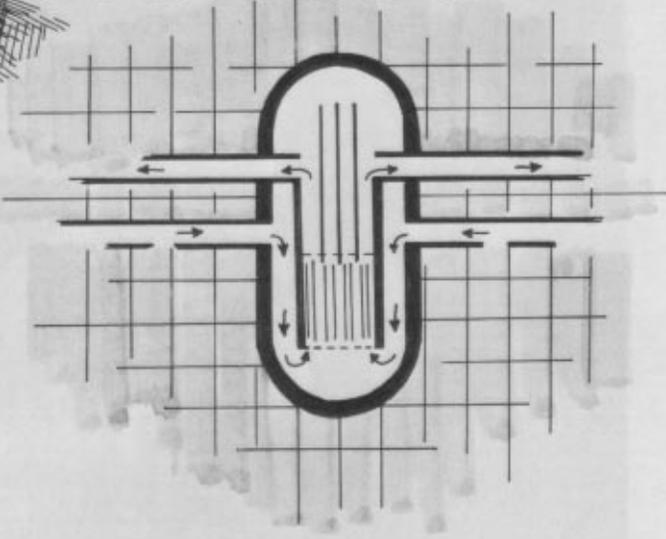
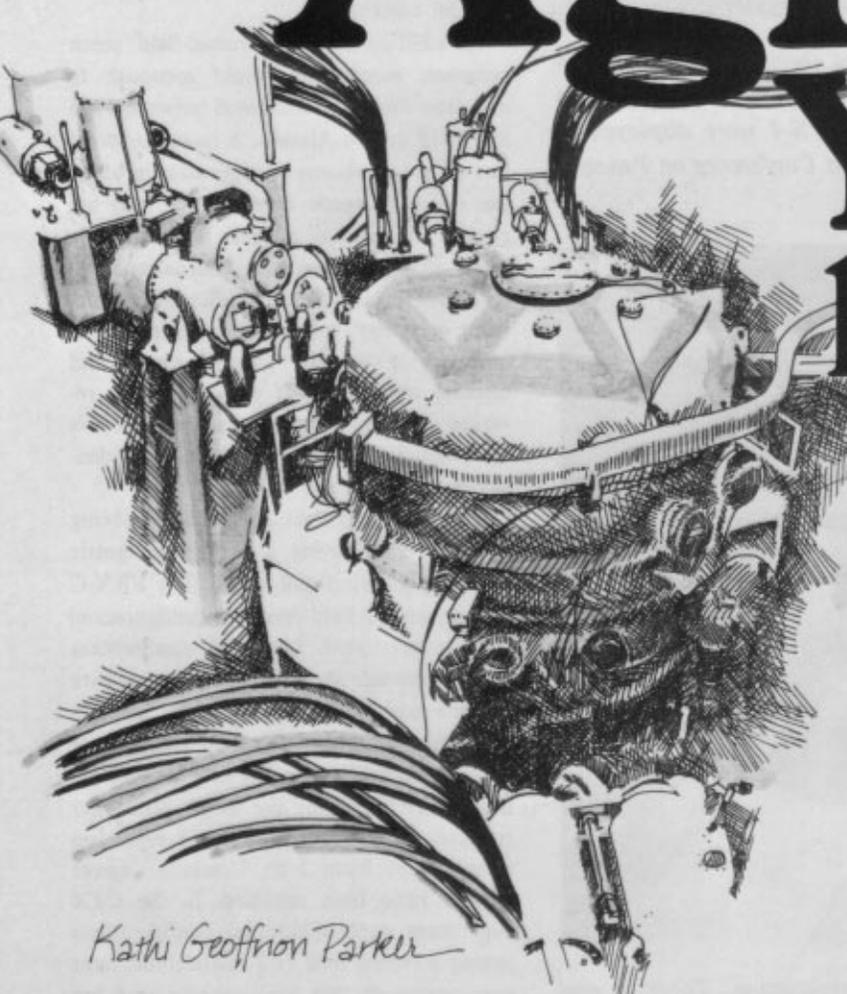


the
Agnew
Years
1970-1979



Kathi Geoffrion Parker



Vintage Agnew

June 15, 1966

talk to Army group

... It does seem to me that with all the basic information we don't know, with all the problems in which we are involved, with all the deficiencies that exist in the world, that a scientist should in some degree . . . stick his head out of his office or his laboratory, whether he is a first year lab assistant or last year's Nobel laureate, and ask himself . . . Is the problem I'm working on one of those whose solution might directly help my colleagues or my fellow countrymen right now or in the future? If the scientist doesn't know, it is probably because in his narrow pursuit of his particular field he actually doesn't know what is going on around him. He may not have taken the time to even find out, or worse, he doesn't want to. This attitude worries me very much.

July 8, 1970

talk entitled Tactical Nuclear Operations

In the last 20 years we and other nations have been engaged in numerous arguments which resulted in physical combat. The political and military approach to these confrontations has been to rely on conventional weapons systems. Although we pretend to have a tactical nuclear capability, we have no doctrine for carrying out tactical nuclear warfare, nor do we seem interested in developing a tactical nuclear capability. Yet, if properly structured, it could conceivably deter these lesser wars—or at least make our forces more effective if they are challenged

Let me take as an example a particular military target in North Vietnam: the Thanh Hoa Bridge. This bridge is about 540 feet long. For military reasons we decided it had to be destroyed. . . .

We flew 657 strike sorties. In addition we employed approximately 300 supporting sorties. We dropped [2.5] million pounds of bombs, we lost 9 aircraft. In addition three optically guided Walleyes were launched at the bridge. Each of the Walleyes actually hit the bridge but the 750 pound warheads were insufficient to seriously damage it. We never were able to collapse a single span. Present rumors state that the bridge doesn't exist but is simply painted on the water. . . .

Had [the] Walleyes carried a [subkiloton] nuclear warhead. . . such as at long last is being provided in the Mk-72, the bridge would have been put out of action. Instead of expending 2.5×10^6 pounds of high explosive in about 700 sorties, the mission could have been accomplished with at most *two* strike sorties and a few cover aircraft The collateral damage from [such] a ground [nuclear] burst . . . would be. . . negligible compared to that actually imposed with conventional explosives as currently delivered with free fall bombs. . . . [Moreover] burial to optimum depth (which maximizes cratering effects and minimizes fallout) is feasible with devices now under development.

July 13, 1971

talk to the National Classification Management Society

Almost my whole professional career has been involved with technical work which has had a running battle with classification. To be very frank with you I've never won an argument with a classification officer and I've never understood why I've continued to lose. . . .

In spite of our country's background in freedom. . . we all know there is a tremendous amount of secrecy and classification involved in government and private industry. Some of it is certainly warranted and will always be required if we are to have a competitive capitalistic industry. But there comes a time when secrets are no longer secrets and impedances imposed by secrecy or classification are no longer warranted. . . .

[For example] I believe that the philosophy or concept of embargoes on materials, products, and technology in today's world is archaic. . . . In fact. . . if the intent of the embargo concept [as embodied in the Battle Act of 195 1] was to guarantee U.S. conventional military superiority it has failed. . . .

Not so long ago the President announced that he was going to attempt to open trade with China. I don't believe there is a person here who doesn't believe that is a splendid idea. But, . . . to pacify our basic fears, which I believe are no longer warranted, the White House quickly stated that of course we wouldn't allow the export of commercial jet aircraft or diesel locomotives. . . which the White House then stated that China very much wanted. . . . Do we really believe that in 1971 a nation of 750 million people shouldn't have commercial jet aircraft? . . . Do we believe that if they don't purchase them from us they won't be able to buy them from France or even Russia? Do we really believe that having jet commercial aircraft will jeopardize the security of the U.S.? . . .

Providing China with a modern airline with aircraft, ground equipment, airfield and navigational aids would be a real shot in the arm for our economy. We ought to sell what we can. . . . Why should ping pong players have to ride in DC-3's or coal burning locomotives?

February 4, 1976

Chemical reactions give a few electron volts per interacting atom. Fission gives two hundred million electron volts per reacting nucleus. This factor of a hundred million has a favorable impact not only on the energy produced but also on the environment with regard to the amount of raw materials required and the wastes produced. A thousand megawatt coal plant produces six million cubic feet of ash per year, a fission plant less than a cubic yard.

