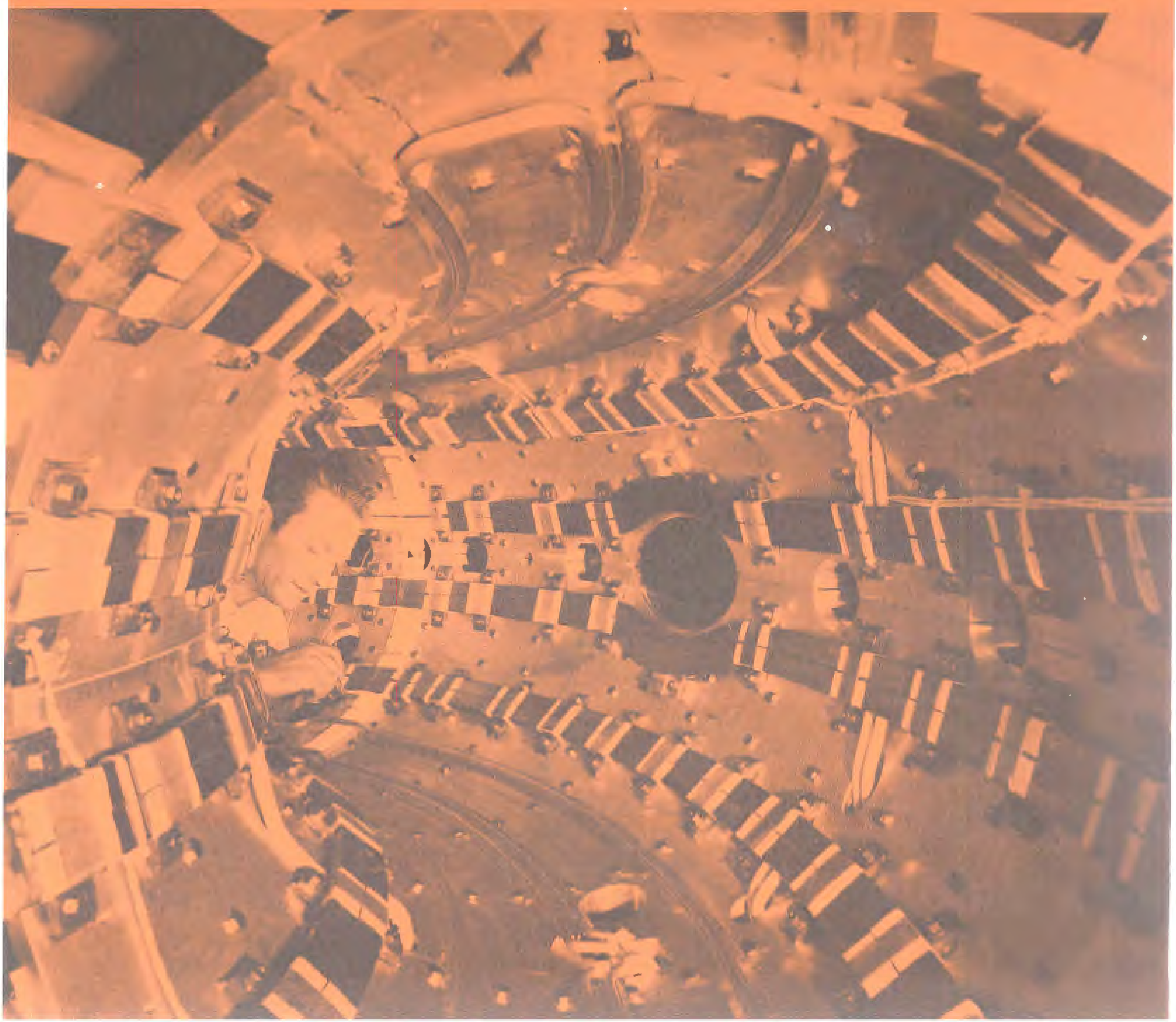
State of the Laboratory-1973

Review

WINTER 1974

OAK RIDGE NATIONAL LABORATORY



THE COVER: A technician works on the copper doughnut that will hold the stream of sun-hot ions that is the plasma in ORMAK. On page 12, Mike Roberts narrates the events that led to the Laboratory's success with its preliminary experiments in thermonuclear fusion.

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Review

OAK RIDGE NATIONAL LABORATORY

VOLUME 7, NUMBER 1 **WINTER 1974**

OAK RIDGE NATIONAL LABORATORY
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UNION CARBIDE

The State of the Laboratory address this year was delivered, appropriately, by Floyd L. Culler, who served the Laboratory throughout 1973 as its Acting Director. It was a year of extremes, in loss and gain, in painful decisions and sudden turns of fate. In his address to the ORNL staff on January 10, Culler chose to look ahead rather than back, and to reaffirm his faith in the Laboratory's possession of the necessary technical expertise for the nation's effort to solve its energy problems.

State of the Laboratory - 1973

1973 — Time of Transition, by Floyd L. Culler, Deputy Director

1973 can best be described as a year of many transitions for the Oak Ridge National Laboratory. First, it marked the end of an era that started with the creation of Clinton Laboratories, extended through the period of our major growth, and is ending as the men responsible for the Laboratory's present form and strength relinquish the responsibility for our direction. These founders, who conceived each discipline basic to our program, set our directions, and created our style, have given us standards of excellence by which to judge our personal worth and the Laboratory's scientific stature.

The second transition was actually a blessedly short turnaround in our funding, and in the prospects for major segments of the Laboratory's program. In January, we were plunged into despair by sizable and immediate cuts that forced the largest and most painful reduction of employment level in the Laboratory's history. By June, our hopes were raised by the proposals of new energy research and development initiatives. By December, with broad new support and prospects of major increases in almost all of our technological and basic research programs, we were whole again. I am confident that we are entering into a period

". . . there is good reason to believe that we will be heavily involved in this broader energy R&D effort."

of renewed opportunity. Programs, dead last January, have been reborn. For example, the Molten Salt Reactor Program has been reactivated at modest levels; the radioisotope research program is still alive with prospects for revitalization.

To each of you I wish to express my thanks for the exceptional vitality, imagination, and hard work which turned the tide.

The third transition occurred in our management. After 25 all-too-short years as Research Director and Director of ORNL, Dr. Alvin Weinberg has resigned to become the first Director of the Energy Research and Development Office in the Federal Energy Office. No one in the United States is better qualified to fill this highly important post. There is no one to whom ORNL is more indebted than Alvin. He is the architect of its disciplinary and programmatic structure, the creator of its style, the gentle mentor to its staff, the arbiter of its disputes, and a dear friend to all of us who have worked with him. From every member of the staff, from the Oak Ridge community and especially from me, go heartfelt thanks and our wishes for success in his new position.

Dr. Herman Postma has replaced Dr. Weinberg as Director of ORNL. No more capable, vital, or brilliant man could have been chosen to steer our course through the coming years. I am delighted to have the opportunity to work closely with Herman, and look forward to an exciting new cycle of achievement for ORNL. I'm sure that the entire staff of the Laboratory will wholeheartedly support his efforts.

Over the past 30 years, ORNL has evolved to its present multidisciplinary and multipurpose form through many stages of development. When it was established during World War II, it had a single mission: to demonstrate the safe production of plutonium. The Atomic Energy Act of 1946 directed the new Atomic Energy Commission to develop nuclear energy as a source of power, to proceed with a broad program for basic research in the physical and biological sciences, to undertake isotope production, and to develop chemical and metallurgical technology necessary for the nuclear program. A basic unifying purpose of the Laboratory then was established: to produce safe, economical energy from nuclear processes. It was a result of this decision to pursue fission as an energy source that the applied technology divisions were formed from the original Technical Division. Oak Ridge became the center for radioisotope production and research, and using the calutrons originally built to separate U-235, established the stable-isotope program. Dr. Alexander Hollaender started the Biology Division, and the basic research programs in the physical sciences were broadened to support these new activities.

In the late 1940's then the Commission embarked on a major expansion of production capability for fissile materials and began to develop a domestic supply of uranium; the Laboratory expanded its heavy, applied development programs related to these efforts. At the same time, there was a push to proceed experimentally toward practical nuclear power as well as to exploit the unique scientific instruments and staff at ORNL to study fundamental nuclear processes. ORNL acquired the dual aspect that has characterized it ever since: a heavily applied technological development laboratory working in beneficial mode with a basic research institution dedicated to advancing nuclear energy and science.

During this exciting period of conception and experiment, most of the main lines of reactor development were started and the principal chemical and metallurgical approaches to fuel and fuel recycle were established. We built, tested, and expanded both the technological and the scientific base for practical nuclear energy production. At ORNL, we built the LITR and we participated in the design of the MTR. The Molten-Salt Reactor Program began as an aircraft propulsion concept. We built and operated two aqueous homogeneous reactors in our early effort to develop a breeder. The high promise of cheap energy as a power source for the millennium had not yet been restrained by the concerns of society nor dampened by practical experience, and the urgent enthusiasm of the period was embellished with all of the dreams and ardor of youth.

Production of nuclear power from fission was demonstrated by the early 1950's. Until then, the Commission itself had been the only user of the

". . . ORNL's . . . central theme . . . has continued to be the development of safe, clean, abundant, economic energy systems."

nuclear technology being developed; but in 1954 and 1955, under the impetus of the Atoms for Peace Program, U.S. and foreign industry were introduced to the declassified atom. Then followed reduction to practical industrial use, during which the Commission's role changed from being the principal user of its own developments to that of coordinator, supporter, and regulator of a growing nuclear industry.

It was during the early 1960's that the Commission's second and independent role as a protector of public health and safety became important. The needs for research in reactor safety and for standardized design criteria, codes, and performance standards for a multibillion dollar industry became urgent. ORNL expanded its activities in these areas, leading to our present substantial safety and standards programs for the various power reactors being built or developed in the United States.

When, in the early 1960's, nuclear industry became competitive economically with other energy sources, the goal of the AEC's reactor development program was an inexhaustible energy resource through breeding. Although the aqueous homogeneous reactor program had expired because of technical difficulties, the Laboratory's molten-salt reactor concept had evolved into a promising breeder. In addition, our role in support of fast breeder reactor development began to expand to become the Laboratory's major reactor-related activity.

In the 1960's, the possibility of producing energy in a controlled manner from fusion of the isotopes of hydrogen was recognized. The thermonuclear research program was initiated at ORNL and other laboratories, both in the U.S. and abroad. Thus, the second major nuclear process for the production of energy entered our programs, along with related research in plasma physics, radiation damage, superconductivity, magnet development, and the various methods of plasma heating and analysis.

Two major developments then broadened ORNL's scope and basic strengths. With AEC encouragement, and sanctioned by a change in the Atomic Energy Act, the Laboratory undertook work in fields related to public health and environmental protection for federal agencies other than the AEC. In biology, the early initiatives were toward understanding the effects of irradiation of living systems and ameliorating the deleterious sequelae to radiation insult: programs in mammalian genetics; immunology; viral insult; biochemistry; aging; pathology; cancer induction, detection, and treatment; chemical carcinogens and mutagens; biotechnology; and biophysics. These programs at ORNL were parallel to major interests of the National Institutes of Health, particularly in the broad field of cancer research. With NIH support the work in cancer and basic research in biology was expanded, and the biology program at ORNL was recognized internationally for excellence and diversity.

