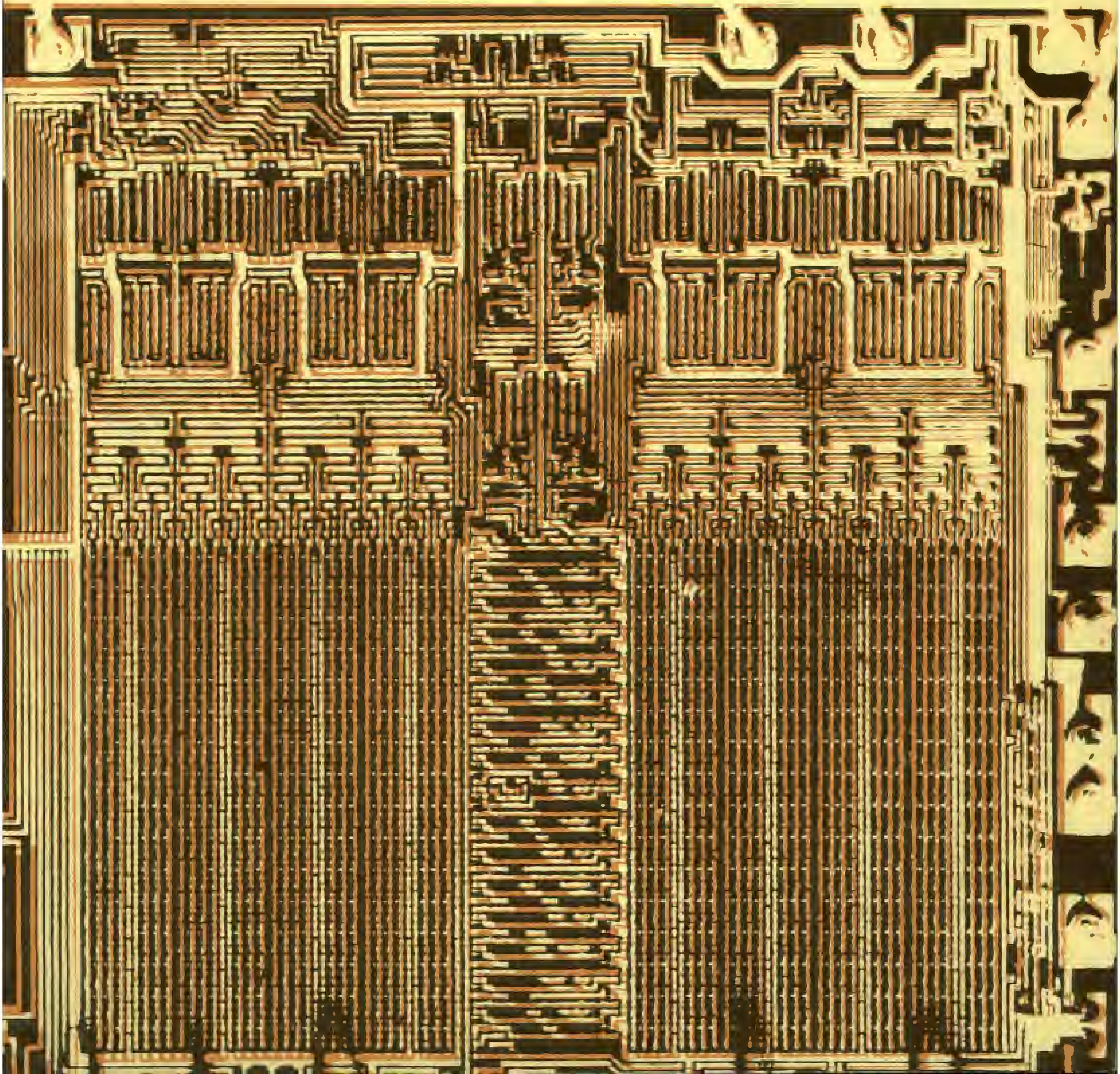
REVIEW

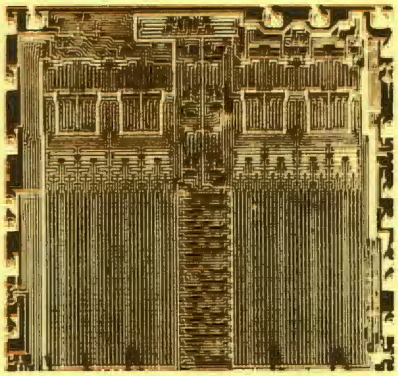
OAK RIDGE NATIONAL LABORATORY · SUMMER 75



REVIEW

Volume 8 Number 3 Summer 1975

FEATURES



THE COVER: Memories are made of this. You are looking at a blowup of a semiconductor chip, 3.2 mm square, called a PROM (programmable, read-only memory), that is capable of storing 2048 bits of computer information. It can be programmed to provide all the memory a microprocessor needs to do a small job. Twenty-four such chips can store as much information as a small PDP-8 minicomputer. For an account of the latest development on the computer scene and what it can do for your research, read the article by Adams and Roberts on page 12.

Editor
BARBARA LYON

Staff Writer
CAROLYN KRAUSE

Consulting Editor
ALEX ZUCKER

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OAK RIDGE NATIONAL LABORATORY

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EDITORIAL

The Paper Plague

Paper, like entropy, proliferates in a closed system; institutions have been the living proof of this second law. We all deplore it, but none more than members of research laboratories like our own, as we dimly perceive the day when the Laboratory will do no research at all, but will produce the most complex and confusing mass of proposals, plans, reviews, evaluations, budgets, inventories, audits, cost analyses, safety reports, accident reports, incident reports (major and minor), mini-fund requests, maxi-fund requests, and milestones (kilometer stones?).

Paper work has been increasing steadily according to natural law, but the last few years have witnessed a veritable explosion—all with seemingly good cause. For example, the transition from AEC to ERDA, from nuclear to comprehensive energy research, has been a time of confusion. Under these circumstances, the rule is to "saturate all channels"; and what manager will dare restrict his flow of proposal documents when the penalty may be loss of a program and the price is but a few pages of written material? Such compliance is understandable. Agencies have become more demanding in their requirements as they seek to justify their own proposals before internal and external review boards and committees. This, too, is understandable. New management systems, increased auditing practices, a plethora of Federal regulations dealing with health, safety, radiation, environmental quality, nuclear safeguards, etc., account for additional increases in paper work.

As one looks at these mountains of paper, two general classes can be distinguished. One is working paper, that is, a class that includes technical publications, progress reports, fund requests, proposals, long-range plans, and program reviews. The working paper is generally high on technical content, it is tailored to address a particular audience, and it dwells heavily on accomplishment and expectation. The second class we may call documentary paper; it includes budget information, requirements of management systems, responses to the myriad requests of internal and external bureaucracies to assure them that not only is the job performed responsibly but there exists documented proof that this is, in fact, so. Documentary paper is heavy on control, management, legal requirements, etc.

Is there no help? Are we destined to drown in paper? Is the Xerox machine the true symbol of the age?

We recall that at least two people are involved with every piece of paper: the writer and the reader. To slow the paper plague, one might think of a writer's Bill of Rights:

No one shall be asked to prepare a working paper without a clear understanding of (a) who will read it, (b) what action will be taken as a result of it, (c) what would be the consequences of not writing it, and (d) what is the minimum information required? Documentary paper can be subjected to further constraints in addition to the four listed above: (e) how long will the document be used, (f) are there existing documents that will serve the purpose?

If writers insist on substantive answers to these questions, paper work might diminish, writers could return to work on things that are really worth reporting, and all of us, writers and readers, might get to know our families better.—A. Z.

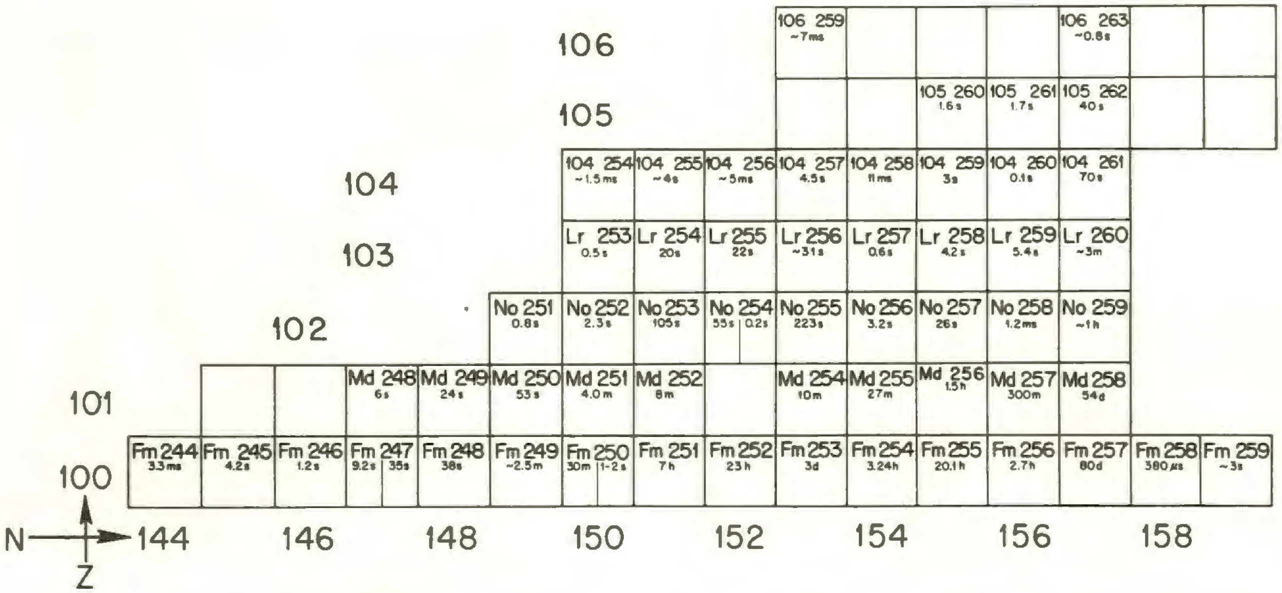
For a couple of nanoseconds there, they knew they had it: a firm grasp on a new element that was at the time hovering in a cloud of international dispute. Then, when the expected readings did not appear, a group of West Coast colleagues, using the same methods, carried off the laurels, and the chance for lasting fame was temporarily lost to the alchemists of the Laboratory for Transfermium Element Chemistry and Physics in Building 6000. The story is one of elegant science, tenderly executed, told by a member of the group. Curt Bemis received his doctorate from MIT in Nuclear Chemistry and Physics, and after spending a year at Sweden's Nobel Institute on Fulbright and Sweden-America Foundation fellowships, joined ORNL in 1965 on a postdoctoral appointment with Dave O'Kelley in the Chemistry Division. To guide the nonphysicists through Curt's forest of superscripts, a section of the upper reaches of the Table of Elements is shown here. Vertically, the *Z* or atomic numbers march; horizontally, (*N*) the number of neutrons in each nucleus; add the two and you have the isotope number, which represents the atomic weight. This is the superscript; the subscript is the atomic or identifying number (except where the element has not been named, in which case the number is in the name's place). The lower line in the squares denotes the isotope's half-life; two such numbers appear when the isotope has been observed to have two isomers: the ground state (*l.*) and the excited state. So, just add up the superscripts to learn what the alchemists have been doing.

Alchemy Updated

or, the Genesis of an Element

By CURT E. BEMIS, JR.

THE QUEST FOR THE "philosopher's stone," an imaginary device believed to have the power of transmuting base metals into gold, occupied a substantial fraction of the productive research career of many a medieval alchemist. Searches for the "elixir of life," the panacea for all diseases that plague mankind, were also important activities of our ancient brethren. The modern day



practice of the ancient art of alchemy — a mixture of science, mysticism, and faith in unknown proportions — is still concerned with both activities. However, progress in the transmutation of the elements has been considerably more rapid than that in the search for the elixir of life. Thanks to Lord Ernest Rutherford, modern alchemists are now equipped with the philosopher's stone — the ability to transmute the elements artificially by means of nuclear reactions. Lord Rutherford, an eminently successful alchemist, found that nitrogen could be transmuted into oxygen when bombarded with alpha particles (i.e., helium nuclei containing two protons and two neutrons bound together).

Progress in extending the chemical periodic table beyond uranium (U), element 92, to the present limit of 106, has been most dramatic. Isotopes of these elements, with the exception of one isotope of plutonium, element 94, are not seen in nature now because their half-lives are much shorter than the age of the earth ($>10^9$ years). Thus, nuclear reactions are the essential tool in extending the number of known chemical elements. At present, some isotopes of all of the elements from uranium to fermium (Fm), element 100, can be produced by the long-term irradiation of suitable target materials with neutrons, usually in the core of a nuclear reactor.

Isotopes of the elements beyond the atomic number (*Z*) of 100, form a special case for the alchemists, as they can be produced only in charged-particle-induced reactions, mostly using an isotope of one of the man-made transuranium elements as target material. The charged-particle bombardment method was used in the discovery experiments that produced the first small quantities of plutonium (Pu, element 94), curium (Cm, element 96), berkelium (Bk, element 97), and californium (Cf, element 98). This technique later was surpassed by the more advantageous method of neutron irradiation.