Sooner or later the whole world will realize that they cannot turn their backs on the benefits of the nucleus. Today fission, hopefully in the next century fusion.

paper presented at the
Annual Joint Meeting of the
American Physical Society
and the American Association
of Physics Teachers

April 14, 1977

... [Most of] the world's population. . . [has] great expectations. Part of their expectations are due to the sort of instant discontent that we through the media have been beaming for many, many years. They expect in a very short time to achieve a standard of living that's commensurate with ours, and I would submit that we're not going to achieve this standard of living unless they have plentiful relatively inexpensive energy. This can be provided, but. . . only. . . through what I'll call technology. It's not going to be achieved through wishful thinking or abstinence in certain technologies.

talk at Belgium American
Chamber of Commerce Luncheon
in honor of Dr. Agnew

April 19, 1977

. . . I do not believe we can maintain a technology base or the necessary cadre of first-class scientists and engineers to enable the USA to have a nuclear weapons design capability for more than a few years if testing ceases.

letter to Congressman Jack F. Kemp

September 8, 1977

. . . If it is the considered opinion of the Senate that the United States has no further needs now or in the future for new untested types of warheads having yields substantially greater than the 150 kilotons limit of this agreement, then the [threshold test ban] treaty [under consideration] will have no appreciable impact on our defense posture in the immediate future. However, if you believe that there will be requirements far new untested designs of yields considerably larger than 150 kilotons, then if this treaty is ratified our defense systems will eventually have to bear a penalty in payload weight, physical size, and perhaps even in the additional use of fissile materials. . . . It simply will not be prudent to put into the stockpile designs which represent a large extrapolation from tested designs...

testimony before
Senate Foreign Relations Committee

I personally would not support any treaty further limiting nuclear testing until meaningful agreements on SALT and Mutual Balanced Reduction of Forces have been ratified. . . . I stress this relation to other arms control progress because we need some clear sign of Soviet restraint in their weapons build-ups and because our own nuclear posture must be appraised as a consistent whole. . . .

For those of you who may wish to remind me of the destruction caused by a nominal 15 kiloton bomb, may I remind you that I flew on the Hiroshima mission and have participated in the major thermonuclear tests which this country has conducted. As an aside, I firmly believe that if every five years the world's major political leaders were required to witness the in-air detonation of a multimegaton warhead, progress on meaningful arms control measures would be speeded up appreciably,

October 2, 1977

talk at 1977 National Conference
for Advancement of Research

I still remember when Seamans took over the AEC, he said, "ERDA will not be a warmed over AEC." He was right; except for the weapons program and a few other areas, it became a half-baked NASA . . . I believe the dismal track record of ERDA was due to the lack of appreciation of how fundamental [our] basic but relevant research is to the successful implementation of any development or engineering project. . . .

Hopefully, this attitude will not prevail [in] the DOE [under Schlesinger] . . . because of [his] past attitude when he was with the AEC. For tens of years under the most absurd secrecy . . . the AEC had been conducting research on centrifuges. Their engineering was superb, but their basic understanding . . . of how centrifuges really work, which involves complicated fluid dynamics, was lacking. After Schlesinger came on board. . . he simply directed that the weapons people, with their advanced, basic science capabilities in . . . fluid dynamics, be brought into the program. In a few months . . . the weapon design theorists attacked the problem, developed codes to analyze the action of the gas inside the centrifuge, and allowed the centrifuge to become a viable option. . . for uranium enrichment. Had Schlesinger not broken down the compartmentalization. . . the centrifuge developers would still be using an Edisonian, build-and-try technique with a six months turnaround time. . . .

Many people don't realize the . . . stimulus given to major scientific programs in the U.S. today, which started from work initiated through the weapon's supporting research program of the AEC Some originating at Los Alamos are:

1. SHERWOOD - controlled thermonuclear fusion
2. LAMPF - medium energy physics facility
3. ROVER - nuclear rocket research
4. LASER FUSION
5. JUMPER - laser isotope separation
6. VELA - nuclear test detection
7. SMES/SPTL - cryo-engineering
8. NUCLEAR SAFEGUARDS
9. GEOTHERMAL ENERGY

. . . the support of basic science is vital to any development work; it can't be programmed and micromanaged. It must be supported as if it were one of the art forms, which it really is.

However, one can insist in these trying times, where we are confronted with specific problems, that for the most part research be conducted in relevant fields, but not that it be necessarily relevant today If one does not provide this freedom and enlightened management, then the country will end up with the run-of-the-mill, average, plodding, pseudo-research institutions, which will be busy supplying the last digit after the decimal point that is so dear to the handbook publishers. The innovative wild men and women who are always on the leading edge of science and technology will not be part of the team, And we need them.

1954

State Senate campaign slogan

"A person of integrity stays bought!"

The Times They Were a Changin'



Raemer Schreiber (left) joined the Laboratory in 1943. In the '50s he was the Leader of the Weapons and the Nuclear Propulsion divisions and then, in 1961, was appointed Technical Associate Director. He remained in that position after Agnew became Director until "Harold, in 1972, decided I was really Deputy Director, so he changed my title." Robert Thorn (right), currently the Deputy Director, first joined the Laboratory's Theoretical Division in 1953. His numerous administrative positions included Theoretical Design Division Leader, Associate Director for Weapons, and, from March to July, 1979, Acting Director of the Laboratory.

SCIENCE: *Schreib, you were Technical Associate Director from 1962 to 1972 and as such were part of the transition between the Bradbury and Agnew eras. What do you feel was Agnew's vision of the Laboratory when he became Director?*

SCHREIBER: Only Harold can answer that question definitively. I do know he was always intensely proud of the capabilities of the Laboratory and did not feel that its expertise needed to be confined to nuclear physics. He was willing to tackle any scientific or technological problem worth solving. Generally he took the attitude, "If we don't

have the experts, we can get them." You should remember that at this time reactor work was shifting over to commercial utilities, and the AEC was clamping down on new reactor concepts. Harold saw that the future of the Laboratory might well be in other directions than just pure nuclear physics.

SCIENCE: *Bob, you were the Theoretical Design Division Leader and then later Agnew's Associate Director for Weapons during the '70s. What do you feel he hoped to accomplish when he became Director?*

THORN: I think Harold felt we needed to

regain the initiative in weapons development that we'd lost to Livermore. In 1970 this Laboratory was still largely a weapons lab, but Livermore was doing a better, more aggressive selling job and was pushing for the enhanced radiation weapons and all the strategic weapons—the nuclear warheads for Minuteman and Polaris. Their reputation was better than ours, or at least perceived to be so by some people. Harold's vision was to restore the luster that Los Alamos had lost. It's true that he thought the Laboratory was premier in all fields and he would undertake anything, but above all he wanted to be first in our principal mission of weapons development.

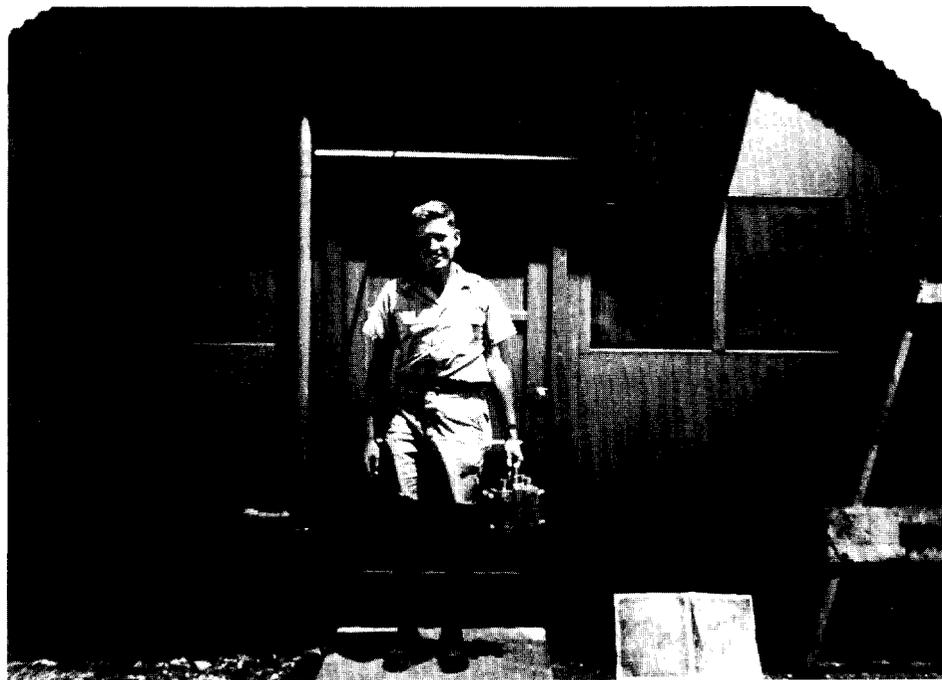
SCHREIBER: There's another aspect to the Bradbury-Agnew transition that I feel is also important to recognize. At the end of World War II, when Norris became Director, a lot of people who had served during the war years on Laboratory advisory boards simply disappeared. Norris really didn't have an existing management structure to work with, so he was able to start with a clean slate. Twenty-five years later the Laboratory was firmly established, and Norris was working with a senior staff of people he'd worked with for years. He knew what they could do and what they were interested in doing, so he was able to take a low profile and run a fairly relaxed ship. But many of these people were also approaching retirement. Norris knew and they knew that major changes would have to be made in a few years. However, Norris did not want to make changes that would obligate the incoming director. When Harold took over he had the chance to assert his leadership at once. It was an appropriate time to reshuffle personnel and his reorganization took place over the first couple of years.