In problems associated with the environment, the necessity for understanding the behavior of radionuclides resulted in a major program in environmental research, which had started at ORNL as a discipline in the mid-50's under Dr. Stanley Auerbach. Consequently, we had a leg up when the urgency for environmental protection and restoration became a national demand. Our programs in environmental science and waste processing technology and our knowledge of power-generating systems led in time to broadly based joint efforts with the National Science Foundation and the Environmental Protection Agency and finally to nuclear reactor station environmental impact evaluations for the AEC's Directorates of Licensing and Regulatory Operations. It was then that many of our ecologists began their transformation from academic research to environmental systems analysis in cooperation with technologists and economists, a process which continues.

The social sciences at ORNL are just developing under the stimulus of several quite dissimilar programmatic requirements. In the early 1960's, because of his concern for civil defense, Dr. Eugene Wigner initiated studies in civil defense directed to the problems and potential for population protection in the nuclear age. Social scientists were required to explore attitudes concerning civil defense and the behavior of people confined to shelters. The nature of urban growth and the characteristics of cities, particularly the possibility that facilities provided to protect the populace could also provide useful services under normal conditions, led to programs of joint interest with the U.S. Department of Housing and Urban Development. Need for information about population dynamics led to the introduction of demography at ORNL as a corollary science.

A second requirement for social scientists at ORNL developed in National Science Foundation-supported programs to evaluate (and try to predict) the effects of technological change on the environment and society. These activities have engendered programs in regional modeling, economic analysis, and resource use and recycle, with particular focus on energy use and conservation.

The third social science stimulus has come from our broad program of study with the U.S. Department of the Interior of energy production—desalting—agro-industrial complexes as possible instruments for economic and social improvement in developing countries, an important concept now languishing in the political vicissitudes, a region in the political world that corresponds to the horse latitudes in the geographical world.

To make the results of past investigations readily available for use and to help avoid repetition of work already done, comprehensive information systems are necessary. We have established about 17 information centers at ORNL and are now studying ways to consolidate this massive activity into a more coordinated system.

Our early research programs thus have differentiated to separate programmatic thrusts, the more important of which are no longer encompassed by the rationale in which they originated. In truth, the Laboratory now serves many purposes: it is multipurpose. So, too, is it multidisciplinary with particularly high competence in the physical sciences, the life sciences, and engineering development; the social sciences are still developing.

But, throughout ORNL's evolution, its central theme, and one not in conflict with its many missions or its many disciplines, has continued to be the development of safe, clean, abundant, economic energy systems. To this we have added the need to understand energy production well enough to protect the environment, human health, and the society from its deleterious effects. The Laboratory is now in a uniquely strong position to undertake a multimodal attack on the nation's energy problems primarily because this theme has been sustained to focus our attention rather single-mindedly on energy.

And there is good reason to believe that we will be heavily involved in this broader energy R&D effort. During 1973, the Chairman of the AEC was asked by the President "... to undertake an immediate review of Federal and private energy research and development activities, under the general direction of the Energy Policy Office, and to recommend an integrated energy research and development program for the Nation ...." This program recommendation was sent to the President on December 1, 1973. Further, the President proposed to the Congress that an Energy Research and Development Agency be created. A bill that responds to this request has been initiated and passed in the House, and the Senate version has been introduced (HR 11510, S 2744). The ERDA bill states that "... to assure the coordinated and effective development of all energy sources ... it is necessary ... to bring together and direct Federal activities relating to research and development on various sources of energy ...." To accomplish this, it was proposed that the Atomic Energy Commission be dissolved and its research and development programs be included in a broad energy research and development initiative.

Even though this proposed reorganization could falter or be delayed, it is very likely that the Laboratory's program will include work on energy sources and systems other than nuclear. The transition to an expanded research and development initiative in energy, already in progress in a preparative sense, will have profound effects on the technological programs of the Laboratory and upon the opportunities for important new thrusts in basic research.

THE ENERGY CRISIS

During much of 1973, we participated in many of the groups who studied broad energy research and development needs which culminated in "The Nation's Energy Future," a report to the President by the Chairman of the AEC (December, 1973). Let me review some of the main aspects of the considerations which led to the recommendations in the report as a basis for projecting the impact of proposed R&D in the broad field of energy upon ORNL.

The Proposed National Program for Energy R&D

Most of the studies that preceded Chairman Ray's report to the President in December, 1973, *"We are planning now to expand already existing efforts and to initiate new programs . . ."*

and the conclusions in that report, tend to converge on similar approaches to solving the national energy dilemma. This emerging national consensus is based in part on the following considerations:

1. ". . . energy is the *sine qua non* of a modern society's ability to do what it wants to do." (AEC report to the President.)

2. Demand for energy exceeds supply, and is increasing. Demand must be reduced and supply increased by both administrative and technical means.

3. Today's energy shortages result in no small measure from the absence of a national energy policy in the past. Energy policy, based on serious analysis, is essential to the solution of both short- and long-term problems of supply, environmental protection, economic stability, et cetera.

4. The immediate problem in energy supply is a shortage of oil from domestic sources. Our dependence on foreign oil imports must be reduced as rapidly as possible. Current shortages in oil will increase because of our dwindling domestic reserves; oil now supplies about 45% of our energy, and about 35% of it is imported. Given current expansion of demand, unless substitutes are provided, this imported fraction will increase sharply to about 45% in 1975 and 65% in 1985.

5. The nation has more than sufficient energy resources to regain and maintain self-sufficiency: for hydrocarbons there are large quantities of coal, lignite, and oil shales; for electricity generation there are adequate reserves of uranium and thorium as well as coal and coal-derived products to provide fuel for thousands of years. At present, electricity supplies only 25% of our total energy, of which fraction nuclear reactors provide 5%, coal about 41%, gas and oil 38%, and hydro 16%.

6. Every effort must be made to increase oil and gas production by developing new sources and improving recovery (less than 35% for oil). Economic and environmentally acceptable methods for producing oil from oil shale must be brought into use.

7. Coal must be substituted for oil and gas; first, whenever possible, by systems where coal is burned directly (power, home heating, industrial applications). Next, gas and petroleum substitutes must be produced from coal.

8. The highest priority must be given to the protection of the environment and to human health and safety. Since environmental and safety restraints have been one of the causes of the current shortage in energy — at times on a questionable scientific basis — we must place priority on understanding these restraints at the most fundamental level.

9. Conservation must be given high priority; it is reasonably obvious that there is no way to reduce oil imports significantly in the near term without employing stringent conservation. The transportation sector, which accounts for 25% of our energy consumption, can improve efficiency to some extent by technical means (smaller cars, higher performance, etc.), but a reduction in consumption through legislative action may be necessary. Conservation measures are mostly non-technical and can be counted on with less certainty than methods for increasing supply.

10. The goal of the energy R&D program is to regain and maintain energy self-sufficiency. It is obvious to most that this probably will not be achieved by 1980, or even by 1985, with results from R&D. Although the development may be completed on techniques for increasing our energy supply, productive capacity, employing the results of R&D, will lag.

In the long term — after 1985 — this will be possible, but a number of approaches to energy production need to be supported to guarantee that this objective can be met.

11. Research and development to reduce oil imports in the short term will involve technologies now reasonably advanced or in such a stage of development that scale-up of production levels can be done with reasonable anticipation of success.

Research and development will have its greatest impact in the long term.

We must be very careful to avoid promising results from the energy R&D program that will alleviate energy shortages in the short term.

12. The use of nuclear energy as a primary source of electricity must be validated. In the short term, light-water-cooled reactors and HTGR's will be used; breeders will serve in the long term.

In the short term, R&D will emphasize safety research and development for both LWR's and

"I think we are ready for the task, but, ready or not, we shall be asked."

HTGR's, radioactive waste disposal, development of an adequate uranium ore production potential, uranium enrichment process development in gas centrifuge and laser-stimulated isotopic separation, completion of the fuel recycle development for HTGR's, and improvement of reactor licensing procedures.

Breeders are essential if we are to realize the potential for fission reactors to provide energy for a very long time. The LMFBR should be developed as rapidly as possible, with urgent attention given to high-performance fast reactor systems possessing doubling times of ten years or less using as yet undeveloped advanced fuels (carbides, nitrides), more radiation-resistant alloys, and short-cooled methods for reprocessing spent fuels.

Achievement of success in the breeding option is so important that alternates to the LMFBR must be explored. The Gas-Cooled Fast Reactor will be supported. The Molten-Salt Breeder will receive modest support; and the Light-Water Self-sustaining Reactor will receive support to test a core for this concept in the Shippingport facility.

13. Recent successes in fusion-related experiments with magnetically confined plasmas indicate that this long-term effort should proceed to the proof of the scientific feasibility; at which point the experimental burning of deuterium and tritium should be attempted. Supporting R&D for feasibility and the early work required for a fusion power reactor should be done. But fusion as a power source can only be counted on on a long-term basis.

Laser-stimulated fusion experiments will be pursued.

ORNL in the Energy Program

After six months of intensive review and careful analysis performed by experts from all sectors of the energy community, the Energy R&D Plan was submitted to the President by Dr. Dixy Lee Ray. The plan is well developed; all possible energy production systems have received attention, and a balanced five-year program is proposed.

It will be some time before we know to what extent and how the proposed program will be implemented. The recommendations will probably appear in the administration's budget message to Congress for FY 1975, thus providing us with the first solid indication of the intent to proceed. I am reasonably sure that this plan, or some version of it, will be initiated during FY 1975. We are planning now to expand already existing efforts and to initiate new programs likely to be assigned to ORNL.

I think that the following ORNL programs will expand as a result of the acceleration of energy R&D:

1. In the reactor development, to assure performance and acceptance for nonbreeding reactor systems, the following will receive emphasis:
   - High-level waste disposal: continued work on disposal of solid wastes in salt and other geological formations; process development for krypton, iodine, and tritium removal from chemical plant gaseous wastes; development processes for the removal of transuranics from high-level wastes for recycle to the reactor.
   - Reactor safety research for Light Water Cooled Reactors, Gas-Cooled Reactors, and the Liquid-Metal Fast Breeder Reactor.
   - Development of design methods for high-temperature reactor materials.
   - Study of radiation damage of reactor materials by neutrons.
   - Preparation of uranium-233 oxide fuel for the Light-Water-Cooled Breeder.
   - Continued assistance to the Divisions of Licensing and Regulatory Operations.
   - Reactor siting studies, including nuclear parks.
   - Development of improved waste heat disposal systems with cold vapor cycles.
   - General expansion in the materials program.
   - Fuel transportation studies and experimentation.
2. For breeder reactor systems:
   - Reactivation of Molten-Salt Breeder Reactor Program at a modest level.

- For the Liquid-Metal Fast Breeder: fuel reprocessing demonstration; fuel refabrication and recycle; development of radiation-resistant alloys and studies of radiation damage; shielding studies; nuclear design studies; steam generator development and testing; reactor control research; fuel development for advanced high-performance breeders.
- For the Fast Gas-Cooled Reactor: fuel development and safety studies.