A Brief History of Conflicts

Mendelevium (Md), element 101, the first of the elements that could not be produced in neutron irradiations, represented a substantial challenge to the alchemists. The discovery of mendelevium in 1955 marked the first occasion that a new element was produced and identified on a one-atom-at-a-time basis. In this case, mendelevium was produced in the reaction,

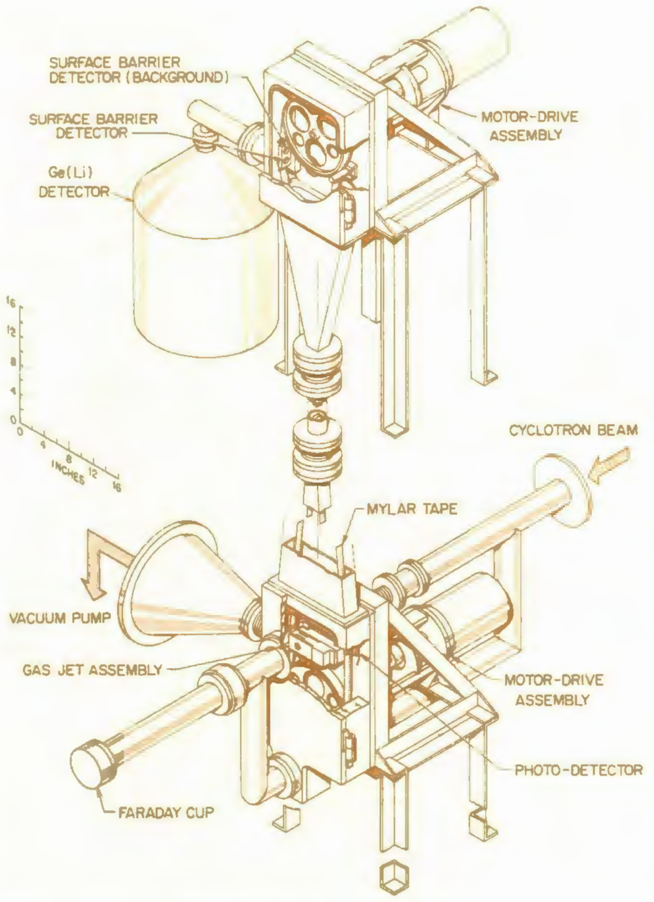
$$^{253}_{99}\mathrm{Es} + ^{4}_{2}\mathrm{He} \rightarrow ^{256}\mathrm{Md} + ^{1}_{0}n\ ,$$

using only about 10^9 atoms of einsteinium as a target. The ^{256}Md was identified chemically by Ghiorso, Harvey, Choppin, Thompson, and Seaborg at Berkeley, where the discovery was based on the decay of only 17 chemically separated atoms of ^{256}Md! Identification of a new element based on the observation of only a few atoms is certainly nontrivial and, as you might suspect, is the basis for all of the controversies that now surround the discovery claims for the remainder of the very heaviest elements. Element 102, nobelium, is a classic example as it was the first of the very

Pete Dittner inspects the equipment in the transuranium element bombardment room of ORIC.

controversial heavy elements. Element 102 is also a milestone for the alchemist in that isotopes of this element can be produced using only heavy-ion projectiles, that is, charged particles with an atomic number greater than 2 (heavier than helium nuclei). Theoretically, one of the fermium isotopes, ^{257}Fm, can be used as a target to produce element 102 using $^{4}_{2}$He ions, but there is not enough of the fermium isotope to do this. The fine art of accelerating heavy ions fast enough to overcome the mutual electrostatic repulsion between the ion and the target nucleus (Coulomb barrier) to cause nuclear reactions to occur in heavy element targets had its inception at ORNL in the early 1950s. Rapid technological developments subsequently occurred in the United States at Berkeley and at Yale with the construction of two heavy-ion linear accelerators (HILAC), in Stockholm by accelerating heavy ions in the Nobel Institute for Physics' classical cyclotron, and in the Soviet Union at Moscow and Leningrad. An international scientific group at the Nobel Institute in Stockholm prematurely claimed the discovery of element 102 in 1957. This group bombarded a target of $^{244}_{96}$Cm with $^{13}_{6}$C ions and claimed to have chemically identified an isotope of 102, which they promptly named nobelium. The name was officially accepted by the International Union of Pure and Applied Chemistry (IUPAC). Researchers, both at Berkeley (A. Ghiorso et al.) and at the Kurchatov Institute in Moscow (G. N. Flerov et al.), were unable to confirm the Stockholm work, but on the basis of their own work, both research groups claimed the discovery of element 102 in publications in 1958. Later work by both groups about 1958 has resulted in the production, study, and characterization of 10 different isotopes of element 102, from $^{250}_{102}$No through $^{259}_{102}$No, but the mutual resolution of the discovery of this element by the research groups involved has never been realized.

Element 103, the second of the controversial heavy elements, was first reported in 1961 by Ghiorso et al., who bombarded a mixed isotopic target of californium with $^{10}_{5}$B and $^{11}_{5}$B ions, using



Schematic view of the tape transport system used in the ^{263}106 synthesis experiments. Recoil reaction products emerging from the target, located in the gas jet assembly, are thermalized in helium gas and pumped out through a small orifice where they are collected on mylar tape. Periodically, the tape is advanced a distance of approximately 11 ft to position the active spot between the detectors in the counting assembly.

the HILAC at Berkeley. The Berkeley workers suggested the name lawrencium (Lr) for element 103, after E. O. Lawrence, and this name was subsequently adopted by the IUPAC. This work was challenged in 1965 by the group headed by G. N. Flerov, which had since moved from the Kurchatov Institute in Moscow to the Joint Institute for Nuclear Research at Dubna. The Soviet group reported the production of ^{255}Lr, ^{256}Lr, and ^{257}Lr in reactions of $^{243}_{95}$Am with $^{16}_{8}$O and $^{18}_{8}$O ions during the period 1965—1970, but none of these isotopes were reported to have the same decay characteristics as the discovery isotope for element 103 reported in 1961 by the Berkeley researchers. This discrepancy was resolved by Eskola, Eskola, Nurmia, and Ghiorso at Berkeley in 1971 in a comprehensive study of all the isotopes of 103 from $^{254}_{103}$Lr thru $^{260}_{103}$Lr. These isotopes were produced in a large variety of different target and heavy-ion projectile combinations, but, most importantly, they confirm the 1961 experiments for ^{258}Lr.

The Soviet group under Flerov first reported element 104 in 1964 when they produced an activity decaying by spontaneous fission with a half-life of approximately 0.3 sec. This activity, produced in the bombardment of $^{242}_{94}$Pu with $^{22}_{10}$Ne ions, was identified by the predictable variation of production yield with changes in the incident $^{22}_{10}$Ne energy (excitation function) and later by the chemical similarity to hafnium, its periodic table homolog. In 1966, the Flerov group suggested the name kurchatovium (Ku) for element 104 after I. V. Kurchatov, the noted Soviet nuclear physicist; however, no action was taken by IUPAC. The Ghiorso group at Berkeley was unable to confirm the Soviet work on ^{260}104, but in 1969 the group reported the production and identification of two new alpha-emitting isotopes of element 104, ^{257}104 and ^{259}104, in reactions of $^{249}_{98}$Cf with $^{12}_{6}$C and $^{13}_{6}$C ions. Identifications were based on excitation functions and on the establishment of a genetic alpha decay link to known isotopes of element 102, which are the daughters of the parent 104 isotopes. Evidence was also presented for the isotope ^{258}104, a spontaneous-fission activity and somewhat later for the isotope ^{261}104, a relatively long-lived alpha activity with approximately a 70-sec half-life produced in reactions of $^{248}_{96}$Cm with $^{18}_{8}$O ions. Elemental identifications for ^{261}104 were based on the establishment of a genetic link to $^{257}_{102}$No and on comparative aqueous chemical studies done at Berkeley by Bob Silva of ORNL. The alternative name rutherfordium (Rf) for 104 was suggested by the Ghiorso group because they were unable to produce the isotope ^{260}104 reported in 1964 by Flerov. No official name for element 104 has been adopted, and the controversy, much more complicated than can be indicated here, continues to this date.

The story of element 105 is repetitious of the history of the discovery for element 104. The Dubna group in 1968 reported the production of a short-lived alpha emitter and later (1970) a spontaneous-fission activity produced in the $^{22}_{10}$Ne reactions on $^{243}_{95}$Am. Comparative gas-phase chemical studies were reported and claimed to support

L. D. Hunt, left, and J. R. Tarrant adjust the oscilloscope used to monitor the course of an experiment.

the assignment to element 105. In 1970, Ghiorso et al. at Berkeley reported that they were unable to confirm the Soviet work but were able to produce the isotope $^{260}105$, a 1.6-sec alpha activity produced in the $^{249}_{98}\mathrm{Cf} + ^{15}_{7}\mathrm{N}$ reaction. Identification was based on the excitation function and by the establishment of a genetic link to $^{256}_{103}\mathrm{Lr}$, the alpha decay daughter isotope of element 103. Two additional isotopes of element 105, $^{261}105$, and $^{262}105$, both alpha active, were produced at Berkeley in 1970. Berkeley's suggested name for element 105 is hahnium; the Dubna group suggested the name nielsbohrium. The controversy continues, and no official name has been adopted by IUPAC.

This extremely brief history of the developments surrounding the discoveries of elements 102 through 105 indicates that new element work is not trivial; it is never straightforward, and more often than not, it is very controversial. The production rates for isotopes of these elements are measured in terms of a few atoms per hour; they require the most intense ion beams available and the maximum amount of target material dictated by details of the experiment. Moreover, the elemental identification methods must be totally unambiguous. A direct measure of the atomic number is nearly impossible, and indirect methods must be used. Comparative chemical studies, establishment of a direct genetic link to an isotope of a known element, details of the nuclear reaction mechanism such as the product yield as a function of incident ion bombarding energy (excitation function), and systematics of the nuclear decay properties, half-lives, and energies, etc., are all clues to an element's identification. When controversy arises, it is more often concerned with establishing the atomic number for an isotope of a proposed new element in question than with the actual experimental work. The Dubna group, for instance, relies on the observation of spontaneous-fission activity, which by itself, contains no atomic number information.

In contrast, the Berkeley work is usually concerned with alpha emitters and with the establishment of a genetic link to known isotopes of previously established elements. These two groups do similar but not identical experiments, because the Dubna group, in its work with elements 102 through 105, has used relatively light targets, $^{242}_{94}\mathrm{Pu}$, $^{243}_{95}\mathrm{Am}$, etc., in combination with heavier ions such as $^{22}_{10}\mathrm{Ne}$, and the Berkeley group has used heavier targets such as $^{249}_{98}\mathrm{Cf}$ and somewhat lighter ions such as $^{11}_{5}\mathrm{B}$, $^{12}_{6}\mathrm{C}$ and $^{14}_{7}\mathrm{N}$.

The ORNL Contribution

This was the situation in 1968–1969 regarding the heaviest elements. At ORNL, the Transuranium Element Research Laboratory had been in operation for about one full year and the HFIR-TRU complex in Melton Valley was steadily producing and separating substantial quantities of $^{248}_{96}\mathrm{Cm}$, $^{249}_{97}\mathrm{Bk}$, $^{249}_{98}\mathrm{Cf}$, $^{252}_{98}\mathrm{Cf}$, and $^{253}_{99}\mathrm{Es}$ for research purposes. Indeed, as part of the U.S. transuranium production program, some of this material was in use at Berkeley as targets in their heavy-ion—new-element discovery experiments. Merrit Mallory, then a postdoctoral appointee attached to the Oak Ridge Isochronous Cyclotron (ORIC), was transferred to the Chemistry Division to aid in the development of heavy ions. His assignment to the transuranium research effort, as a result of active lobbying on the part of the TRL staff, was a giant step in heavy-ion research at ORNL. Merrit joined Bob Silva, Pete Dittner, Lew Keller, Dick Hahn, and me, and in a short period of time, he

Dick Hahn changes detectors in the second-floor tape system while Dave Hensley makes a minor adjustment.

and Ed Hudson, of the former Electronuclear Division, had developed respectable beams of ${}^{12}_{6}\mathrm{C}^{+4}$, ${}^{14}_{7}\mathrm{N}^{+5}$, and ${}^{16}_{8}\mathrm{O}^{+5}$, and the multiply charged ion source development program was well under way. Energies available at ORIC for these ions were high enough to overcome the mutual electrostatic repulsion that occurs with even the heaviest targets; hence, our heavy ion program was starting to bloom. Improvements in the art of heavy-ion sorcery and acceleration in ORIC are still being made by Merrit and Ed, both now in the Physics Division, but the initial successes with heavy ions at ORIC were directly due to their efforts.

Bob Silva, truly a chemist's alchemist, and Lew Keller, then director of the TRL and now director of the Chemistry Division, were interested in chemical properties of the very heavies, and Dick Hahn, alchemist who now directs TRL, together with Ken Toth, alchemist at the cyclotron, were interested in nuclear reaction mechanisms and new alpha emitters. Pete Dittner, molecular and atomic physicist and blossoming alchemist, and I had just started to investigate the interesting characteristic *K*-series x-ray spectra for ${}_{96}\mathrm{Cm}$, ${}_{97}\mathrm{Bk}$, and ${}_{98}\mathrm{Cf}$, research quantities of which had recently become available as a result of the HFIR-TRU program. With Jim Tarrant and Lee Hunt, our electronics experts at the TRL, we made a formidable research team ready to explore the heavy-ion frontiers at ORIC. Our initial experiments, deliberately chosen to initiate us to the field, resulted in the production and study of two new isotopes of californium, ${}^{240}_{98}\mathrm{Cf}$ and ${}^{241}_{98}\mathrm{Cf}$. These new isotopes were produced in ${}^{12}_{6}\mathrm{C}$ ion reactions on ${}^{233}_{92}\mathrm{U}$, ${}^{234}_{92}\mathrm{U}$, and ${}^{235}_{92}\mathrm{U}$ targets. Bob used his chemist's touch and promptly provided chemical identifications for our two new isotopes that supported our physical identification by the excitation function method. This work, with Merrit as coauthor, was completed in 1969—1970 and sent to *Physical Review* in June 1970.