THORN: I agree. Both Oppenheimer and Bradbury operated with small staffs and were able to stay close to all aspects of the effort because there were only a very few major programs. For example, I think when

Harold took over there was the Weapons program, the Space Nuclear Reactor program, and the Fusion program. By the end of Agnew's directorate there were 600 programs! Harold realized that things were getting more complicated and set up two associate directors, one for weapons and one for research, to handle the technical programs. He inherited a Technical Board from Norris made up of the director's immediate staff, division leaders, and department heads, but as time went on this function was largely replaced by the associate directors working with their divisions.

SCHREIBER: In fact, Norris and Harold had different personalities, different approaches to management, and the Tech Board meetings show some of these differences. All major policy decisions under both directors were discussed or announced at these meetings. Norris' favorite technique was to state the question, perhaps offer some possible answers, and then sit back with his feet on the table and let people talk. He might pose some questions from time to time, but generally he let everyone have his say. Quite often a consensus would be reached, in which case he'd simply say, "OK, let's do it that way." Or there might be times when violent differences of opinion would emerge. Then he'd either rule one way or another or suggest that we adjourn and think it over some more. Harold preferred to research the subject first, make up his mind in advance, then announce his decision at a Tech Board meeting. He would listen to contrary arguments to see if anyone really couldn't live with the decision. As a result, he might modify his stand, but he did not encourage prolonged debate.

Harold could be fairly hard-nosed when it came to the shuffling of senior personnel. Perhaps he had to be since he was dealing with entrenched incumbents, but he also believed that the future of the Laboratory depended on bringing in fresh people with new ideas and on rotating responsibilities to provide management training. This was a deliberate stirring of the Laboratory by



Agnew at Tinian in 1945.

Harold, and he put his priorities for the Laboratory above the feelings of those displaced. On the other hand, he was quite compassionate in dealing with hardship cases anywhere in the Laboratory.

One thing was the same under both directors: it was implicit that management get their jobs done without formal directives or instructions. The general attitude was, "If I have to tell you how to do it, you shouldn't be holding down that office."

SCIENCE: *How did management change from the beginning to the end of the Agnew era?*

SCHREIBER: It got more complex. Because of the small number of major programs, interdivisional coordination under Bradbury was handled by steering committees or working groups usually chaired by one of the division leaders. As a result, program direction was quite decentralized and the Director's staff was small. But then the AEC discovered "program direction," which is a

polite way of saying that it was building its staff to participate more directly in calling the shots out at its laboratories. Moreover, it was subdividing its budget and personnel to enforce compliance with its directives. This process has continued through the ERDA and DOE regimes and is largely responsible for the large growth in administrative positions in the laboratories themselves.

For example, the Budget Office under Bradbury had two men and a secretary. Harold had to set up the Financial Management Office which grew to about fifteen to eighteen people. Periodic reports and what were called Form 189's were required for every project. This resulted in an enormous amount of bookkeeping, so the accounting office had to grow. There were a number of requirements from Washington that Harold at first just flatly refused to comply with. He won some of these, but lost others.

THORN: In fact, by the end of Harold's tenure it was obvious to many, including



Harold and Norris about the time of the transition between the two directors in 1970.

Harold, that substantial management changes had to be made. The changes were largely necessary because of the increase in programs, program direction from Washington, and accountability. As a manager, you had to control and review the yearly proposals to make sure that they went to Washington in the proper form and that they were the kind of thing the Laboratory wanted to do. In addition you had divisions over which you had to exercise line management. So you were both program manager and line manager. And then you presumably were supposed to remain technically competent. It was just too much to do—too much for a director and two technical associate directors to do, Harold wisely held reorganization in abeyance and allowed his successor, Don Kerr, to implement his own management system.

SCIENCE: *Bob, getting back to Agnew's desire to regain the initiative in weapons development, what were the major ac-*

complishments in the Weapons program in the '70s?

THORN: When Harold took over, Livermore was responsible for the development of all the strategic missile warheads, which were the big prestige items in the eyes of the public and the Defense Department. But Harold fought vigorously to acquire new warhead responsibilities.

SCHREIBER: Harold was a very aggressive salesman.

Thorn: Yes. He started the Weapons Program Office and the Weapons Planning Office. These were supposed to be part of what you might say was our marketing group. By backing up this group with the technical people in the design and engineering divisions, we could be more aggressive about going out and getting these weapons systems. He also tried to reinvigorate the Weapons program here by splitting the old Theoretical Division—the design part away from the theoretical physics part—so as to

provide more emphasis to weapons design. As a result of these efforts, we were awarded responsibility during his tenure for the W76 used in the Trident warhead, the W78/Mark 12A used in the Minuteman III warhead, and the W80 used in the air-launched cruise missile warhead. Also, the Laboratory introduced the first enhanced radiation bomb into the stockpile and developed new versions of the air-carried B61, a general purpose bomb and warhead for short-range attack missiles. One of the weapons developments that Harold felt most proud about was the introduction of insensitive high explosive that makes the stockpiled weapons containing it much safer to handle. An accidental detonation that scatters radioactive plutonium becomes highly unlikely.

SCHREIBER: Another point is that Harold took over at the time when the national emphasis was shifting from aircraft to ballistic missiles, so the major weapon developments were aimed at matching the bomb to these new carriers. Microelectronics and the ability to communicate or to install elaborate instructions in missiles opened a new era in the mating of warhead to delivery system. Ideas such as smart missiles that could track a target or the concept of multiple independent re-entry vehicles (MIRVs) were growing. These ideas required new weapons, but not in the sense of changing the basic physics of the innards of the device. Rather they were new weapons in the sense of changing the configuration to match size, weight, and shape requirements of the missile warhead or in changing how the weapon was told to behave to match the safing, arming, and fuzing requirements of the delivery systems. These requirements led to significant and detailed changes involving highly intricate engineering of the warheads. Also changes were made to improve yield-to-weight ratios and to extend the useful stockpile lifetimes of the warheads. Because of the necessarily close relationship between warhead and delivery system, this period was one of very intensive collaboration with the

Defense Department.

THORN: The collaboration was revitalizing. Originally I think Los Alamos slipped because many of the people here had been in the business since the beginning—twenty-five years—and some of them had grown tired of the arms race. Their attention shifted to diversifying into other fields. As a result, the Laboratory was not putting the kind of attention into weapons development that a weapons lab should be putting into it. After all, we're not here to argue for arms control, we're here to design weapons. But in this period we started to participate more actively with the Defense Department, both by designing to meet their stated weapons needs and by developing our own ideas and trying to sell them.

SCIENCE: *The diversification into non-weapons programs, then, did not start with Agnew?*

SC HREIBER: In one sense, yes. There was a strong effort under Bradbury to diversify into nonweapons applications of nuclear energy, but this was generally limited to nuclear reactors and nuclear fusion. In the '60s there was considerable encouragement by the AEC to try out all sorts of ideas for building reactors, and Los Alamos had projects in nuclear rocket propulsion, the thermionic reactor for generating electricity directly, the graphite-based, ultra-high-temperature reactor, reactors in which the fuel was molten at operating temperatures, and so forth. It was a time when anybody who had an idea that would stand up under peer scrutiny could try it out. But, as I said earlier, about the time of the Bradbury-Agnew transition there was a budget squeeze, and the AEC curtailed support of new reactor work to concentrate on the commercial development of the light-water reactor and on research and development of the liquid-sodium-cooled breeder reactor. This created an immediate need at Los Alamos to find other activities for many of the people who had been in the field of reactor development.