3. In fusion research and development there will be a major expansion, the principal components of which are:
   - Conversion of ORMAK to a higher field and additional injection heating.
   - Preliminary design, possible development and construction of a fusion feasibility experiment, with optional conversion to deuterium/tritium burning.
   - Large superconducting magnet development.
   - Major expansion in supporting technology for a fusion power reactor.
   - Expansion of supporting research, particularly in materials.
4. In environmental sciences there will be a major expansion in all existing areas plus probable new investigations for: fossil fueled power systems, geothermal wastes, shale processing wastes, coal and coal conversion wastes and products; also, the testing of SO_2, NO_2 to establish a more scientific base for effects.
5. In biology: an expansion in radiobiology, effects of energy resource materials and their wastes for both somatic and genetic effects; also, processes for the production of methane, H_2, and other possible fuel materials by biological processes; expansion of basic biological research.
6. A comprehensive energy information system may be developed.
7. In conservation activities, an expansion of studies on methods for reducing energy consumption in all sectors of consumption; recycle of wastes for primary metals and other energy-intensive major products.
8. In efficiency of energy conversion: expanded effort in potassium vapor combined cycles, in cold vapor bottoming cycles, and uses of waste heat.
9. In basic research there will be a significant expansion: materials research to produce more radiation-resistant alloys; alloys for high-temperature use; chemical research on coal and basic coal conversion processes; battery research; solar energy conversion research; chemical research on production of synthetic fuels, such as H_2, methane, methanol; and other areas.

There are very good possibilities for new research and development programs in:

1. Coal conversion processes, particularly liquefaction, which will include all supporting basic and applied development possibly at pilot plant levels.
2. Chemical research and development in support of geothermal heat source development, plus development of energy conversion cycles based on cold vapors.
3. Evaluation and analysis of energy systems.
4. Training and education.

How much will these activities increase our programmatic funding? I do not know, but, assuming that the National Energy R&D program is approved, we may find that our program is limited by the rate at which we can expand our staff and acquire the necessary experimental equipment and supporting facilities. Based only upon the proposals for broader energy research and development, and not upon fully approved and funded programs, I think that it is possible that major multipurpose labs, such as ORNL and Argonne, might well be asked to increase at the rate of 30 to 50% per year for several years. But I have heard such siren songs before, so I urge caution while expressing my optimism for ORNL's future.

If I believed in destiny, I would be tempted to think that ORNL was predestined to play its most important roles in the next scenes of the great energy dilemma. Destiny or no, we now have the challenge to participate in the most difficult and complex research and development program ever to be proposed. I think that we are ready for the task, but ready or not, we shall be asked.

John Poston joined the Health Physics Division in 1964 with a B.S. in mathematics from Lynchburg College in Virginia, and plunged immediately into the Oak Ridge School of Reactor Technology. While employed at ORNL he earned his doctorate in nuclear engineering at Georgia Institute of Technology, and shortly thereafter settled down in his chosen field of interest, medical physics and internal dosimetry. It is in this role that he supervised two graduate students recently in their project to give corporal substance to a mathematical description of a "standard man." Paul Stansberry, shown on the opposite page, and Stephen Garry built the phantom in the little laboratory of Bldg. 2008 on the hill overlooking the cafeteria. Stansberry is at the Laboratory under an ORAU Graduate Participation Grant from the School of Nuclear Engineering at Georgia Tech, and Garry, now no longer here, was an ORAU Special Fellow in Health Physics from The University of Tennessee's Physics Department.

The Phantom on the Hill

By JOHN POSTON

"Hey, Paul, how are things going with Henry Walter?"

"Well, Steve says he has at least two broken ribs but new ones should be ready in a couple of days."

"How about his other organs?"

"The new stomach and bladder are almost ready but we must design something to hold his stomach in place."

No, the Laboratory is not involved in organ transplant research. Henry Walter happens to be a phantom built by the Health Physics Division for use in the research of the Medical Physics and Internal Dosimetry Section. He is a representation of an adult human designed by ORNL health physicists Walter S. Snyder and Henry F. Fisher, from whom he gets his double name. The phantom was designed in 1966 and has been revised and improved by Dr. Snyder, with the assistance of M. R. Ford and G. G. Warner. Henry Walter has a skeleton, lungs, skin, and about

23 internal organs. He stands 5 ft $8\frac{1}{2}$ in. (174 cm) and weighs 154 lb (70 kg).

Until about a year ago the phantom existed only on paper, and except for a few sketches, only in the form of mathematical equations. This geometric description of an adult human was used in a Monte Carlo computer code employed in the calculation of absorbed dose to the organs of the body when a radioactive material was deposited in another organ of the body. These calculations, using the Snyder-Fisher phantom, are known and recognized throughout the world as the best data available for internal dosimetry. The phantom geometry has been employed more recently in cases where the source is external to the body, e.g., in a cloud of radioactive gas or in a situation similar to a diagnostic x-ray exposure.

For some time, experimental data have been needed to provide a comparison between calculated and measured results. Such a program was outlined about a year ago and the project to construct a physical representation of the Snyder-Fisher phantom was started then.

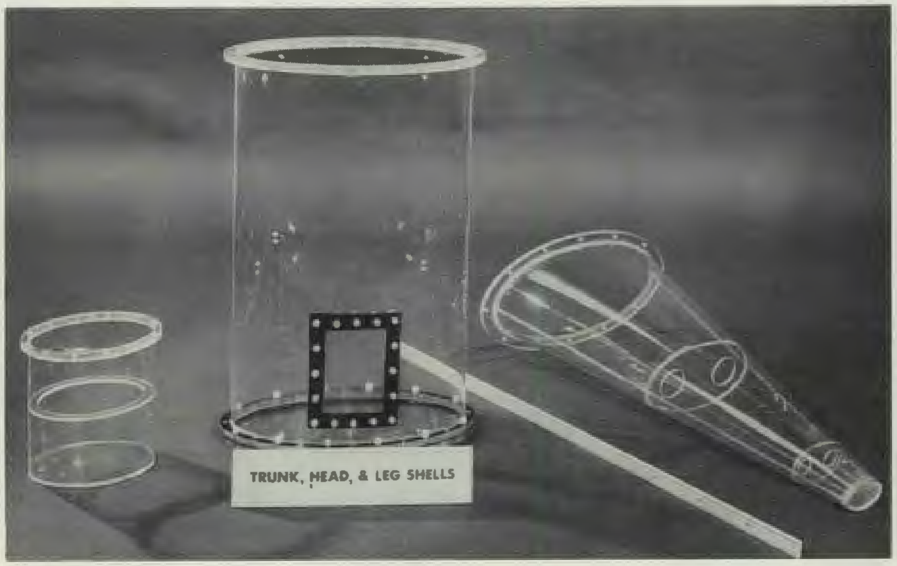
The design, fabrication, and assembly of Henry Walter has been the responsibility of Steven Garry and Paul Stansbury. Steve is an ORAU Special Fellow in Health Physics from the Physics Department at The University of Tennessee, and Paul is here under an ORAU Graduate Participation Grant from the School of Nuclear Engineering, Georgia Institute of Technology.

Paul Stansberry feeds shredded Teflon to a blender to make the tissue-equivalent stuffing for Walter Henry's lungs.

These students have worked closely with Bryan Cook and the craftsmen in the Plant and Equipment Division's Fabrication Department. The interest in this project shown by Cook and his staff, W. F. Bunch, W. C. Carothers, and H. S. Roach, has contributed directly to the success of the project.

The phantom has been constructed in three separable regions. The head region is an elliptical cylinder containing materials analogous to the skull, brain, soft tissue, and a portion of the spine; the trunk region is a larger elliptical cylinder containing representations of ribs, pelvis, arm bones, lungs, soft tissue, and the remaining portion of the spine; and the leg region, a truncated elliptical cone, holds simulated leg bones and soft tissue.

The lung, skeletal, and other exterior shells have been fabricated of Lucite by



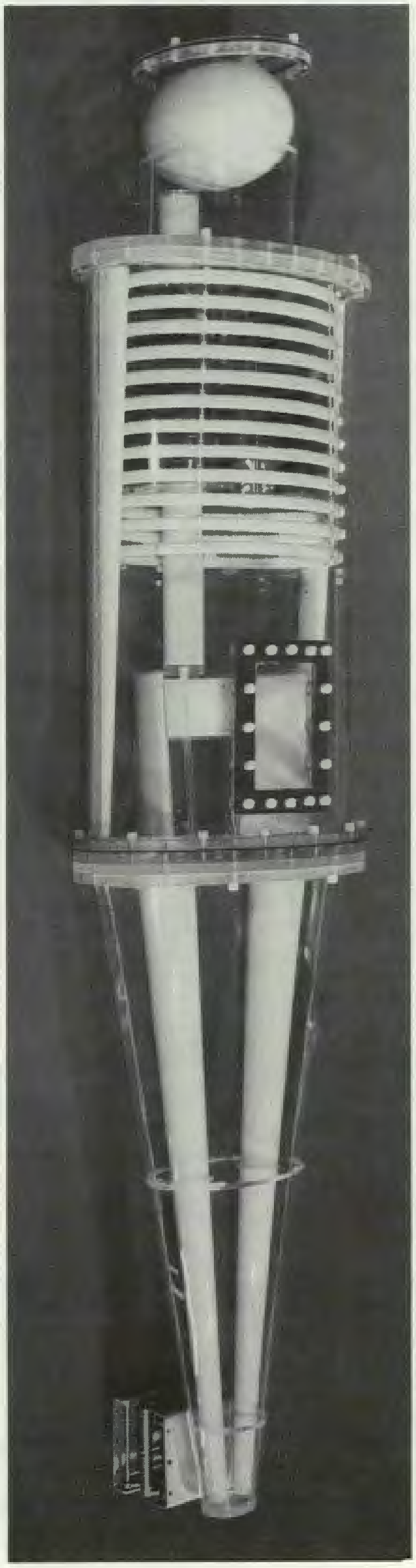
conventional molding techniques. The lungs are filled with shavings of a tissue-equivalent material (commercially available as Shonka A-150 TE Plastic) to give a density of 0.3 g/cm^3. The skeletal components are filled with a homogeneous, liquid, bone-equivalent material having a density of 1.5 g/cm^3. In the mathematical description of the phantom the skeleton is assumed to be a homogeneous mixture of hard bone, soft bone, marrow, and blood. The skeletal fluid used in the simulation is a mixture of bone flour, sucrose, water, ammonium phosphate, ammonium nitrate, and an anionic surfactant to keep the insoluble bone flour in suspension. The rest of the phantom is filled with a soft-tissue-equivalent liquid material having a density of 1 g/cm^3. This liquid is a solution of water, sucrose, methanol, and sodium chloride.

The initial use of the phantom has been to provide experimental data to compare with internal dosimetry calculations. Two special organs, the bladder and stomach, have been fabricated, filled with radioactive solutions, and used as source organs for these measurements. Experimental results have been in general agreement with calculated doses obtained from the Monte Carlo code. These measurements have provided the first set of experimental results obtained in a geometry similar to the Snyder-Fisher mathematical phantom.

Henry Walter is very suitable for use in such dosimetry experiments. He is capable of standing in a radiation field for long periods without complaining. There are no occupational exposure limits set for him and to date no radiation effects have been noted. He doesn't even mind being poked and stabbed with all sorts of dosimetry probes and he is always willing to have someone remove his head to show it to a visiting scientist. In addition, Henry is a very willing worker with a pleasant personality. He loves to work at night and on weekends with the graduate students and, of course, plays an important part in their success.

Needless to say, Henry Walter will not be allowed to retire. Experimental measurements of photon dose and spectra inside the phantom resulting from typical diagnostic x-ray procedures are scheduled to begin in the near future. In addition, the use of the Monte Carlo code is being extended into the realm of 14-MeV-neutron cancer therapy. Thus, he can expect to be called upon to serve as the target for these neutron beams and the receptacle for other dosimetric devices.

An effort has begun already to provide the phantom with a rounded head and separated legs. Henry Walter is very enthusiastic about these modifications as they are certain to make him more acceptable in his peer group. In addition, the modifications should provide a more realistic geometry for dosimetry experiments, an improvement that is certain to increase the acceptability of certain portions of the experimental results.



Don't get the idea that the Health Physics Division is a male chauvinist organization. Actually, Henry Walter is a hermaphrodite. But the Monte Carlo calculations also include phantoms representing a woman in various stages of pregnancy, as well as children of ages 0 (newborn), 1, 5, 10, and 15 years. Calculated results have been published recently which give doses to a fetus in ten stages of development, postulating a source of radioactivity located in the mother's bladder. In addition, these mathematically described phantoms are being used to investigate doses to populations, for example, from immersion in a radioactive cloud. When I am asked if these phantoms would be constructed for use in the dosimetry program, I can only say that, regardless of Henry Walter's feelings, I don't think we need another woman and five more children around the house!

The Birth of ORMAK...
A Personal Recollection

By MICHAEL ROBERTS

A little over five years ago George Kelley, John Clarke, and I began an odyssey called ORMAK that now involves the greater part of the Thermonuclear Division and many parts of the rest of ORNL and Union Carbide Nuclear Division. The story really begins in 1968 when six staff members from Thermonuclear went to an international conference in Akademogorodok near Novosibirsk in the Soviet Union. It was in many ways the culmination of much of the work conducted in the Thermonuclear Division over the years leading up to 1968. At the meeting, John Clarke represented the DCX-2 work; Norm Lazar, target plasma interests; Gareth Guest, the theory efforts; Rodger Neidigh, the turbulent heating experiments called Burnout; Herman Postma, the division in general; and I reported on the toroidal multipole program. One of the most dramatic but also the most controversial topics of that conference was the work from the tokamak groups at the Kurchatov Institute in Moscow. Their favorable interpretation of their own results met general disbelief, and when the six of us toured the Kurchatov Institute, we looked with only casual interest at the tokamak device.

In the early part of 1969, we were persuaded to look more closely at tokamak research. At that time, the late L. A. Artsimovich, who guided the tokamak scheme, came to this country for a series of lectures at M.I.T. Herman Postma had decided to ask the three of us to look at this potentially exciting field of the diffuse toroidal pinch ap-

Mike Roberts, a doctor of electrical engineering from Cornell, became a member of the ORNL Thermonuclear Division in 1966. There he initiated, with Igor Alexeff, the plasma-physics-oriented levitated toroidal multipole program, one of the contributing factors underlying ORNL's toroidal containment experiments inspired by the USSR's successful Tokamak. In 1969 the program that led to the Oak Ridge Tokamak, ORMAK, was launched by Mike, George Kelley, and John Clarke (now director of the division). Mike was responsible for the machine design, construction, and subsequent modifications of the history-making device, and today is leader of the group charged with planning and engineering in the ORMAK section. However, his dedication to Oak Ridge National Laboratory is not confined to r and d work. He is an ORNL on-campus (Cornell) technical staff recruiter, and served in 1971 as one of the first members of the Long Range Planning Office under David Rose. Here, in the ORMAK bay, he confers with Kelley, right, ORMAK section leader, who worked closely with him in the preparation of the following article.

proach to fusion, more commonly known as the tokamak experiments. We agreed with Herman, and while Artsimovich was at M.I.T., John Clarke and I went with Igor Alexeff to Boston to talk with him. That interview increased our enthusiasm, and so, when the AEC's Office of Controlled Thermonuclear Research called a meeting for June to look at the advisability of doing tokamak research in this country, we were ready. It's important to remember that in those days the tokamak results were not proven, and there were many unknowns. Bill Halchin, who had worked with me on the toroidal multipole (a baby cousin of the tokamak), became the chief mechanical engineer; Sam DeCamp, who had been operating engineer and electrical engineer on DCX-2, got involved; and we came up with what now seems to have been the thinnest of documents on the wispiest of information, which was what little we knew about toroidal devices.

Looking back on that conference in the summer of 1969, I can say that even though our foresight was not terribly good, it looked good at the time and was probably the best of those expressed there. We came home in late June to await developments; much to our surprise, approval came on July 1.

So there we were, off and running, in a race on strange ground. Ours was almost purely a mirror geometry laboratory, that is, all straight solenoids, with but one piece of toroidal work. We began immediately to assemble a staff mostly from our division, but with help from General Engineering. Our initial idea was to make a simple device in a short time. Little did we know that for FY 1970 and in each of the years thereafter, we would need to spend nearly $2 million. We had thought it would take about a year and a half to complete construction, which it did, but then we spent many frustrating months debugging the machine, and it wasn't until late 1972, after a year of initial operation, that it began to produce useful physics results. This was a difficult period for us and for the people depending on us, but actually the time required was about normal for bringing a large, new experimental device into useful operation.

By midsummer 1969, Ray McCarrell started drawing layouts and some details that have since grown into reams of paper. Since the device was to involve large transformer cores and copper coils, we started to contact those manufacturers and eventually wound up with what has to be the world's biggest, heaviest doughnut, a 10-ton grain-oriented silicon steel torus that has a circular minor cross section.

Recognized very early was the need to cool the whole machine to the temperature of liquid nitrogen and put it inside a vacuum tank. This feature would permit small coils and was important to the machine's high performance. Ward Wright got involved with the design of the nitrogen system, which had its own checkered history of "do it right," "do it cheap," and "do it again."

It was at this point that we learned of another international conference, this time specifically on closed confinement systems, to be held in Dubna, just northeast of Moscow. It was clear that the burden of this meeting was to present the latest successful tokamak findings by a team of British scientists working in the Kurchatov Institute. John Clarke and I were able to wangle invitations, and so we set off in the fall to spend a week in Dubna and a few days in Moscow. This time we pored over the machine and talked to everyone there and came back with valuable information about the conduct of the tokamak experiment. At home, we made some design changes and made the vertical field programmable rather than a static field — something that has turned out to be crucially important.

Those next few months also saw us involved with another phase of the project — plasma heating.

One of the acknowledged problems is the difficulty of getting the plasma in these devices hot enough. The tokamak had demonstrated the ability to *contain* a "quite warm" plasma, but simple calculations showed that the conventional heating method — by the friction of the electrical current in the discharge — could not make a reactor or a "reactor-like" plasma. George had thought briefly about the possibility of using energetic neutral beams of hydrogen to add heat, but a rough calculation was discouraging. Director Bas Pease of Culham Laboratory, visiting ORNL, urged a more careful look. George did a more detailed calculation and found that injection was indeed a promising means for providing supplemental heating. Bill Morgan, who had experience in this work, began developing injectors and very soon had an ion source that produced a beam of suitable intensity, so sufficiently suitable in fact that his designs are now used worldwide.

Our early rationale for the program, confinement rather than confinement *and* heating, must be viewed in the light of the uncertain state of tokamak research at the time. We had proposed in the beginning a two-phased project: we would start out on a very conservative approach doing what was then called ORMAK-I. This device would match up with one of the existing Russian experiments called TM-3 but with more attention paid to symmetry — thought at that time to be very important. The TM-3 was a small device oriented to plasma physics rather than to fusion physics. Remember that in 1969, when not everyone believed in tokamaks, a conservative step looked like a very reasonable one. The injection heating was to be used on ORMAK-II, a second step in which we increased the magnetic field considerably and took a large step beyond what the Russians had achieved.

Our original view had begun to seem outmoded by late spring in 1970, as more facts were fitted into a consistent picture of tokamak operation. The initial step of a small device to touch base with the Russians began to be less relevant — particularly so when successful operation of the ST tokamak experiment at Princeton was announced. On the other hand, because of the requirements for ORMAK-II, we could not bypass this step without seriously delaying the experimental program. In the last days of July 1970, George, Herman, and I were in Princeton listening to the first results ot the first U.S. tokamak experiments with stiff upper lips. That night, George spent a sleepless night, and by morning he had worked out a scheme to put us once again in the running.

The new approach involved a rearrangement of some of the parts and the building of some new ones to produce a fatter, i.e., low-aspect-ratio, torus at a moderate magnetic field. It was intuitively apparent to George that, relative to other experiments, what this device lacked in magnetic field strength it more than made up for in size and shape.

It was hard to tell some of the people involved in the project what we had decided. I remember informing Ray McCarrell that a year's worth of his drawings on the ORMAK magnetic field coil would now be scrapped. He accepted this gracefully with the comment, "Okay, let's go the other way," and then tied into designing and detailing this new scheme. In early August we advised the program office of the AEC of our change in plans. They were understandably taken aback and, in fact, had to be convinced. Roy Gould, the assistant director for thermonuclear research at that time, called on a panel of thermonuclear experts from the other laboratories to review our program here.

At this time, we now required a thin conducting shell that formed a large annular doughnut. It had to be made either out of copper or aluminum, and the decision became simple after I found that aluminum was the only material we could get to produce this shell in two weeks. In mid-August, with the committee's approval, we started cutting metal. The Y-12 general machine shop supported us strongly, and, in fact, there was a competition between Joe Tilson's general machine shop and Keith Kahl's special machine shop in manufacturing four of the aluminum shells. These were done 24 hr a day, five days a week, for about six weeks. It was an exciting time as the aluminum curls piled up on the floor and there slowly emerged from the machine above, our 1-in.-thick aluminum shells with thousands of holes drilled in them for the bolts that would support the coils.

The coils were interesting themselves. John Monday, who had just joined Y-12, became our man, along with Homer Clayton, in the Y-12 electric shops. John and Homer guided through their shops production of some 60 (circular) toroidal field coils; they then started in on the next set, the peculiar-looking coils, each a half circle of different radius and conductor tilt since they covered the surface of the doughnut.

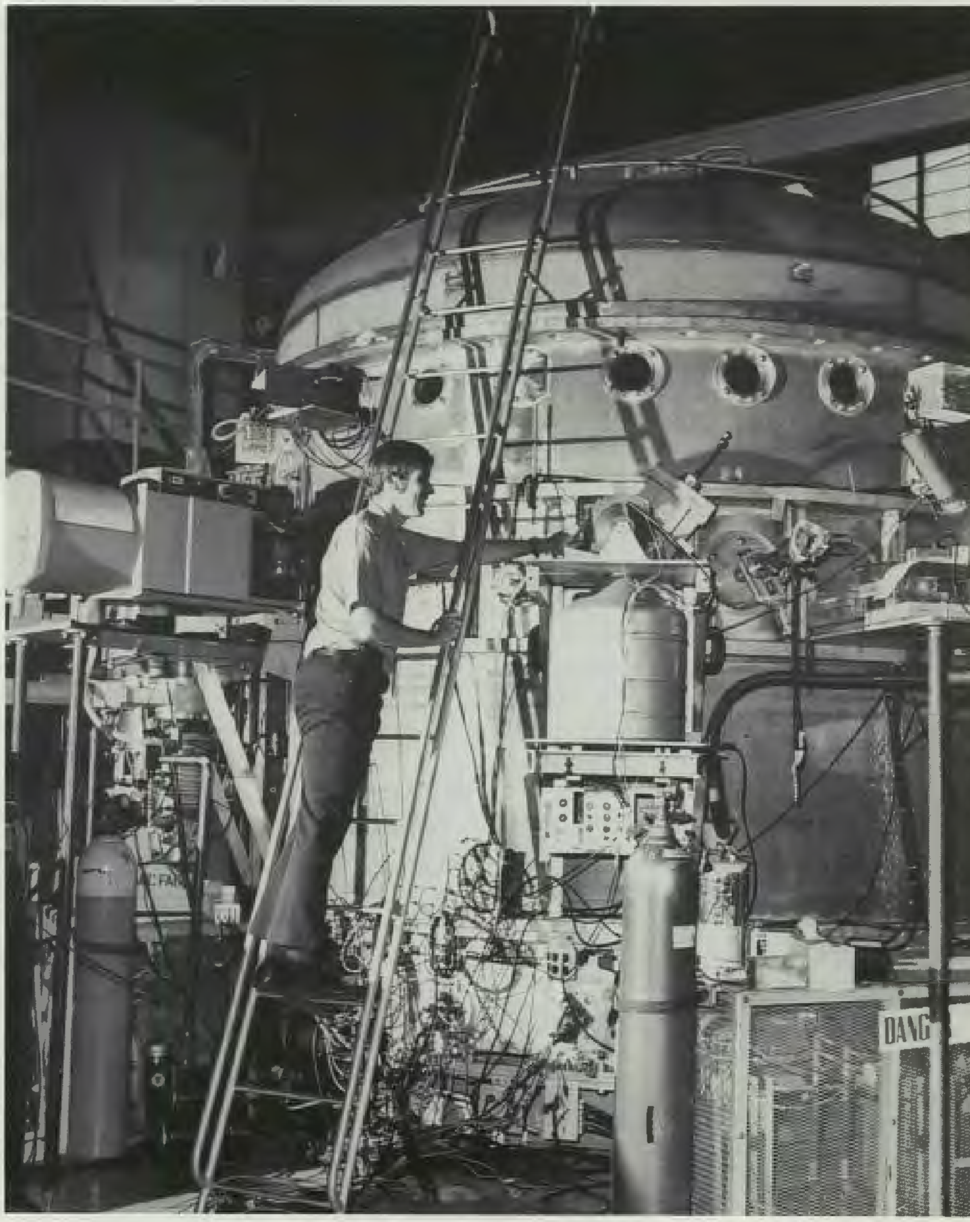
There were then many months of dedicated banging and smashing and coercing of copper coils by John Monday's four skilled craftsmen, who gave their all in this peculiar job that frequently had pieces coming the day after they were due, arriving in coffee cups and by the handful with many redos. There came a time when we (i.e., S. O. Lewis, Lloyd Vineyard, and many others) were producing drawings and revisions to drawings so quickly they had to be specified not just by dates, but by the exact hours and minutes of the drawings. Somehow, we kept on, working two and three shifts, and 1970 came to a close with three-shifts-a-day installation in the last months. Those were very exciting days. I was in on all of the day shifts, half of each night shift, and every shift change. We had craftsmen coming in who didn't want to go home at the end of the shift. The excitement grew as one saw the machine being assembled, and each day as people would come in they would see it in a later stage of being put together. The first coils went on hard. We had some measure of heart failure when the first one refused to go on the torus, but with a big enough mallet, and enough perseverance, it went on. After that, the others went on easily. This really was our first experience with large-scale toroidal devices and the difficulty of their assembly — which is like that of putting an orange inside an orange inside an orange, all from the outside.

One of the other real achievements was the production of the plasma liner under Sam DeCamp's supervision. This was done principally by two mechanics, Hubert Boyd and Homer Jeffers of the Y-12 Maintenance Division. These fellows did true artisan jobs in producing the 10-mil-thick stainless steel "tin can" (later gold-plated) that serves as the plasma liner. Each piece was made by hand, and then welded together in this enormous mitered-section device that we call a liner. It was welded by Jake Chance without very much equipment but with lots of skill. All this time, Herschel Bailey, who was responsible for coordinating all the Y-12 craftsmen, worked under great pressure trying to meet our almost impossible demands.

It was in the middle of January 1971 that we finally closed the vacuum tank door, sealed it up, and began to pump the machine down. We were to do this many times in the next five months. Sam had said in 1969 that the problem was going to be vacuum, not the more elaborate things we had talked about. Well, he was right. We spent the first half of 1971 trying to get the machine pumped down effectively. Although that was not the only problem, it was one of the most severe. It came from the fact that there were some 250 insulating connections inside the machine joining the coolant headers to the coils. These joints had been designed on the bench, and they seemed okay when they were tested, but they didn't work in the field. So, after some thought, we replaced them all with a better joint. That one didn't work. It was only on the fourth try that we were finally able to get something that worked. (Now, in 1973, I have just talked with Jake Meece, who has been an operating technician since the beginning with us. We have just installed the second injector system. The machine was open for three weeks. People walked around inside. We put the machine together, pumped down, and Jake reports that there was not the slightest indication of a leak in any one of the 250 insulators.)

At the same time in 1971, Cal Ezell was having difficulty persuading the four generators to feed the ORMAK field coils properly. At the time, we could not afford a dummy load, i.e., a way of testing the generator separately, and had to do the final development of the regulating system on the device itself. To add to our problems, there were electrical insulation breakdowns inside the device that delayed testing. We would get one fixed, and the other would break down. We try now to make sure that every piece can be tested individually and not necessarily in the system as a whole.

In 1970, Masanori Murakami joined us from M.I.T., the only physicist on the ORMAK staff to be hired from outside. Masanori began working on the crucial Thomson scattering laser system. He inherited a jumbled system that was the best we could do by proxy (i.e., with the help of Moshe Lubin from University of Rochester, the FTS lines, and a few overnight working visits), and he then spent two years trying to make it work. At the end of 1972 I saw the first results, a set of measurements that represented Masanori's two-year herculean effort under very trying circumstances. During that time, Phil Edmonds came to help Masanori with the experiment, Joe Culver with the engineering, and Rand McNally had also joined us as a spectroscopist. Shortly thereafter, we began full-time experiments, and more plasma physicists joined us. The burnout—turbulent heating program was concluded, and Rodger Neidigh, Lee Berry, and Bill Wing pitched in with their full creative talents.



Lee Berry reveals his obvious pride and pleasure in the big fusion furnace he helped to build.

It was not terribly surprising in the summer of 1971, when we first generated the plasma, that we did not have the world's best tokamak plasma. Our budget-priced liquid-nitrogen cooling system was not adequate, so the first operation had to be at room temperature, where only one-third of the full design magnetic field could be produced. By the end of the year, we had proved out the engineering, installed the cooling system, and were operating at liquid-nitrogen temperatures.

Then came the very good year of 1972. When we first began operating, we had a plasma that was characterized in the lingo as "dirty, resistive, and non-interesting." By thinking and trying and pushing and eventually by cleaning up the liner by baking and discharge cleaning (that's running unconfined plasma to scrub the walls), we found a great change, from a very dirty plasma with lots of x rays to a very clean plasma with very few x rays and more interesting properties. This understanding in operation technique — proper cleaning of the device; running with the appropriate pressures, densities, and magnetic fields — led us to work with ever more interesting plasmas. Unfortunately, some of our most important diagnostic equipment still was only marginally working at this time, and our somewhat intuitive appreciation of ORMAK's performance was not properly documented. So 1972 was spent developing and perfecting the diagnostic techniques that were all to come to fruition at the end of the year.

The early part of 1973 saw three months of very exciting, productive thermonuclear research that led directly to the recent successes in the ORMAK program. There were a number of specific diagnostic inputs. Besides his important contribution to the laser development with Masanori, Bill Wing was working on two x-ray measurements that provided corroboration for the laser measurement of the electron temperature. John Clarke was joined by Don Bates from Instrumentation and Controls Division, and together they sweated through the year trying to make a microwave interferometer work to measure the plasma density as it changed during a discharge. Rodger and Rand worked on many techniques in spectroscopy to get a handle on the impurities inside. And as one of his many jobs, George succeeded in getting the large, complicated transistor-controlled battery supplies he conceived to behave. Joining the effort in 1972 was Clarence Barnett, who, with Jack Ray, developed the low-energy neutral-particle energy analyzer which measured ion temperature, and it too proved successful at that time. Lee Berry became, and has continued to be, the man on the spot, making the operating decisions on an hourly basis.

While all of this was going on on the experimental side, the theoretical side was slowly developing. John Clarke has been bridging the yawning gap between experimental data and theoretical bases with existing specific applications or by adding his own newly developed theories to the available information. In the last couple of years, John Hogan and Jim Rome had been working steadily in the tokamak theory area with Bob Dory and, more recently, Jim Callen, on leave from M.I.T. These fellows are involved in two kinds of efforts. One is computer simulation of the ORMAK plasma to let us quantify some of the models; the other is their analytical plasma physics work on some of the more basic questions of limitations in the tokamak idea, and the importance of such items as neutrals and impurities and wall problems.

John Clarke's liaison with this group and his personal inclination to the theoretical side of the experiment fit well with the organizational structure, in which Lee Berry is the leader of the ORMAK operating group; John, until he became division director, was the tokamak confinement physics group; I have been heading up a group concerned with the planning and engineering aspects of future devices; and George who has contributed to all three areas, is chief of the entire ORMAK section. George and Lee reported many of our findings at an international specialized conference in Munich last March. At that point, we could really say "it looks as if your trust paid off" to Herman Postma, who, in the best philosophy of technical management, had, while director of the division, *enabled* us to do these things.

The fall of 1973 saw the first effects of the encouraging physics results achieved by the ORMAK operating and confinement physics groups. In addition to the heightened excitement of our new understanding of the physics, there is an ever increasing esprit de corps among the staff. Dick Colchin, Julian Dunlap, Glen Haste, Jim Lyon, and Norm Lazar have all joined the physicists on the job, and their help will strengthen the crucial diagnostic areas immeasurably.

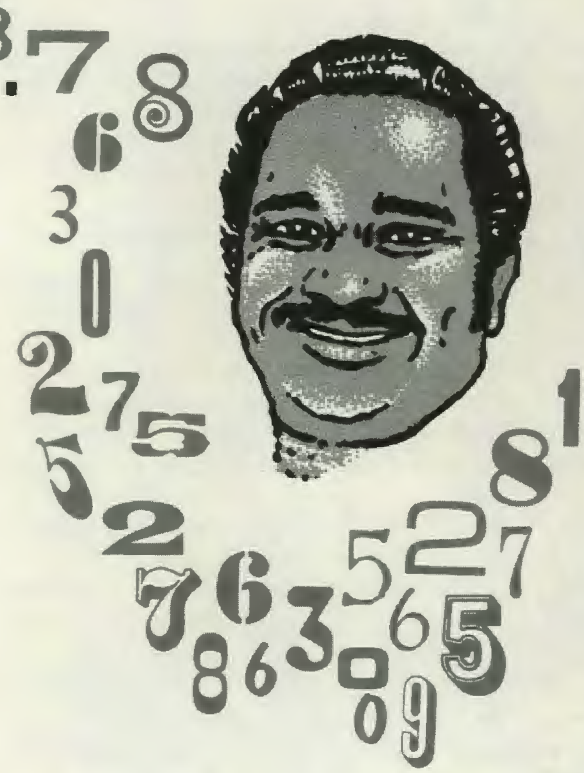
New projects are also under way, such as adding injection heating to ORMAK and a large-scale modification to the machine to double its magnetic field. These two have depended centrally upon our growing engineering staff, including Jim Rylander, Dave Lousteau, Ellis Hill, and Don Wallace (all UCCND engineers). We have just started a conceptual design for a feasibility and deuterium-tritium burning experiment over the next five to eight years. Paul Haubenreich has joined the group from Reactor Division as program manager for this design study, which includes Don Cannon and Bill Kunselman (Engineering) and Dick Lord (Physics) in addition to many others in the division.

Four and a half years ago we started with three people in one room, and now we represent the focus for about 80% of the Thermonuclear Division's dollar effort. Building 9201-2 is bursting with people, and the direct ORMAK budget in FY 1974 is what the entire Division budget was in the late sixties.

Although I have mentioned contributions of a number of the key people in ORMAK, too many more people, unfortunately, go unmentioned. Without the thoughtful assistance of members of the ORMAK and other Thermonuclear groups, other ORNL divisions, Y-12 Maintenance and UCND fabrication facilities, UCND Engineering, and UCND central support facilities such as Purchasing and the Legal Department, all the ideas, no matter how good, would have come to naught.

Take A Number.....

BY V. R. R. UPPULURI



CRITERION FOR PRIMALITY

A positive number is said to be a prime number if it has only two divisors, the number itself and unity. The first few odd primes are 3, 5, 7, 11, 13, 17, 19, 23, 29, ... Recently Henry Mann and Daniel Shanks (1972) came up with the following interesting criterion to test the primality of a positive integer in terms of divisibility of binomial coefficients.

The binomial coefficient

$$\binom{r}{s} = \frac{r!}{s!(r-s)!}$$

may be considered as the number of ways of selecting s individuals out of a set of r distinct individuals. For example, $\binom{4}{2}$ may be considered as the number of ways of selecting 2 individuals out of a set of 4 distinct individuals, denoted by A, B, C, and D. Clearly, we have the following six possible selections: (A, B), (A, C), (A, D), (B, C), (B, D), and (C, D). Thus $\binom{4}{2} = 6$.

CRITERION: n is a prime number if and only if, for all integers j between $n/3$ and $n/2$, j divides $\binom{j}{n-2j}$.

Let us use this criterion to check whether $n = 19$ is a prime or not. We first determine all the integers j between 19/3 and 19/2; these are $j = 7$, 8, and 9. Next we find $\binom{j}{19-2j}$ and see whether the particular j divides this number or not. $j = 7$ divides $\binom{7}{5} = 21$, $j = 8$ divides $\binom{8}{3} = 56$, and $j = 9$ divides $\binom{9}{1} = 9$. Therefore 19 is a prime number.

If we wish to check whether $n = 20$ is a prime or not, we first find that j takes the values $j = 7$, 8, 9, and 10. For $j = 10$, $\binom{j}{20-2j} = \binom{10}{0} = 1$ is *not* divisible by 10, and hence $n = 20$ is not a prime.

QUADRUPLETS AVAILABLE – QUINTUPLETS KNOWN?

The triplets (1, 3, 8), (2, 4, 12), (3, 5, 16), (4, 6, 20) have the following property in common: "Take the product of any two distinct numbers in a triplet, and add 1 to it; the result obtained is a perfect square." For example, in the triplet (1, 3, 8) we have $1 \times 3 + 1 = 4 = 2^2$, $1 \times 8 + 1 = 9 = 3^2$, and $3 \times 8 + 1 = 25 = 5^2$. We may now form a quadruplet (1, 3, 8, 120) and still preserve the above property, since $1 \times 120 + 1 = 121 = 11^2$, $3 \times 120 + 1 = 361 = 19^2$, and $8 \times 120 + 1 = 961 = 31^2$. With a little effort we can also find the following quadruplets: (2, 4, 12, 420), (3, 5, 16, 1008), (4, 6, 20, 1980), and so on.

In 1969, A. Baker and H. Davenport proved mathematically that we cannot add any positive integer other than 120 to the triplet (1, 3, 8) to make a quadruplet which has the said property. This implies that we cannot form a quintuplet with the above property from the quadruplet (1, 3, 8, 120). The corresponding results for other quadruplets seem to be still unknown. Can we find a quintuplet with the above property?

Charles Barton is now in the Environmental Sciences Division, where he is group leader for dose estimation studies related to underground uses of nuclear explosives. He came to Oak Ridge, however, as an analytical development chemist for Warren Grimes in 1948, and has contributed most of his work at the Laboratory in the Molten Salt Reactor Experiment. A native of East Tennessee, he received his first two degrees in chemistry at The University of Tennessee, and his doctorate at the University of Virginia. At ORNL, he has also worked on nuclear safety research, the separation of zirconium and hafnium, and, very briefly, aqueous homogeneous reactor studies. The following report is a less technical version of a paper delivered to a meeting of the Atomic Industrial Forum in Washington, D.C., in 1970, and later published in *Nuclear Technology* (July 1971).

Potential Radiation Doses from Plowshare Gas
An ORNL Study

By C. J. BARTON

Work summarized in this article has been in progress at ORNL under the direction of E. G. Struxness since 1968, first in the Health Physics Division. Stephen V. Kaye and C. J. Barton have been in charge of this investigation since D. G. Jacobs, the first group leader, left in April 1971 to take a two-year assignment with the International Atomic Energy Agency. Other ORNL contributors to the project are P. S. Rohwer, M. J. Kelly, and R. E. Moore, while C. R. Bowman and E. W. Chew of El Paso Natural Gas Company, the industrial sponsor of Gasbuggy, provided data used in the estimation of doses related to that project and participated in the studies. S. R. Hanna, G. A. Briggs, W. M. Culkowski, and F. A. Gifford, Jr., of the Atmospheric Turbulence and Diffusion Laboratory, National Oceanic and Atmospheric Administration, have also made substantial contributions to the investigations.

Shortages of electricity, fuel oil, gasoline, propane, and natural gas convince us that the energy crisis is here. The demand for natural gas grows unchecked because the price has been regulated at a low level and gas is a clean source of energy. However, United States production of gas has decreased since 1970, when total consumption was approximately 22 trillion cubic feet, and it seems unlikely, according to Sam Smith, assistant vice president of El Paso Natural Gas Company

(EPNG), that alternate sources of gas, such as overland imports from Canada and Alaska, liquefied natural gas from Africa and the Soviet Union, and coal gasification, can bridge the gap between supply and demand.

One potential source of natural gas is low-permeability gas-bearing rock formations in Colorado, Wyoming, Utah, and New Mexico. Gas from these formations, which are estimated to contain more than 300 trillion cubic feet of gas, approximately equal to the country's currently known reserves of available gas in the "lower 48," apparently cannot be recovered as economically by other techniques as by use of nuclear explosives. Some people feel that hydrofracturing, possibly with slurried explosives, can do the job, and the AEC has asked for money to investigate this possibility. Large-scale use of nuclear explosives involving development of hundreds of wells with up to five devices per well has the capability of alleviating the shortage of this important energy source. Why don't we do it?

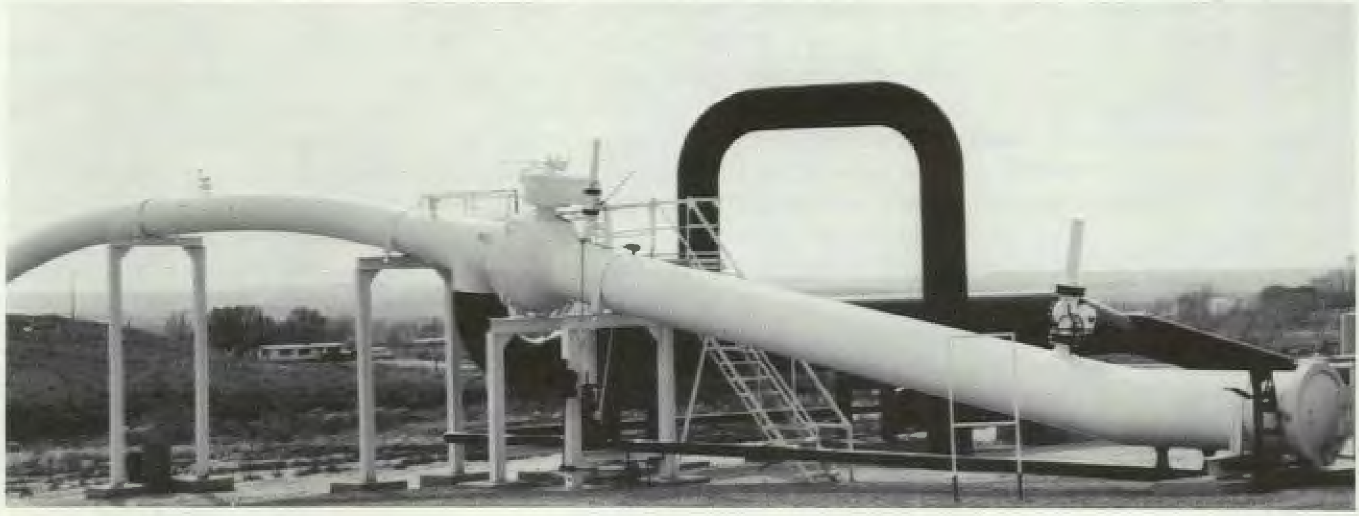
Briefly stated, there are three technical criteria that must be met before significant quantities of Plowshare gas can flow into pipelines: The process must be proven economically feasible, the seismic effects must be acceptably small, and the presence of small quantities of man-made radioactivity must be acceptable to the people who will use the gas. The ORNL studies discussed here dealt exclusively with the last point, but it appears that seismic effects will probably not be a major problem in the sparsely populated areas where most of the gas is entrapped, and, although reliable cost data are not yet available, the projected cost of nuclearly stimulated natural gas — 60 to 70 cents per 1000 cubic feet — compares favorably with the $1.20 to $1.40 cost of imported liquefied gas and synthetic gas from coal gasification. Unreliability of cost estimates indicates that the economic issue can only be resolved by vigorous exploitation of the alternate process. Because of the urgency of the need for energy, there is argument for the belief that more can be lost than gained by waiting for more exact cost estimates before pursuing the alternatives.

The first project, Gasbuggy, encountered relatively little public opposition, but this was certainly not true for the next two. A report in *Nuclear News* describing Rulison following the detonation of the nuclear device in September 1969 was headed "Rulison Stimulates Protests; and Hopefully, Gas." The headline referred to the project's troubled legal history. Intervenors carried their battle against the Rulison experiment all the way to the Supreme Court, where their cause was rejected by Justice Thurgood Marshall without comment. They did not give up the fight then but engaged in an intensive court effort to prevent reentry of the Rulison well and the planned testing program involving the burning of millions of cubic feet of gas at the well site. This effort also failed. Judge A. A. Arraj of the U.S. District Court in Denver ruled in favor of the defendants, Dr. Glenn Seaborg et al.

The loss of this court battle apparently deterred a similar legal confrontation in regard to Rio Blanco, although there was no lack of vocal opposition to this third nuclear well stimulation project. A court hearing was held shortly before the three 30-kiloton nuclear explosives, used to create the Rio Blanco well, were detonated on May 17, 1973, on the question whether the Colorado Water Commission had acted to fulfill requirements of state law on protection of the quality of water supplies. The environmentalists' claims were again rejected, and the experiment proceeded on schedule. The industrial sponsors of the project were apparently successful in their efforts to convince area residents that the experiment was safe and needed. A headline in the Grand Junction (Colorado) *Sentinel* of May 8 read, "Rio Blanco — Those Closest Seem Less Worried."

The soundness of the court decisions rejecting the intervenors' claims of a radioactivity hazard to people in the vicinity of the Rulison well has been fully substantiated. There was no evidence of a surface release of radioactivity as a result of the detonation of the 40-kiloton nuclear explosive, and as a result of an extensive surveillance performed by the National Environmental Research Center of the Environmental Protection Agency during the well testing, the lifetime tritium dose to an individual at the nearest populated location was estimated at less than 0.001 millirem. This dose can be compared to the average U.S. dose of approximately 100 millirems/year from natural background radiation. Thus, it seems certain that people living near the Rulison well did not receive a significant radiation dose from the nuclear explosion or from the burning of Rulison gas. Likewise, there was no measurable release of radioactivity from the Rio Blanco explosions, and the seismic damage was much less than resulted from the smaller Rulison explosion.

The mechanism used for collecting condensed liquids from a gas line, called a "pigging station."



However, the protestors have not yet had an opportunity to test in the courts the more fundamental question: Is an appreciable radiological risk involved in the use of natural gas from a nuclearly stimulated well? Their chance will come within the next year if an application for permission to use gas from the Rulison well materializes. Industry's recognition of the importance of the public-acceptance aspect of the nuclear gas stimulation concept is shown by a statement made by EPNG's Smith: "Technically, there will be no problem, but the psychological impact is a different matter." People will need to be convinced that the small quantity of radionuclides introduced into natural gas by use of nuclear explosives will cause no harm. Public acceptance is a prerequisite for future development of this important source of energy.

Gasbuggy Project

The Gasbuggy project, which was a joint endeavor of the Atomic Energy Commission, the Bureau of Mines, and EPNG, was the first experiment to test natural gas stimulation with nuclear devices. It involved the creation of an underground "chimney" by a 29-kiloton nuclear explosion about 4400 feet underground in western New Mexico in December 1967. The explosion allowed gas to flow from the tight (low permeability) rock formation at a much faster rate than from nearby unstimulated wells. Flaring or burning of gas from this well began in June 1968 and continued intermittently until November 1969. A total of about 250 million cubic feet of gas was flared from the well, and the Gasbuggy experiment was considered to be successful. The radioactive constituents of the gas, mainly tritium and krypton-85, were present initially at relatively high concentrations, but the concentration dropped as the chimney gas was removed by flaring and the remaining gas was diluted with uncontaminated gas from the surrounding formation. The small amount of carbon-14 found in the gas is considered less important than the other two long-lived radioisotopes. The well was reopened in May 1973, and an additional quantity of approximately 108 million cubic feet of gas was removed from the well and flared.

EPNG did not plan to put gas from the Gasbuggy well into its pipelines. However, studies to determine radiation doses that people might receive from hypothetical uses of this gas were initiated in 1968 by ORNL in cooperation with EPNG. Attention was focused first on doses that EPNG employees and members of the public might receive in the area of EPNG's gas gathering and processing system in the San Juan Basin. This portion of the study, designated Phase I, showed that use of gas for cooking or for unvented heating would be the most important routes through which people could be exposed to radioactivity from Gasbuggy gas (the critical exposure pathway), and that housewives living in a camp adjacent to the Blanco gas processing plant would be the people estimated to receive the highest potential doses (the critical population group). It was concluded that tritium was by far the most important of three long-lived radionuclides (tritium, krypton-85, and carbon-14) found in Gasbuggy gas, and only whole-body doses from this isotope expected to be received by inhalation and skin absorption were considered. Whole-body doses from immersion in krypton-85 are estimated to be only about 1/50 of the dose from tritium at the same concentration.

One important result of the Gasbuggy Phase I studies was the estimate, based on data from a field experiment, that operators would receive doses less than 1% of natural background per year during the processing of gas containing tritium at the concentration expected in a well field developed by use of nuclear explosives.

In Phase II of the Gasbuggy studies, the doses that people might receive from use of Gasbuggy gas in two West Coast metropolitan areas, the Los Angeles basin and the San Francisco Bay area, were estimated. Large quantities of natural gas are used at both locations in homes and in commercial establishments as well as for production of electricity. A tritium concentration of 1 picocurie per cubic centimeter of gas was used in the calculations because it is estimated that the tritium concentration in natural gas from a complete field of nuclearly stimulated wells will average 1 picocurie per cubic centimeter or less over the lifetime of the wells. This value was selected on the basis that future nuclearly stimulated wells produced by use of devices especially designed for the job were expected to contain gas with measurably less tritium than that in Gasbuggy gas, which was produced by a weapon-type explosive. These specially designed explosives were used for the first time in the Rio Blanco project.

Dose calculations for the metropolitan areas were aided by models of atmospheric dispersion of pollutants and computer programs developed by staff members of the Air Resources Atmospheric Turbulence and Diffusion Laboratory in Oak Ridge. The estimated average annual tritium dose that would be received by people in both areas was about 0.5 millirem, with maximum individual doses of 2.0 to 2.5 millirems/year. People living in houses having unvented heating systems and unvented appliances would be the critical population group. Radiation doses to population groups in the general public from all sources except medical exposures and natural background are usually compared to the Radiation Protection Guide (RPG) of 170 millirems/year adopted by the Federal Radiation Council. A dose of 2 to 3 millirems/year would not appear to constitute a disproportionate share of the Guide value if a cost-benefit analysis justified the use of nuclearly stimulated natural gas. Also, people living at the site boundary of light water reactors could receive doses from exposure to liquid and gaseous reactor effluents that are in this range. A proposed Federal regulation will limit these site boundary doses to a total whole-body or organ dose of 10 millirems/year from both types of radioactive effluents.

Possible exposures from uses of tritium-contaminated natural gas other than for fuel have also been examined. Examples are drinking tritiated ethyl alcohol and eating margarine produced by use of hydrogen containing tritium. Here again it was assumed that the natural gas had a tritium concentration of 1 picocurie per cubic centimeter. It was estimated that 90 cm^3 of ethyl alcohol, the amount required to give the intoxication level of 0.15% in a 150-lb adult, would contain 0.017 microcurie. Daily intake of this quantity of alcohol would result in an annual dose of about 0.8 millirem. A similar calculation was made for margarine, assuming that hydrogenation of long-chain unsaturated hydrocarbons is performed with hydrogen produced by cracking natural gas. It was estimated that if a person ate a quarter pound of margarine a day for a year, he would receive a whole-body dose of 0.2 millirem.

Rulison Project

Dose studies in connection with the hypothetical use of Rulison gas got under way at ORNL in 1971, and some results obtained were published in *Nuclear Technology* last October. The two gas companies close to the Rulison well that could reasonably be considered as potential distributors of the Rulison gas, the Rocky Mountain Natural Gas Company (RMNGC) and the Western Slope Gas Company, Rifle Division (WSGC), are small in comparison to EPNG.

Production testing of the well, which was completed in April 1971, resulted in removal of 455 million cubic feet of gas and nearly all the tritium initially present in dry cavity gas.

The ORNL dose studies examined two cases involving different well conditions. Case 1 considered estimated doses that could have been received by customers of the two gas companies if the gas present in the well before the testing program began had been introduced into either gas system at a rate of a million cubic feet per day. Case 2 involved possible use of the gas in the well in August 1971 after the gas present at the end of the testing program had been diluted with gas flowing into the well from the surrounding rock formation.

It was estimated in Case 2 that a total of 0.095 curie of tritium remained in the dry cavity gas, while the initial amount was about 1200 curies (Case 1). Large pressurized water reactors release, on the average, 850 curies of tritium per year.

Discussions with representatives of the two gas companies revealed that it would be unrealistic to assume unvented space heating with gas in Colorado, as it is illegal and the companies will not supply gas to homes that do not meet legal requirements.

Potential dilution of Rulison gas differed in the two systems. In the RMNGC system, the data on potential use of Rulison gas over a three-year period showed that it would average about 10% of the total in the first year, when the hypothetical average annual tritium concentration would be relatively high: 107 picocuries per cubic centimeter. In the WSGC system, it was assumed that Rulison gas would only reach two communities. In the smaller one, Rulison gas was assumed to make up 69% of the total used for the single year period considered, while in the other the figure was 39%. Since potential dilution in this system was much lower than in the RMNGC system, estimated doses were correspondingly higher for equal gas usage.

Atmospheric dilution of combustion products was calculated for three types of gas usage: ground-level release from homes and commercial establishments, stack releases from industrial users, and releases from all types of usage dispersed within the valley in which the system is located. Highest estimated doses were from the first type of usage, because a relatively large quantity of gas is consumed annually within a small area, but a maximum first-year Case 1 dose of 0.6 millirem was estimated for Aspen, which receives its gas from RMNGC. The high altitude of this community, coupled with maximum occupancy during the winter skiing season, probably accounts for its high gas usage. The corresponding maximum Case 2 annual dose is 0.0004 millirem.

Although it was assumed that home heating systems as well as gas hot water heaters and clothes dryers are vented, the estimated potential doses in homes having gas kitchen ranges and gas refrigerators were higher than from exposure to gas combustion products dispersed in the atmosphere. The residents of such homes are the critical population group. The maximum estimated dose they could receive in the RMNGC system in Case 1 is 6 millirems the first year; in the part of the WSGC system that would receive essentially undiluted Rulison gas, the maximum estimated first-year dose would be 39 millirems.

Another aspect of the Rulison project that we considered is the radiological impact of the hypothetical use of 94 million cubic feet of gas per day from the Rulison field to generate electricity at the Cherokee Electric Power Station, located just north of the Denver city limit. Since a number of nuclearly stimulated wells would be required to furnish this amount of gas and the change in tritium concentration of gas from such wells with volume of gas flowed cannot be predicted accurately yet, we postulated 10 picocuries per cubic centimeter for the average tritium concentration, from our and others' calculations. Computer programs were developed to calculate whole-body tritium doses to individuals as a function of distance from the plant stacks and total population doses in terms of man-rems. The maximum individual dose of 0.006 millirem/year was estimated to be received by individuals living 5 kilometers north of the station, and the total dose that could be received by 1.6 million people living around the station was 3 man-rems/year. Estimation of these doses required taking into account rather unusual meteorological conditions: the direction of the wind changes from generally north to generally south and vice versa on the average of once a day in Denver. Thus the plume from the stacks will move back and forth over the populated area, becoming more diffuse in the process, until it is blown out of the area. The estimated total population dose of 3.0 man-rems/year gains perspective when compared with the 250,000 man-rems/year received by the same population from natural radiation sources and 110,000 man-rems/year from diagnostic uses of x rays. The 3 man-rems/year population dose estimate is equivalent to the increased whole-body dose that would be received from cosmic rays if the average elevation of the 1.6 million people was increased 4 inches.

It is interesting to compare the estimated population dose from use of nuclearly stimulated natural gas to produce electricity with the dose from light-water nuclear power reactors. The estimated average population dose per pressurized water reactor is 13 man-rems/year. We assume that each reactor generates enough heat to produce 1000 megawatts of electricity. The power produced in the part of the Cherokee station that we considered is 344 megawatts. Krypton-85 gives approximately 2% of the whole-body dose of an equal concentration of tritium, but we assume that the concentration of krypton-85 in Plowshare gas is 7 times that of tritium; consequently, the whole-body dose from krypton-85 in the gas is calculated to be 14% of that from tritium. If we add 14% to the 3 man-rems/year figure for krypton and multiply by 1000/344, we arrive at an estimated total population dose of slightly under 10 man-rems/year for generation of 1000 megawatts of electricity by use of nuclearly stimulated natural gas, about the same as for the average PWR.

The above situations deal only with doses that might be received from fuel and other uses of the gas from nuqlearly stimulated wells. There is another concern that must be considered: the possibility that solid fission products in the chimney may, over a long period of time, contaminate water supplies. This possibility was examined in the environmental statement for the Rio Blanco Gas Stimulation Project. Two aquifers are located between the surface and the well chimney, but the top of the upper chimney rock fractures was postulated to be at least 3000 feet below the bottom of the lower aquifer. Thus, no direct communication path between that aquifer and the chimney was expected. However, the possibility that the more mobile radionuclides, including tritium, rare gases, and carbon-14, might move through seepage paths and eventually contaminate water supplies was investigated theoretically. It was concluded that the likelihood of adverse public health effects by this pathway is very small, and, because no liquid water is expected to enter or leave the chimney area, no mechanism could be hypothesized by the writers of the environmental impact statement whereby the radionuclides deposited on chimney surfaces could be incorporated into mobile groundwater. While further study of the long-term fate of the long-lived solid radionuclides such as strontium-90 and cesium-137 is probably justified because of the large quantities of these fission products that will be produced by large-scale underground use of nuclear explosives, there seems to be no indication at present that contamination of water supplies will be a major problem even over a period of centuries.

Future Rulison dose studies will consider doses that might be received by people in the Denver metropolitan district if gas from a number of nuclearly stimulated wells in the Rulison field were to be used in this area. These will be followed by similar studies in connection with the Rio Blanco project sponsored by CER Geonuclear Corporation and Equity Oil Company and possible future projects like Wagon Wheel. Possible doses through exposure pathways other than those thus far considered, inhalation and skin absorption of tritium and immersion in krypton-85, will be carefully considered in all studies.

Conclusions

The only radioisotopes of consequence remaining in nuclearly stimulated natural gas several months after the detonation are tritium, krypton-85, and carbon-14. Tritium is by far the most important of the three long-lived isotopes from the standpoint of possible whole-body doses that might be received from exposure to gas combustion products. The tritium concentration in future nuclearly stimulated wells can probably be predicted fairly accurately when data become available from production testing of Rio Blanco, where explosives designed for minimum tritium production were used. Dose estimates for typical situations in which gas from the Gasbuggy and Rulison wells could have been used were but a very small fraction of the RPG average dose of 170 millirems per year permitted to a suitable sample of the exposed population under Federal regulations from all sources of radiation except medical sources and natural background.

The anticipated low doses by no means guarantee public acceptance of nuclearly stimulated gas. Intensive programs are clearly called for to provide potential users with data to put these doses into perspective. This publicity may not have the desired effect of overcoming the people's fear of the low dose levels that would result from use of Plowshare gas. Furthermore, regulatory agencies such as the AEC and the Environmental Protection Agency must also be satisfied that the benefit to the public that would result from the availability of such gas would outweigh the risk of exposure of millions of people to even these low levels of radiation exposure.

People accept background radiation because they have little control over it. In this case, society does have a choice, but once it is made, individuals will have limited control over their radiation exposure from this source if Plowshare gas gets the go-ahead. And so, in the final analysis, the decision concerning use of nuclear explosives to increase natural gas production will rest with the people potentially exposed. They will have to weigh the cost, including exposure to low levels of radiation, against the benefits.

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LAB ANECDOTE

Letter to Lab Anecdote Editor

Dear Dr. Pomerance:

I enjoyed reading your lab anecdote in the Fall 1973 issue of the Oak Ridge National Laboratory Review.

You seem to be suggesting that Charles Coryell agreed to provide 100 curies of barium-140 as a means of gaining authorization to build Building 706C. That may be, but I think it is more probable that his real reason for making the barium-140 was to win a nickel bet from Dr. R. L. Doan, who at the time was Director of Research of Clinton Laboratories. At any rate, I have hanging on my office wall a poem (copy attached) written by Doan and given to Coryell on the occasion of the shipment of the first 50 curies of barium-140 to Los Alamos.

I was the one who had designed and made the "hand blown glass system" and the so-called "Stang reactor" to which you refer in your article. Unfortunately, I found it necessary to leave Oak Ridge in September 1944 when things were just getting exciting and everyone was making the same trip in the opposite direction. Like Coryell, I too worked around the clock, putting in 110 hours of work in 706C during my final week and nearly that much during the two previous weeks. Feeling that, as a result of all of this, I should be the custodian of the nickel that he had won from Doan, Coryell subsequently presented it to me along with a copy of the poem signed by some 22 people who were in one way or another connected with the work.

Although the work of the Manhattan Project derived from a tremendous number of people, it is hard to imagine half as much being accomplished except for the exhilarating inspiration acquired when one experienced the Aurora Coryellis emanating from Charles Coryell.

(signed) L. G. (Lou) Stang, Jr.
Head, Hot Laboratory
Brookhaven National Laboratory

ODE TO A LOST NICKEL

A nickel is a piece of dough
Whose value can't be questioned
Since Uncle Sam defines it so
You gotta take it, even though
You know darn well, as alloys go
Intrinsically it's less than.

Now this same nickel can become
A symbol strong and mighty
When harnessed to a proper bet
Involving jobs that must be met
By Coryell and his whole set
Of chemists, bright but flighty.

We lost the bet — we're glad *we lost*
And so, we judge, is Oppie,
Who, wrapped in darkness and in doubt
Not knowing what 'twas all about
But sure that it would not get out
Had almost blown his toppie.

So sing the praises of the coin
And sing also of chemists
Who break the rules and spoil the view
And yet when pressed with something new
They make a bet and then come thru
With much more than the limits.

— R. L. Doan
September 18, 1944

Sent to Louie Stang, one of the Founding Fathers, on the successful conclusion of Prep #3.

(Signatures)

Charles Coryell, Ed Brady, L. S. Goldring, Gerald Strickland, Dick Money, Henry Zeil, Rich Bersohn, Robert Garber, Evan J. Young, Bob Humphrey, Sandy Willner, Earl Purchase, Edward L. Nicholson, F. Robert Lesch, Don Richardson, Art Ross, Sidney Weiss, William P. Bigler, Jere D. Knight, Ted Novey, Henri A. Levy, Jack Siegel, and Harrison Brown

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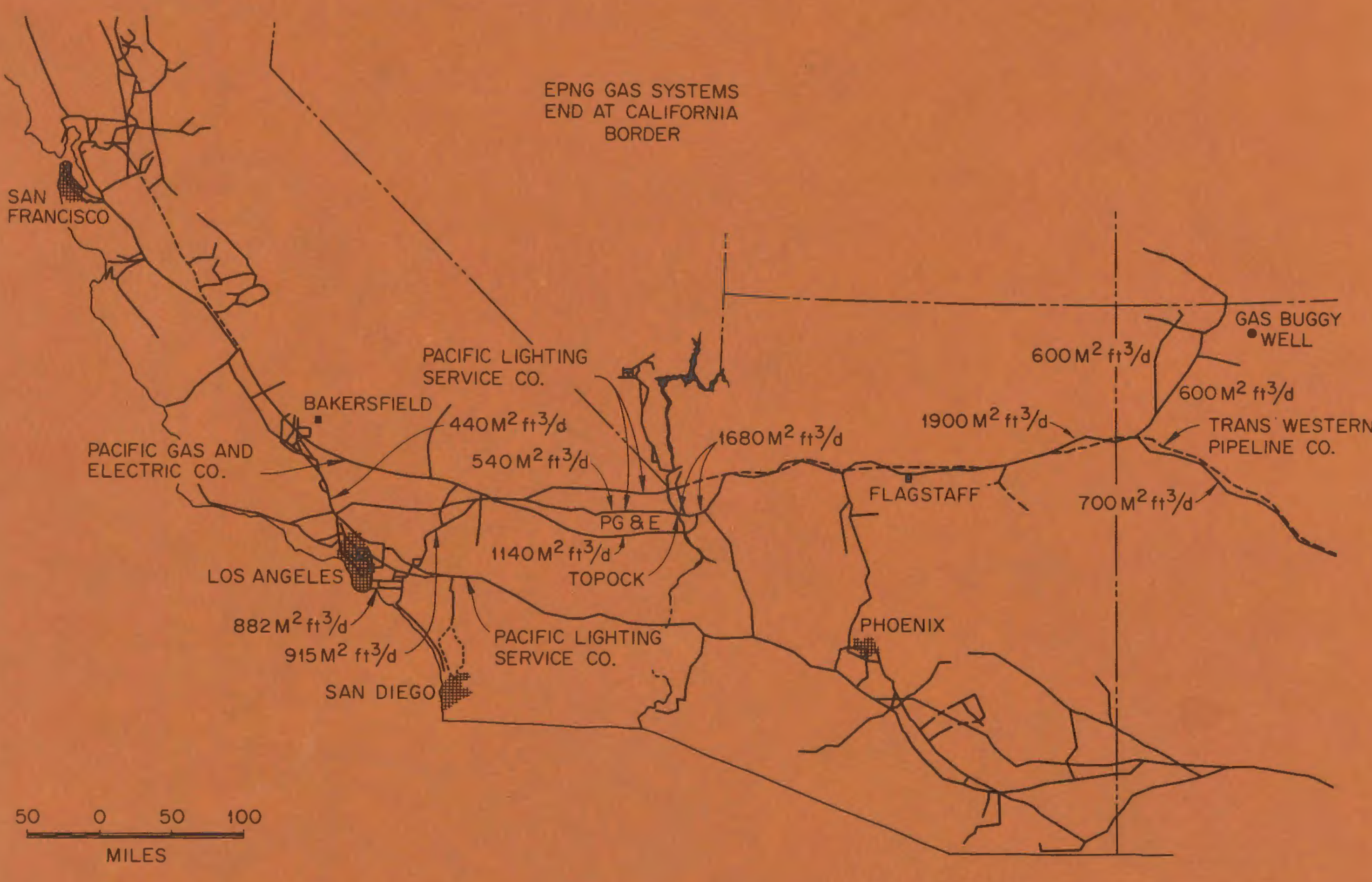

Natural gas transmission systems in the southwest USA, including that of the El Paso Natural Gas Company, for whom ORNL scientists mounted a study on the safety of using nucleraly stimulated natural gas, an account of which begins on page 19.