Riding on the crest of our initial success, we began to think of ways in which we could make a significant contribution to the study of the very heaviest elements through atomic number 105. Taking into consideration that a mere duplication of experiments done by either the Berkeley or Dubna groups would not aid in the resolution of any of the raging controversies, we looked for new techniques that would be far superior to anything tried to date. Bob actively pursued development of fast chemical methods applicable to very short-lived nuclides and in late 1970 worked at Berkeley in chemical studies with an isotope of element 104, ${}^{261}104$. Dick attacked the problem from the nuclear reaction viewpoint and studied the differences between compound nuclear reactions (complete fusion of target and projectile) and multinucleon transfer reactions (partial fusion). Pete and I, intrigued by our experimental *K* x-ray studies in the heavies, were spurred by physicist Tom Carlson, together with Bill Nestor, Barry Malik, and Tom Tucker. Tom et al. had recently predicted election binding energies and *K*-series x-ray energies for all heavy elements from curium ($Z = 96$) through element 126 as theoretical interest in the "superheavy elements" had started to mushroom. Pete and I experimentally checked Tom's calculations from $Z = 96$—99, and both sets of results agreed to within approximately 5 electron volts for the 100- to 140-keV transitions, truly a remarkable theoretical and experimental triumph. Why not use x rays as an unequivocal tool for the identification of these controversial heavy elements? After all, H. G. W. Moseley had done similar work in 1913—1914 in Manchester, England, when unravel-

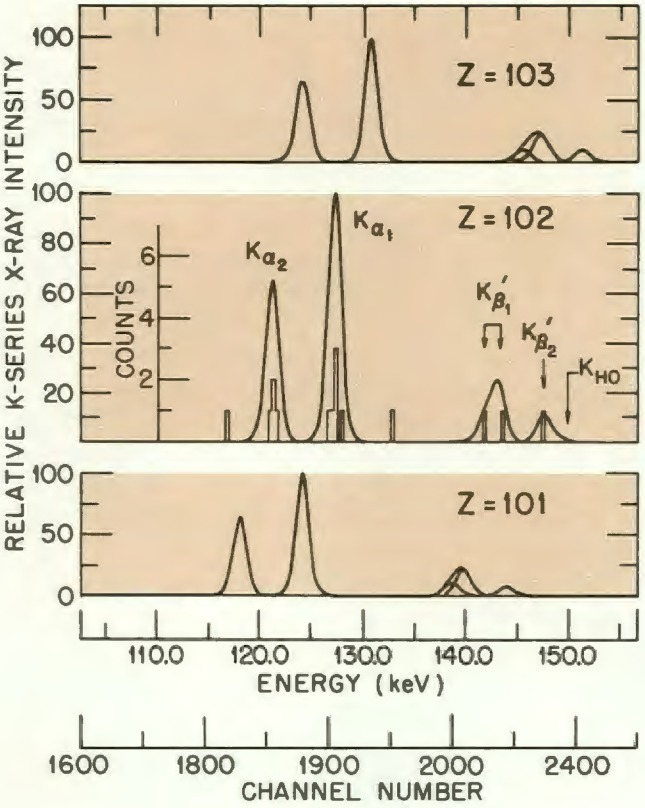


Illustration of the sensitivity and uniqueness of the alpha x-ray identification technique for the heaviest elements. The characteristic K-series x-ray spectra expected for elements 101 through 103 are shown together with the experimental spectrum (histogram) observed in coincidence with alpha particles from the decay of an isotope of element 104, ^{257}104. This observation of element 102 x rays formed the basis of a conclusive identification for element 104.

ing the mysteries of the periodic table, especially the ordering of the chemically similar rare earth elements.

A new twist was needed, however, because we could never hope to excite these nuclides that could be produced only at a rate of a few atoms per hour to fluorescence. We worked out a coincidence method whereby we would detect the alpha particle from the decay of a suspect parent nuclide with atomic number Z; if the daughter nuclide, with $Z-2$, were left in an excited state, x rays could be emitted as part of the nuclear and atomic de-excitation processes. When the daughter nucleus is left in an excited nuclear state, the nuclear de-excitation can occur via the internal conversion process. In this case, a bound atomic electron is ejected from the atom, creating an inner shell electronic vacancy. If this vacancy is in the K-shell, the innermost electron shell, K-series x rays can be emitted when this vacancy is filled by electrons from higher-lying electron shells. The coincident detection of these characteristic x rays (in this case characteristic for the daughter element with $Z-2$) would provide such an unequivocal identification for the atomic number Z of the parent nuclide.

Pete, enthusiastic at this point, convinced me that we should try it, and, together with Bob Silva, we enlisted the aid of Dave Hensley and Charlie Goodman at the ORIC. Meanwhile, Dave and Charlie were just finishing the implementation of a new computer-based, data-acquisition system. This was fortunate for us because, since our proposed experiment was quite complex, we had to use all the data acquisition power available to record the information simultaneously from our several correlated data channels.

We chose the nuclide $^{255}_{102}$No, an approximately 3-min alpha activity that could be produced in the reaction $^{249}_{98}\mathrm{Cf} + {}^{12}_{6}\mathrm{C} \rightarrow {}^{255}_{102}\mathrm{No} + {}^{4}_{2}\mathrm{He} + 2^{1}_{0}n$ at the phenomenal rate of approximately 100 to 200 atoms per bombardment using approximately 72 MeV $^{12}_{6}$C ions. We bombarded for 6 min and pneumatically transferred the reaction products outside of the bombardment room. Then either Pete or I would rapidly insert the foil into our coincident detection apparatus. We had a few false experimental starts, but after completing approximately 180 of these exhausting bombardment cycles, we had a grand total of about 200 K-series x rays detected in coincidence with those alpha particles due to the decay of $^{255}_{102}$No. The various line energies agreed to within ± 30 eV for those predicted by Tom Carlson et al. for the K-series x rays of element 100, fermium. We had made only about 30,000 atoms of element 102, but our work provided the first unequivocal, and hence uncontested, identification of this element. Pete, Dave Bob, Charlie, and I published this work in *Physical Review Letters* in 1971.

After our x-ray identification of element 102, nobelium, we upgraded our apparatus to a greater efficiency and tried similar experiments with various isotopes of element 103, but we saw only L-series x rays and no Ks. We felt that L x rays observed in coincidence were not quite as definitive as the K series x rays, because the photon spectrum is much more complex.

Pete and Dick each left for a year in 1972—1973 on foreign assignment, Pete to Germany and Dick to France. We moved to a new bombardment room at ORIC, improved our apparatus again, and tackled element 104 with the Berkeley discovery isotope, 4.5-sec ${}^{257}104$, which had come under severe attack by the Dubna group. We made only 3000 atoms of ${}^{257}104$ in the ${}^{249}_{98}\mathrm{Cf} + {}^{12}_{6}\mathrm{C} \rightarrow {}^{257}104 + 4{}^{1}_{0}n$ reaction in approximately 30,000 10-sec counting and bombardment cycles. We detected approximately 1000 alpha particles from ${}^{257}104$ and observed 13 coincident K-series x rays of element 102 (16% x-ray yield per observed alpha). Even with only 13 x rays, we demonstrated the uniqueness of the method and provided an unequivocal determination of the parent atomic number, $Z = 104$, thus confirming the prior Berkeley work. We published in *Physical Review Letters* in 1973.

At this time, September 1973, element 106 had not been discovered and the Berkeley HILAC was shut down and being upgraded to the Super HILAC. There were rumors then that the Dubna group was rebuilding their cyclotron to discover the "superheavy elements," those elements with an atomic number of about 110 or greater. What a beautiful time to discover a new element (element 106), provide an x-ray identification that would be universally accepted without controversy, and, of course, suggest a name for the new element! Suggesting names has always been the really fun part of the element discovery business. With the two giants of the new-element game supposedly out of commission, we began planning an experimental effort.

Actually, as early as 1970—1971, Bob had spearheaded an effort to predict alpha energies, half-life, production cross section, etc., for the isotope ${}^{263}106$ which could be produced in the ${}^{249}_{98}\mathrm{Cf} + {}^{18}_{6}\mathrm{O}$ reaction. We expected a half-life in the range of 0.1 to 2 sec, predominantly alpha decay with an energy of 9.25 ± 0.25 MeV for the most abundant alpha group, and under most favorable conditions, we could hope for the production of only about 40 atoms per day! We tried a preliminary experiment in 1970 with a crude experimental system designed only to look at alpha particles, but background problems proved too severe. Fully expecting the half-life for ${}^{263}106$ to be as short as approximately 0.1 sec, we again had to devise a new transport system to do the proposed experiment. Ed Chandler of the Plant and Equipment Division and I devised a system using an endless tape that could be rapidly accelerated, moved a fixed distance, and then stopped. Product atoms could be collected on the tape for a predetermined period, usually about two half-lives for the desired activity, and rapidly moved to position the active spot between the detectors of our alpha x-ray coincidence detection apparatus. Ed masterfully designed the system, and after all improvements had been made, we found that we could transport the active spot a distance of about 11 ft to our counting station in about 150 msec! The final apparatus was installed at ORIC during the spring of 1974. Jim Oliver of the Chemistry Division, in the meantime, had been electrolytically converting hundreds of grams of highly enriched water, $\mathrm{H_2{}^{18}O}$, to ${}^{18}\mathrm{O_2}$ gas just to provide enough feed gas for the ORIC ion source for our 106 effort. Bob went on temporary assignment in 1973—1974 to Germany and was not around for our experimental attempts, but Pete Dittner, Dave Hensley, Dick Hahn, Lew Keller, Jim Tarrant, Lee Hunt, and I felt we could man a 2-week, 24-hr/day experiment on the ORIC. We had purchased the world's largest high-resolution planar germanium semiconductor detector from ORTEC just to enhance the efficiency for x-ray detection. Under our experimental conditions, the coincidence detection efficiency was such that approximately 100 atoms of ${}^{263}106$ had to be produced to get 10 coincident alpha x-ray events. Of course, this assumes that each 106 alpha particle would be accompanied by a coincidence x ray, and we felt that a bare minimum of 10 such events would be required to firmly establish the discovery.

In the interim, ORNL cosponsored an international conference dealing with heavy-ion physics held in Nashville in June 1974. Our hopes for the first claim to the discovery of element 106 were dashed when Academician G. N. Flerov of the Dubna group reported new isotopes of element 104 and the synthesis of an isotope of element 106. They used very heavy ions, ions of chromium, ${}^{54}_{24}\mathrm{Cr}$, to bombard lead targets, ${}^{206}_{82}\mathrm{Pb}$, ${}^{207}_{82}\mathrm{Pb}$, and ${}^{208}_{82}\mathrm{Pb}$, in a novel experiment to produce ${}^{259}106$, a spontaneous-fission activity with a half-life of approximately 7 msec. I felt that this Dubna experiment was not entirely unambiguous, because the mere detection of fission activity, although perhaps more sensitive, does not convey any atomic number information. The Dubna group did not and has not proposed any name for 106 to avoid any possible hassle. All this time, we had presumed that the Dubna group was looking for the superheavies or, at least, rebuilding its cyclotron!

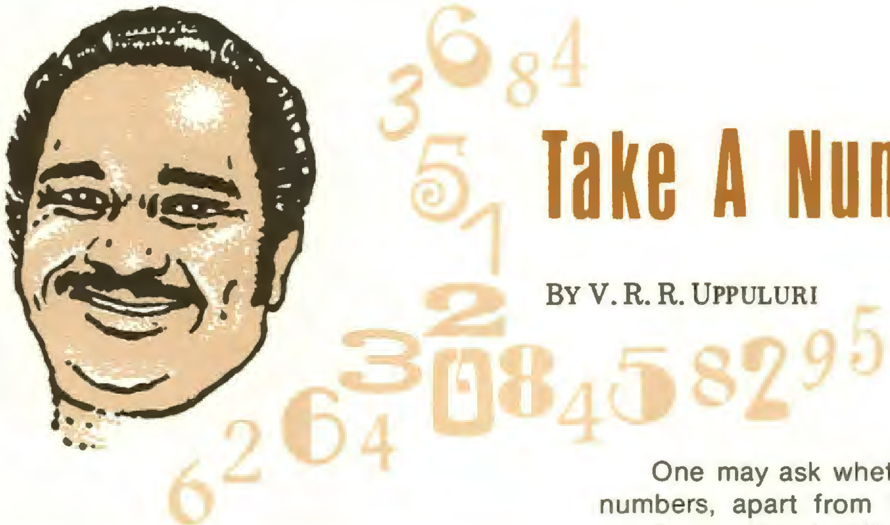
Undaunted, we returned from the Nashville conference with hopes of providing an x-ray identification for element 106 that we believed would be much more definitive and readily accepted by the international scientific community in contrast to the Dubna experiment. The whole tape system, including detectors and electronics, was tested in early July using $^{12}_{6}C$ ions from ORIC and a target of lead, which results in the production of various known short-lived isotopes of radium ($Z = 88$), francium ($Z = 87$), etc., with tremendous yield. As unforeseen minor problems were corrected, we were completely prepared to look for element 106.

Our 106 experiment started on Monday, July 22, 1974. We were bombarding a target of $^{249}_{98}Cf$ with an O_{18} ion beam. We had previously determined that approximately 4 μA of $^{18}_{8}O$ ion beam current, measured as the fully stripped ion, was the maximum amount of beam we could pass through our target assembly safely without melting it, and we continually needled the cyclotron operators to run as close to this maximum as they could. The experiment ran 24 hr/day from July 22 through August 3 with a brief shutdown for the weekend. The experimental team rotated shifts to avoid complete exhaustion with the exception of Dave, who was on call any time of the day or night to kick the computer in the proper place to make it function. Merrit, although not with our group anymore, continually suggested and made improvements in the new ORIC heavy ion source to enable us to get maximum beam current on our target. At the end of the experiment, we had completed well over a quarter of a million 2-sec counting and bombardment cycles, had consumed about 80 liters of $^{18}O_2$ gas, and completely filled up a half dozen or so magnetic tapes with data. What were the results of our search for ^{263}106? We saw two new alpha peaks, a total of about 30 events in the alpha spectrum that could not be attributed to any activity we had ever made before or even heard about. These alpha groups, at 9.06 MeV (90%) and 9.25 MeV (10%) decayed with a half-life of 0.74 ± 0.23 sec and fit our 1970 predictions for ^{263}106. So far so good; but after carefully searching our many data tapes, we could not find any x rays, neither *K*-series nor even *L*-series, that were in coincidence with these alpha events. We were nearly certain that we had made ^{263}106, but just convincing ourselves is not the same as satisfying the scientific community. We had made it, but we could not prove it because we had seen no coincidence x rays.

We had heard rumors of a similar experiment being done at Berkeley at nearly the same time as our experiment. Indeed, contacts with various members of the Berkeley experimental team confirmed our suspicions, and in September of 1974, the story broke. Element 106 had been produced at Berkeley by bombarding $^{249}_{98}Cf$ with $^{18}_{8}O$ ions, and they had made a 0.9 ± 0.2-sec alpha activity with 9.06-MeV and 9.25-MeV alpha particle groups. The Berkeley group had used their favorite identification technique, that of establishing a genetic link, this time to the ^{259}104 daughter isotope and further to the $^{255}_{102}No$ granddaughter isotope. The Berkeley team, consisting of A. Ghiorso, J. M. Nitschke, J. R. Alonso, C. T. Alonso, M. Nurmia, G. T. Seaborg, E. K. Hulet, and R. W. Lougheed, had beaten us to the discovery. Their work was published in the December 16 issue of *Physical Review Letters*. Berkeley has postponed suggestions of a name for element 106 until the discovery situation with respect to the Soviet group has been clarified.

Epilogue

Our experimental group bounded back after having missed element 106 by only a few days and a few x rays. Bob came back from Germany and we stepped back one element to 105 and provided an x-ray identification for ^{260}105, the Berkeley discovery isotope. We have attempted to provide clues to the identity of ^{260}104, the Soviet spontaneous-fission discovery isotope, with a 0.1-sec half-life, but as yet the experiments are inconclusive. The entire problem of spontaneous-fission half-life systematics for the heaviest elements has been the major subject of our cyclotron efforts in the past few months. New isotope and/or element discovery experiments, while constituting only a small fraction of our group's varied research activities, are perhaps the most glamorous and exciting of our efforts. The new heavy-ion accelerator facility, due to be completed in the late 1970s, will greatly add to our capabilities as well as open new frontiers in other exciting areas of heavy-ion physics. Now let me see...we could make element 107 on the present ORIC if we used $^{19}_{9}F$ ions and....



Take A Number.....

BY V. R. R. UPPULURI

A SEQUENCE OF ULAM

Special sequences of numbers have drawn the attention of mathematicians for centuries. A classic example is the Fibonacci sequence, which was studied in detail. The first few numbers of the Fibonacci sequence are 1, 2, 3, 5, 8, 13, 21, 34, 55, 89,.... The most striking appearance of Fibonacci numbers in plants is in the spiral arrangement of seeds on the faces of certain varieties of sunflowers. An unsolved question about Fibonacci numbers is whether there are an infinity of Fibonacci primes. It is interesting to note that there is a journal called *Fibonacci Quarterly*, published since 1963 by the Fibonacci Association.

Stanislaw M. Ulam, who is associated with Los Alamos Scientific Laboratory, introduced sequences of positive integers constructed in the following way: "Let u_1 and u_2 be given integers; we construct an increasing sequence of integers by adjoining those which can be represented in just one way as the sum of two distinct preceding members of the sequence." Take $u_1 = 1$ and $u_2 = 2$ and consider the *u*-sequence formed by the above rule. The first few numbers of the *u*-sequence are 1, 2, 3, 4, 6, 8, 11, 13, 16, 18, 26, 28, 36, 38, 47, 48, 53, 57, 62, 69, 72, 77, 82, 87, 97, 99,.... There are many interesting questions one can ask about these *u*-numbers.

One may ask whether the sum of two *u*-numbers, apart from 1 + 2 = 3 can be *u*-numbers. Frank W. Owens (Ball State University, Indiana) has obtained $u_{19} = 62$, $u_{20} = 69$ and $u_{31} = 131$ ($= u_{19} + u_{20}$) and has observed no other occurrences of this phenomenon with the sum $\leqslant 99{,}933 = u_{7584}$. Not much seems to be known beyond that.

MATHEMATICAL GEMS

There are not too many stories about mathematical problems that seem to spot a genius. One such is attributed to Karl Friedrich Gauss (1777–1855), who at the age of seven was able to add the first hundred natural numbers instantly. He divided the numbers (0, 1, 2,...100) into the fifty pairs 0 + 100, 1 + 99,...49 + 51, and observed the answer to be 50 × 100 + 50 = 5050.

Another such problem is attributed to a Hungarian mathematician, Louis Pósa (born in 1948), who at the age of twelve was able to solve the following problem instantly: Given $n + 1$ positive integers less than or equal to $2n$, show that some pair of them are relatively prime. Pósa observed correctly that if we have $n + 1$ positive integers less than or equal to $2n$, some two of them have to be consecutive and hence relatively prime. According to his personal account, a simple proof of this result has taken Paul Erdös, the great number theorist, about ten minutes.

When Mike Roberts (r.) joined the Instrumentation and Controls Division last year with a fresh Electrical Engineering Ph.D. from the University of Tennessee, he and Ray Adams formed a strong team. One of the products of this happy partnership is the availability to the Laboratory's research staff of the ingenious, programmed-to-order microcomputers described in their article. Ray has been with the Laboratory since 1954 and holds a M.S. in Engineering Science from UT. He has a long experience in process control and computer applications to research here, and he was the originator and leader of the Digital Systems Development Group in the I & C Division. At present, he is leader of the Development and Metrology R & D Group in the Division.

Put a Brain in Your Rig

Microcomputers for ORNL

By RAY ADAMS and MICHAEL J. ROBERTS

A NEW GENERATION of electronic intelligence in the form of small but powerful digital microcomputers is available for use now at the Laboratory. Such a device is capable of operating an experiment, gathering data, computing results, and making logical decisions, and it can fit into a space no larger than a cigar box. The brain of a microcomputer is the microprocessor, a single, large-scale, integrated circuit on a chip smaller than a matchbox. This is incorporated with an assortment of auxiliary equipment that gives the entire array the capabilities customarily associated with a PDP-8. In some ways, it can outperform the PDP-8.

The development of the microcomputer represents the confluence of two trends in two different areas in engineering in the last few years: the trend (which began in the 1960s) toward smaller and less expensive computers using transistors and the more recent trend toward larger integrated circuits with great circuit complexity on a single wafer of silicon. The

Mike Roberts lectures on microcomputers during a two-day course given to Laboratory personnel in May. Ray Adams, sitting to the left of Roberts, also served as an instructor.

significance of its development can be more fully grasped in the light of the history of the digital computer at the Laboratory.

History

The first digital computer at ORNL was the ORACLE, a vacuum tube device that was engineered by the I & C Division and that began operation in 1953. For about two weeks, ORACLE was the fastest digital computer in the world. Many old-timers will recall that the ORACLE was located in Building 4500N where the present PDP-10 stands, a computer thousands of times as powerful as the ORACLE.

In the late 1950s and throughout the 1960s, when transistors replaced vacuum tubes in digital computer designs, the Laboratory procured a succession of larger and more powerful transistorized computers for general-purpose scientific and engineering computations. Along about 1965, the minicomputer came on the scene. With the minicomputer, which started out about the size of a two-drawer file cabinet, the ORNL researcher, for the first time, could have a significant amount of computing capacity with which to control and monitor his experiments right in his laboratory. The centralized computers served well for solving large mathematical and scientific problems, but they were not adaptable for controlling or gathering data for the experimenter's needs.

Bill Busing and Henri Levy of the Chemistry Division used the first minicomputer at the Laboratory. Their system was engineered by Jim Woody and Ron Roseberry in I & C. Sharon King of the Mathematics Division worked with Bill and Henri to program it for use on the x-ray crystallographic diffraction system.

There are today more than 100 minicomputers at ORNL; without this capability, large-scale and even much small-scale research and development could not keep abreast of state-of-the-art experimentation.

In 1971, the microcomputer first appeared. Even though the first microcomputers were slow, they were quickly put to use. One commercial application was the development of the "intelligent cash register" used by some fast food chains. This device can store in its memory the price of each food item and calculate each order, computing the tax, as it comes in, saving the clerk both work and the risk of error.

What is a microcomputer? In a sense, it is a miniaturized version of the massive vacuum-tube computers of yesteryear. As the vacuum tube was replaced by the transistor, which consumes less power and takes up less space, the physical size of computers became progressively smaller for the computational power they embodied. As a corollary, however, as transistors began to be used, the computational power in the large-scale scientific computer also increased very quickly. Microcomputers evolved when methods for miniaturizing arrays of transistors were perfected.

A transistor is composed of solid state chemical elements called semiconductors. Most transistors are made out of a small amount of silicon, which has an ideally suited crystal structure. Silicon is grown in zone-refining furnaces in the shape of bars and sliced to make transistors. With the advent of microelectronics, hundreds of microscopic transistors, invisible to the naked eye, could be arranged on a single slice of silicon, making it unnecessary to saw a separate silicon slice for each transistor. Hence, *Samples of water are extracted from this weir so that health physicists can monitor pollution in White Oak Dam. The samples are taken at precise intervals with the help of a microprocessor.*

microcircuits resulted, as a couple of dozen or more transistors could be put on one chip of silicon, sometimes as small as a tenth of an inch square. These could then be packaged into what were called integrated circuits, or "chips" (because the active part of a circuit is on a small chip of silicon).

The microcomputer resulted from a kind of engineering challenge in the electronics industry: How much of a computer can you really put on one chip? In late 1971, the Intel Corporation announced that it had, in fact, put a significant portion of a computer on a chip of silicon. It was fairly slow and unable to process many bits of information at one time, but it was a major step, and it remained only for subsequent refinements to produce chips with more significant amounts of computing power.

Microcomputers vs Minicomputers

The electronics industry has yet to approach the limit to the number of transistors that can be put on one small chip. At the present time, integrated circuits less than a quarter of an inch square can incorporate more than 20,000 components. The overall increase in capability has reduced the cost per component on a chip in ten years by a factor of more than a hundred, from about 20 cents to a small fraction of a cent. Much of the success of the minicomputer industry is a result of these dramatic advances in microelectronics just in the last decade. The size and cost of systems based on minicomputers during the 1960s decreased at a rate of between 20 and 30% per year.

In the area of cost, the price tags of different microcomputer and minicomputer systems vary all over the map. You can buy the components for what is legitimately called a complete microcomputer—that is, a microprocessor with enough peripheral devices around it to actually do computation and communicate with other devices—for about $29 in large quantities. That is only the minimum end of the cost scale, however. On the other end, you can buy full-blown complete operating systems for software development of microcomputers for upwards of $4000. The minimum minicomputer system probably costs no less than $2000. But price tags of minicomputers with the addition of many peripherals can go beyond $100,000.

In the area of architectural differences, the microcomputer may have word length (i.e., the number of bits required to form an instruction) ranging from 4 bits for the smaller machines up to 16 bits for a few of the larger microcomputers. Most use an 8-bit word. On the other hand, most minicomputers use at least 12 bits, typically 16 bits, and sometimes as many as 32 bits for their word size. Microcomputers also can have a few locations on a chip where data may be stored temporarily and recalled quickly.

Of course, the important advantage of a microcomputer is its size. Most minicomputer systems are at least as large as a bread box, and usually a lot larger. But a microcomputer system can be designed in a size as small as 2 in. square, 2/10 in. thick, and weighing only 2 oz. Microcomputer systems have been purposely designed so that they can be built in small, modular packages and lose their identity as computing machines, whereas the minicomputer is usually recognizable as a computer. The microcomputer may be embedded in an instrument so that the user cannot see or necessarily even be aware of it.

Ken Cross inserts a read-only memory unit into a control logic card, which can be plugged into the microcomputer in the background.

When it comes to speed of computation, the comparison between microcomputers and minicomputers becomes more difficult because of the great range and variety of the former. Most microcomputers traditionally have been slower, but a few of the special-purpose models built with different architectures are actually faster than minicomputers and can be used in special applications in which their speed is an important requirement.

The microprocessor can be either a single chip or built on multiple chips, that is, two or three wafers of silicon. The single-chip microprocessor, which is made with the metal oxide semiconductor (MOS) technology, uses less power but is slower in its calculation speed than the multiple-chip device, which is produced by technologies with lower packing densities of transistors to chip.

The quality of software, or programming aids, supplied by the manufacturer has a direct bearing on the value of the device to the laboratory experiment, both in time and in money. A good set of programming aids translates directly to greater efficiency and the consequent savings in cost.

Semiconductor Memories

A companion development in the microelectronics industry that has been important to microcomputers has been the creation of new semiconductor memories. These are produced with the MOS technology, the same technology that made the microprocessor chip. These new memories take less power than the more traditional magnetic core memories, they are easier to use, they take up less space, and for a while they were less expensive. Very recently, however, the magnetic core memory manufacturers have become competitive and lowered their prices, so the comparison in cost is still a moot point.

Because MOS memories are far easier to integrate into an MOS microcomputer system than are magnetic cores, they are the overwhelming choice these days for microcomputer users. Two basic types of semiconductor memories, volatile and nonvolatile, are used. The nonvolatile memory is one that retains its memory capacity even when the power goes off. All present *semiconductor* read-write memories are volatile; *core* memory is both read-write and nonvolatile. A read-write memory holds data that can be taken out or changed by the computer; a read-only memory, which contains data that can be extracted but not changed, is nonvolatile. Some read-only memories can be erased with ultraviolet light.

Scientific Applications

At ORNL we have been investigating microcomputers for about two years. We have built a complete development system for writing, editing, and debugging programs, and we have a staff actively developing systems using microcomputers. We recently taught a Laboratory-wide course on the features of microcomputers and how to program them, and we have also given seminars in several research divisions. We have developed microcomputers for a number of applications, some of which are now in operation.

Now that pilot applications are well under way, we believe that we should take major steps to apply microcomputers to the Laboratory's needs.

From the large number of microcomputers available in industry, we have chosen a model for development that we think combines the most advanced technology with the necessary support structure. Not only is proven hardware available, but also, more importantly, convenient software tools are available to make programming easy and relatively rapid for each specific application.

Some electronic companies say that they prefer to wait a little longer before learning about microprocessors and building devices that use microcomputers. They may very well be right; to predict how and where microcomputers can effectively be used and, indeed, how many more fantastic changes there will be in the microcomputer field is difficult. Some questions, however, on how microcomputers might be used in research are already partially answerable.

Microcomputers are already being used as scientific computers, and this use will undoubtedly increase in the future. The logical structure of the microcomputer suggests that distributed systems of microcomputers are more likely to appear. Remember that the microcomputer is quite slow by conventional large computer standards today, so that it may take a while for its development to reach the point where it should be applied to large-scale scientific calculations. On the other hand, of course, in a primitive way the microcomputer is already being used for calculations, since the scientific desk or pocket calculator is undoubtedly a microcomputer. It is hard to predict whether large numbers of microcomputers will be used in parallel processing arrays as distributed systems, or in ways that we have only marginally imagined in the past.

As many ORNL researchers know, the most complex computer built is the Illiac IV machine conceived at the University of Illinois, which consists of 64 high-capability elements, each with very significant computing power, hooked together in a large two-dimensional array. A problem with this sophisticated computer is knowing how to program the parallel structure effectively to use such a fantastic amount of computing power. Another arrangement of computer elements is the distributed system, such as the Livermore "Octopus," which seems to lend itself to solving a wider variety of problems and is more amenable to programming.

Will the microcomputer be used in these ways? Possibly. If someone can figure out how to program the University of Illinois machine, we may then know how to program microcomputers in such array configurations.

Many instrument manufacturers are incorporating microcomputers to make their instruments "intelligent." It now looks as if the microcomputer will be very pervasive, moving into much of the measurement, control, and computation field. It will surely replace some of the current computer types, and it will make possible many applications that have heretofore been ruled out because of cost or complexity.

These are not miniaturized minicomputers. They are usually significantly more powerful and more capable computing machines than the early minicomputers. They are even somewhat more capable and sophisticated in their instruction repertoire than the ORACLE was. However, the microcomputer cannot do everything. Some tasks are more difficult for it than for previous designs of digital computers; for example, it cannot easily handle calculations involving ten-digit numbers.

How do present microcomputers compare with the world's best-known minicomputer, the Digital Equipment Corporation's PDP-8? The PDP-8 has seven types of instructions, plus an input-output instruction by which one can specify a number of devices with which the computer can communicate. The microcomputer that we are using at the Laboratory today has 74 basic instructions. The PDP-8 can address directly only 128 memory locations, but our microcomputer can communicate directly with 65,536 memory locations. Although these comparisons are not the whole story, the microcomputer represents not a reduced capability, but rather a powerful computer in a very small package.

Uses at ORNL

The first application at ORNL was when a microcomputer using the Intel 8008-1 microprocessor was installed as a dedicated controller for a system at the White Oak Dam at the Laboratory's west boundary. Here it controls the sampling of water to determine its pollution level. The program is structured so that samples can be taken at very precise intervals of integrated water flow over the dam. This enables the health physicists to inform EPA, once they have gathered and analyzed samples, of the exact ratio of pollutants to water.

In a project supported by NASA and NIH, as well as by ERDA, we are also applying the

The two boards on the left are packed with small-scale integrated circuits from a PDP-8 minicomputer. The 8080 microprocessor chip held on the right has roughly the equivalent amount of computing power as the PDP-8 boards.

microcomputer to the Miniature Fast Analyzer, which has been developed in the Chemical Technology Division. This is a small, portable machine that can analyze samples of a few drops of blood while one waits. Because there is much computation involved in this type of analysis, a microcomputer is ideally adapted to this.

Another microcomputer forms the intelligent element of our Johnson Noise Thermometer, a new device being developed in the I & C Division. We are going to put this microcomputer in a small package where it can carry out the complex computations and iterative calculations necessary in this method of measuring temperature. Without the microcomputer, the Johnson Noise Thermometer would require the larger minicomputer.

The microcomputer is also proving useful in the experimental development of a power controller. This device will be used to control the power being delivered to an electrically heated mockup of a nuclear reactor core. This effort is part of the Core Flow Test Loop Experimental Program in the Reactor Division. In this case, the microcomputer will very precisely control the power to simulate a variety of conditions postulated to occur in a reactor—a loss of flow, temperature cycling, etc., as well as its own normal operation.

A potential application of microcomputers is in the development of a portable data acquisition system. A researcher could carry such a system in a suitcase or briefcase to the site of an experiment, plug it in, record the data on the spot, and return to a central location where he could analyze the data with a large computer.

Other possible applications of microcomputers already in trial are:

- An "intelligent terminal" to relieve the large computer of some mundane functions so that it can dedicate itself to what it does best: complicated, long-winded, mathematical calculations.
- Sophisticated electronic games of strategy or reaction tests such as electronic hockey and ping pong.
- Replacement of hard wired logic so that less circuit rewiring is needed in integrated circuits when changes are to be made.
- Traffic light controls that are programmed to adapt to changing traffic density and patterns as well as to normal daily and weekend changes.
- Cash registers for department stores and supermarkets that will collect, store, and transmit data as each sales transaction is made.
- Machines that will lead salespeople through the sequence of operations required for a complicated sale, such as a charge account sale, so that it cannot be closed if an incorrect procedure is attempted.
- Automobile applications that control the timing of the ignition, depending on information the microprocessor receives from sensors measuring such variables as the speed and torque of the engine, the crankshaft position, engine coolant temperature, and outside temperature.
- Intelligent instruments "smart" enough to do such things as monitor their own operation, check for procedural errors by the operator, calibrate themselves, and carry out computations to produce desired answers.

In summary, the microcomputer costs less, takes up less space, uses less electricity, and, in special computing applications, it is faster than its predecessor computers, many of which are prevalent at ORNL. In the future it will surely take its place alongside the transistor and the operational amplifier as a standard module that can be configured as a control and data acquisition system to make possible advanced experimental research.

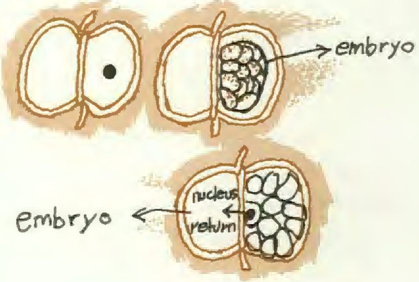
BOOKS

STEPS IN MODERN BIOLOGY

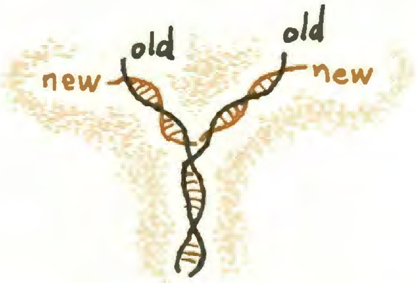
"Thirty-Six Lectures in Biology," by S. E. Luria. MIT Press, Cambridge (1975). 439 pages. Paperback, $8.95; hardcover, $17.95. Reviewed by Charles S. Shoup.

I approached this book with a good deal of curiosity from my personal viewpoint, for I remember a younger Salvador Luria, who long ago spent part of a Guggenheim appointment in the laboratory of Max Delbruck at Vanderbilt University in Nashville next door to my physiology laboratory. This was in 1942, and I also recall at least one visit by Alfred Hershey from Saint Louis. The results from early work on bacteriophage and replication of the virus then being composed into an orderly body of knowledge eventually led to this same triumvirate sharing the Nobel Prize in 1969. This volume by Luria is the condensation of a General Biology course he offered at the Massachusetts Institute of Technology during the spring terms of 1973 and 1974. Luria has been on the faculty at MIT since 1959. He admits that he was so pleased with teaching bright and well-prepared students that he dedicated this book to them with the hope that the lectures might set a pattern for other modern biology courses suited to good students. Recommended outside reading references are an important feature of the book.

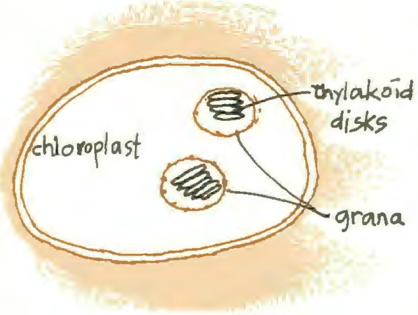
While the lectures are of wide scope, encompassing five main subjects—Cell Biology and Cell Chemistry, Biochemistry, Genetics, Developmental Biology, and Physiology—the teaching centers around one concept: Living organisms are possessors of a program, a set of genetic information, underlying all vital processes and undergoing an evolution through mutation, genetic recombination, and natural selection. Cell composition, growth, and reproduction are considered preparatory to a large section on Biochemistry, which includes a study of DNA and the properties and role of transfer RNA, messenger RNA, and ribosomes. From time to time, the author mentions important contributors to modern biology such as O.T. Avery (who later settled in Tennessee), who first concluded from his pneumococcus experiments in 1943 that genes consisted of DNA and that the DNA of each gene is specific. I am a little disappointed that Oak Ridge contributors to the subject of molecular biology are not mentioned, but these lectures are in outline only, and doubtless the author as lecturer had much more to say. He does mention Oscar Miller and his electron micrographs, which were prepared when he was at the Biology Division, showing RNA polymerase transcribing DNA segments into RNA molecules.



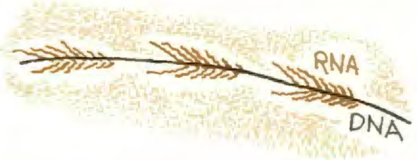
About 80 pages are devoted to the science of genetics; the author first gives a brief look at Mendelian inheritance and then moves on to the modern outlook, which includes biochemical genetics as shown in *Neurospora* and *E. coli* with isolation of mutants, DNA transformation, gene mapping, enzyme induction and repression, and the operon concept. There is a section on the special contributions from phage studies, but recent elegant electron micrographs of the ultrastructure of bacteriophage are not included (perhaps the author provided these with his lectures). Radiation biology and radiation genetics are not included either, because these fields are considered as specialties outside the scope of these lectures. In the genetics section, the author discusses human genetics and diseases of genetic defect. Actually, here may be found a source of terse statements on the basic causes of some of these disorders. The author has a good deal to say about evolution and natural selection and the hope (vs only the possibility) that Man's chances of survival will be longer than that of past dominant species.



The next section of the book deals with problems and phenomena associated with the development of the whole organism. Embryology is seen as the unfolding of a program that is held within the genetic material, leading to the turning on of genes in the sequence of the developmental stages of the multicellular organism. Vertebrate embryology is discussed with regard to the chemical relations between cells, aggregation, gradient, cell multiplication and growth, and the fate of the germ layers. The author indicates that genetically controlled chemical markers must serve to sort out the cells and cause them to take their places to form the various embryonal tissues and organs.



The final eight lectures constitute the section on Physiology. Because function is approached from the molecular biology point of view in various plants and animals, I call this part a résumé of general physiology. Highlights of control by the hormone-producing organs, cyclic AMP, ionic exchanges, the generation of ATP in glycolysis as an energy source, and the chemical and physical basis for muscular contraction are included. Other topics concern the heart and circulation, electrophysiology of the heart, blood and its composition, and the efficiency of hemoglobin in oxygen transport. The author does not, however, mention haemocyanin, the copper-containing body fluid of the mollusca and crustacea, a step in the evolution of biological oxygen carriers. There is a section dealing with immunology and the immune mechanisms prior to an ending portion on neurobiology, electrophysiology of nerves, the receptors, and the phenomena of vision. In all, there are about 260 topics covered in the book. As mentioned earlier, the course was a one-term series of lectures, but it seems to me that there are enough topics to permit the series to be expanded into a year-long course. Possibly that is the author's intention. If one examines the content of the basic courses in the University of Tennessee-Oak Ridge Graduate School of Biomedical Sciences, one will find subjects that are also contained quite broadly in these lectures by Luria. A skilled teacher could use this volume as a framework to build a modern biology course quite adaptable to "plug-ins" of new information.



The book includes several appendixes: a section of background material for the student on chemical kinetics, review questions covering most of the lectures, and a set of searching open-book examination questions. An index is provided. Printing is from the original typescript, interspersed with sketches and diagrams prepared as the lecturer must have drawn them when he turned to the blackboard. This book is a contribution from a good teacher who enjoys his students and teaching and delights in his subject matter.

"Thorium: Preparation and Properties," *by J. F. Smith, O. N. Carlson, D. T. Peterson, and T. E. Scott. Iowa State University Press, Ames (1975). 382 pages, $9.95. Reviewed by Sigfred Peterson.*

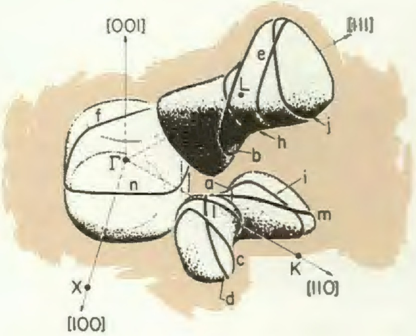
This is a good book. It is an authoritative, thorough, and well written account of nearly all aspects of the metallurgy of thorium, running the gamut from diffusion through ductility and fabrication through Fermi surfaces. This laudatory conclusion must precede the bulk of my review, because enumeration of limitations of the book may otherwise be interpreted as disapproval. The authors are professors of metallurgy at Iowa State University, the source of much of the modern knowledge of thorium.

The extractive metallurgist will be disappointed. Chapter 1 gives a judicious review of all methods of metal preparation, their advantages and shortcomings. Chapter 2 treats purification in a similar manner, but gives no coverage of ores and their extraction. The fabricator, however, may be more satisfied. Chapter 3 gives an interpretative treatment of the successes and failures of all processing methods used on thorium and its alloys, even presenting what is known of deformation textures.

The physical and mechanical metallurgists will find much of interest. Nearly a hundred pages in Chapter 4 present data (largely in graphic form) on tensile, creep, impact, fatigue, and hardness properties, along with explanations of their variability of composition, temperature, strain rate, and irradiation. Much attention is given to rather significant effects of impurities, particularly carbon, and available data on properties of alloys are included. Chapter 6 on Physical Properties starts with band theory and continues with experimental data and theoretical interpretations of all properties measured. Diffusion data are collected in a separate Chapter 5. Metallography is ignored except for citations of supporting evidence in discussions of various properties. This lack may have been imposed by the printer and publisher.

Chapter 7 presents extensive information on all binary systems of thorium. The treatments reflect the extent of data available. They range from mention of miscibility limits with scandium and the existence of a compound with technetium to a twelve-page presentation on the thorium-carbon system, with two proposed versions of the complex phase diagram and considerable information on the allotropy, crystallography, and thermodynamics of the thorium carbides. Corresponding information, where available, is given for all other binary systems and compounds of thorium. The coverage of the literature is not as complete as in the more metallurgical parts of the book, but it nevertheless provides a worthwhile extension of the coverage.

The book was apparently reproduced by an inexpensive offset process with typewriter composition. The type font chosen has excellent readability, although it appears to lack a much-needed degree sign. No photographs are used (hence the neglect of microstructural information). The numerous well executed graphs adequately illustrate the material presented. However, many would look better with more reduction; the cost of smaller prints would be offset by the pages saved. The authors have not seen fit to convert the data to SI (International System) units. Although dual units are cumbersome, the effort of conversion would be justified, since this monograph will remain the authority in its field after the transition to SI has been completed. The finicky editor will find several instances of departure from best editorial practice, but these will go unnoticed by the reader for content.



THE THIRD AND LAST volume in a series on "Advances in Molten Salt Chemistry," edited by J. Braunstein and G. P. Smith of ORNL as well as Gleb Mamantov of The University of Tennessee, is now available. It explores molecular dynamics calculations of molten salts, gas solubility in melts, organic reactions in molten tetrachloroaluminates, and the chemistry of thiocyanate melts. It goes into experimental techniques for molten fluoride melts in detail, and contains a compilation of binary and ternary fluoride systems. Among its seven contributors are Carlos E. Bamberger and Roy E. Thoma of the Laboratory.

R&D *Accomplishments* at ORNL

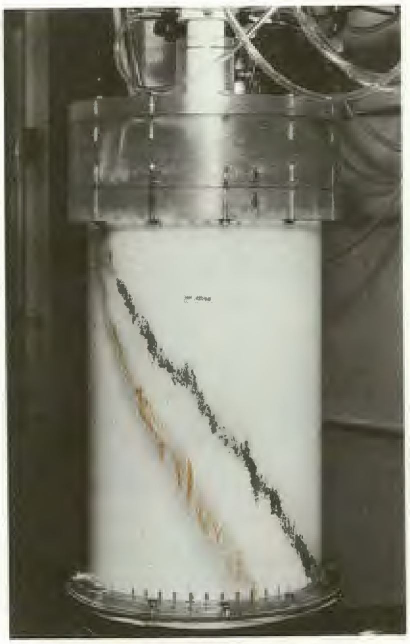
This description of Chuck Scott's continuous chromatographic separations device introduces the *Review's* new department, a periodic account of exceptional technological development that has originated at ORNL. In this regard, the Laboratory truly enjoys an embarrassment of riches, which means that mention in this column can neither be considered a comprehensive list nor indication of the most outstanding.—Ed.

A prototype of a continuous liquid chromatographic separations system has been developed and successfully operated by the Chemical Technology Division. The development shows promise as a means for large-scale recovery of usable nuclear materials (uranium, plutonium), fractionation of radioactive wastes, and isolation of biological fractions (enzymes). In the latter case, enzymes of interest may be obtained by growing microorganisms, breaking them up, and introducing the crude mixture in liquid form into the separations system. Several enzymes in purified form are desirable because of their capability of degrading hazardous chemical pollutants and of producing hydrogen as an energy source.

Unlike conventional chromatographic systems, ORNL's continuous chromatography system, based on a principle derived more than 20 years ago, can accept feed material to be separated continuously. In conventional systems, separation of a large amount of material into various fractions requires several columns and a sequence of steps to process a batch of feed. In ORNL's continuous system, feed can be introduced continuously into the top of only one column while the system isolates various constituents and collects them at the bottom at different points.

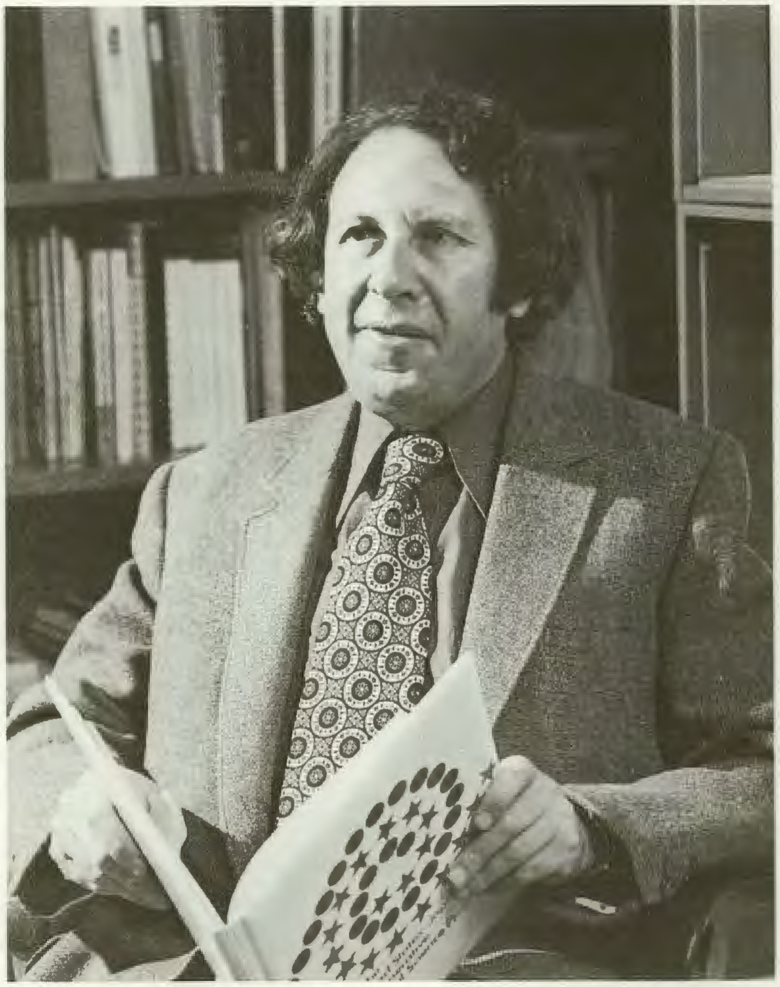
The ORNL device consists of a rotating annulus made from two concentric Plexiglas cylinders, 50 cm long and about 1 cm apart. The annulus is sealed at both top and bottom by flanges. Spacers are used in the annulus at top and bottom to help maintain a constant annulus width while, at the same time, segmenting the eluent (entering) and eluate (exiting) streams. A double O-ring seal is used at the top of the chromatograph to provide for pressurized operation. A slow-speed drive system allows rotation of the annulus at speeds of approximately 2 to 20 revolutions/hr. Metering pumps introduce the feed and eluent streams (which wash down through the cylindrical system).

One type of separations medium used in such a system is a white, powdery "molecular sieve" composed of porous particles that are distributed throughout the rotating annulus as a packed bed. With this type of separation, the feed material is introduced through a stationary tube. At the same time, eluent streams enter the system through several other stationary tubes. Molecules of the feed that are larger than the pores in the particles of the separations medium do not interact with the sieve particles and are flushed down through the system by the eluent streams. Molecules smaller than the pores in the sieve interact with the solid particles enough to be retarded and therefore pass through the system at a slower rate than do the large molecules in the feed. Interaction with the molecular sieve by feed molecules includes penetration of the particles, formation of weak chemical bonds with the particles, and adsorption (adhesion of the liquid molecules to the surface of the solid particles). All the feed molecules eventually are washed through the system, with the retarded ones exiting at different angles from the faster-moving molecules.



Charles D. Scott, chief of the Division's experimental engineering section, and William Moon, technician, developed the prototype and are now optimizing the design. It is hoped that the new system can be used this fiscal year for the isolation of biological fractions.

Willie Lijinsky has for a long time been concerned with the relationship between chemical structure and carcinogenesis and the biochemistry that occurs in the conversion of the normal cell to the tumor cell. This has led him to a search for possible carcinogens in the environment, resulting in the study that he describes here. He is a British citizen, Dublin-born, who came to the U.S. in 1952 with a doctorate in biochemistry from the University of Liverpool. He spent two years at the California Institute of Technology as an A. A. Noyes Research Fellow, and the next year at the Montreal General Hospital Research Institute as a Damon Runyon Research Fellow. From there, he moved to the faculty of the Chicago Medical School, and ultimately to ORNL's Biology Division, where he has conducted his research since 1971. He tells here of the direction in which it has led him.



Nitrites and Nitrosamines

... are they related to human cancer?

By WILLIAM LIJINSKY

CANCER IS A COMMON disease in man, especially in societies that have a long expectation of life; it is generally considered a disease of old age and is classed with the degenerative diseases. Human cancer has its parallels among other species, and the availability of so many experimental animal models for a human disease is rare. We are able to simulate in laboratory animals almost all types of cancer seen in man by administration of a variety of chemicals called carcinogens.

Radiations can also induce cancer in laboratory animals, but their versatility is small. Viruses have induced a variety of tumors in certain species, but, although it seems likely, no definite evidence has emerged that any human tumor has a viral etiology. By default, we are left with the conclusion that most human cancer (possibly as much as 90%), is caused by chemicals.

Indeed, in the few cases of clustering of a certain type of cancer such that epidemiology has shown a cause of the effect, a chemical or group of similar chemicals has usually been the agent. Historically, this type of association goes back 200 years to the observation by Percival Pott that London chimney sweeps, who started their exposure to soot as small boys, had an abnormally high incidence of cancer of the scrotum. We now believe that the polynuclear compounds in soot were the carcinogenic agents, and that these same compounds were responsible for the skin cancer of coal tar and creosote workers and for wax pressmen's cancer in oil refineries. Similarly, the bladder cancer of dye workers was related to exposure to beta-naphthylamine, and more recently, a rare cancer of the liver in some chemical workers has been ascribed to exposure to vinyl chloride.

However, all these cases of industrial exposure to carcinogenic chemicals amount to a handful of the more than 500,000 cases of cancer that are diagnosed and the more than 300,000 persons who die of cancer each year in the United States.

There can probably be no comprehensive explanation of cancer, which might be a collection of diseases rather than a single disease. On the other hand, we have become acquainted during the last 20 years with a group of carcinogenic chemicals of remarkable versatility, the nitrosamines, or more properly, *N*-nitroso compounds. These are simple compounds, known since the beginnings of organic chemistry, formed by reaction of secondary or tertiary amines with nitrous acid, which is usually generated from a nitrite and a mineral acid.

Since the observation 20 years ago that the simplest nitrosamine, dimethylnitrosamine, was toxic in the form of severe damage to the liver (again a finding made first in man), more than 100 *N*-nitroso compounds have been tested in laboratory animals. In long-term experiments, usually with repeated doses of the compounds, the vast majority have been carcinogenic nitroso compounds, and some nitroso compounds have induced tumors in every organ and tissue of one species or another.

As a class, *N*-nitroso compounds are most effective when given by mouth. And, like most carcinogens, they are also most effective when given in repeated small doses over a long period, rather than in large single doses. This gives credence to the theory that cancer is a disease of old age only because our exposure to carcinogens is limited to very small amounts and it takes a long time to accumulate a dose sufficient to form a viable cancer.


Wayne Taylor and George Singer watch extraction of an amine from food.

Because it is unlikely that man would be the only species resistant to the carcinogenic action of nitroso compounds, we seem only to need to link the presence of nitroso compounds to the occurrence of human cancer induced by them. This has not been possible. Nitroso compounds have been found only rarely in the environment and then only in minute amounts, and there has been no case in which any human cancer can unequivocally be attributed to exposure to one or more nitroso compounds. The very fact that they are such broadly acting carcinogens might be hampering our search for a connection; there are several nitroso compounds that induce tumors in several organs of the same species, depending on dosimetry.

However, nitroso compounds must be considered prime candidates as human carcinogens, because they can be formed in the acid of the stomach from nitrites and secondary or tertiary amines, exactly as happens in a flask containing acid. The reaction is subject to the usual physical chemical factors as well as to catalysts and inhibitors that affect the rate of reaction. Nevertheless, Sander showed in 1969 that, by feeding secondary amines together with sodium nitrite to rats for a long period, sufficient nitrosamine was formed to give rise to the tumor characteristically induced by the nitrosamine product in those rats.

My coworkers and I have pursued this aspect of nitrosamine carcinogenesis for the past six years, more than three of which have been at ORNL. Nitrites occur in nature in widely varying amounts: they are found, for example, in plants grown on deficient soils, or after bacterial reduction of the ubiquitous nitrates; considerable quantities are added to meat and fish as coloring and flavoring agents, as well as preservatives. Because of this prevalence in nature we have turned our attention to nitrosatable secondary and tertiary amines, because they are more easily brought under control. The numbers of these that man might ingest is huge, many hundreds, including natural components of food, food additives, agricultural chemical residues and drugs. We have examined the reaction with nitrite of more than 20 typical compounds in wide use and have found that all form a measurable amount of the expected nitroso compound in an hour or two, even at body temperature. We have complemented these chemical studies with animal feeding tests, in which several of the compounds and nitrite have been given in drinking water to rats every day for a year, and the rats kept until death. Careful pathological examination of the animals is performed by Wayne Taylor to detect tumors that might have been induced by nitroso compounds formed in vivo (as in most species, there is in rats an incidence of "spontaneous" tumors originating in endocrine and other secretory organs and in the genital and lymphatic systems).

Twenty amines are being tested in this way; three of these tests are now complete because the animals have died. The secondary amine heptamethyleneimine has induced lung cancer and cancer of the esophagus in more than 60% of the animals, thereby demonstrating that a possible contributor to lung cancer in smokers could be nitroso compounds formed from amines in swallowed tobacco smoke and nitrite in food. The tertiary amine, aminopyrine, formerly a commonly used analgesic in the United States (and still much used elsewhere in the world), has given rise to liver tumors in almost 100% of the animals, even at the quite low dose of 250 ppm in water. This agrees with chemical findings that aminopyrine reacts more readily with nitrite than almost any other amine to form the potent carcinogen dimethylnitrosamine. Oxytetracyline, on the other hand, fed together with nitrite, has not given rise to tumors not occurring spontaneously; this also agrees with the chemical findings that oxytetracycline reacts readily with nitrous acid at high concentrations, but very little at low concentrations. The control tests of nitrite or of the amines have been entirely negative.

Even though we must wait for the results of most of the experiments in progress to make a complete judgment, it seems obvious that the observations of our chemical systems can predict roughly the reactions occurring in vivo. That is, that small amounts of nitroso compounds can be formed when nitrite and secondary or tertiary amines are ingested together, but that these amounts might or might not be sufficient to give rise to tumors within the lifetime of our rats. In the case of some highly reactive amines, tumors are formed, and the combination of nitrite with these amines is as carcinogenic as if the nitrosamine product itself had been administered.

We are now extending our studies to include some widely used insecticides (*N*-methylcarbamates) which react readily with nitrous acid to form nitroso derivatives. Typical of this group are carbaryl and carbofuran.

Through extensive collaboration with ORNL biologists Jim Regan, Rosalie Elespuru, and Jane Setlow, we have shown that nitrosocarbaryl and five or six similar derivatives of insecticides are very potent mutagens in bacterial systems and react with human DNA in vitro to form alkali labile bonds. The implications that these findings have for people who are exposed to them has led us to set up a program to look for these insecticides and their nitroso derivatives in water, food, and

Willie Lijinsky and Wayne Taylor examine a lung tumor induced in a rat by feeding an amine plus nitrite.

elsewhere in the environment. This will require high-pressure liquid chromatography to isolate and identify these nonvolatile compounds, but at least we will have the means to investigate this new area of concern about environmental carcinogens.

In parallel with these very practical studies of the distribution and formation of nitroso compounds and their precursors, in which George Singer makes the major contribution, we are investigating the more academic, but possibly no less important, matter of just how the carcinogenic *N*-nitroso compounds do their damage. In this area we are hampered by not having enough knowledge about the chemistry of this group of compounds, and unfortunately we have no mandate to study this subject. Most of the work done so far on the mechanism of action of nitroso compounds has concerned their interactions with DNA in the cell nucleus, which may or may not be the important reactions in carcinogenesis. In either case, the results have been unsatisfactory when looked at as a whole. We have been performing a long series of tests in which closely related nitroso compounds are administered to groups of rats under identical conditions of dose and time of administration. These structure/activity studies have yielded some interesting and even surprising results,

Peggy Braden trims a tumor to be sectioned and made into a histology slide.

which might help us understand the process by which nitroso compounds help change normal cells into cancer cells.

For example, there is a pronounced lessening of carcinogenic activity when the hydrogen on the carbon atoms adjacent to the nitroso group is replaced by deuterium, and almost complete inhibition of carcinogenic activity when these hydrogen atoms are replaced by methyl groups. On the other hand, methyl groups in other positions seem to enhance the carcinogenic activity and chlorine or bromine substitution even more. This means that the key initial reaction, probably with an enzyme, is probably removal or replacement of an alpha hydrogen atom of the nitroso compound.

The program thus far includes a series of nuclear magnetic resonance studies of conformation and reactivity of a series of nitroso compounds, using the new advanced NMR facility at ORNL. We are using three tagged nitroso compounds synthesized by Russell Jones, in biochemical studies of their distribution and interaction in vivo with various cell components in target and nontarget organs. There is reason to hope that we may eventually arrive at an understanding of the chain of events culminating in the formation of some cancers.

Staff quote:

"Emphasis is growing, on the one hand, on the contributions of basic physics and chemistry to biology, and, on the other hand, on the reduction of basic biological problems such as those of photosynthesis and vision to the physico-chemical level. It is exciting to see that basically the whole problem of vision is being reduced to the energy-level scheme and character of the lowest electronic states of a few molecules."—**L. G. Christophorou,** *describing a meeting of physicists, chemists, and biologists to discuss "Excited States of Biological Molecules," in the spring of 1974.*

lab anecdote/HERBERT POMERANCE

A SIGMA PILE IS NOT A REACTOR

The Oak Ridge Graphite Reactor was made of 3000 tons of concrete, 650 tons of graphite, and 54 tons of uranium. The graphite is still encased in concrete in the historical reactor, but perhaps 15 tons of the same quality of graphite can be found in Building 3088 near the Bulk Shielding Reactor.

A frame building where the Oak Ridge Research Reactor (ORR) now stands housed all the graphite work in the construction days of 1943. The carpenters (including Ray Oakes, still at ORNL) planed the 4-ft-long surfaces of the graphite blocks and cut the 4-in.-square ends with a circular saw. Then the laborers stacked the graphite blocks (or stringers, as they were called) into piles about 6 ft square and 10 ft high for a measurement of the neutron diffusion length; on the following day, they stacked the graphite inside the concrete shield. Because the diffusion length includes in its theoretical definition the ratio of absorption cross section to scattering cross section—$\sigma a{:}\sigma s$—the pile was always known as a "sigma pile." Neutron-absorbing impurities in the graphite that would impair the operation of the pile were quickly found this way.

In the official account entitled "Graphite-Uranium Production Piles" and edited by Lyle Borst, the range of diffusion lengths from 45 to 50 cm is stated in a short paragraph without any mention of the method of determination. The diffusion length measurements were made by a team of six or seven people, including Charlie Clifford, now with the Neutron Physics Division, and Rube McCord, now with the Operations Division. They inserted a radium-beryllium neutron source near the base of the sigma pile, put indium foils on the axis at 4- or 8-in. intervals, and counted the foils in a nearby counting room. These scientists—who had been trained at the Metallurgical Laboratory in Chicago when the West Stand Pile that Enrico Fermi had built was renamed CP-1 and transferred to the Argonne Forest Preserve (from which Argonne National Laboratory gets its name)—later measured all 650 tons of graphite at the rate of 15 to 20 tons a day at the Clinton Laboratory in Oak Ridge, now ORNL.

Grace McCammon (now Estabrook) at a pile in 1952. The standard pile, a remnant of the construction of the Oak Ridge Graphite Reactor, is in too small a building to be photographed. This sigma pile is similar and makes a satisfactory stand-in.

One sigma pile with a half-gram radium-beryllium neutron source was saved to be used as a standard pile. At Slot 11 the neutron flux was exactly 703 neutrons/cm^2/sec. With the advent of absolute beta counting methods, the need for a standard pile diminished. The National Bureau of Standards now provides a standard.

When the sigma pile at ORNL was restandardized in 1952, editors at AEC were under orders to suppress the word "pile" and to use the word "reactor" instead. However, since a standard pile has no uranium in it for a chain reaction, the editors relented—the term "pile" stands. The restandardization also brought out the differing attitudes of the experimentalist and the theorist. Thelma Arnette found a slope of the line on her graph of 3.34; the theorist found a slope from his least-squares fit of 3.35. He complimented her on coming so close to his computed value. She replied, "Of course. That was the only line I could draw through those points."

When James R. Lawson, now president of Fisk University, was assigned in 1955 to work with Henry Morgan of the Physics Division, a close relationship was formed between ORNL and the predominantly Black university in Nashville. Through this interaction, both the Fisk Infrared Institute and the Department of Physics have gained international reputations in molecular spectroscopy. In 1966, at the Fisk Centennial Celebration, Henry was granted an honorary Sc.D. for his contributions to the University science programs. Henry received his formal training at St. John's University in New York and at the University of Minnesota; he has worked as a physicist at ORNL since 1950, specializing in the spectra and structure of molecules using infrared, Raman, and microwave spectroscopy. In 1973 he received a grant from the National Academy of Sciences to participate in IUPAC committee meetings in Munich and to present a paper at the IUPAC Congress in Hamburg. In 1973 and 1974 he visited Latin America, with U.S. State Department support, to confer on science education. This past July he served as faculty member in a course on lasers and holograms given at the Instituto Technológico de Aeronáutica, near São Paulo, Brazil. Henry, Professor Enrique Silberman, and Percy Staats (l. to r.) are shown here during a recent visit to ORNL by Prof. Silberman, who moved to the Fisk faculty from Argentina in 1966.

The ORNL-Fisk Connection

By HENRY MORGAN

ONLY 20 YEARS AGO, in the mid-1950s, the demand for equal rights, which we now accept so readily, was sweeping across the nation and meeting strong resistance. In those times, most capable black scientists were trained in the North, and showed an understandable reluctance to move to the southern states; there was one black scientist on the ORNL staff. In 1955 the first Negro summer participant was chosen, Dr. James R. Lawson, then a professor of physics at Tennessee State A & I University in Nashville. His training and research interests lay in molecular spectroscopy, and he was assigned to work with me that summer—the beginning of a close ORNL-Fisk relationship continuing to this date. Jim Lawson soon transferred to Fisk University as professor of physics, then became, in succession, department chairman, vice-president, and president of the University, the position he now holds.

The Fisk physics staff was directed in the late 1940s and early 1950s by Dr. Nelson Fuson, also an infrared spectroscopist. Both he and Lawson, who had a B.S. from Fisk, had received a Ph.D. in spectroscopy at the University of Michigan under Prof. Randall. This was not coincidence, for Fisk had been a center of spectroscopy in the South since the 1920s. Dr. Elmer Imes, a Fisk graduate who also took his Ph.D. at Michigan (1918) and a chairman of the Fisk Physics Department, carried out as much research in infrared spectra as his facilities would permit during the 1920s and 1930s. His limited equipment and strong traditions were passed to Lawson and Fuson in the late 1940s after World War II.

Jim Lawson spent two summers, 1955 and 1956, conducting his research in Building 9734 in the Y-12 area, one of the few one-story brick buildings of wartime construction that were suited

to temperamental laboratory equipment such as spectrometers. His studies were carefully planned not only for the summers at the Laboratory, but also for the instrumentation available at Fisk and Vanderbilt, so that a basic cooperative program could be built with ORNL. The only difficulties in those summers came not at the Laboratory, but in the Oak Ridge area. Clinton was undergoing bitter racial tensions that led to the bombing of the high school, and rumors were always in circulation about the possible harassment of those "northerners" in Oak Ridge. Jim Lawson was living in Gamble Valley, and on more than one occasion, because of these rumors, moved briefly to the Knoxville College campus. Nevertheless, despite some sleepless nights, his work on inorganic solid spectra progressed and led to several publications.

In 1957, Lawson transferred to Fisk University as professor of physics, and the cooperative studies continued on that campus, providing material for several master's theses. With strong support from ORAU, much of the work of Lawson's students was performed here at the Laboratory. Similar work on inorganic solid state spectra was pursued by Dr. William Trawick of Louisiana Technical University at Ruston, a summer faculty participant, in 1960 and 1961. The studies of Lawson and Trawick complemented each other so well that they were combined with ORNL data to form a comprehensive survey, and a paper was scheduled for presentation at the International Meeting of Molecular Spectroscopists, held in Amsterdam in 1961. The paper was coauthored by Trawick, Lawson, and Morgan, and only after its submission and the award of travel support to Jim Lawson for delivery of the paper, did the problems arise. In 1961 the Louisiana legislature was in a turmoil over the civil rights issue, and the state universities, always suspect, were investigated by members of the legislature who visited each campus. Too late did we realize the significance of the paper coauthored by Trawick of the Louisiana State facility and Lawson of the Negro Fisk University. Because Bill Trawick and his science department obviously were in for major problems, Trawick had arranged a move to the faculty of Georgia State College in Atlanta by September of 1961. He eventually became chairman of the Chemistry Department and could look back on the affair with some humor.

And indeed, it is easy to forget that 15 years ago racial attitudes in the South were remarkably different from the tolerant attitudes of today. Almost every year a paper coauthored by members of the Fisk and ORNL staffs would be given at the annual Ohio State Symposium on Molecular Structure and Spectroscopy held in Columbus. For convenience and economy, we would drive together, but of necessity we would pack our own food, because at no restaurant south of Cincinnati and few south of Columbus, would a mixed group be served. Only on the Ohio State campus was there complete freedom.

Earlier, in 1950, Lawson and Fuson, together with Dr. E. A. Jones of Vanderbilt, held the first one-day "training course" in infrared techniques, open to all students and faculty in the Nashville area. Infrared technology, having proved its value during the hectic years of World War II, was one of the most promising research areas for universities and industries, and the course, named the Fisk Infrared Institute, in a few years expanded to five full days of lectures and labs. As it grew, instrument manufacturers shipped their newest spectrophotometers for use in laboratory experiments and sent their instrument designers to lecture and demonstrate the equipment. Industrial and university faculty from all parts of the country enrolled as students; arrangements had to be made for food and housing on campus and tuition set to cover faculty and administrative costs. In 1956 I joined the Institute faculty as lecturer, and in 1957 Percy Staats took charge of the laboratory, developing a program that effectively, in five concentrated sessions, introduced as many as 50 participants per class to experimental infrared techniques.

The racial turmoil in the South severely affected the Fisk Infrared Institute. Northerners, reading of the strife and violence in the southern states, were reluctant to come to Nashville to study and live on a black campus. Yet they came, in increasing numbers, bringing more sophisticated problems. In the early 1960s, Percy Staats and I saw the need for extending the Institute to two full weeks of classes and separating it administratively from the Physics Department. Still the enrollment stayed at our maximum of 50 students per class, and applicants were regularly turned away. Each year, despite our explanations that the Meharry Medical School and Hospital were directly across the street from the Fisk campus and that ambulance sirens could be expected at any hour of the night, some nervous students, after one sleepless night in the dormitory, would move the next morning to a nearby motel and commute to class by taxi.

A chance remark by one of the instrument manufacturers turned the Institute international. Sales of infrared equipment to South American countries were growing rapidly, but once installed, the instruments had little use because so few people actually knew the theory or techniques, much less the industrial applications. A little investigation showed only two spectroscopy research groups in all of South America: Prof. Hans Stammreich's institute at the University of São Paulo, Brazil, and Prof. Enrique Silberman's group at the University of Buenos Aires, Argentina. Both groups readily agreed on the need for training, and as a direct result, the Fisk Institute held a three-week infrared course in São Paulo in 1965. The invitation of Professor Hans Stammreich, strongly backed by the Brazilian scientific community, led to the choice of São Paulo. The course was conducted at the Instituto de Pesquisas Tecnológicas, roughly the equivalent of our National Bureau of Standards, which, with the Brazilian Atomic Energy facility, is located adjacent to the campus of the University of São Paulo. The Institute was jointly sponsored by the U.S. Agency for International Development, the Organization of American States, the Brazilian Academy of Sciences, and the University of São Paulo. The course was truly Latin American, attended by students from South and Central America. The faculty, too, came from Argentina, Brazil, and Mexico. Communication was not a problem, despite lectures and explanations in three languages—English, Spanish, and Portuguese—due primarily to the tremendous enthusiasm shared by students and faculty alike. The only difficulty turned out to be the summer rains and relative humidities of 90%, which made working with alkali halide windows a real challenge. Nevertheless, the Institute was a success by any standard and set a pattern for the training of spectroscopists in universities and industries throughout Latin America.

Prof. Silberman of Argentina, serving as a member of the faculty, became one of the Institute's most enthusiastic supporters. He planned a second institute for Buenos Aires in January 1967 (vacation period in the Southern Hemisphere), again with the participation of Fisk and ORNL staff. As fate would have it, the military assumed power in the government of Argentina on June 28, 1966, and brutally took possession of the University. The physics faculty resigned en masse, leaving professors without classes, research facilities, or salaries. At a hastily arranged meeting in Nashville between Jim Lawson, then vice-president of Fisk, Percy Staats, and me, budgets and plans were arranged and rearranged to provide funds for a temporary joint Fisk-Vanderbilt faculty appointment for Silberman. He accepted the offer and arrived in September 1966 to teach during the fall term. Over the next several months, we brought five of his best graduate students to this country. One completed his thesis at The University of Tennessee-Knoxville, and the other four received their Ph.D. degrees from Vanderbilt. All did parts of their doctoral work in my lab at ORNL, and their thesis publications are in joint authorship with Laboratory staff. Collectively, they were nicknamed the Argentine Mafia. Prof. Silberman is still at Fisk, the one-year appointment of 1966 having been extended, as Argentina remained under military rule, to what has now become a permanent position. As Jim Lawson moved to the office of president, far removed from the infrared laboratory and the annual Institute, Silberman became chairman of the physics department and now is the director of research in molecular spectroscopy.

The Fisk Institute, held annually in Nashville, relies to a very limited extent on Fisk and Vanderbilt staff; lecturers come from all parts of the country and from abroad. For example, Lionel Bellamy and George Wilkinson of London have been frequent faculty members. In 1967, one of the guest lecturers was Dr. C. N. R. Rao of the Indian Institute of Technology in Kanpur. His enthusiasm for this type of spectroscopy course was great, and upon his return to India, he sought financial support for a similar Institute on his campus in Kanpur. By 1970 the National Committee on Science Education, the sole Federal funding agency for science in the Indian Central Government, finally gave its approval. For political reasons, however, the Committee determined that the course would be held at Punjab University, some 600 miles away in the northern city of Chandigarh. Punjab University had no spectroscopist on the faculty, and no funds were provided to bring Dr. Rao, his staff, or his equipment from Kanpur. To compound the problem, this was to be the First All-India Science Institute, assembling some 40 students who were, in turn, faculty members at universities all over India.

Because the situation had the potential for a major disaster, the U.S. National Science Foundation office in New Delhi, headed by Dr. Max Hellman, took action. On the reputation of the Fisk Infrared Institute, I was asked to participate in the three-week infrared course as a consultant to the Government of India, and Fisk went international a second time. Due to the years of experience with the annual institute in Nashville, I was able to organize the lectures and laboratories and actually to give most of the course myself. Again on the reputation of the Fisk Institute, instrument companies loaned or donated supplies worth thousands of dollars, which were shipped by NSF. As one example, Harshaw Chemical Corporation donated a 20-lb crystal of NaCl, which, because of the scarcity of optical quality alkali halide in India, was promptly placed in the department safe.

Despite temperatures of 120°F on the northern plains and 220-V mains that frequently ran at 140 V, the "First All India Special Institute in Spectroscopy" was a real success. The practical value was so well proven that tentative plans were made for similar courses at Kanpur and Bangalore. However, just as the army coup of 1966 in Argentina ended the Latin American adventure, so the India-Pakistan problem brought to an end much of the U.S.-India science cooperation, including plans for future spectroscopy institutes.

Although we have kept the title "Fisk Infrared Institute," the program has been a joint Fisk-Vanderbilt-ORNL effort. The 25th anniversary was celebrated last year with a special two-day program, including the participation of noted spectroscopists from this country and Europe, many of whom had been lecturers in past years. A special recognition was given ORNL, and accepted by Alex Zucker, for the generous role the Laboratory has played in the Institute's 25 continuous years of service to the physics and chemistry community. P. A. Staats, for his personal commitment over these many years, was granted an honorary Sc.D. Fisk University, a small quality southern school with an enrollment of approximately 1500 students, has been able to achieve a truly international reputation in the science community through the help of ORNL. This reputation has been a major factor in maintaining enrollment and academic standards during a period when private colleges were hard-pressed financially.

In research, the reputation of the molecular spectroscopy laboratory under Prof. Silberman, in the Department of Physics, has also become international. All the spectroscopy research by Fisk and Vanderbilt students and faculty is conducted in the Fisk laboratories, and a significant portion is performed cooperatively with the spectroscopy group here at the Laboratory. This work provides material for M.S. and Ph.D. degrees, awarded by Fisk and Vanderbilt Universities. In this relationship, ORNL gains the enthusiasm and stimulation of the many graduate students who spend time at the Laboratory. Today, Fisk is well-equipped for research and attracts good students who undertake challenging, significant thesis problems, and the ORNL-Fisk relationship is now truly one of equal cooperation. We at ORNL look back with pride on this Fisk connection, which indeed has produced a first-rate physics department at the best-known Black university in the South.

Awards and Appointments

Wallace J. McAfee has received the Hetenyi Award, bestowed by the Society of Experimental Stress Analysis upon the author of the outstanding research paper of the year in the field of engineering mechanics.

The University of Tennessee has selected **Charles D. Scott** for its Outstanding Engineering Alumnus Award.

Melvin J. Feldman was elected president of the American Nuclear Society.

The 1975 American Nuclear Society's Special Award for Waste Disposal and/or Management, including an honorarium of $1000, has gone to **E. G. Struxness.**

John Storer has been named director of the Biology Division.

Academic Press's Volume 5 of *Advances in Radiation Biology* lists **Howard Adler** as coeditor.

The *Nuclear Safety Journal,* edited by a staff that includes **W. H. Jordan, Myrtleen Sheldon, Angelyn Puckett, William B. Cottrell, William R. Casto, Edward W. Hagen, Dunlap Scott, Donald G. Jacobs, Frank N. Browder, J. Richard Engel,** and **Joel Buchanan,** received the Award of Merit in the technical journal category of the sixth international publications competition sponsored by the Society of Technical Communication at the annual conference in May.

Robert J. Gray has been appointed associate editor of *Metallography,* a journal of the International Metallographic Society.

Elected to a six-year term on the National Council on Radiation Protection and Measurements is **Chester R. Richmond.**

The American Welding Society has selected a paper written by **G. M. Goodwin, R. T. King,** and **J. O. Stiegler** to receive the William Spraragen Award, given each year for the best research paper published in the research supplement of the Society's *Welding Journal.* The award includes an honorarium.

O. L. Keller is one of nine members of an international committee to consider the claims of priority of discovery of elements 104 and 105. Research groups at Lawrence Berkeley Laboratory and the Soviet Union have claimed to be first in discovering these elements. To resolve the issue, a group of neutral experts has been named by the International Union of Pure and Applied Chemistry (IUPAC) and the International Union of Pure and Applied Physics (IUPAP). The committee consists of three members from the United States, three from the Soviet Union, one from England, one from the Federal Republic of Germany, and one from Switzerland.

Replacing S. E. Beall, Jr. as director of the Energy Division is **William Fulkerson.**

Walter S. Snyder was selected by the Health Physics Society as the 1975 recipient of its Distinguished Achievement Award for his contributions to the field of internal dose calculations, to the establishment of international standards for radioisotope use and exposure, and to health physics research. The Society awarded its Meritorious Performance Award posthumously to **James C. Hart,** president of the Society at the time of his death last year.

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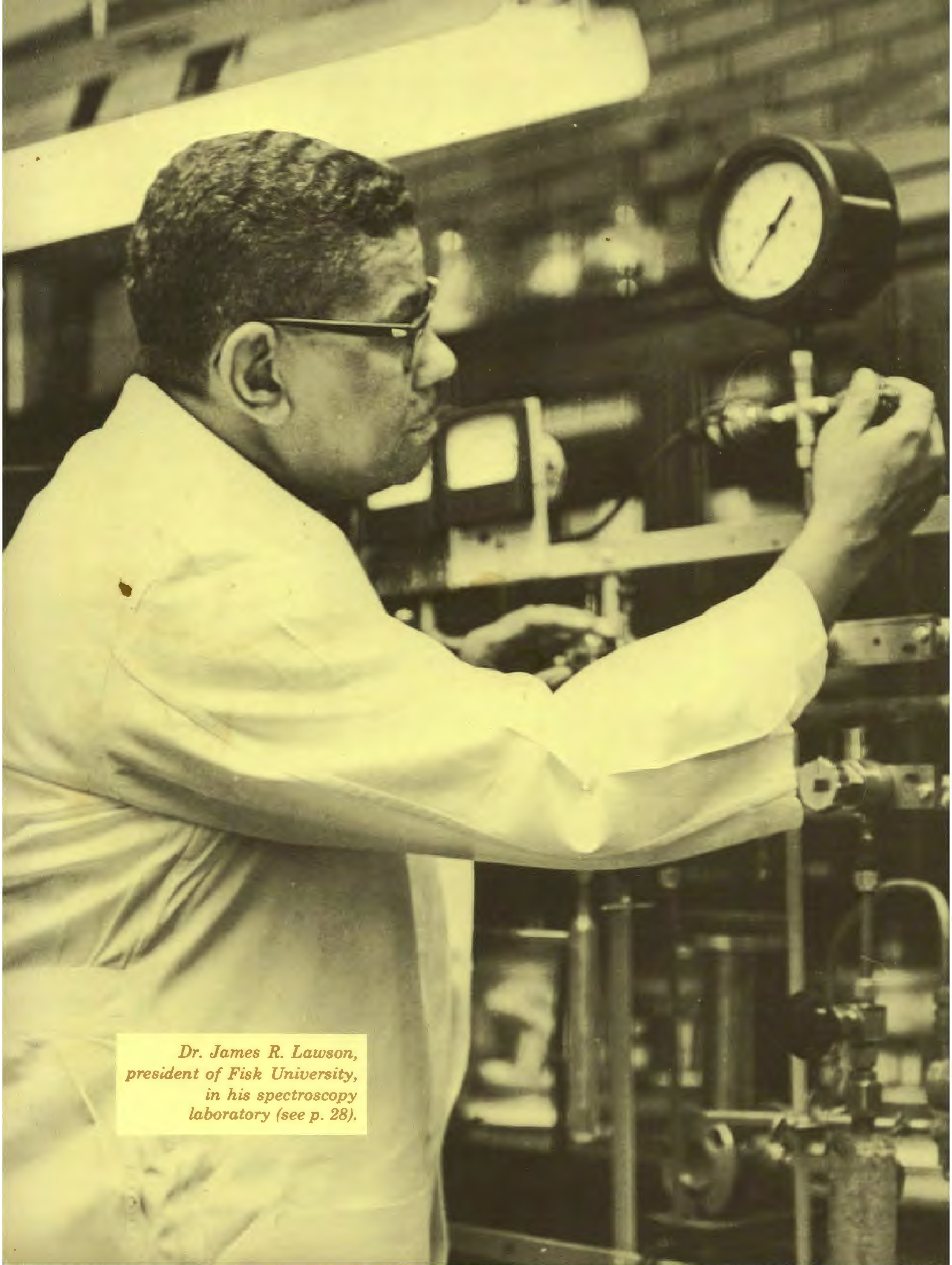

Dr. James R. Lawson, president of Fisk University, in his spectroscopy laboratory (see p. 28).