Harold with Edward Teller in 1973.

Part of the need was satisfied by a push into energy programs. For example, the potential of lasers to do isotope separation and to initiate fusion reactions was brought to Harold's attention, and he authorized an immediate expansion of this work. A bit later the oil crisis of '73 and '74 stimulated interest in alternative energy sources, and that led to substantial programs in solar energy, hydrogen as a fuel, and hot dry rock geothermal systems. Other energy programs included synthetic fuels, fuel cells, and superconducting transmission lines. Our large computer facility made possible demographic and socio-economic studies of energy resources and energy distribution.

THORN: In fact, the push into the energy programs during the '70s was so vigorous that the Laboratory, rather than shrinking, almost doubled in size. Harold had correctly recognized that times were changing. He responded by infusing the Laboratory with a spirit of experimentation based on the exper-

tise we'd acquired over the years dealing with multidisciplinary problems in weapons research. It was a period of excitement and challenge.

It was also true that many of the programs were unrelated to our principal mission, and the Laboratory lost a great deal of the cohesive spirit that bound it in its first twenty-five years. What happened was that in response to the energy crisis the AEC had its charter broadened: it could look into other energy programs besides nuclear. The government thought the way to solve the energy problem was with an influx of money, and the fastest way to get started was at the level of the national laboratory. Of course, they found some eager people here quite willing to work on these problems. But as far as having any overall coherent plan—that was missing! The result at the Laboratory was a multitude of programs. When everyone had been paid from the same source—the weapons program—you could



Harold spearheaded the drive for the Laboratory's National Security and Resources Study Center, shown here under construction in 1976.

walk up to somebody, ask him to do something, and he'd get it done. Today you ask, and he'll say, "I can't do that. I'm working on another program, and my sponsor won't allow me to work on yours unless you give me some money." That's an example of what I mean by a loss in the spirit of cohesiveness.

SCIENCE: *What were some of the outstanding nonweapons programs under Agnew?*

SCHREIBER: Well, as I mentioned before, laser fusion and laser isotope separation were initiated by Agnew. A great deal of excellent research has come out of those programs. There's LAMPF—the Los Alamos Meson Physics Facility—which was

conceived in the Bradbury years, then realized in the Agnew years. LAMPF, of course, is a story in itself.

We have the new plutonium facility, which is the finest plutonium research and development facility in the country, perhaps in the world. That such a facility was necessary had been recognized at Los Alamos for years, but Harold was the one who convinced the AEC. The old DP site had been built in a hurry as a temporary facility and was being kept in a safe operable condition at considerable maintenance cost. So first the AEC had to be made aware that something should be done. If they were just going to shut the old site down, what then? There

were two other reasons the decision was held up: environmental requirements had been changing so that it was hard to pin things down, and it was going to be a very expensive bit of construction because of the need for safeguards and protection against everything from a laboratory fire to an airplane crashing into the building. In essence the AEC was committing itself to having all plutonium research done at the new facility wherever it was built. Much of the selling was to point out the expertise in plutonium research that already existed here at Los Alamos. Construction of the new facility finally started in 1974.

The hot dry rock geothermal concept was an outstanding program under Agnew. Morton Smith should be given credit for initiating and selling this one—he probably made two thousand speeches on the subject. As I recall, preliminary exploratory work had been authorized by Bradbury, but a full-scale effort was not mounted until later when manpower, including chemists and materials fabrication people, became available when the Rover (space nuclear reactor) and UHTREX (ultra-high-temperature reactor) programs were halted.

In a similar vein, work on reactor safety analysis was a natural spin-off from the various experimental reactors that had been designed and built here. People who had been in the UHTREX and LAMPRE (molten plutonium reactor) programs and who were familiar with the safety requirements of reactors moved into that field.

THORN: I agree, Schreib, except I would attribute the reactor safety program more to Kaye Lathrop and other theoreticians who were using large computer codes for weapons simulation and started developing similar codes for reactor safety analysis. They expanded weapons transport codes by adding the appropriate equations of state, accounting for two-phase flow of water and steam, and so forth. But more important, they brought with them the experience of using large codes to model complex problems.

In contrast to many of the other nonweapons programs, the nuclear energy programs at Los Alamos have always complemented the weapons effort. Much of the work involves transport codes used in weapons calculations or involves the plutonium facility or provides useful neutronics data. In that sense, these programs have been cohesive, not divisive.

SCHREIBER: Nuclear Material Safeguards was another outstanding program: it was well under way toward the end of Bradbury's stewardship, then was expanded under Agnew. I was directly involved in its development but can take little credit since Bob Keepin was the founder and chief salesman. He badgered me into authorizing a small initial program, then parlayed that into a major effort by selling it to key officials in the AEC. He acquired equipment and laboratory area from defunct reactor programs using the "camel in the tent" approach. This approach comes from the old Arab story in which the camel outside the tent says his nose is freezing, so the owner tells him he can stick his nose in, then the camel says his ears are freezing, and so on. Bob used a lot of the equipment from the defunct UHTREX, including a building adjacent to it that had been built for reactor experiments. But the real success was the fact that he recognized a very real need—accountability and safeguards for fissionable materials—and then did something about it.

SCIENCE: *What about the theoretical effort?*

THORN: Well, Harold, although he was an experimentalist, respected theoretical physics, and he wanted a first-class theoretical research effort in the Laboratory. Peter Carruthers was hired by Harold and given that charter, which Pete was largely able to fulfill. Also, Harold started the Laboratory Fellows program to help bring eminent external scientists to the Laboratory. Early Fellows were Herbert Anderson, Richard Garwin, Gian-Carlo Rota, Bernd Matthias, and Anthony Turkevich. This program has



been continued and expanded under Kerr, who has also instituted a Fellows program composed of outstanding scientists within the Laboratory. And there was a major expansion in computing under Harold, including purchase of the first Cray computers.

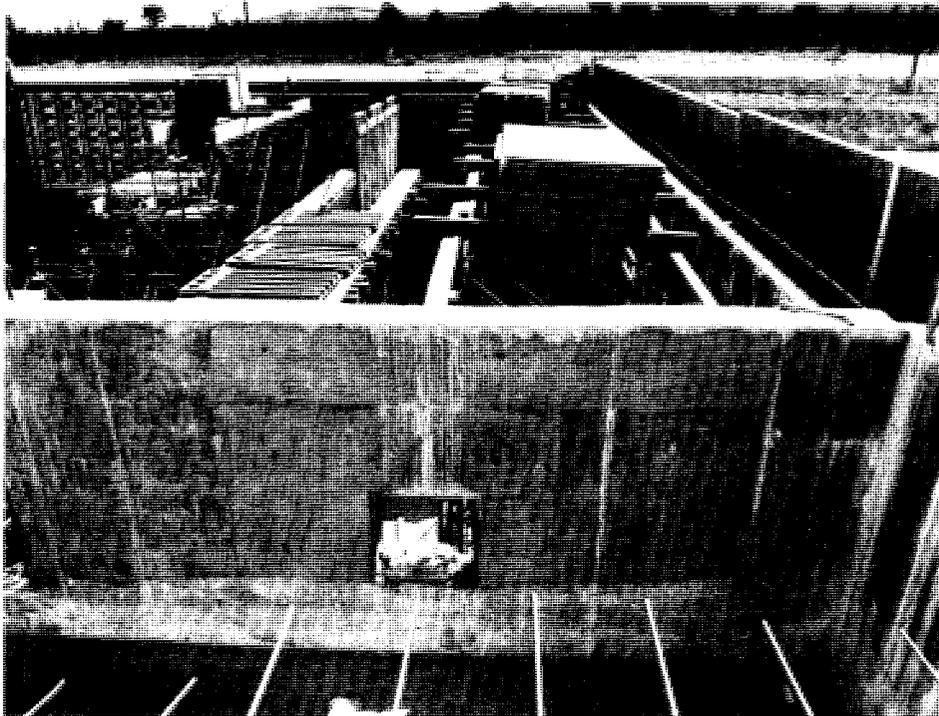
SCHREIBER: One of Harold's objectives was to find ways to finance the growth of basic research, including the theoretical efforts, up to a level of perhaps ten percent of the total Laboratory effort.

SCIENCE: *How did the funding sources and amounts change during this period?*

SCHREIBER: As we've already indicated, budgeting was not a major problem for most of Bradbury's tenure because the money came in a few large chunks accompanied

only by general directives. However, the AEC eventually began to exert its muscle in program direction, and then the Laboratory had its first budget crisis in the early '70s with the cancellation of the UHTREX, LAMPRE, and Rover programs.

THORN: Essentially the entire experimental reactor program was wiped out, then Rover, plus there were cuts in the weapons program. The first thing that Harold did was to say, "Let's do reimbursables. Besides the AEC we'll work for the Defense Department, we'll work for any other federal agency." Harold was never just negative about a situation; he always had a solution or two. The idea of reimbursables was an important solution that not only helped the Laboratory survive a crisis, but opened new doors such as



Harold helped convince the AEC of the absolute necessity for a new plutonium research and development facility. Construction started in 1974.



The Helios facility was constructed during the mid '70s to further explore the use of the CO₂ laser as a driver for inertial confinement fusion. Helios is an eight-beam system with an output of 10 kilojoules in 1 nanosecond.

developing productive ties with industry.

SCHREIBER: The Laboratory had already done a limited amount of reimbursable work, but mostly at the initiative of the sponsor of the work. With the AEC cutbacks, active solicitation of reimbursable work was started and a full-time employee was assigned to sell the ideas. In the early period, this was encouraged by the AEC. However, when reimbursable work grew above ten percent of the AEC budget to the Laboratory, worries were expressed about possible wholesale layoffs if, for any reason, reimbursable work stopped. Most of the contracts were for a period of one or two years, so the worry was real, both to the AEC and to Laboratory management. An informal compromise was reached with the agreement that reimbursables would be held approximately to the ten-percent level.

As matters turned out later in the '70s, the AEC budgets grew and the Laboratory continued to expand. However, it 'was not all that easy. Each year's budget was a cliff-hanger, but Harold was an excellent salesman and knew how to bargain successfully.

THORN: He was indefatigable. He understood that good public relations were becoming necessary. He was good at it, but he needed to be. He traveled extensively, addressed groups, served on committees, and maintained contacts with Congressional delegations.

SCHREIBER: Considering the wholesale cuts at the beginning of the '70s, the Laboratory definitely needed that kind of effort.

THORN: Harold never stopped believing in or selling the expertise and the potential that exists in this Laboratory and its people. ■

The Laser Programs

by Keith Boyer

The laser programs of Los Alamos had their inception in 1968 when I was directing the test activities of the Nuclear Rocket Propulsion program (Project Rover) in Nevada. At that point decisions were being made that would shift much of the program's test activities over to Aerojet and Westinghouse, and it was an appropriate time to explore new activities.

The main concept of the Rover nuclear rocket was to generate a high-temperature exhaust stream for propulsion by passing a gas, such as hydrogen, through the hot core of a nuclear reactor. However, I thought that a system based on fusion rather than fission might provide an extremely high-temperature exhaust stream for efficient propulsion. One possibility was the "Orion" concept in which a series of thermonuclear explosions "pushes" the spacecraft by ablating a replaceable layer of material, such as water, off a pusher plate. This process could produce high thrust and a very high efficiency system.

But what would ignite the thermonuclear explosions? Because of my interest in lasers, I was aware of the development of a high-energy carbon dioxide (CO_2) gas-dynamic laser system by the Air Force Weapons Laboratory. Our calculations indicated that if the energy then predicted for this laser could be released in a short enough time (about a nanosecond) and focused uniformly onto a small pellet of thermonuclear fuel, an efficient fusion process might be achieved.

Another feature that made the gas-dynamic laser attractive to our program was the manner in which the laser's population inversion was generated. The CO_2 gas was pumped to higher energy states by heating the gas, then the inversion was formed with rapid cooling through an expansion nozzle. Our early systems could use the Rover reactor as the heat source for driving this laser at high energies. Thus, the investigation of laser fusion seemed appropriate. The Space Nuclear Propulsion Office in Washington agreed, and a modest effort was

started that year at the Nevada Rover test site. Of course we recognized that fusion as a commercial energy source was the most important application and one that would surely precede any propulsion application, but we had found our first sponsor.

Design studies soon revealed difficulties in achieving the desired short, high-energy pulses at the low CO_2 pressures necessary in the gas-dynamic laser system, so other pumping mechanisms for the laser were considered, including optical, electrical, and chemical energy sources. Also, more information was needed about the effective absorption efficiency of the laser energy by appropriate targets, about the physics of the interaction process, and about energy transport and utilization in initiating fusion.

Raymond Pollock, a weapon designer, agreed to collaborate on this study and was able to derive the scaling laws and calculate the requirements to achieve thermonuclear burning of small pellets of fuel by assuming ideal interaction physics of the laser light with the target.

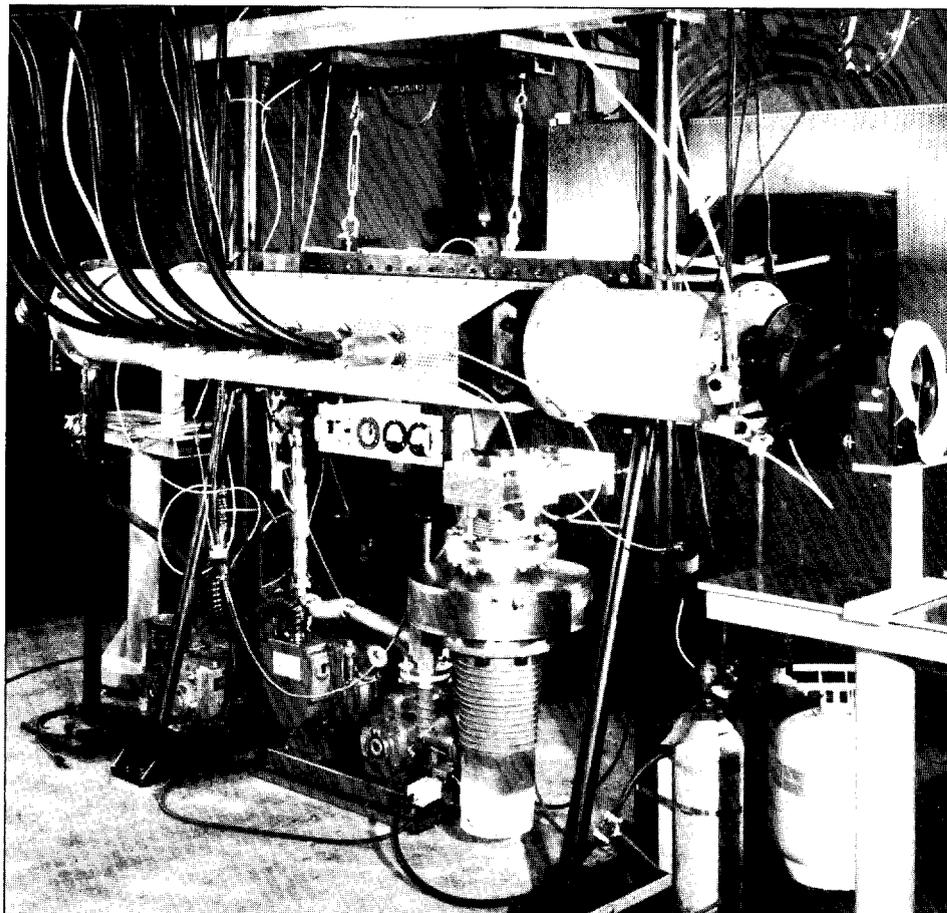
In early 1969 Bill Ogle, then the Weapons Testing Division Leader, agreed to authorize a small experimental exploratory effort. This activity included about ten staff members and initiated a three-pronged experimental effort: development of a one-joule, picosecond glass laser for the light-target interaction studies, investigation of electrical-discharge-pumped CO_2 lasers that could be scaled to high energy, and development of chemical lasers. Although chemical lasers would serve as backup for the undeveloped CO_2 laser, we intended to pursue both laser development and laser applications, and we recognized the potential of chemical lasers for studying photochemistry. For the CO_2 laser one of the early innovations, in which Charles Fenstermacher played a key role, was an electron-beam-controlled discharge capable of pumping large volumes of high-pressure CO_2 gas.

A year later we had established estimates of key parameters for laser fusion, such as

laser energy, pulse width, and preliminary pellet design. We were able to outline a program designed to determine the feasibility of laser fusion, including several different laser options. About this time we became aware of other programs in various parts of the world, including those at Livermore, Sandia, the University of Rochester, the Lebedev Institute in the Soviet Union, and the Osaka University in Japan, but all of these were based on glass lasers. Moreover, apparently only the Los Alamos and Livermore programs initially considered a target design that used laser energy to compress the fuel strongly as well as to heat it, a technique that reduced the laser energy required by many orders of magnitude. This situation changed soon as the various programs, including a new one at KMS Fusion (a commercial venture), discovered the necessity of compression.

Harold Agnew, recognizing the importance of developing new and promising activities at Los Alamos, asked me in January 1971 to set up an expanded laser program. This program was run out of the Director's Office in order to enlist Laboratory-wide support. Our effort soon had a wide base of activities, including a theoretical group organized by Richard Morse in the Theoretical Division; an interaction physics and target group under Gene McCall, who played a key role in the Laser Fusion program; a CO_2 laser development group under Fenstermacher; a glass laser group under Dennis Gill; and a chemical laser group directed by Reed Jensen. A series of seminars was established to review the existing state of laser technology and interaction physics and to explore new applications such as laser photochemistry.

By early 1972 the program had achieved sufficient size and complexity so that a new Laser Division was established. Two new groups were added, one on laser applications and one on target fabrication. At this time the first large CO_2 laser chain was being built and plans were in progress for a series of CO_2



One of the amplifiers used in the early '70s in a CO₂ laser chain that generated a 1-nanosecond, 0.5-kilojoule pulse.

laser systems of increasing size, including a two-beam, 2-kilojoule laser later called Gemini; a six-beam, 10-kilojoule laser now operating under the name of Helios; and a 100-kilojoule system whose configuration was being debated and which evolved into the present Antares system.

The early interaction data was obtained using a 50-joule, picosecond glass laser. Meanwhile, work proceeded on development of a larger 500-joule, glass laser system. Frequency-conversion crystals were also planned to be used with this laser to give green light and ultraviolet light, although at lower energies. These latter frequencies were needed to explore fully the question of the most efficient wavelength for the laser fusion process, a question that has not yet been resolved. The chemical laser work proceeded with the development of hydrogen fluoride lasers, which promised to provide the highest energy output of any laser system.

A coordinating committee was established in Washington to provide guidance for the laser fusion programs in the United States with representation from the Division of Military Applications of the AEC, the Mag-

netic Fusion program, and the heads of the various AEC Laboratory laser programs. The Los Alamos budget approximately doubled each year through the early '70s.

Our plan to pursue a broadbased laser technology program included a small project in the Chemical Laser Group to investigate the use of laser energy to separate uranium isotopes. This particular activity captured the interest of Paul Robinson, who had transferred from the Rover Reactor Division, together with a number of other staff, as the Rover program decreased in size. Paul had earlier been active in the gas-dynamic laser effort in Nevada and now, together with Reed Jensen, played a major role in the isotope separation project. The separation was based on the photolytic dissociation of uranium hexafluoride vapor cooled by a supersonic expansion to permit isotopic selectivity using a combination of infrared and ultraviolet laser photons. This activity continued to grow until it was split off from the Laser Division as the Applied Photochemistry Division with Paul as Division Leader. This division also became involved in both high-repetition-rate, high-

power laser development and in broad aspects of laser photochemistry. Projects included high-resolution laser spectroscopy, photochemical processing, laser sound generators for potential military uses, and chemical and biological warfare agent detectors. Although a recent Washington decision terminated the Los Alamos molecular uranium isotope separation process in favor of the Livermore atomic vapor process, the molecular process was close to engineering demonstration and was judged by many of us to be the superior process. In spite of the uranium decision a growing Los Alamos program on the separation of plutonium isotopes is doing well.

The laser fusion programs are still vigorous, but many problems have developed, and the final utility of laser fusion for energy production remains uncertain. Inflation and budget stretchouts reduced the design energy of the Antares CO₂ laser, which has just begun its checkout phase, from 100 to 40 kilojoules. The estimates of laser energy needed for a useful thermonuclear yield have risen from a few hundred kilojoules to a few megajoules. The longer wavelengths of both CO₂ and glass lasers produced undesirably large hot-electron components in the absorption process. The resulting self-generated magnetic fields are believed to reduce the lateral heat conduction that was originally counted on to symmetrize the implosion of the fuel pellet. Shorter wavelengths appear to be more satisfactory, and work is proceeding on ultraviolet excimer lasers, such as krypton fluoride, but the optics problems for these wavelengths are severe. Glass lasers can be frequency shifted to the third harmonic with good efficiencies, although the basic efficiency of the glass laser itself is too low to provide the driver for a laser fusion reactor. However, this technique is being pursued at other laboratories.

The Los Alamos program is now emphasizing investigation of physics problems of interest to the weapons programs. Because this effort appears to be increasingly productive, program funding and support is expected to continue. In spite of the apparent difficulties associated with the long wavelength of the CO₂ laser, it may be possible to find clever target designs that permit the many advantages of this laser to be used for successful initiation of the fusion process. Other laser activities, such as the Free Electron Laser program, are now expanding both the Laboratory's interest in and its commitment to laser technology. ■

The Reactor Safety Program

by Kaye D. Lathrop

Although Los Alamos has had a long history of individual contributors to the safety of reactors, including Hans Bethe, George Bell, and William Stratton, the reactor safety research program now conducted by the Energy Division began in 1972 in the Theoretical Division. At that time, in reactor physics and safety circles, there was a slowly increasing realization that our ability to predict the consequences of possible reactor accidents was woefully inadequate. The safety review process for the Fast Flux Test Facility at Richland, Washington had resulted in a heated and prolonged debate between the safety analysts at Argonne National Laboratory and the construction project managers at Hanford because the results of the safety analysis implied greatly increased design and construction expense. Somewhat earlier, the first major performance tests of a simulated light-water reactor emergency core-cooling system at the Semiscale Facility at Idaho Falls gave an unforeseen result. The emergency cooling water, instead of penetrating the core and cooling the system, simply flowed around the upper annulus of the apparatus and exited through the simulated pipe break. Although the Semiscale apparatus was about one-thousandth as large as an actual reactor, these disturbing results precipitated a lengthy set of hearings that culminated in a Code of Federal Regulations that limited the operating temperatures of existing and future reactors. Because of a lack of understanding of what would happen in a full-size reactor, these regulations embodied many "conservatisms" and in this sense were arbitrary.

So there existed a desperate need for an analytic predictive capability, especially because expense had prohibited and always would prohibit complete full-scale testing of safety systems. Jay Boudreau, William Reed, and I, members of the Transport Theory Group of the Theoretical Division, saw this need as an opportunity, each in a different way. Boudreau, who had written his doctoral thesis on possible supercritical configura-

tions that might emerge from core rearrangements during fast reactor accidents, wanted to turn from his transport theory assignments to solve what he believed were truly important problems. Bill Reed, who had already demonstrated a brilliant mastery of computational transport theory, was anxious to extend his talents to hydrodynamics. And I had an implicit faith in the ability of a properly designed computer code to make correct predictions and was anxious for a new challenge. Further, in the reduction-in-force days of the early seventies, I needed new financial support for my group.

In my first 1972 foray to Washington, I was greeted by a skeptical branch chief with the sally, "Who are you, and what are your credentials?" However, in a widely attended Washington meeting on October 31, 1973, we presented a detailed proposal, authored by Jay Boudreau, Frank Harlow, Bill Reed, and Jack Barnes, for the development of the SIMMER (an acronym for S_n , implicit, multifield, multicomponent, Eulerian, recriticality) code to analyze fast reactor core-meltdown accidents. Although Los Alamos was outside the reactor safety community, the Laboratory's acknowledged leadership in computational methods and the existence of three groups in the Theoretical Division devoted to transport theory, hydrodynamics, and equation-of-state research convinced the AEC of our competence,

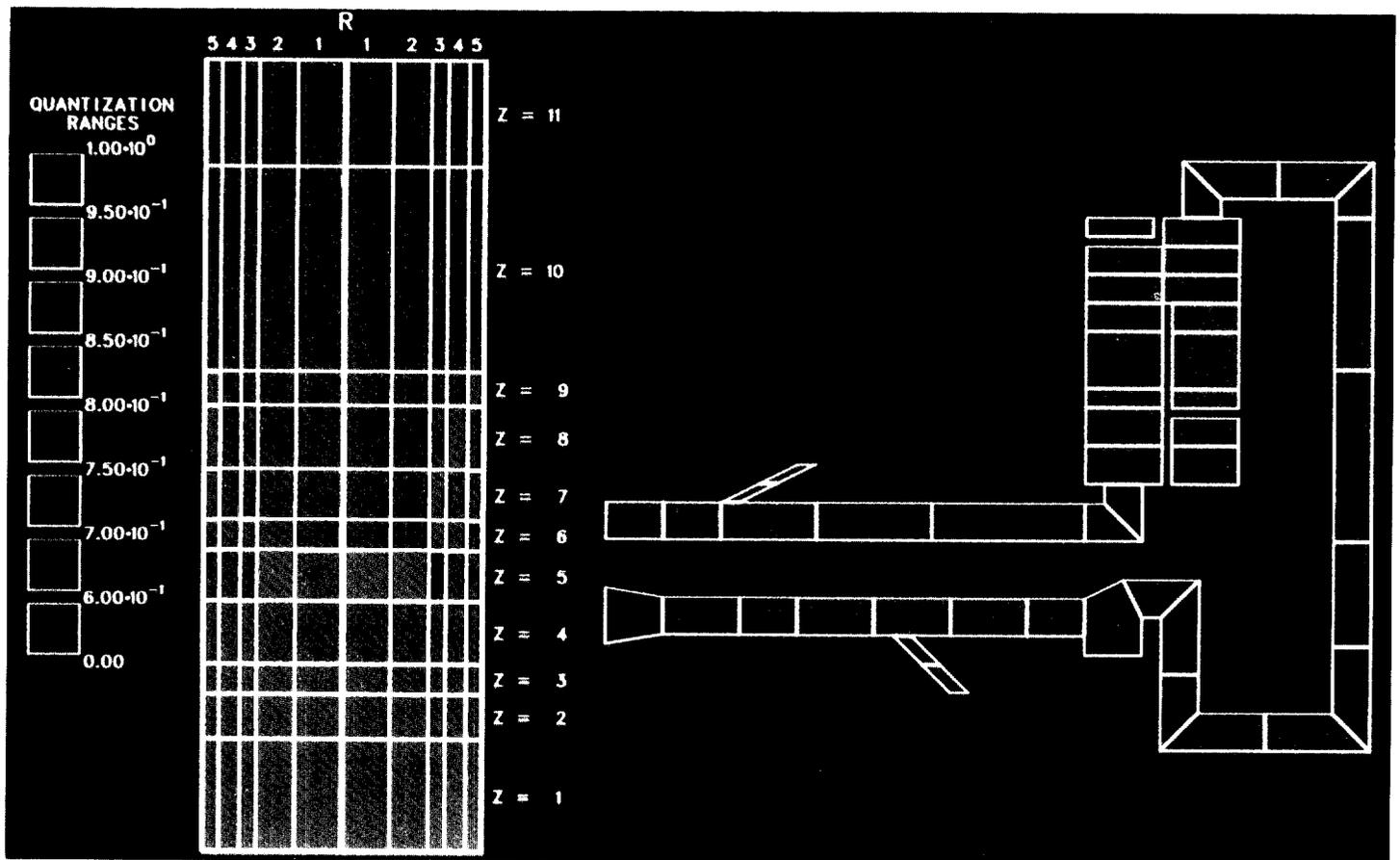
The proposal was funded, and work on SIMMER began in earnest in 1974. That same year, William Kirk and I began a more broadly based reactor safety research program on high-temperature gas-cooled reactors. Simultaneously, and almost as an afterthought, Reed and I agreed to develop a best-estimate computer code (subsequently named TRAC for transient reactor analysis code) to predict the effects of emergency core-cooling systems in light-water reactors. In retrospect, our self-confidence was astounding. We were blissfully ignorant of the difficulty of the task, and Los Alamos,

despite long experience with high-temperature gas-cooled reactors and fast reactors, had no expertise with light-water reactors.

The Transport Theory Group grew rapidly in 1974 and 1975, becoming three groups in December of the latter year. Two of these groups formed the nucleus of the present 125-man reactor safety program in the Energy Division. The research of this program is the theme of the Summer/Fall 1981 issue of *Los Alamos Science*. The third group, headed by Warren Miller, remained as the Transport Theory Group of the Theoretical Division.

The success of the SIMMER and TRAC computer codes has been especially noteworthy because they must extrapolate. That is, they must make believable predictions outside the domain of experimental results. Versions of TRAC, in particular, have been used to predict results for dozens of experiments on many reactor components of scales up to full size and on integrated systems of various miniature scales. (The only full-scale, full-system data point for a light-water reactor emergency cooling system is Three Mile Island.) TRAC has a convincing predictive record. No other computer model of similar complexity, certainly not those of weapons design codes, can extrapolate with such confidence. SIMMER, while not yet as exhaustively compared with experiment as TRAC, has made two valuable predictions. First, contrary to previously accepted dogma, secondary and subsequent critical configurations can occur because of a core rearrangement during the course of a fast reactor accident. Second, and notwithstanding this first prediction, the energy released (and hence the containment expense) in fast reactor core-melt accidents is computed to be much less than previously predicted.

In addition to these technical achievements and of equal importance, the growth of the reactor safety program brought to Los Alamos many extremely capable people. These include Jim Jackson, who came from



Two examples of TRAC results. The graphic output shown here is color coded (left) according to the fraction of vapor or steam in each computational cell. One example (middle) shows liquid water (blue) in the bottom of a pressurized-water reactor vessel filled with steam (red) following a postulated complete break in the largest coolant pipe leading into the vessel. The unique ability of TRAC to analyze 3-dimensional fluid motions in a vessel coupled to a full reactor system is proving valuable in addressing a wide variety of possible accidents in

pressurized-water reactors. The output on the right shows steam-water flows in a loop of the Upper Plenum Test Facility (UPTF). Now in the design stage, this West German facility will include a full-sized vessel and several coolant loops to allow accurate simulations of fluid behavior during the core-reflooding stage of a large-break loss-of-coolant accident in a pressurized-water reactor. TRAC is being used extensively in the design of UPTF as part of a \$300-million cooperative program among the United States, Japan, and West Germany.

Brigham Young University to take charge of TRAC development during a crucial phase and is now head of the Energy Division; his deputy, Mike Stevenson, who came from Babcock & Wilcox via Argonne to head the high-temperature gas-cooled reactor analysis effort; Charlie Bell, who came from Atomic International to solve SIMMER heat-transfer and hydrodynamics problems; Walt

Kirchner, who finished his doctorate at MIT in time to write TRAC heat-transfer routines; Dennis Liles, an expert in two-phase flow hydrodynamics from Georgia Tech who has been invaluable to TRAC development; John Mahaffy, a postdoctoral astrophysicist from the University of Illinois whose numerical hydrodynamics expertise has made TRAC faster; Rich Pryor, a

Savannah River reactor physicist whose experience with methods and large codes was very valuable; Jim Scott, a Hanford fuel-behavior specialist; Ron Smith, from Argonne; Ken Williams, from Georgia Tech; Dominic Cagliostro, from SRI; John Ireland, from General Electric; Thad Knight, from EG&G; and many more. ■

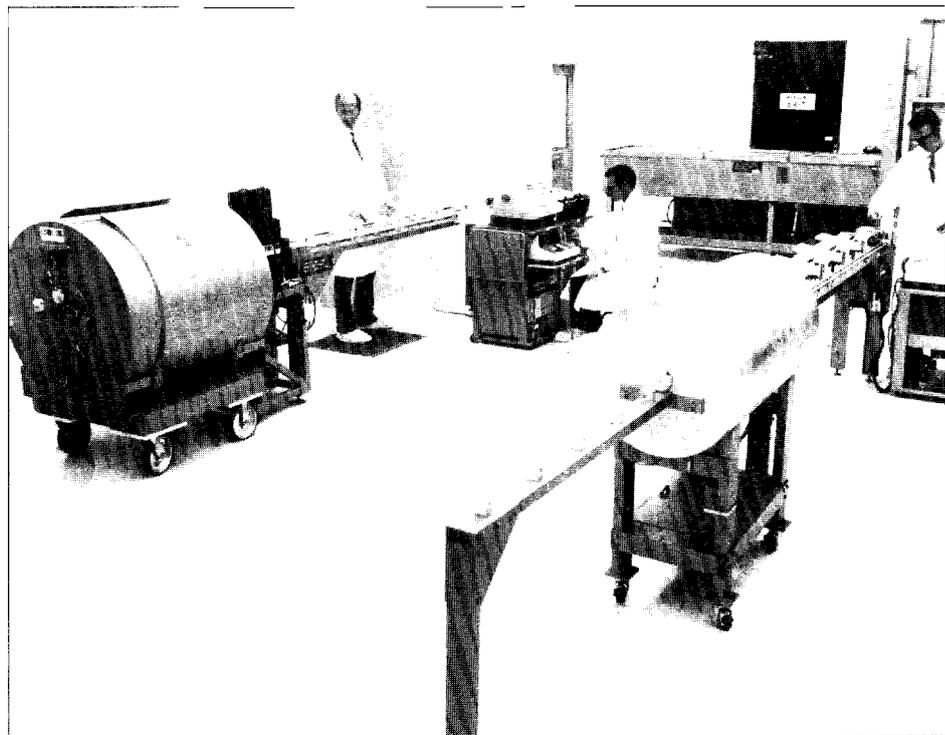
The Nuclear Safeguards Program

compiled by Darryl B. Smith

“Los Alamos’s interest in safeguards . . . should not really surprise you. Our pioneering work in nuclear weapons has left us . . . with the profound concern that these devices never get used in anger, never get used surreptitiously, never get made by surprise, by theft, or by diversion,” Dr. Norris E. Bradbury used these words in his welcoming remarks to the more than three hundred and fifty participants in the Second AEC Symposium on Safeguards Research and Development held in Los Alamos in October 1969.

Immediately following the end of World War II there was a hope that the proliferation of nuclear weapons could at least be delayed by means of rigid controls over all nuclear activities (the Baruch Plan, 1946). Despite efforts by the United States to maintain strict secrecy, by 1952 three additional nuclear weapons states had emerged, and several nations were seeking the benefits of nuclear electric power. In 1953, President Eisenhower announced the “Atoms for Peace” program to promote vigorously the peaceful use of nuclear energy while discouraging or preventing any military use. In the course of implementing this policy, the International Atomic Energy Agency (IAEA) was created in 1957 and entrusted with the international promotion and control of peaceful uses of nuclear energy.

The Los Alamos Nuclear Safeguards program began in 1966 when worldwide interest in nuclear energy for the production of electrical power was rapidly expanding. Bob Keepin, a nuclear physicist in the Nuclear Propulsion Division, had just returned to Los Alamos after two years as head of the Physics Section, Division of Research and Laboratories of the IAEA in Vienna, Austria, and was firmly convinced of the coming importance—both political and technical—of the worldwide nuclear safeguards problem. He was equally convinced that Los Alamos should launch a vigorous program to develop new nondestructive assay techniques and instruments that would in time



Nondestructive assay of fast breeder reactor fuel. Two fuel-rod scanners developed by Los Alamos are being used here in 1974 at the Hanford Engineering Development Laboratory as part of their safeguards and quality control. The device on the left uses a computerized californium-252 system to measure both plutonium content with an accuracy of better than 0.5 percent and pellet-to-pellet uniformity of fissile material loaded into the rod. The system on the right uses a passive neutron-coincidence technique to measure plutonium-240 content, thus providing a cross-check with the first instrument.

provide the technical basis for meeting the increasingly stringent safeguards requirements that were inevitable. Following a lengthy series of briefings, hearings, button-holing, and budget reviews with the AEC and the Congressional Joint Committee on Atomic Energy, the nation’s first research and development program in safeguards was funded and launched at Los Alamos in December of 1966. Six months later, the AEC established a new Office of Safeguards and Materials Management (OSMM) as well as a Division of Safeguards in its Regulatory Branch. The Regulatory Branch is now the

Nuclear Regulatory Commission (NRC). The OSMM is now the Department of Energy’s Office of Safeguards and Security and still provides the lion’s share of the \$12 million Safeguards research and development program at Los Alamos.

Bob was named to head the new program, which began in a small laboratory at Pajarito Site replete with chipmunks in the offices and a rattlesnake on the doorstep. As the program grew, this space was augmented a year later by the addition of a second, larger laboratory at another site. With the encouragement and cooperation of Dick



In-line monitoring of uranium hexafluoride (UF₆) enrichment. This system, shown installed in 1975 at Goodyear's Atomic Gaseous Diffusion Plant in Piketon, Ohio, also uses two independent sensors developed at Los Alamos. The gamma enrichment meter measures the percentage of uranium-235; the neutron detector measures the percentage of uranium-234. This in-line instrument allows instantaneous isotopic analysis (to better than 0.5% accuracy), providing assurance of criticality safety during withdrawal into large cylinders as well as verification that the product selection meets the enrichment specifications. Because uranium-234 is also enriched in the diffusion process, its isotopic abundance in the product UF₆ provides useful diagnostic information for plant operation. The alpha-particle activity of uranium-234 is the principal source of neutrons emitted by enriched UF₆, and this neutron yield is an important signature for safeguards verification.

Baker, Chemistry-Materials Science Division Leader at the time, and the tolerance of Bill Maraman and his Plutonium Chemistry and Metallurgy Group, a special technical liaison committee was set up in 1967 to encourage cooperation among safeguards researchers and those staff whose group or division responsibilities were directly concerned with nuclear materials and equipment. This committee helped to identify needed, practical applications for testing and applying newly

developed safeguards techniques to materials measurement, accountability, and safeguards problems. Such problems were not uncommon in the materials processing, fabrication, and recovery operations carried out routinely at the Laboratory's plutonium facility. The close liaison between safeguards researchers and the Laboratory's plutonium chemists and metallurgists significantly helped the Los Alamos Safeguards program get off to a head start in the safeguards field

with a commanding lead that has been retained ever since.

The Agnew years saw the Los Alamos Safeguards research and development program grow by more than an order of magnitude. At the beginning of the '70s, most of the nuclear industry was unaware of the importance and economic impact the nondestructive assay techniques could have on their operations, so in the spring of 1970 Los Alamos fielded the Mobile Nondestructive Assay Laboratory (MONAL) to serve as a demonstration unit and assay laboratory and as a staging area for conducting in-plant assay using portable instrumentation. During the next few years, MONAL traveled to nuclear facilities nationwide, addressing special measurement problems. The first Los Alamos instrument installed in a nuclear facility for routine production use went to the General Electric fuel fabrication plant in Wilmington, North Carolina, in the spring of 1971 to assay reactor fuel rods. By the end of the decade, instruments and techniques developed by Los Alamos were in use throughout the world. In November 1973, the Safeguards staff conducted its first formal course in nondestructive assay techniques. By 1980 nearly seven hundred people had received training in safeguards techniques at Los Alamos, and currently about two hundred students participate each year in the eight to ten courses offered, including all new IAEA inspectors, who come to Los Alamos for their initial training.

Today, the Los Alamos Safeguards program is recognized worldwide as the fundamental source for state-of-the-art safeguards technology and has been designated as the DOE's lead laboratory in nuclear materials control and accountability research and development. It encompasses all aspects of the design, development, testing, and in-plant evaluation of new techniques, instruments, and integrated systems for safeguarding fissionable materials in all types of civilian and national defense nuclear facilities. ■

The Hot Dry Rock Program

by Morton C. Smith

It is not often possible to trace the ancestry and list the immediate family of a new idea, but in this regard—and some others—the Hot Dry Rock Geothermal Energy program is exceptional.

Since its establishment as Site Y of the Manhattan Project, the primary mission of Los Alamos National Laboratory has required information that could be acquired only from experiments done in nuclear reactors, and reactor expertise has always been one of its greatest strengths. It was therefore quite natural, when a national need appeared for higher-performance rocket-propulsion systems, that the Laboratory should propose the use of compact, gas-cooled nuclear reactors. The result was the Rover program.

One of the reactor concepts considered in the early days of the Rover program was Dumbo, a fast reactor with a refractory-metal-composite core built as a honeycomb structure. To demonstrate the heat-transfer characteristics of such a structure, a resistively heated laboratory-scale model of a core section was built and used to heat a hydrogen jet to above 3000 degrees Celsius. The demonstration was impressive, and when Dumbo was abandoned in favor of a graphite-core reactor, some of the Dumbo advocates felt that a gadget that good must have other uses. In particular, Robert M. Potter (now a Laboratory Fellow), after rereading the Edgar Rice Burroughs novel, *At the Earth's Core*, concluded that something like it could as well be pointed down as up and used to melt holes in rock more rapidly and efficiently than they could be produced by drilling or tunneling. The result, some years later, was the Subterrene program—development of a rock-melting earth penetrator.

In 1970 the late Eugene S. Robinson assembled an *ad hoc* committee from several Laboratory divisions and disciplines to examine the possibilities and problems of the Subterrene. One of the obvious problems was disposal of the molten glass produced when a rock is melted. