

Review

FALL

1971

OAK RIDGE NATIONAL LABORATORY





THE COVER: Held aloft is the quartz disc on which the very first beam of the Oak Ridge Isochronous Cyclotron impinged. The point of penetration, which glowed brightly in the beam, is visible here as a smudge of radiation-damaged quartz. See article beginning on opposite page for ORIC's history.

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OAK RIDGE NATIONAL LABORATORY

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In this two-part presentation, Jim Ball and John Pinajian give us a four-dimensional look at accelerator research at ORNL. Jim, who received his doctorate in physics at the University of Washington, offers a lovingly researched history of the Electronuclear Division, reaching back to its beginnings in the electromagnetic separations plant at Y-12. John, whose Ph.D. is from Purdue, takes it from here and projects new capabilities, now in the planning stage, that will open the door to ever more sophisticated work, primarily the study of nuclei far from stability, but perhaps also eventual production of the predicted superheavy elements. Jim joined the Division in 1951 after a year as a fellow with the National Science Foundation at Washington and another year in Oak Ridge as a research associate of the National Research Council. John Pinajian, a nuclear chemist specializing in radioisotope production in the Isotopes Division, first came to the Laboratory as a member of the Electronuclear Research Division in 1956, from the faculty of the University of Tennessee. He has been on the Isotopes Division staff since 1963. The two authors are pictured here at the entrance to ORIC's beam room with Prof. J. H. Hamilton of Vanderbilt University, chairman of the steering committee in UNISOR's Users Group (l. to r., Ball, Hamilton, Pinajian).

The Electronuclear Division

A Backward Look

By JAMES BALL

ON JUNE 1, 1971, the Electronuclear Division and the Physics Division of ORNL were merged to form a new Physics Division. As the term "Electronuclear" disappears from the Laboratory vocabulary, it seems appropriate to review briefly the history of this division with the unusual name.

The Electronuclear Division first joined the Laboratory in 1950 under the name of the Electromagnetic Research Division. Before this, the

division had been part of the Y-12 research laboratory, having originated in a group established at Oak Ridge in 1943 by the University of California Radiation Laboratory to carry on the development of the electromagnetic process for the separation of uranium isotopes.

By the later 1940's, the work of the division had demonstrated clearly the basic limitations of the electromagnetic separation process, and it became obvious that this technique could not



Fabrication of the magnet coils for ORIC in the large bay of Building 9201-2 at Y-12. Construction of the cyclotron, authorized in 1958, was completed in early 1961.

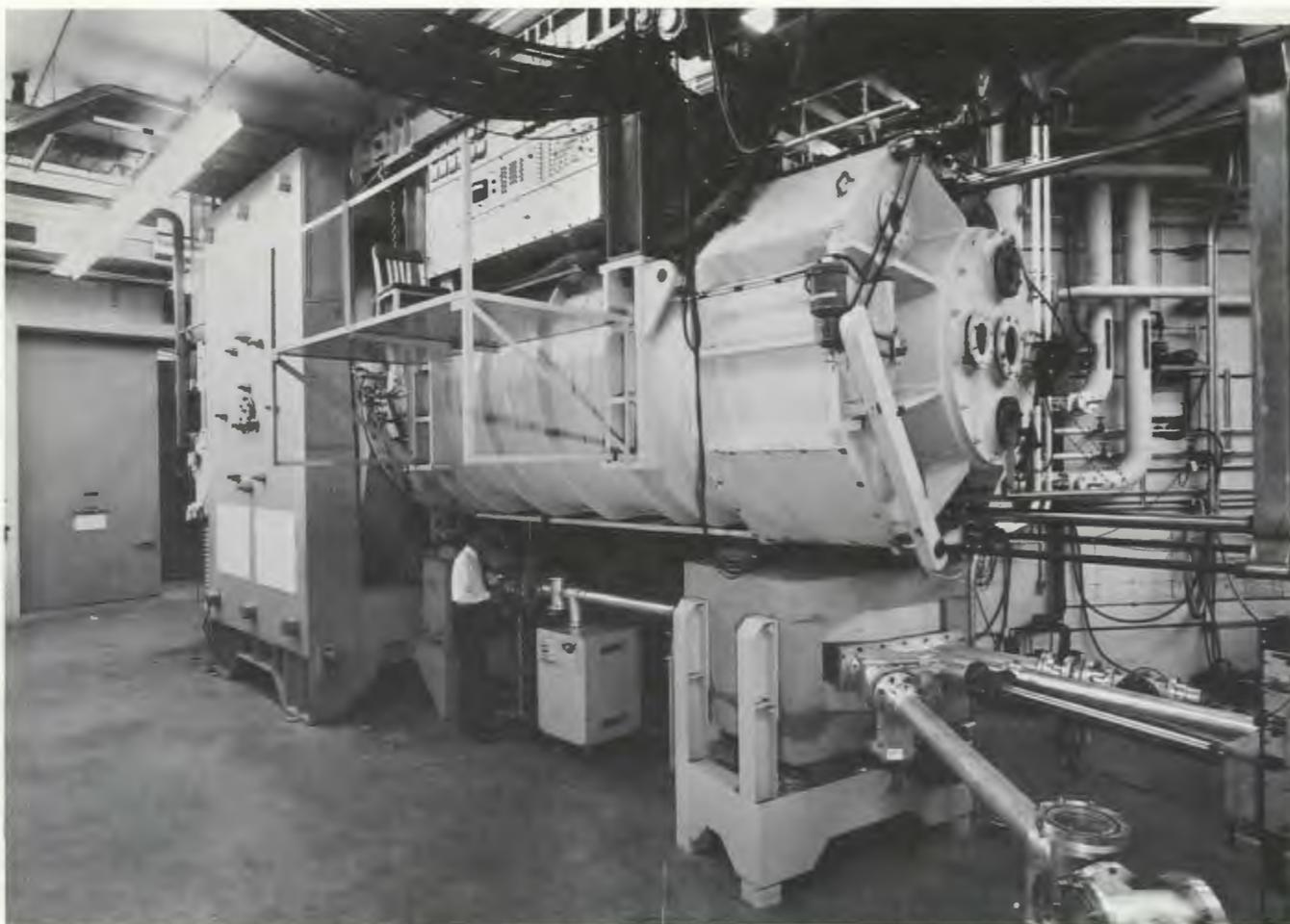
compete economically with the gaseous diffusion process for production of ^{235}U . So the efforts of the division were changed in line with the changing objectives of the Atomic Energy Commission to increase emphasis on fundamental research. The applied research and development work on the "calutron" isotope separator units was supplanted by programs aimed more at understanding the electromagnetic process itself, particularly those involving the formation and motion of ions in electric and magnetic fields. This change initiated a vigorous program of research on particle accelerators, ion sources, and plasma devices.

In 1949, design studies were started for a high-current proton cyclotron capable of proton



E. L. Olson stands beside the giant "dees" of the 86-inch cyclotron.

energies up to 25 MeV. As part of the development of such a machine, a small test cyclotron was built and operated in a modified calutron magnet gap. This 22-inch test cyclotron accelerated protons to about 2 MeV. A later improved version of this machine produced 5-MeV protons. Late in 1949 construction of the large cyclotron, to be called the 86-inch cyclotron, was started in the building at Y-12 that now houses the Thermonuclear Division. During this same period plans were started for a cyclotron to produce large currents of nitrogen ions. This machine would become known as the 63-inch cyclotron and would be built in Building 9204-3 at Y-12. An important part of these projects was the development, based on



Conferring with a mechanic under the completed ORIC is R. J. Jones, who directed the construction and initial operation of the facility.

calutron ion-source experience, of the well-known hollow-anode ion source which became the standard source for cyclotrons throughout the world.

Many of the research staff associated with the Electromagnetic Research Division at the time of transfer to ORNL became familiar names as the Laboratory research programs continued to grow. The 86-inch cyclotron group, under the direction of A. L. Boch, included F. H. Neill, E. D. Hudson, and R. S. Lord. The basic research section included J. L. Fowler, R. A. Charpie, and B. L. Cohen; the electromagnetic fundamentals group included J. S. Luce, R. V. Neidigh, C. F. Barnett, and J. A. Martin; and the 63-inch cyclotron group included A. Zucker and R. J. Jones.

The 86-inch cyclotron achieved initial operation on November 11, 1950, and became the world's first truly high-intensity cyclotron with internal beam currents in excess of 2 mA. The attainment of proton energies above 20 MeV pushed the relativistic limit for fixed-frequency cyclotrons higher (by almost a factor of 2) than

had been done before. This accomplishment stood unique until the advent of sector-focusing cyclotrons in the early 1960's. The 86-inch cyclotron was originally intended for isotope production, in particular the long-lived polonium-208, with the internal beam. At the termination of the polonium program in 1953, design studies were initiated to extract the beam from the cyclotron for nuclear physics research. An extracted beam of 22-MeV protons was obtained in 1955 and used in succeeding years to revitalize interest in medium energy nuclear physics. Among the important contributions from the 86-inch cyclotron were the studies of elastic and inelastic scattering processes and the demonstration of the importance of the direct interaction mechanism for the study of nuclear structure. This machine was transferred to the Isotopes Division on December 1, 1961, to

allow the operating staff to move to the new cyclotron (ORIC) under construction.

The 63-inch cyclotron started up on May 20, 1952. It was the world's first accelerator designed for multicharged heavy ions and supported a pioneering research program in heavy-ion reactions. Almost all aspects of nuclear research were touched upon in this work, many for the first time with heavy ions. The first transfer reactions with heavy ions were observed with this accelerator. The operation of this machine was terminated on October 13, 1961, to enable the operating staff to begin testing of the ORIC.

The Y-12 portion of the Electronuclear Division history is also marked by continued development by the Special Separations Groups. This group, headed by B. Harmatz, included F. N. Case and H. C. McCurdy. In 1951, for the first time gram quantities of ^{236}U with enrichment greater than 95% were separated for experimental research. In 1953, this group achieved the first separation of plutonium isotopes in gram quantities. On July 1, 1957, the activities of this group were transferred to the Isotopes Division.

During the early 1950's, the ion source group became more and more interested in basic plasma phenomena. By 1952 a group on thermonuclear research had been formed. This group continued to expand within the Electronuclear Division until in July 1957, it separated to become the Thermo-nuclear Experimental Division.

In 1952, a series of experiments on radiation damage to materials induced by the 22-MeV proton beam of the 86-inch cyclotron contributed valuable data to the nuclear-propelled aircraft program. In 1953, a study was launched to investigate the development of relatively small and inexpensive nuclear reactor facilities for generating electrical power for special applications. This program, under the leadership of A. L. Boch, developed into a complete design study for a "package reactor" for the Reactors Branch of the U.S. Army. Key members of the group included H. G. Blosser, A. M. Perry, E. E. Gross, F. H. Neill, H. C. McCurdy, and J. E. Mann. In 1955, the Army awarded a contract for construction of the Army Package Power Reactor at Fort Belvoir, Virginia. The Electronuclear Division performed the fuel element assembly testing and criticality experiments. The reactor was completed in about 18 months. The APPR was one of the first reactors to employ the concept of burnable poisons in the fuel plates to extend the life of the core and still

maintain safe control. On October 1, 1957, the activities of the Package Power Reactor Group, which now included advisory and development services to the AEC on several other reactor projects including the Nuclear Ship Savannah, moved to the newly formed Reactor Projects Division.

In 1956, the accelerator design group of the division, working toward the design of a high-energy proton cyclotron, began construction of an electron cyclotron analogue to test the effects of various cyclotron resonances on orbit dynamics. The first beam from Analogue I was obtained in March of 1957. This same year design of a second analogue cyclotron to accelerate electrons to the rest mass equivalent energy was begun under the direction of J. A. Martin. Analogue II was successfully operated on August 4, 1961. Extensive studies with this device established the practicality of acceleration of protons to at least 1 GeV in a sector-focusing cyclotron. Successful extraction of the electron beam from Analogue II was demonstrated in 1962.

The theoretical group of the division made major contributions to the field of nuclear structure research. In 1960, a major step forward was taken in the extraction of quantitative information from single-nucleon transfer reactions: R. M. Drisko and R. H. Bassel wrote the distorted-wave Born-approximation code SALLY in collaboration with G. R. Satchler of the Physics Division. This was later superseded by a more general code, JULIE, which was to become the world standard for extraction of nuclear structure data from direct reaction studies. It was during this time that an optical model search code, HUNTER, was prepared for analysis of elastic scattering data. In 1965 a large shell model program was completed under the direction of E. C. Halbert. This program, the most sophisticated of its kind, has been used extensively for computing detailed nuclear properties and investigating the applicability of the nuclear shell model.

In 1955 the division proposed to replace the 63-inch heavy-ion cyclotron with a more modern heavy-ion machine. In 1957 the AEC approved such a project. Meanwhile, progress with the cyclotron analogue studies had demonstrated that, with sector focusing, acceleration of protons to 75 MeV in a fixed-frequency machine was quite feasible. This advance in cyclotron technology along with an increased interest in precision reaction studies, due in part to the success of the ORNL

reaction codes and experimental studies, led to a re-examination of the cyclotron and a new proposal for a much more versatile machine. In October 1958 Congress authorized construction of the Oak Ridge Isochronous Cyclotron. This machine promised a capability of accelerating many types of particles over a wide range of energies. Construction of the building at the X-10 site was completed in January 1961, and the first testing of ORIC, under limited power, occurred in 1962 with the first deflected beam obtained in June of that year. The accelerator became fully operational in late 1963 with a full range of particle energies demonstrated. The experimental program on ORIC began in 1964. The first heavy-ion beams from ORIC were obtained in 1968, but these were limited in intensity until the development of the Penning source in 1970. This cyclotron is now one of the most versatile in the world with a large variety of both light- and heavy-ion beams available for nuclear physics and chemistry research. In late 1970 the ability to accelerate polarized protons and deuterons was added to ORIC to increase its flexibility still further.

In 1967, part of the accelerator engineering group under R. E. Worsham, in collaboration with members of the Physics Division, started to apply techniques developed in modern accelerator research to the problem of designing and building a high resolution electron microscope. The first of these microscopes is now nearing completion.

In 1970, as an outgrowth of discussion of possible research collaborations on a proposed new heavy-ion accelerator (APACHE), a group of Southern universities joined to form a users group to study short-lived isotopes with an on-line isotope separator at ORIC. The group raised funds from a number of universities and colleges to purchase a commercial magnetic separator, and negotiated with the AEC for matching funds for operating and equipment budgets. This facility, to be called UNISOR, marks a new phase in collaboration between the Laboratory and University scientists.

During its history, the Electronuclear Division took an active organizational role in many conferences. Included are the first Conference on Reactions between Complex Nuclei (Gatlinburg, 1958), the first Conference on Sector-Focused Cyclotrons (Sea Island, 1959), an International Conference on Nuclear Physics (Gatlinburg, 1966), and the IEEE National Accelerator Conferences. As a service to the accelerator community, the division has for a

number of years maintained an accelerator information center under F. T. Howard.

Electronuclear Division Chronology

- 1948: Electromagnetic Research Laboratory formed. First isolation of gram quantities of ^{234}U .
- 1949: 44-inch cyclotron successfully operated. Construction begun on 86-inch cyclotron.
- 1950: Joined ORNL as Electromagnetic Research Division. 86-inch cyclotron achieved initial operation. First isolation of gram quantities of ^{236}U .
- 1951: Construction begun on 63-inch cyclotron.
- 1952: 63-inch cyclotron achieved initial operation.
- 1953: Name changed to Electronuclear Research Division. Start of Package Reactor program. First gram quantities of plutonium isotopes separated.
- 1954: Conceptual design for Army Package Power Reactor (APPR) completed.
- 1955: Deflector installed in 86-inch cyclotron to provide 22-MeV protons for nuclear physics experimentation.
- 1956: Construction begun on electron cyclotron Analogue I.
- 1957: Successful operation of APPR achieved. Analogue I operational.
- 1958: Construction of Oak Ridge Isochronous Cyclotron (ORIC) approved.
- 1959: Construction begun on ORIC. Construction begun on electron cyclotron Analogue II.
- 1961: Analogue II operational.
- 1962: First deflected beam from ORIC. Electron beam extracted from Analogue II.
- 1963: ORIC operational under full power.
- 1964: Nuclear physics research begun at ORIC.
- 1967: Electron microscope program initiated.
- 1968: First heavy-ion beams at ORIC.
- 1970: Successful operation of polarized particle source on ORIC.
- 1971: UNISOR project approved. Electronuclear and Physics Divisions combined to form new Physics Division.

Perhaps two factors, in particular, distinguish the 20-year history of the Electronuclear Division. The first is the high degree of cooperation and professional collaboration between the technicians, engineers, and scientists who have comprised the professional staff. The degree of interchange between the different disciplines involved in designing, fabricating, and performing research with the accelerators provided a unique and stimulating atmosphere to those present. The remaining factor was the wise counsel and guidance provided by R. S. Livingston, who served as sole director of the Electronuclear Division from its inception until June 1971, when he joined the Director's Division of ORNL.

Unisor:

University Isotope Separator at Oak Ridge

A Look Forward

By JOHN PINAJIAN

THE BIRTH OF UNISOR is related to the proposal for a new heavy-ion accelerator, dubbed APACHE (for Accelerator for the Physics and Chemistry of Heavy Elements), for ORNL. Universities in the region, as part of their active participation in this project, held a University Users Group meeting in March 1968 at which a steering committee was formed. In November 1969, the chairman of the committee, Prof. J. H. Hamilton of Vanderbilt University, inquired of the university people about the possibility of an on-line isotope separator for ORIC. With such a device nuclei far from stability could be produced on a continuous basis, opening new areas in nuclear physics research. Indeed, of the approximately 5000 theoretically possible nuclei, less than 1900 have thus far been identified, and of these less than half have even reasonably well known level properties. Having received enthusiastic support earlier from the Tennessee governor's office for the APACHE Users Group program, Hamilton approached the governor's office again at the turn of the year for support of the ORIC on-line separator concept and again was successful.

Early in 1970, R. S. Livingston and Alex Zucker, director and associate director of the Electronuclear Division, were approached, and the groundwork was laid for the unprecedented approach. It was the first project, to my knowledge, to which universities, a state government, a federal agency, and a national laboratory each combined to contribute significant capital funds and operating expenses.

It was becoming increasingly apparent that an isotope separator on line with ORIC could be one of the best facilities in the United States for research in tailor-made nuclei far from the stability line. For during this time the Electronuclear Division had developed beams of carbon, nitrogen, oxygen, neon, and argon in ORIC at energies up to 10 MeV per nucleon. Heavy ions offer many advantages in producing nuclei far from stability. With enriched targets and choice of ion beams and energy, one can select the desired nuclei and the mass chains. Heavy ions bring into the target nucleus large linear and angular momenta. Thus high spin states can be reached which are inaccessible by other means. In addition, large linear momenta give rise to nuclear recoil, providing a quick way of getting reaction product nuclei out of the target. With this device, the researcher is not limited only to volatile elements. Nor is he hampered by too short a half-life in the diffusion of the product in the target. Indeed, heavy ions present an opportunity to produce short-lived nuclei more than 10 neutrons off the stability line.

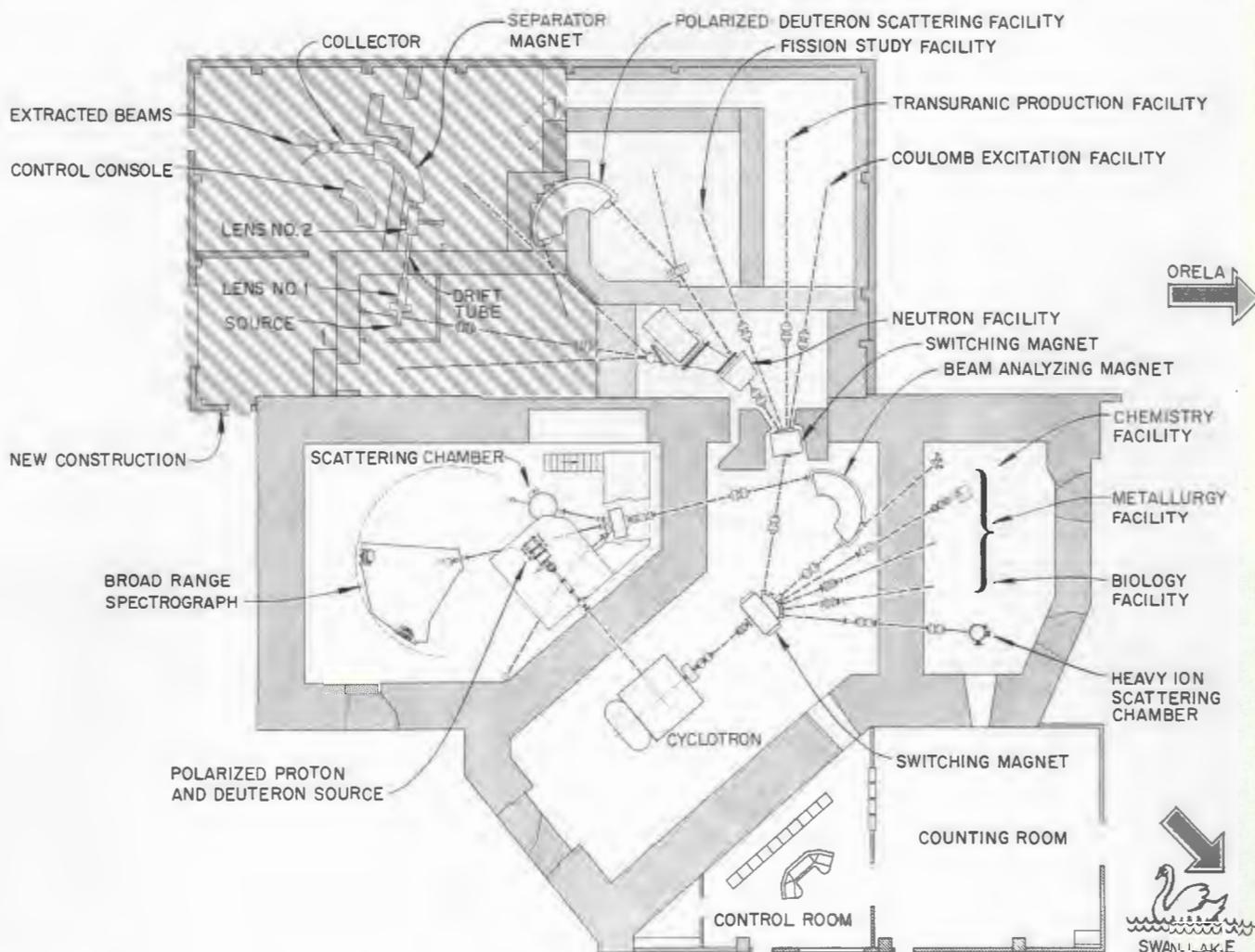
The first of a series of bimonthly meetings at ORNL of the representatives from the Southern universities was held in February 1970, and, in rapid order as excitement grew over the possibility of opening new areas in nuclear physics, committees were organized to draft a proposal, draft bylaws and a charter for the new organization, examine other on-line separators, and formulate a plan of action. Members of the group attended the conference in Leysin, Switzerland, on studies of Nuclei Far from the Region of Beta Stability and in Marburg, Germany, on Isotope Separators. In addition, visits were made to all of the European laboratories where isotope separators were being operated on line. Reports of the initial planning of the ORNL separator project have been given at a separator conference in Israel and at the Soviet Nuclear Spectroscopy Conference in Moscow.

Hamilton continued his search for support for the project. His efforts, in combination with those of physicist William M. Bugg, of the University of

Tennessee faculty, succeeded in getting the State of Tennessee to commit \$90,000. Commitments were sought for operational funds for a period of five years to assure the continuity needed for successful operation, as well as capital funds from the universities. As these commitments were acquired, the administrative portion of the work was turned over to Oak Ridge Associated Universities, who was to act as fiscal agent. An executive committee was formed with representatives of founding institutions as well as ORNL and ORAU. The AEC, through its Division of Research, contributed 60% of the initial capital and first-year cost of operating UNISOR. The AEC participation includes \$212,000 for construction of an addition to the ORIC building to house the isotope separator and for interfacing it with the beam, \$100,000 for capital equipment, and \$70,000 toward UNISOR's first year of operation. The sponsoring

universities to date have committed themselves to \$58,000 per year for five years. The DANFYSIK organization in Denmark is to supply the separator and an extracted beam station at a cost of \$147,000. An index of the excitement and enthusiasm may be gathered by the fact that one of the quadrupoles for the beam transport system and a 2000-liter-per-second pumping station have been ordered. In addition, much electronic equipment has been ordered for use on the project. To date the founding institutions include Emory University, Furman University, Georgia Institute of Technology, Louisiana State University, Tennessee Technological University, University of Alabama at Birmingham, University of Kentucky, University of Massachusetts, University of South Carolina, University of Tennessee, Vanderbilt University, and

Diagram of the foundation of Building 6000, showing proposed addition to the cyclotron, supported by the Universities Users Group.



Virginia Polytechnic Institute and State University. The Oak Ridge Associated Universities, under the leadership of W. G. Pollard, assumed an active role as a contributing institution in addition to its role as fiscal agent.

A full-time staff of one Ph.D. and one technician will coordinate the work. The separator is to be delivered in late 1971, and the building will be completed in January 1972.

The combination of an isotope separator and heavy-ion cyclotron would constitute the largest such program in the United States and is particularly well adapted to the needs of a university users group. A large quantity of data can be accumulated

in a relatively short time. Data reduction and interpretation can then be carried out by the users at their own universities. The only other major on-line separator program with a heavy-ion accelerator is at Dubna, USSR, where interest lies chiefly in the light elements, helium and neon, and the transuranic region.

Among the areas of research planned by the UNISOR groups are the exploration of new regions of deformed nuclei; further development of our understanding of the energy-level structure of transitional regions between deformed and near-spherical nuclei; examination of single neutron or proton states in nuclei far from stability for

The overall responsibility of overseeing the operation of UNISOR lies with the Executive Committee which also handles the appointments of personnel, expenditure of funds, and appointments of committees such as the Scientific Committee, Program Committee, and Technical Committee. At present, the Executive Committee is composed of

- J. H. Hamilton, Vanderbilt University,
Chairman;
- F. T. Avignone, University of South Carolina;
- W. M. Bugg, University of Tennessee;
- J. L. Duggan, Oak Ridge Associated
Universities;
- R. W. Fink, Georgia Institute of Technology;
- K. J. Hofstetter, University of Kentucky;
- J. A. Jacobs, Virginia Polytechnic Institute
and State University;
- R. S. Livingston, Oak Ridge National
Laboratory;
- A. R. Quinton, University of Massachusetts;
- E. L. Robinson, University of Alabama at
Birmingham;
- E. F. Zganjar, Louisiana State University;

with one additional representative-at-large to be elected from Furman University, Emory University, or Tennessee Technological University.

The Technical Committee, charged with the design and purchase of the initial capital equipment, currently comprises

- C. R. Bingham, University of Tennessee,
Chairman;
- C. E. Bemis, Oak Ridge National Laboratory;
- E. Eichler, Oak Ridge National Laboratory;
- R. W. Fink, Georgia Institute of Technology;
- M. A. Ijaz, Virginia Polytechnic Institute and

State University;

- W. D. Schmidt-Ott, Georgia Institute of
Technology;
- K. S. Toth, Oak Ridge National Laboratory.

This Committee, having set the specifications for the separator and evaluated the bids, has moved on to other technical areas, for example, the task of drawing up specifications for a computer-based multi-parameter system which will be tied into the present computer at ORIC, but will be able to operate independently as well. Indeed, as Toth and his group keep close watch on the actual construction and Bingham and Eichler are working on the multi-parameter system, Bingham, Schmidt-Ott, and Toth are concerning themselves with the problem of getting a beam to the ion source and Fink and other members of the group are concerned with an ion beam extension, i.e., a 3-meter station which will receive an extracted beam and deposit it on a moving tape transport system.

The organization as a whole and the committees individually are active and viable. The committees draw on skills and background of the membership at large, and subcommittees are quickly formed as the need arises. In any undertaking of this size there is a substantial amount of hard and apparently unrewarding work. The most necessary and important chore of a temporary secretary to the meetings, for example, was undertaken by R. L. Robinson of Oak Ridge National Laboratory until it could be turned over to the ORAU organization. There are many others contributing to the program. The success of the program is a reflection of the unstinted efforts of the membership and the air of cooperation existing in the entire organization.



UNISOR Members

UNISOR representatives at last summer's meeting here.

Standing, left to right, are:

<i>J. Lin</i>	<i>Tennessee Tech</i>
<i>R. Sayer</i>	<i>Furman University</i>
<i>W. Pollard</i>	<i>ORAU</i>
<i>C. Bingham</i>	<i>University of Tennessee</i>
<i>M. Ijaz</i>	<i>Virginia Polytechnic Institute</i>
<i>F. Avignone</i>	<i>University of South Carolina</i>
<i>E. Zganjar</i>	<i>Louisiana State University</i>

Seated, left to right:

<i>J. Duggan</i>	<i>ORAU</i>
<i>A. Quinton</i>	<i>University of Massachusetts</i>
<i>R. Fink</i>	<i>Georgia Tech</i>
<i>E. Robinson</i>	<i>University of Alabama</i>
<i>W. Bugg</i>	<i>University of Tennessee</i>
<i>J. Hamilton</i>	<i>Vanderbilt University</i>

theoretical calculation of nuclear shapes, masses, and properties of superheavy nuclei; production of high-spin isomers which may provide important new radioisotopes for application in nuclear medicine and industry; improvement in our understanding of how heavy nuclei are generated in stars and the prediction of the properties of these new elements; study of heavy particle radioactivity; and the determination of cross sections and production rates of various nuclear reactions that may offer

new insight into the structure of the nucleus and mechanisms of heavy-ion reactions.

The areas that can be developed are not limited to nuclear physics. Indeed, solid-state physics will benefit. The decay of the nuclide implanted in a crystalline structure results in perturbation of the directional correlation of the gamma rays by the internal field gradients. Thus one can get information on solid state aspects of the crystalline material. At present, one of the major obstacles in the application of radioisotopes in nuclear medicine is the presence of other radioactive isotopes of the same element. Advanced electronics and computer data reduction enable medical workers to circumvent these problems in part. The on-line separator will enable workers to produce individual radioisotopes tailor-made to their needs.

The fact that this project has been funded in these days of reduced budgets for both the universities and the Atomic Energy Commission, while most people are concerned with retrenchment policies, is a major indication of the strong scientific merit of the proposal showing foresight and imagination on the part of the initiators. It shows that a good idea can still receive attention. The project, by providing forefront nuclear research, will strengthen the nuclear research effort in the region.

AMW COMMENTS

Herceg-Novi is a lovely resort on the Adriatic Coast of Yugoslavia, a few miles south of Dubrovnik. Early in July some 200 scientists, engineers, philosophers, lawyers, social scientists, writers, and artists — from East and West — gathered there for a week to talk about Science, Man, and the Environment. This was the fourth conference in the series "Science and Society" sponsored by the Yugoslav Federal Council for the Coordination of Scientific Activities. These conferences grew out of similar meetings we once held in Oak Ridge, and usually Oak Ridgers, from both ORNL and ORAU, help to organize them.

The world has discovered the environment with an urgency that would have been hard to predict only a few years ago. Prior to the 1971

conference, I had little opportunity to learn what Eastern Europe was thinking about the environment; my impression had been that the West was much more concerned about these matters than was the East. After all, as one Marxist speaker at the conference suggested, isn't environmental pollution an exclusive diseconomy of the capitalist world, a world dominated by profit-seeking individuals intent on maximizing their personal gain? But this is obviously not the case: the West, being the more highly developed and more responsive to open social criticism, simply has appreciated the problem earlier and more sharply. There is plenty of pollution in the industrializing East, as well as the West; the Herceg-Novi Conference was notable for being one of the first at which intellectuals from the socialist world took hard notice of the environment. The naive idea that production alone is sufficient, whether in capitalist or collectivist states, was realized even by the doctrinaire Marxists to be false.

A conference of this sort — ranging very broadly from mercury in birds' feathers to the sociology of the environment, from population to science policy, from discussion of participatory democracy to the safety of nuclear reactors — is bewildering and

diffuse. The participants brought to the conference many different views, many different degrees of sophistication. After a week of such broad and diverse discussion I asked, *What did I learn?*

I learned, first, that most of us at ORNL are "Jacobins" — this is the term Professor Paul Meadows (a sociologist who spent a few weeks at Oak Ridge some years ago) used to characterize those optimistic technologists who believe basically in the infinite perfectibility of human institutions and of the human condition through science (both hard and soft). We have a problem with the environment? O.K., we shall solve the problem by the use of human rationality — this view harks back to the Jacobins who started the French Revolution. Such a view is contrasted to that of the Malthusians, who are pessimists — about population, about resources, about institutions.

I learned, second, that — despite our protestations to the contrary — the environment is not a central issue for the underdeveloped countries. This is nothing new. But at meetings such as the one at Herceg-Novji, there are generally very few representatives of the overpopulated, underdeveloped Asian and South American countries. To them, technology in its crudest sense — as a means of increas-

ing production, of outwitting Malthus — remains primary. Environment is something that comes second.

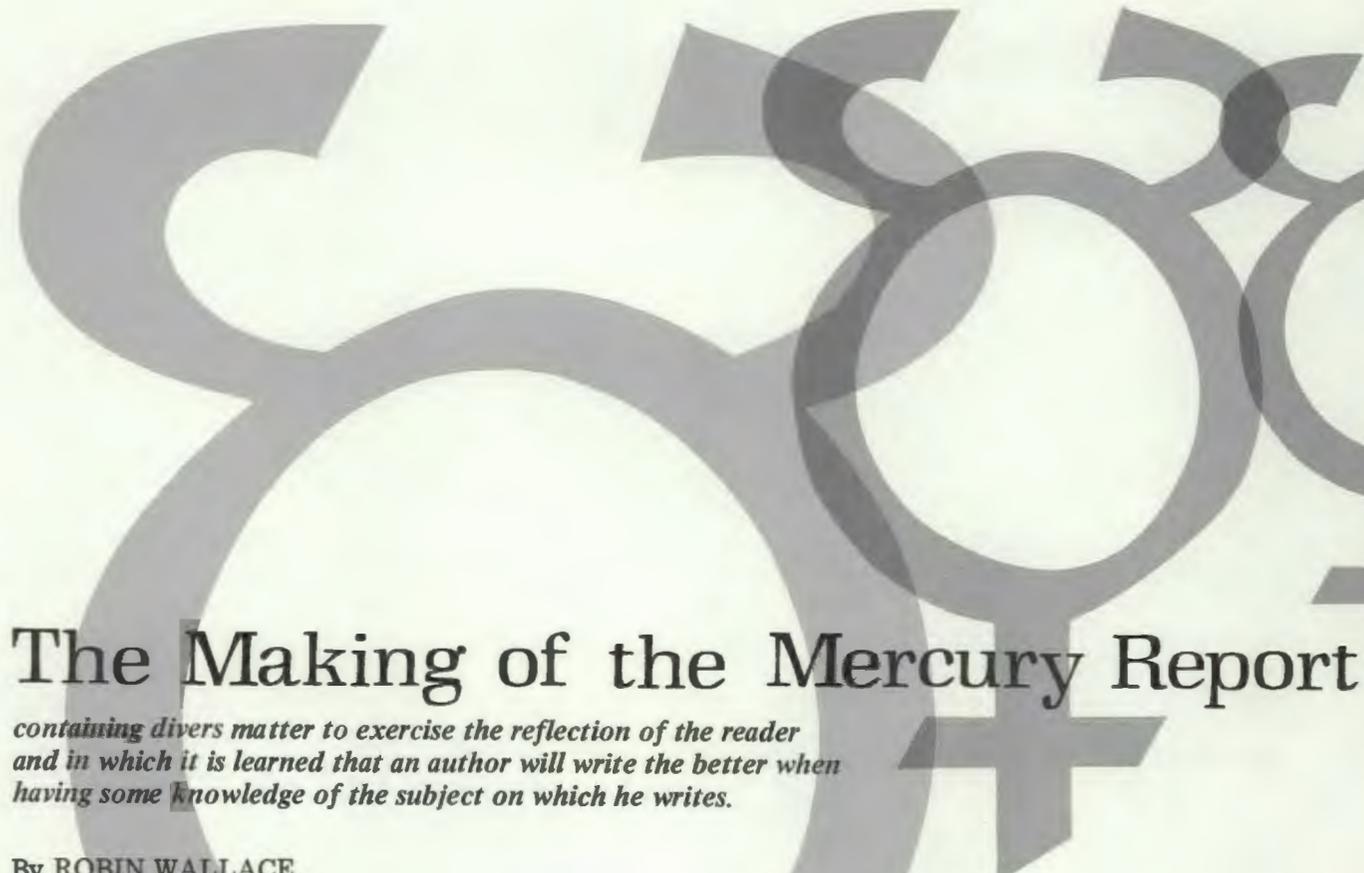
I learned other things — that technology and science are indeed on the defensive, that many persons are struggling with the question of how the people can participate more fully in decisions as to environmental impact, that places like ORNL do have an enormous contribution to make in integrating the broad social elements of the environmental problem with the hard technological ones. I even learned how remarkably obscure dialectical Marxist philosophers (of whom there were many at the meeting) sound — how the abstract idiom in which they revel carries almost no information to Westerners like me.

But most important was the feeling that this conference engendered of a time of profound change in our basic human ethic. The idea that nature is for man and is to be exploited for man without regard to the side effects of such exploitation — this basic view was denied by the conference. Great transitions in man's perceptions and roles usually occur gradually and without fanfare. Occasionally a conference marks such a transition — as the 1927 Solvay Conference which established the Copenhagen interpretation of

quantum mechanics. The Herceg-Novji Conference, along with the June 1972 United Nations Conference on the Human Environment, scheduled in Stockholm, and other similar conferences perhaps will owe their full significance not to the actual information that exchanges at them, but rather to their symbolism: they mark this great transition in man's thinking toward the new environmentalism. But environmentalism will not be easy — it will require tough, specific choices as the situation at Herceg-Novji attested.

Adjacent to the conference was Hotel Topla which was crowded with many young tourists, mostly from Germany. Every evening they danced to a Yugoslav rock and roll band — played through that inhuman technological invention, the rock and roll amplifier, which seemed to put out at least a kilowatt of pure sound! So we didn't sleep very well at Herceg-Novji — but we were indeed treated firsthand to an example of the deleterious side effects of technology. Oh, what I would have given for a neat technological fix — ear plugs that work, or even a shillelagh that would do the amplifier in!

Alvin M. Weinberg



The Making of the Mercury Report

containing divers matter to exercise the reflection of the reader and in which it is learned that an author will write the better when having some knowledge of the subject on which he writes.

By ROBIN WALLACE

I HAVE BEEN ASKED to write about the making of the Mercury Report, since this document was not only the first official publication of the ORNL-NSF Environmental Program, but also was the recipient of (to me) a surprising amount of attention throughout the country. [As well as in the United Kingdom, the Netherlands, Guam, Saipan, Samoa, and the Virgin Islands. — Ed.] What is expected here, I suppose, is a trenchant documentation of the orderly and tedious progression whereby a circumspect assessment is coldly made: (1) Identification of the Problem; (2) Securing the Available Information; (3) to (∞) Etc. and So Forth. Perhaps when everything is sifted out, something approaching this classic sequence actually did occur; but somehow the spurts and pratfalls have always seemed more real to me. Along with the Mock Turtle, I feel that mathematics and much of everything else is composed of a good share of ambition, distraction, uglification, and derision. So, rather, a personal tour seems more appropriate.

But first allow me to set the stage. I have been cloistered for the past several years, quietly prob-

ing the biochemical intricacies that govern the growth of oocytes, or female germ cells. My colleagues in this endeavor around the world can probably be counted on the fingers of one hand, a situation which has alternately seemed to me to reflect an unconscionable neglect or a delightful lack of competition. When not thinking about oocytes, I have concerned myself and bored my friends about "The Environment," a vague, all-encompassing abstraction invariably defiled, of course, by others. It was thus when the 1970 Environmental Summer Study Program was initiated that Tom Thomas invited me to join his Environmental Indices group in what I suppose was a gesture of "put up or shut up." The point of all this is that I did not know and probably still do not know very much about mercury: my familiarity with the metal extended to the use of home and laboratory thermometers. If this is somewhat disconcerting, read on: there is more to come.

Tom's group was fun, beginning with pizzazz and pizza at the Back Door and continuing with weekly meetings at someone's house with beer, pretzels, and other items considered *de rigueur* for,

Robin Wallace, shown here in the quadrangle of Trailer City, headquarters of the ORNL-NSF Environmental Program, is really a biologist, and has been on the staff of the ORNL Biology Division since 1963. With a Ph.D. in zoology from Columbia University, he spent a year in Ottawa, before coming to Oak Ridge, as a visiting investigator with the National Research Council of Canada. He has spent many summers at Woods Hole with the Marine Biological Laboratory, and is a member of the MBL Corporation. He comes by his interest in the NSF Program through his activities in the Environmental Action Committee of Oak Ridge, but he has also taken active part in local politics, Scientists and Engineers for Appalachia, and, for recreation, the White Water Club. His best seller, at one time available at the Laboratory under the distinguishing letters ORNL-NSF EP-1, is now in reprint form and can be obtained from the National Technical Information Service, U.S. Department of Commerce, 5285 Port Royal Road, Springfield, Virginia 22151. Printed copy, \$3.00; microfiche, 95¢.



shall we say, "open discussion." I was only a part-time member of the group and so escaped most of the daily paper work and what seemed like an endless succession of conferences endured by the regular members of the Study Program. But as the summer progressed, I had some difficulty defining a role for myself, and from this frustrated and limited perspective the Study Program as a whole gradually seemed to acquire all the aspects of a Parkinsonian climax-bureaucracy: a mill of discourse, deliberations, and memo-passing that had become completely self-sustaining, no longer in need of any informational input. Francis Galton, I recalled, once calculated the relative longevity enjoyed by the clergy and the Royal Family because of the extra prayers offered up on their behalf. He came out with a negative number. I also began to sulk about the relative benefits derived from the extra discussions and effusive paper work offered up to The Environment.

Nevertheless, by some inevitable and evolutionary process a focus was invisibly being drawn, and eventually the question of "tolerance limits" came up one evening. I belatedly realized that here was the crux of many environmental and public health considerations. Such standards are invariably entangled in a web of countervailing vectors that include a perceived environmental degradation or health hazard, economic considerations, politics,

possibly a dash of public outcry, and only a limited number of facts possessed by those who are trying to make a sincere evaluation of the problem. I developed the idea of taking, say, three substances that are artificially introduced into the environment and documenting how difficult it is, for a variety of reasons, to arrive at a "tolerance limit" or "standard." I finally chose:

Mercury, because it exists in numerous chemical forms, some of which seem infinitely more toxic than others, giving rise to the question, Does it make sense to talk about "ppm mercury"?

Sulfur dioxide, because a bona fide synergistic effect had been documented for sulfur dioxide and benzopyrene together, leading to another question: How valuable is it to establish a tolerance limit for sulfur dioxide alone?

And coliform counts in water as a measure of fecal contamination. Here the problem takes on another perspective since most American communities, even when discharging "raw sewage," will at least douse the effluent with chlorine. This, of course, will generally kill the coliform organisms and any associated bacterial or amoebic pathogens, but the effect on viral pathogens (such as hepatitis and polio) is less certain. Thus, in the developing countries where bacterial and amoebic pathogens are endemic, coliform counts make some sense, but are they an adequate and accurate indication of the

safety of our North American rivers and lakes, in which viral pathogens may play a more important role?

I did not know the definitive answer to any of these rather elementary considerations; I simply wanted to take a look at the problems involved and then write up three short but integrated reports.

And so, with more than half the summer gone by, I started out on mercury. A brief brush with several undigested facts provided a quick portrait, and as a final touch I considered whether a "buzz-word" summary would be appropriate. This is an exercise in official cant, developed into a well-defined discipline (see Betty Zisk in the *Western Political Quarterly* for March 1970), that has enjoyed a recent vogue among writers of government and scientific reports. My initial construct was not bad, I thought; it served to obscure my fundamental ignorance of the subject and at

BASIC RULES FOR "BUZZ-WORD" CONSTRUCT

Assemble alphabetically two columns of adjectives and one column of nouns, with each column containing ten words. The words should pertain only in a general way to the matter at hand and be chosen from among the current buzz-words for a given field. Working phrases are then obtained by proceeding through a table of random numbers, three at a time. With a sprinkling of verbs and prepositions here and there, the working phrases can then be developed into a construct worthy of the finest government report or research proposal.

- | | | |
|-----------------|--------------------|-----------------------|
| 0. comparative | 0. dynamic | 0. assessment |
| 1. complex | 1. ecological | 1. diseconomy (ies) |
| 2. explicit | 2. economic | 2. hazard(s) |
| 3. hypothesized | 3. empirical | 3. impact |
| 4. quasi- | 4. environmental | 4. indicator(s) |
| 5. relevant | 5. longitudinal | 5. insult(s) |
| 6. significant | 6. operational | 6. inter-relationship |
| 7. similar | 7. policy-oriented | 7. paradigm(s) |
| 8. tentative | 8. social | 8. program(s) |
| 9. valid | 9. systematic | 9. variable(s) |

EXAMPLE. The hypothesized environmental impact (343) of mercury as judged by explicit operational indicators (264) necessitates a comparative longitudinal assessment (050) of the problem within the near future. However, a valid empirical paradigm (937) is required for the complex and dynamic variables (109) involved before either the quasi-systematic diseconomies (491) of the recycle market or the relevant ecological hazards (512) of various mercurials are understood.

the same time contained about the right amount of misty profundity. None of it made much sense, of course, and my minimal efforts served simply to gnaw at whatever feelings of responsibility I tenuously possessed. A discussion with Tom, a solicited review (with elaborate criticism) of a journal article, several (perhaps more) martinis, and a touch of the flu were then added to the ferment and eventually helped brew a desire to go back and obtain additional information.

The Environmental Mutagen Information Center is a cluttered and cozy two rooms in Building 9207, brimming with computer cards, file cabinets, books, journals, Senate Hearing Reports, and seemingly endless stacks of miscellaneous articles, all efficiently tended by John Wassom, note- and article-clipper nonpareil: The Center is only one floor below my lab, so I casually dropped in and asked John if he had any specials on mercury. He pulled off the shelf a notebook stuffed with clippings taken from Eastern and Southeastern U.S. newspapers and magazines over the preceding six months and on which was written "The Strange Case of Mercury." He then piled on top of this notebook a file drawer full of articles, three books, several monographs, and a Senate Hearing which touched on mercury fungicides and other pesticides. I became immediately apprehensive (depressed?) about this informational overkill, to say the least. Warily, I carted the pile back to my office, and there I began to read.

And read some more.

What followed next is difficult to document. I was drawn into an entirely new experience as one intriguing aspect after another unfolded with each page I turned and with each new report I chased down. I can estimate the amount of coffee drunk, the cigars smoked (an environmentalist yet!), the hours of sleep not slept, but the exact process whereby three weeks later I emerged from my burrow with a 60-page report and a much-harried secretary must remain somewhat of a black box. As a last move, I slapped a cover page in the typewriter and, assured that no one would read it anyway, poked out in my best two-finger style, "all you may ever want to know about MERCURY. . . not for external distribution," and appended the name of the Environmental Indices Group. So with some satisfaction that I had at least pulled my own weight, I handed my report in to Tom, said good-bye, and caught up on some sleep. That was it as far as I was concerned, since I had to prepare an invited paper on oocytes for an im-

minent meeting, and my sojourn with mercury (or any other environmental additive) was ended.

Or so I thought.

It must have been the title that intrigued someone into actually reading the report. It was apparently passed on and Xeroxed and circulated and reproduced again until in a pyramidal ascent through an invisible network it ultimately surfaced, despite the caveat on the cover, at many points in what can loosely be described as "official circles." There were still some glaring omissions in the report, a slipshod treatment of several matters, and certainly nothing really new or original. As a plain matter of fact, its pU value was distinctly high at this point in time. It apparently turned up, however, at an hour when a harassed officialdom needed what I like to feel was a reasonably objective evaluation of the available information. Bertrand Russell once classified scientists as being either muddleheaded or narrow-minded. Perhaps this report, about a specific topic, written for the "intelligent layman" and employing a minimum of technical terms, contained a certain hybrid vigor of muddle-minded narrow-headedness.

Regardless, the principal actors were now moving across the ORNL stage. Bill Fulkerson and his Materials and Recycling Group had independently prepared an excellent study concerning the flow of mercury through U.S. society. Decisions were also made to bring some of the considerable expertise at ORNL to bear upon mercury abatement problems. Two reports on mercury were thus out, the subject was definitely in, and it was now time to get down to work and tackle the problem.

Feedback loops are always very important and in this case not only made me very aware of shortcomings in the initial document, but also

pU , equal to $-\log_{10}U$, has been defined by Pardee (Amer. Scientist 50: 130A, 1962) as the "Uniqueness Coefficient" and serves as a measure of Uniqueness (U) of a contribution. Research on this matter (loc. cit.) has indicated that the simple empirical relationship

$$pU = N$$

can generally be applied, where N is the number of investigators who simultaneously publish a similar article. Thus, if two other authors publish on the same subject, one's own article is only 1/100 as unique as if it were truly unique. It should be noted that as $N \rightarrow \infty$, pU becomes identical to the Unimportance Coefficient (pUg).

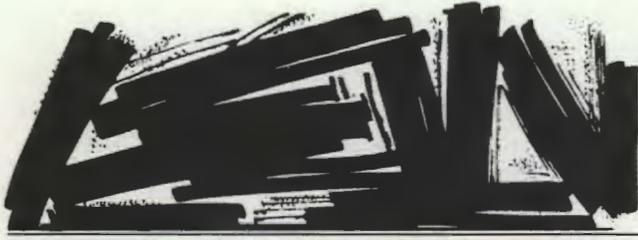
motivated me to put it into some definite shape. I also had become known by this time as The Mercury Man to many people, the most important of whom were the librarians. Through their fingers much information passes, and this was now helpfully sent on to me. Jack Gibbons encouraged us to add into the assessment some of the information on societal flow, and excellent sections on chemical analyses were contributed by Dub Shults and Bill Lyon. Another intense three to four weeks yielded the final product, which was then carefully tended in January by Bill Colwell in the Graphic Arts Department (who drew up an original cover) and by a highly proficient editorial staff (who patiently handled all the oddball titles and offbeat quotes).

In retrospect, only one suggestion of mine was not accepted, so I mention it here for perhaps future consideration. I wanted the author to be John (or Jane, lest we be accused of male chauvinism) Ornl, our instant expert on just about anything. Since, in reality, so many different people contributed to the final report on mercury, and presumably an ever-changing flux of personnel would handle future evaluations, it seemed appropriate to establish a figurehead around which we could all rally, as well as one with whom at the same time we could be identified.

Also in retrospect, I remember that sunny winter day I left Trailer City for what was to be the last time. I had just gone through one of the more frenetic experiences of my life, and I felt in some respects like the poor derelict who several years back was peacefully sleeping by a curb in New Orleans when a huge street cleaner rumbled up, swept him into its churning bowels, and ultimately disgorged him several blocks down the street, apparently not too much the worse for wear. At the time of my leaving, his experience symbolized to me much of the modern condition, and I looked forward to the anticlimactic obscurity of oocyte research.

The mercury assessment was, of course, just a very small part of the Environmental Program. Its activities over all were and still are strenuous and many-faceted, and a good number of hard-working people are right now moving the Program down the road. And as I now watch the dust on the horizon, the value of my experience has begun to dawn. The chance to talk and argue with, and, yes, even to pass memos to, people with so many perspectives and perceptions provided a rare exhilaration.

Thanks for the opportunity!



BOOKS

By ALEX ZUCKER

OP, POP — AND SCIENCE?

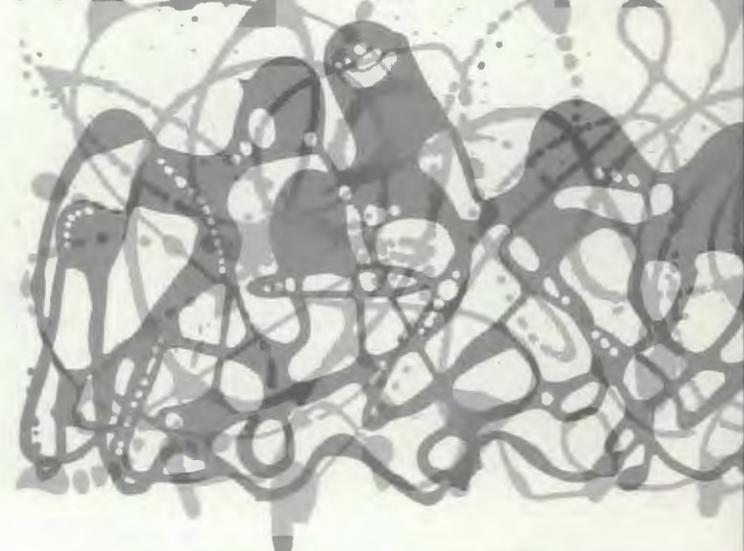
Behind Appearance, by C. H. Waddington, MIT Press, Cambridge, Mass., 1970. 256 pages, \$25.

THE TWENTIETH CENTURY has witnessed many explosions, revolutions, reversals of form and taste; up close it appears as a confused pattern of brilliant strands, violent and twisting, never at peace with itself, never sure of a direction or a goal. Has not Jackson Pollock painted its portrait? This is the kind of question C. H. Waddington might well ask in his exhilarating book, "Behind Appearance," in which he tries to show the relationship between two of the brightest and most unruly threads in our culture; between science and painting in this century. The book is a beautiful example of its genre. First, it is marvelously produced, a work of art in itself. There are over 70 brilliant color reproductions and 136 in black and white. Although the choice was limited by the theme of the book, they represent some of the finest painting done in this century. Second, the text is not the usual bland, scholarly-sounding pap that one usually finds in expensive adult picture books. On the contrary, "Behind Appearance" is a book full of ideas, well worth reading as well as looking at.

We all suspect from time to time that science and art are somehow related; they both spring from careful observation of nature, they involve imagination and abstraction of a high order, and they give us, each in its own way, a picture of the world, and a picture of ourselves. But it remains for Waddington to lead the way across "the shallow cleft between the cultures." No one could

be a better guide. It is evident that Waddington, a distinguished geneticist at the University of Edinburgh, is at home on both sides of the cleft, that he loves both science and painting with an acceptance and understanding of the difficulties and the rewards. He comes through, in this book, as that admirable product of British civilization, a man of culture and erudition, of precise and penetrating eloquence, of clear judgment and understated persuasion. He recounts the development of modern painting from Cubism to Op and Pop, with care for the essential relationships between succeeding schools, yet with an economy of language and a precision that point undeniably to the author's original craft. No turgid agglomeration of words here, to describe the emotional effect of a painting, no attempt to replicate the nonobjective canvas through nonobjective prose.

In so many ways the book succeeds. It is a rich but concise history of painting that begins with Cubism, with Picasso and Braque, to Mondrian who surprises us with the statement, "... art must disclose what science has discovered, that time and subjective vision veil true reality." Kandinsky, on the other hand, is disenchanted: "This discovery (the divisibility of the ultimately indivisible atom) struck me with terrific impact, comparable to that of the end of the world. In the twinkling of an eye, the mighty arches of science lay shattered before me. All things become flimsy, with no strength or certainty. I would hardly have been surprised if the stones had risen in the air and disappeared. To me, science had been destroyed. In its place — a mere delusion, guesswork by the scientists, who, instead of erecting, stone by stone, a divine and unshakable edifice, had — or so it then seemed to me — gropingly, as if in the dark, fumbled for the



OAK RIDGE NATIONAL LABORATORY *Review*

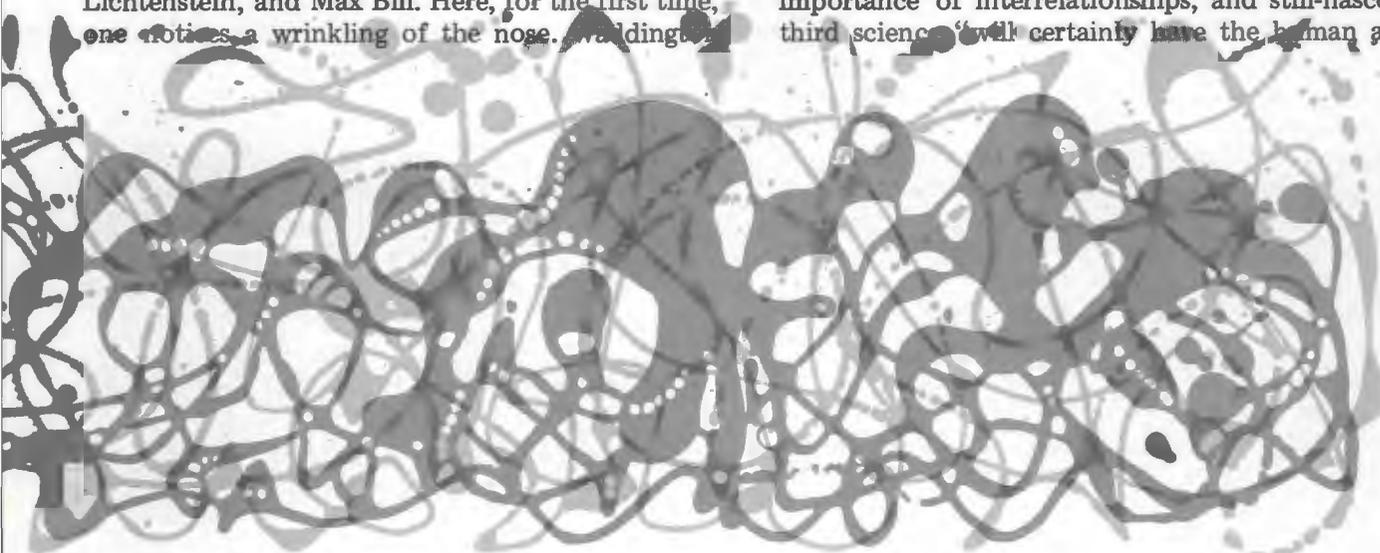
scientific verities, often — in their blindness — mistaking one thing for another.” His faith shattered, Kandinsky was led to despise science and as a reaction to it pioneered his anti-realistic, purely abstract style. I fear that, on the whole, modern painters follow Kandinsky, rather than Mondrian; they view art as an emotion-conveying antidote for a society fed on the cold poison of science. Rare indeed are statements such as this by the writer on Cubism, A. Ozenfant, “The more science gives us new realities, the further progress true poetry makes. The more science prolongs its sight, the wider do dreams extend their wings and their flight.” I have the opposite feeling, that as science reveals more about the world we live in, the artist dives deeper into himself for a different kind of truth.

From Cubism Waddington proceeds to surrealism. He declares it a generally interesting cul-de-sac, full of ideas that link it to science, but the main development of painting leads to the exciting efflorescence of the New York School that erupted violently on the world around 1950, with DeKoonig and Pollock, followed later by Rothko, Kline, and so many others. Waddington’s treatment of this school is honest and straightforward; he makes the reader aware of the inner motivations of the artists, of the external forces of the society in which they find themselves, of the influences and strong emotions that would sometimes produce distorted searing portraits, and at other times deceptively placid, but potentially explosive, huge canvases of closely reasoned color patterns.

The final stage is Op-Pop-Cool as exemplified in this book principally by Bridget Riley, Roy Lichtenstein, and Max Bill. Here, for the first time, one notices a wrinkling of the nose. Waddington

sympathizes with Op, he is an admirer of Max Bill’s mathematically derived Cool, but Pop is too much. His sensibilities cannot stretch to encompass the Campbell Soup can, or a much enlarged detail of a comic strip. Waddington brings understanding to this art form, he knows what the artist is trying to say, but cannot swallow it whole. No matter; it is all there, the reader can form his own judgment.

Into the interstices of his description of the development of twentieth century painting, Waddington has loaded a parallel history of science. Again his professionalism, his lucidity, his experience as a teacher serve him well. He ranges over wide terrain: relativity, quantum mechanics, Gödel’s theorem, Whitehead, Wittgenstein, many of the old horses are trotted out; handsomely caparisoned, they appear fresh and shiny. We learn a new trinal classification of science: *First Science* encompasses the work of ancient cultures, the postulation of axioms, which by logical deduction yield theorems. *First science* is principally deductive, and occasionally observational. *Second Science* is an offspring of the Renaissance. Its principal characteristic is “the interrogation of nature by . . . experiment.” The concepts of second science are familiar objects: atoms are like billiard balls, forces are like springs, light is wave motion. The epoch abounds with visual models of what is not readily seen. *Third Science* is a child of this century. Concepts lose their realistic appearance. Abstract fields replace the springlike forces, fuzzy probability distributions replace hard-edged spheres, and entirely new ideas of information and organization take center stage. Especially the biological sciences develop in a way that points up the importance of interrelationships, and still-nascent third science “will certainly have the human and



social sciences. . .in its makeup.”

This is an interesting ordering, and precedes the consciousness triad recently launched by Charles Reich. It suits Waddington's purposes admirably, since he can relate the disappearance of reality from painting to a contemporaneous heightening of abstraction in science. Wave functions extend to infinity, and some large paintings of the New York School almost achieve that impression. The “all-over” quality of a painting by Tobey is reminiscent of the gestalt approach in psychology, or systems analysis in communication engineering. Waddington stretches such analogies a good deal. For example, he quotes a number of artists and art critics of the twenties to the effect that Cubism or its Italian cousin, Futurism, is earnestly trying to depict “the fourth dimension.” This is all very unconvincing, especially since Waddington never presents any real historical evidence that the artists of the age ever seriously read or thought about modern scientific development. This, in fact, is one of the major flaws of the book. It should be possible, by the established methods of the historian, to ascertain whether and to what extent the painters of this century were influenced by science, what they said about it, and what the central notions were in this transfer of ideas.

There are other, and more serious, omissions in the book. Painting and science must be related at the source. They are both attempts by man to understand the world, to describe it, to react to it, with all the resources he commands. They are both ventures into uncharted regions; they are the outriders of society. One may, for example, explore the predictive nature of art and science. Rollo May points out in “Love and Will” that artists form the most sensitive component of any society; they feel what is only dimly perceived by the rest of us, and sometimes succeed in putting it on canvas. Who can deny that the violence of DeKoonig or Kline, expressed in the placid Eisenhower fifties, was a prophecy for our time? In their way, scientists predict the future too. Today's esoteric discovery may well be tomorrow's major social force. Again, who can deny that the transistor radio has had the most profound effect on social and political development in the less advanced countries? The future is there for us to examine in the art galleries, and in the journals of scientific societies; our problem is that we are unable to read it. Or is it that we are unwilling to believe unfavorable auguries?

Western culture worships originality, as much

for the sake of novelty as for the weight of the content. Nowhere is this more apparent than in art. We have come to consider innovation in art as a sine qua non of the great artist, a trait by the way that is by no means universal; the Chinese, for example, do not despise a copy as inferior to the original. How much this quest for originality in art has influenced the development of science is open to inquiry. The fact is, however, that the discovery of a new effect, a new element, or a new galaxy, is ever so much more appreciated than a careful synthesis of existing data, or improvements in the accuracy of existing measurements. I don't mean to condemn the search for the unexplored; it is the hallmark of our culture, and much of who we are, and how and where we live derives from it. But it is a common thread in those two front runners of society, art and science, and I wish it had been explored by Waddington. Why is it that the scientist and the artist strive so passionately for innovation? What is it in our society that drives them in that difficult direction?

Lastly, I would just mention one more unexplored connection between art and science, in this case one that is not unfamiliar to the readers of this review — the axiology of the two fields. How you can tell good art from bad is very much the same kind of question as what kind of scientific research is worthy of support. When we deal with pure art, as with pure science, decisions of ranking and relative value become extremely difficult. In science as in art it is difficult to set down criteria by which one can judge whether a piece of work is good or not, but in both fields the experienced critic will say that he can tell when he sees one. And in both fields experienced critics are frequently wrong. Here again Waddington is silent, and I am sure that he has much to say.

My reservations about the book then derive not so much from what Waddington has said, but from what he failed to address. I could wish for nothing better than a follow-up volume to “Behind Appearance” in which he discusses the deeper interrelationships between science and art. He has the reader hanging on his words, “Man does not only haunt many various worlds; he lives simultaneously, at one and the same time, in worlds which are distinguishable, but not truly divided. He cannot produce anything which has solely an aesthetic impact and no conceptual meaning; nor anything which is purely scientific without providing any basis from which aesthetic creation is possible,” — but then fails to explore the opening.



Take A Number.....

BY V. R. R. UPPULURI

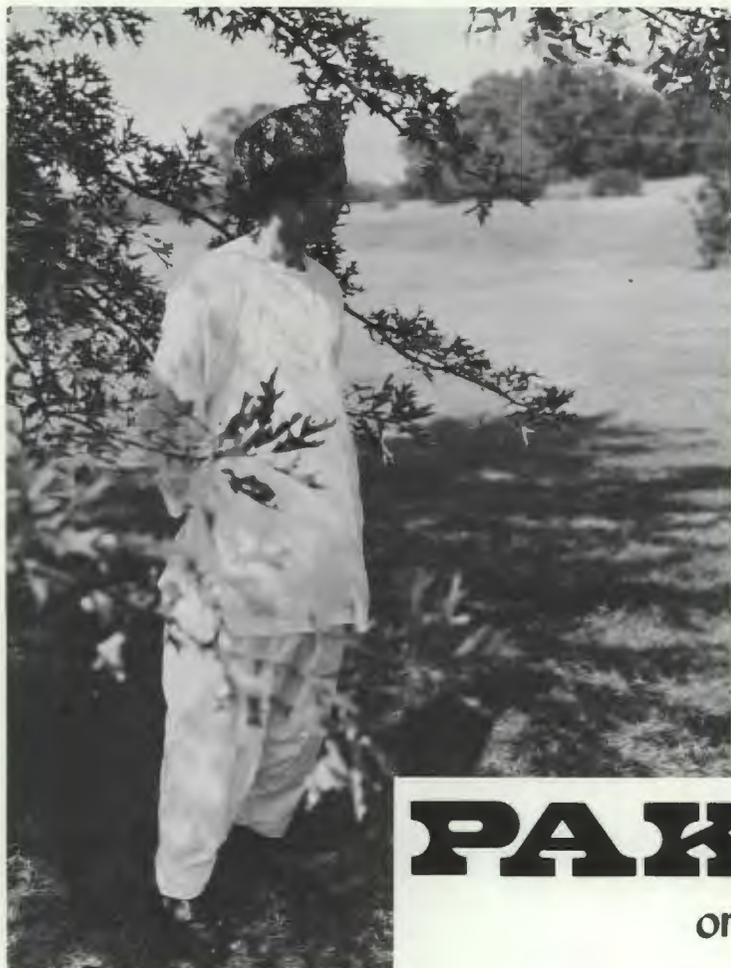
145 in a Cycle

The numbers 1, 2, 3, 4, ... are called natural numbers. Take any natural number (e.g. 17057) and then add the squares of its digits ($1^2 + 7^2 + 0^2 + 5^2 + 7^2 = 124$). Repeat this on the number obtained ($1^2 + 2^2 + 4^2 = 21$) and proceed in the same way ($2^2 + 1^2 = 5$, $5^2 = 25$, $2^2 + 5^2 = 29$, $2^2 + 9^2 = 85$, $8^2 + 5^2 = 89$, $8^2 + 9^2 = 145$). It can be shown that unless this procedure leads to 1 (in which case 1 will recur indefinitely), it must lead to the number 145, and the following cycle will appear over and over: 145, 42, 20, 4, 16, 37, 58, 89.

It may be checked that it does not take more than 12 steps to hit either 1 or 145 (for the first time) in the first 10,000 natural numbers. For any large natural number, the number of steps needed to reach 1 or 145 is a finite number, though one cannot set a bound on this valid for all numbers.

The Prisoner's Dilemma

Alan, Bernard, and Charles are in jail unable to communicate among themselves. Alan knows that two of them are to be executed and the other set free; and in his opinion the probability that any one of them will be set free is $\frac{1}{3}$. Alan says to the jailer: "Since either Bernard or Charles is certain to be executed, you will give me no information about my own chances if you give me the name of one man, either Bernard or Charles, who is going to be executed." Accepting this argument, the jailer truthfully replies that Bernard will be executed. Thereupon, Alan feels happier because now either he or Charles will be set free and he has no reason to think it is more likely to be Charles, so his chance to be set free is now $\frac{1}{2}$ and not $\frac{1}{3}$ as before. The question is, Is Alan justified in feeling happier? There are two schools of thought. One group (which includes M. S. Bartlett) feels that Alan has a perfect right to feel happier, and the other group (includes D. V. Lindley) feels that Alan has no reason for feeling happier. What do you say?



Frantisek Plasil was born in Prague 32 years ago, leaving Czechoslovakia with his family in 1948 to live in Geneva. From there he attended the University of London for his undergraduate work in chemistry, and then came to the U.S. to earn his doctorate in nuclear chemistry at Lawrence Radiation Laboratory. He arrived at the ORNL Physics Division in 1967 from a post-doctoral appointment at Brookhaven, pursuing his interest in heavy ion induced fission in experiments at ORELA, ORIC, and on the Van de Graaff. Recently he found himself in Pakistan at a crucial moment in that country's history, and here offers his account of it.

Incident in **PAKISTAN** or, Fission: Nuclear and National

By FRANK PLASIL

Our microbus rattled at high speed down the dusty country road. It shook violently as it occasionally darted onto the rutted shoulder to avoid an oxcart or a camel. We had left Rawalpindi, and now we fought our way against the stream of workers headed for the city on bicycles, in horse-drawn tongas, and in overcrowded buses. The other scientists and I sat silently and watched with sleepy eyes the mud houses of the villages flying past. It was my first trip to the Pakistan Institute of Science and Technology (PINSTECH), located in the middle of nowhere, about 20 miles from Islamabad, the capital. The road passed through an undulating, heavily eroded countryside. There were few trees, and the harshness of the brown dust was relieved by patches of fresh green wheat and by the startling yellow of the mustard fields. In the distance were the foothills of the Himalayas. Behind a bend against the backdrop of the Punjab hills I caught a glimpse of PINSTECH.

Its white splendor was like a modern Taj Mahal: a vertiable temple of science. I was startled, even though I had been prepared for the sight.

PINSTECH was to be my home for the next two months. I had arrived to try to perform a fission experiment with the 5-MW reactor, which is housed in a beautiful white and gold dome, surrounded by a fountain-studded reflecting pool. My visit was part of the Sister Laboratory arrangement between Pakistan AEC and Oak Ridge National Laboratory, funded by the U.S. Agency for International Development (AID). Pakistan is a country with limited power resources and is setting out on the road to industrialization. It is a country with a good use for nuclear energy. It has one nuclear power station almost completed near Karachi, and another one planned for East Pakistan. This is the reason for the existence of PINSTECH, and also for the Sister Laboratory program. A small reactor can serve as a training

ground for engineers needed to staff the power reactors. Research on the small reactor can support a group of nuclear scientists who are familiar with reactor problems, and can in turn provide fruitful interaction with engineers. Together they can form a small center of nuclear know-how. Basic reactor-oriented research in a developing country can be very relevant to international development and thus a legitimate recipient of AID funds. I won't try to describe all the aspects of the Sister Laboratory arrangement. The program is administered at ORNL by Lewis Nelson of the Director's Division, H. W. Schmitt of the Physics Division, and M. K. Wilkinson of Solid State Division. Among its activities is training Pakistani scientists in reactor-oriented research at ORNL and sending ORNL scientists to Pakistan to help them begin their own independent research. A recent visitor from Pakistan under this program was G. Dastgir Alam, who worked in our fission group in the Physics Division for 15 months, returning to PINSTECH in September 1970.

Our microbus stopped at the back entrance in front of a red carpet lined with potted flowers and palms. The welcome, it turned out, was not for me. The prime minister of Mauritius had visited the place a few days earlier. The back entrance was used because the front entrance, which is to be graced by a modern version of a Moghul water garden, was not yet finished. We were invited to inspect the institute. It consists of a two-story concrete canopy of graceful lines forming four sides of a quadrangle. In the center will be a garden, patterned after the famous Shalimar garden at Lahore. Under the canopy, the buildings, all in the same style, line the edges of the garden on three sides of the quadrangle. The fourth side is open, revealing a view across the countryside to Rawalpindi; the reactor dome itself stands in its reflecting pool near the opposite side of the quadrangle. The exhaust stack, also gilded, stands at a discreet distance from the dome in the same reflecting pool. The dome, an obvious target, was covered with mud during the Indo-Pakistani war of 1965. It has never lost the scars of that camouflage.

Most of the buildings are not yet complete, and our footsteps echoed as we walked through them. Although the laboratory will some day employ 1,000 people, at this time, only the building near the reactor was occupied, and total employment stood at about 200. We passed through air-lock doors into the reactor dome, wearing white over-

shoes of the type that tourists sometimes wear when they visit a mosque. The floor of the reactor hall was, in fact, not unlike a holy place. It was clean, uncluttered, and quiet — an excellent place for contemplation; the reactor was down. Within one week the serenity was gone. A team of Polish engineers arrived to install a neutron diffraction spectrometer, and our fission experiment was getting off the ground, generating a frenetic activity of the kind we Western physicists found more normal.

The experiment we planned had been agreed on before my arrival. The idea was to compare fission fragment distributions obtained from resonance neutron-induced fission of plutonium-239 with distributions obtained from thermal-neutron-induced fission of plutonium-239. The distributions were to be obtained by measuring energies of both fission fragments from fission events in each of the two cases. I brought with me the solid-state detectors used to measure fragment energies, and a zinc crystal used to select neutrons of the required energy by diffraction techniques. I also brought with me a variety of experimental odds and ends, and felt relieved that I had not been put to explain them in customs searches in Katmandu and Varanasi.

The next few weeks were a drama of persistence. Dastgir Alam and I, with our several helpers ranging from janitors (referred to, widely, as "peons") to junior scientists, battled overwhelming odds. Our techniques ranged from Boy Scout-style self-help to cunning, from pleading to browbeating.

"Let's move that beam stop," I would say.

The answer might be, "Yes, we will on Monday."

"I meant today," I would counter.

"Fine, after lunch and the Friday prayer period at about 2 PM."

"NOW!"

Sometimes it worked.

Our first task was to map out the reactor beam and to install a rotating platform to hold the crystal and the fission chamber. Next came orientation of the crystal, and identification of the refracting planes. This enabled us to obtain a diffracted neutron beam of the same energy as the plutonium-239 resonance. At this point we found that the construction of the fission chamber was proceeding at a rate that would complete the job in a little over a year. We took the matter into our own hands. Starting from scratch, we built a simple vacuum-tight chamber in four days. This was

The differences between East and West Pakistan include that of climate: in the west (l.) the rugged terrain of the Himalayan foothills has little in common with the rice paddies of the tropical eastern section (r.).

possible by our continuous physical presence in the machine shop where we worked alongside three machinists. There was no fear of union grievances, and all were impressed by the crazy pair of Ph.D.'s actually working lathes and drilling holes. All that remained before the actual experiment was the stacking of shielding, assembling of the electronic gear, and testing of the detectors.

Every day our senior scientist microbus delivered us to the laboratory at 8 AM, after a one-hour ride. The next hour or so was spent drinking tea, discussing the news, waiting for the air conditioning (or heating) to be turned on and the reactor to be brought up. The highlight of the day came at noon when half a dozen of us would crowd around a small table in our joint office and wait for the janitor to bring our food. My lunch came from the mess hall; others brought food from home. All the reheated curries were placed in the center of the table. Eating with our hands, we helped ourselves from any and all of the dishes. The food was excellent. It occurred to me the ORNL cafeteria might experiment a little with curry. At 3:30 PM everything was turned off, including the reactor, and at 4:15 we headed back to Rawalpindi.

I spent the evenings roaming about the bazaars on a rented bicycle, eating mutton kebabs and tikkas from the street vendors. I was the subject of great curiosity. Perhaps my place was really in a chauffeur-driven, air-conditioned AID limousine, avoiding crowded areas, or in the dining room of the Hotel Intercontinental.

Weekends were something to look forward to. They brought such adventures as falcon hunting, a visit to the tribal areas of the historic Khyber Pass, a wedding ceremony at which the bride was never seen by male guests, getting snowbound in the mountains when the temperature in Rawalpindi was 95°F, camel rides, flying past Nanga Parbat, nearly 27,000 feet high, and watching water buffaloes bulldoze their way through crowds in narrow streets of the old city in Lahore.

The most exciting event was an extended weekend trip I took to Dacca, the capital of East Pakistan in Bengal. To understand what happened, some political background is necessary. Pakistan is



ruled under martial law by President Yahya Khan. He decided to return power to the people and declared free elections. These were orderly, but everybody was in for a surprise. When the results were announced, West Pakistan had given a majority of its votes to the Pakistan Peoples Party led by a wealthy pro-Communist, anti-American landowner called Bhutto. East Pakistan, in a show of surprising unity, was completely carried by the Awami League led by the somewhat pro-American Sheik Mujibur "Mujib" Rahman.

East Pakistan has been politically dominated for many years by West Pakistan. For example, of the fifty generals in the army, only one is from East Pakistan. The Awami League won on a platform of autonomy for East Pakistan, which has 70 million inhabitants to West Pakistan's 50 million. Mujib had a clear majority, and the convening of the National Assembly to draft a constitution was set for March 3.

On February 27 I arrived in East Pakistan to spend a weekend with friends in Chittagong, and to visit the AEC research laboratory in Dacca. March 1 was a Monday, and I had spent the morning visiting the Dacca Research Center. After a splen-



did lunch, I was about to leave a downtown restaurant with my host. We found the exit blocked, and customers and employees were peering anxiously through cracks in closed shutters. Outside, people were streaming past, carrying sticks and shouting. They poured out from a cricket stadium where a game had been interrupted by the announcement that the president, bowing to a threat by Bhutto to set West Pakistan ablaze from Khyber to Karachi if the National Assembly were held, decided to postpone the session. This was a blow to East Pakistanis, and crowds headed for their leader's house.

In the confusion, we blended into the crowd and made our way to the Research Center. It was barricaded, and the employees were headed home. After some embarrassment about what to do with a foreign visitor, my hosts loaded me onto a Jeep which took me to the Center's guest house near the airport. I found it deserted, and decided to walk to the airport and inquire about flights to West Pakistan. The airline staff said they had no news and could not get in touch with the town office. They suggested I avoid the center of town, but curiosity won out, and by means of a scooter

rickshaw I headed for the airline office downtown. Only small groups of people with sticks milled about the main intersections in town, but there was a large crowd outside the Pakistan International Airline (PIA) office. The windows were smashed, desks turned over, and the office was ransacked and deserted except for a few policemen. Apparently PIA was a symbol of West Pakistan's supremacy. I learned that a general strike had been declared for the next three days, and decided that no further purpose could be served by staying in East Pakistan.

At about 10 PM I headed for the airport again. The situation there had deteriorated. Lights were off, no PIA officials were to be seen, and a crowd consisting mainly of West Pakistanis milled about the lobby. The access to the runway was guarded by soldiers, and the runway itself was lit by kerosene lamps. A PIA Boeing 707 arrived from Karachi. A military cordon went up around the plane several hundred feet from the terminal. Bewildered passengers deplaned in confusion. I was told that the plane would return without passengers.

Then I noticed that some important looking

officials were taking some luggage to the plane. On their next trip I joined them, pretending to be one of them, carrying my bag confidently. We were stopped by soldiers a few times, and in my best colonial arrogance (so easy to acquire) I told those who stopped us that I had nothing to discuss with them, but that I had to talk to the captain of the aircraft. Thus I made it inside the plane, and introduced myself to the captain. With great politeness I regretted his predicament, and told him that I had to be in Rawalpindi the next day where I was needed urgently to operate a nuclear reactor. The bluff worked, the captain was impressed, and I was allowed to stay on the plane. The next hour, confusion reigned. The captain won an argument with the military and obtained fuel. The crew debated what to do, and most decided to return. A few more foreigners were allowed aboard later, as well as about 20 important-looking Pakistanis. Luggage and passengers were all in the cabin together. Near midnight the plane took off. It was the last plane for several days.

The next few weeks saw hectic political activity with speeches, statements, strikes, and negotiations. Mujib was the *de facto* ruler of East Pakistan, but did not have things sufficiently under control to prevent reported cases of atrocities against some West Pakistani citizens living in East Pakistan. Just when negotiations between President Yahya Khan and Sheik Mujibur Rahman seemed to be going well, the final crisis came. There was a tentative agreement, but it was unacceptable to West Pakistani leaders. Mujib remained firm, and the President, acceding to the objections, changed his mind, cut off negotiations, arrested Mujibur Rahman, declared him a traitor, and outlawed the Awami League. The country plunged into civil war. During the many political discussions at PINSTECH I had been determined to remain neutral. Soon, however, I found myself taking sides and disagreeing with most of my colleagues. Strangely enough, the other foreign visitors, consisting of the Polish engineers and a British professor assigned to the reactor school, had the same reaction.

My stay was coming to an end, and the experiment was ready to be performed. We needed at least 100 hours of reactor time. This represents only four days of around-the-clock operation, but at the rate of six hours per day for four to five days per week it would have required more than three weeks. Chances of equipment failure during such a long period are great. We started to agitate

for continuous reactor operation. This was unheard of. There were only two competent engineers who could be in charge of shift operation. Other support departments also had inadequate manpower to cope with the problem. The reactor engineers were reluctant to set a precedent by working 12-hour shifts since their request for additional manpower had been turned down earlier, and they did not want continuous operation to become a habit. The laboratory director was not inclined to lose sleep over the problem. He told the engineers to do it if they could and to forget it if they couldn't. Luckily the engineers (splendid fellows) succumbed to our persuasion and pleading, and agreed to run the reactor. We all hoped that nothing would go wrong — and nothing much did.

On the last day the reactor scrambled as the result of a power failure. A trip to the power substation revealed employees happily performing routine maintenance. They assured us that they had informed PINSTECH as to their intentions, but obliged us by turning the power back on.

Our experiment was a success, and we collected the data we needed. The results are not analyzed yet, however, since PINSTECH will first have to negotiate with a Rawalpindi bank to obtain permission to use their computer. We felt a sense of accomplishment. More data collection, and work on other targets will now follow.

On one of my last days at PINSTECH I could not resist being ostentatious, and hoped I would not, as a result, be considered an Ugly American. I knew that laboratory parties were very popular. These were usually held in midafternoon. Tea and very sweet items called sweetmeats were served, together with oranges and other fruit. I decided to finance singlehandedly such a party for all shift workers involved with the memorable reactor operation. About 40 people showed up and devoured the sweetmeats with great gusto. In a speech, mandatory on such festive occasions, I pointed out that since all were worried about setting the precedent of continuous reactor operation, I was going to set another precedent. Anyone requiring continuous reactor operation must throw a party at the end. This will keep the requests serious. The cost of my extravagance? A total of \$3.50. But then, that is more than a third of the janitor's monthly salary!

A few days later, my work completed, I left. And to Pakistan, may the peace of Allah be with you.

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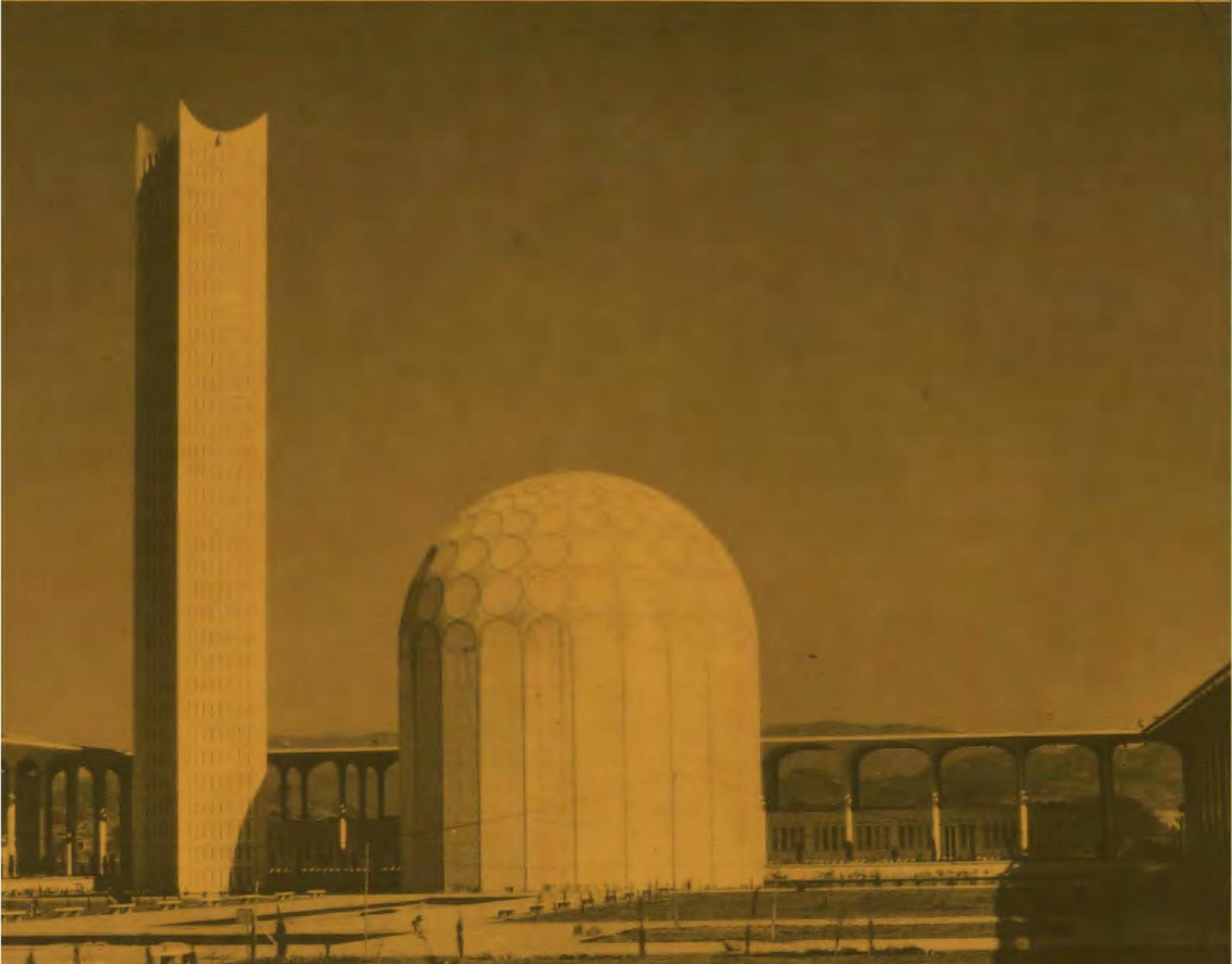
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Nuclear reactor in Pakistan (see p. 20).