell suggested that the braze assemblies be sectioned by electrical discharge machining (EDM). We followed their advice, and within a day Claude Benge (Plant and Equipment Division) was at work sectioning the nickel Skylab assemblies. It took longer to cut sections by EDM, but the resultant ring faces were parallel and exhibited negligible metal flow. Analysis on these Skylab samples along with those tested on earth provided some very interesting and useful information. It showed that on earth the liquid braze tended to flow to the "bottom" of joint areas with large clearances that in space were adequately filled by the braze alloy. Comparison also showed that in space there was decreased resistance to braze flow, as silver-110 was detected in braze alloy that flowed well beyond the nickel sleeve. Moreover, the radiation intensity maps illustrated a general movement of braze alloy both on earth and in space toward the two areas where the exothermic igniters were located. (Earth assemblies ignite at 90- and 270-degree angles from the original silver-110 pellet location; Skylab assemblies ignite at 45- and 225-degree angles from the original silver-110 location.) These igniter areas would have experienced higher temperatures initially. Thus the addition of the radioactive tracer to the M-552 brazing experiment also provided a unique picture of the thermal history of braze melting which would have otherwise been undetected.

M-558 — Molten Zinc Diffusion

Skylab experiment M-558 was a fundamental study of liquid zinc diffusion in a 4.68-centimeter-long by 0.73-centimeter-diameter cylinder with a rather steep temperature gradient imposed over the length of the cylinder. In this experiment the zinc-65 isotope was the diffusing species for the tracer analysis. Our part in this experiment was to fabricate zinc cylinders with zinc-65 at specified locations, to conduct ground tests on a number of samples as specified by the principal NASA investigator, Dr. Tony Ukanwa, and to use sectioning and radioactive counting techniques to analyze zinc specimens from both ground tests and Skylab-III. The final analyses to determine diffusion constants, etc., will be accomplished by Dr. Ukanwa, who is currently a professor at Howard University, Washington, D.C. Work on this project actually began at ORNL in June 1972 under the direction of Frank O'Donnell, who is now in the Environmental Sciences Division. When he left our group in October 1972, I was asked to fill in.

The first step, and perhaps the most difficult in the entire program, was fabrication of the zinc specimens. Two types of specimens were required: the first, Type A, had a 0.64-centimeter-long cylindrical pellet containing zinc-65 joined to one end and the second, Type B, had a similar zinc-65 pellet in the center of the 1.842-inch zinc bar. Fabrication of the zinc bars and irradiation of the zinc pellets to form zinc-65 were no problem to the ORNL shops and the Bulk Shielding Reactor. However, joining the zinc-65 pellets to the zinc cylinders to form Types A and B specimens posed some unique challenges. Since M-558 was a diffusion experiment, it was mandatory that the joining be essentially a "cold" process to prevent the diffusion of zinc-65 across the bonding interface. Once we were restricted to a cold joining process, the range of choices became small or, as you will see, "far out." Frank O'Donnell tried cold welding the zinc surfaces in a vacuum system. He used dc sputtering to clean each of the surfaces before pressing them together under hydraulic pressure. Although a theoretically sound procedure, it failed because of difficulties in producing clean-enough surfaces. Cold pressing of the zinc cylinders in a steel die also failed because of the ever present surface oxide on the bonding surfaces. Finally, our boss, Ed Kobisk, had a brainstorm and suggested not only using the steel die, but also adding three or four drops of dilute hydrochloric acid between the zinc cylinder and zinc-65 pellet before applying the load. He reasoned that this procedure might produce the clean surfaces necessary for cold joining. Frank and I had visions of acid spraying

Jim Gibson, center, examines the capsule for the M-558 zinc experiment for Skylab that was fabricated in B Shop, as R. L. Neubert, left, and Rush Hamley, shop foremen, look on. Gibson, now at Y-12, was, during the work described here, the Plant and Equipment field engineer assigned as liaison between Isotopes and B Shop.

out the ends of the die or acid being trapped in the bond interface and were admittedly pessimistic. All three of us were pleasantly surprised to find that the first attempt at using "acid" joining succeeded in producing an excellent cold-bonded specimen. However, we were still not out of the woods completely, because about 50% of subsequent joining runs were unsuccessful. Tom Rice and I worked late one night trying to meet a deadline. It appeared to us that the specimens were breaking as they were ejected from the die. A quick check of the *Handbook of Chemistry and Physics* provided what I had hoped for — a sizable difference in the right direction between the thermal expansion for steel and zinc. So we simply placed the die in a solution of dry ice and acetone for several minutes before ejecting the bonded zinc specimen. The success ratio for joining went up to about 95%, and we were on our way. The next step was to encapsulate the zinc specimens in tantalum capsules. Thin carbon liners were placed in the tantalum to prevent reaction of the molten zinc with the tantalum. Careful TIG welding of the capsules in an argon glove box can be credited to Bill Early. Special chill blocks had to be used to prevent excessive heating of the tantalum which might vaporize the zinc specimen or cause premature diffusion of the zinc-65. After welding, Bill Early and Tom Rice checked the capsules for leaks by immersing them in a bath of warm ethylene glycol and looking for bubbles. Several leaks were detected and the troublesome areas rewelded. Other quality control tests run on the capsules include stat x-ray radiographs of the zinc specimen bond lines, conducted by W. J. Mason of Metals and Ceramics Division. The Plant and Equipment Division's smooth expertise in fabricating the zinc specimens and tantalum capsules enabled us to meet the tight NASA hardware deadlines. Jim Gibson, R. Hamley, R. L. Neubert, Ross Jones, and all the craftsmen who worked on these samples contributed to the success of this Skylab experiment.

Ground tests for M-558 consisted of melting the zinc—zinc-65 specimens under both constant temperature and temperature gradient conditions for times up to 16 hours. These melting tests were conducted both here and at MSFC. After melting, the samples were removed from the tantalum capsules. Tom Rice then cut each sample into 50 or more transverse sections, using a jeweler's lathe set up in Building 3038. Tom became an expert in machining the supersoft high-purity zinc into thin shavings and carefully sealing the shavings into individual plastic envelopes. These zinc sections or shavings were then transferred to Jim Eldridge and Joe Northcutt of the Analytical Chemistry Division for precise gamma counting. Jim and Joe, already established experts in radioactive counting of moon rock specimens, used similar equipment in accurately measuring the gamma radiation from each zinc section.

The results of the ground tests were more than anyone bargained for and almost caused the entire experiment to be scrubbed. But to explain what happened, I need first to say a word or two about the design of this experiment. The transport of zinc atoms in liquid zinc on earth is due primarily to two separate mechanisms: diffusion and gravity-induced thermal convection. Therefore, in space, one could expect to measure only the effects of diffusion and thereby separate the two mechanisms. However, few experimental data have even been generated in earth tests measuring the magnitude of convective transport for zinc, and the principal investigators fixed the length of the zinc cylinder based on limited diffusion data. Unfortunately, with the minimum soak time of 1 hour allowed for the Skylab furnace to operate, all samples melted on earth became completely homogeneous with respect to zinc-65 content. In other words, using the times allowed, it was impossible to measure the transport of zinc in the 1.842-inch sample. Zinc-65 concentration gradients were finally achieved in rapid melting tests of 15 minutes duration, but the chances of M-558 flying on Skylab-IV were seriously jeopardized. However, further analysis of ground-test data indicated that the tremendous increase in zinc transport was probably caused by thermal convection, and so we had reason to hope that zinc-65 gradients would be produced in space with a 1-hour soak time.

Much to everyone's suprise, M-558 flew on Skylab-III instead of the scheduled Skylab-IV flight because the astronauts apparently had extra time available on that third, 59-day flight. I drove to Huntsville and picked up the three Skylab M-558 samples on October 9, 1973, and on the following day Tom Rice cut the capsules open and removed the zinc cylinders. He immediately noticed the shiny appearance of the melted cylinders, which indicated that the tantalum capsules had done their job. I was intrigued by the usual wrinkled appearance of the cylinders, which still lacks interpretation. Sectioning and counting of the zinc samples revealed textbook-like zinc-65 diffusion gradients for each sample. This Skylab experiment was coming out with a scent of the proverbial rose. As a result of it, there is now no doubt as to the effect of gravity on the movement of zinc in the melt, and this effect, as well as accurate diffusion constants, can now be calculated. The results also demonstrate the power of the radioactive tracer technique, which appears to be well suited for use in Skylab experiments. The cooperative effort among the different ORNL divisions for the NASA Skylab program was one of which we can all be proud.

Comment

When opponents of nuclear power argue that it should be stopped dead in its tracks unless, as Nader testified before the Joint Committee on Atomic Energy Jan. 29, reactors "are safe beyond any question of doubt and superior to other energy alternatives," they jeopardize the whole environmental movement, because their short-term alternatives — usually solar power and geothermal energy — are not real options for an energy-hungry society. And when Nader suggests that coal replace uranium, he seems to have no concept that the amount of coal required for such replacement, at least two billion tons annually by the end of the century, involves immense environmental assault as well as exorbitant social costs for the nation.

Given the alternatives, given their availability and their risks, I see nuclear power not only as an acceptable risk for the United States, but also as the only practicable energy source in sight adequate to sustain our way of life and to promote our economy.

— Ralph Lapp, "Nuclear Power," New York *Times* Magazine, February 10, 1974. Reprinted by permission.

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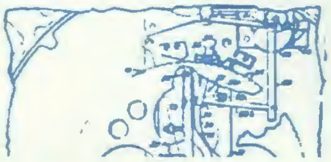
counterbalanced by said automatic mechanism, a group of numerals each representing a value of a load counterbalanced by said manipulative mechanism, means for printing that indicium in said series of consecutive indicia corresponding to that portion of a load counterbalanced by said



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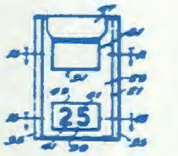
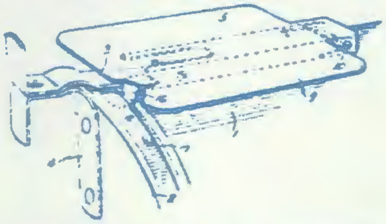
RADIATION EXPOSURE METER
Ernest O. Wollan and Louis A. Pardue, Oak Ridge, Tenn., and Norman Goldstein, Chicago, Ill., assignors to the United States of America as represented by the United States Atomic Energy Commission
Application March 3, 1945, Serial No. 580,785
20 Claims. (Cl. 250—83)



1. A radiation meter of the class described comprising a film holder, a film which is sensitive to X-ray and gamma radiation contained in said holder, and filter means arranged to filter the radiations reaching an area of said film, the absorption characteristics of said filter means and the energy response characteristics of said film being so correlated that the response of said film for equal exposures in roentgen units is substantially independent of the quantum energy of the radiation in the energy range which it is desired to meter.

2,483,992
SIGNAL ATTACHMENT FOR RURAL MAILBOXES
Raymond C. Young, Manitowoc, Wis.
Application June 24, 1948, Serial No. 35,002
2 Claims. (Cl. 232—35)



1. A mail box signal device arranged to cooperate with a mail box provided with a hinged front door and a strap member on its upper side, said signal device comprising a unitary structure including a supporting member arranged to be positioned on the upper side of said mail box and having permanently locked gripping means for gripping opposite marginal edges of said strap member, a signalling flag pivoted to said supporting member and biased for upward rocking

LAB ANECDOTE

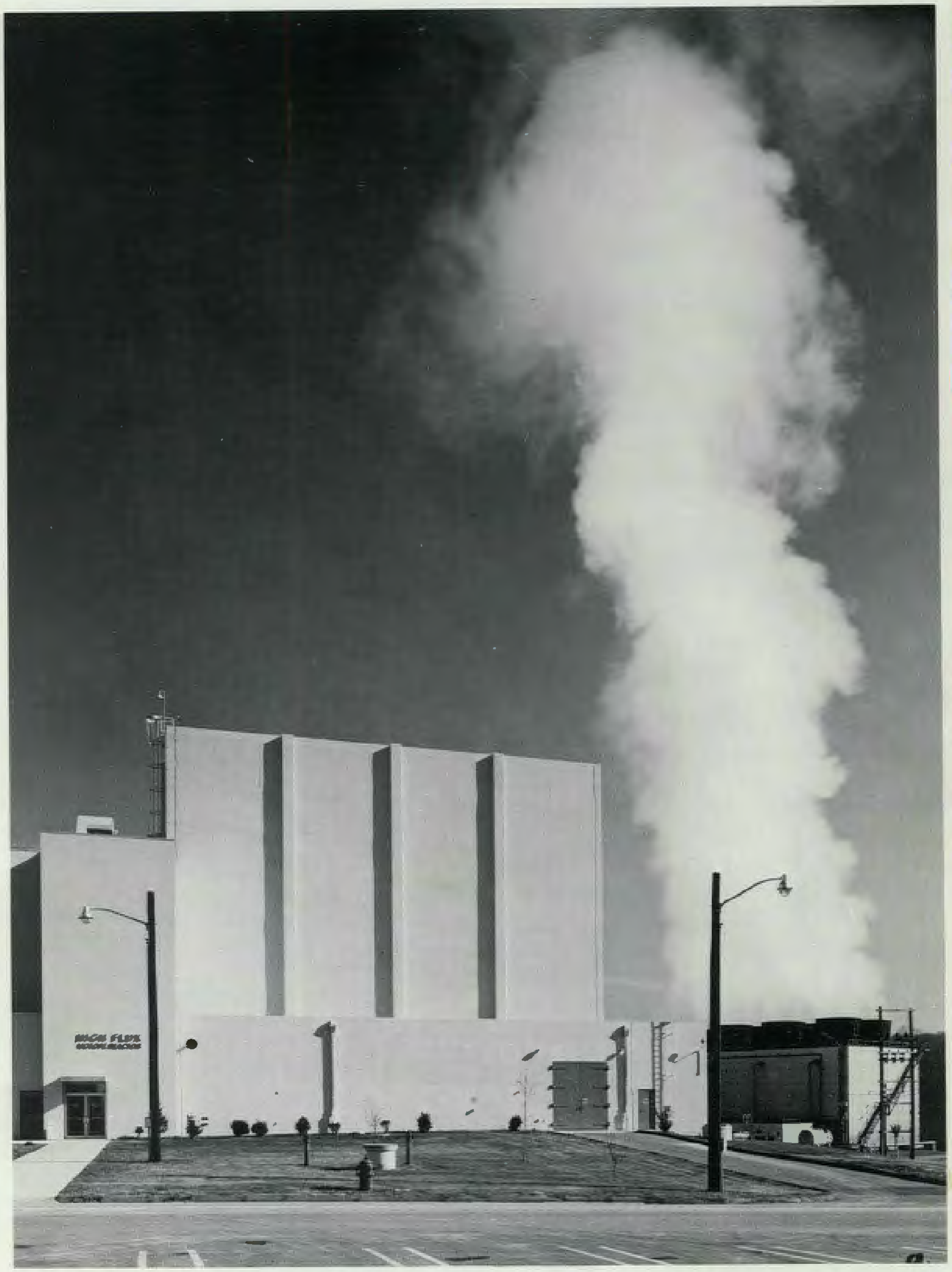
FOR EVERYONE A TIN BADGE

By HERBERT POMERANCE

Hidden behind our portraits on our security badges is a cadmium metal plate over a dental x-ray film. Antoine Becquerel received a Nobel Prize for his discovery in 1896 that the natural radiation from uranium would put the image of a brass key on photographic film. Ernest Wollan, Louis Pardue, and Norman Goldstein received U.S. Patent 2,483,991 half a century later for getting the image of a thin plate of cadmium. Becquerel's discovery was accidental; Wollan and Pardue worked with a purpose.

When Arthur Compton formed the Metallurgical Laboratory (code name for the wartime Plutonium Project) in January 1942, he brought in a former student, Ernie Wollan, for health physics matters. One task was to develop simple radiation meters; the Lauritsen electroscope was cumbersome to use with its microscope viewer and long electric cord and a stopwatch in hand. By systematic tests with dental x-ray film, metal plates, and radium and x-ray sources, Wollan and Pardue showed that a plate of cadmium 1 millimeter thick would give a uniform response for gamma rays from 50 to 1000 kilo-electron volts. Cadmium works because its atomic number, 48, is one unit more than the 47 for silver found in the photographic emulsion. The cadmium was cleated to a frame made of tin-plated steel that held the dental film; the whole thing is referred to in health physics documents as the "tin badge" but in the patent as a "Radiation Exposure Meter."

Ernie Wollan came to ORNL, where he pioneered and worked in neutron diffraction until his retirement. Lou Pardue went to Virginia Polytechnic Institute, where he was vice-president and director of graduate studies. Other people made half a dozen basic changes in the tin badge, including adding chemical and glass dosimeters to make it serve more purposes. Greater than the technical changes was the marriage of the radiation meter to the security badge, which not only cut the number of badges a person wore from two down to one, but also ensured that he would remember to carry the radiation meter. Maybe some modern genius can now figure a way to combine the driver's license with the seat belt to ensure the driver's protection.



An immense vapor plume rises from the water cooling tower during operation of the High Flux Isotope Reactor at 100,000 kW.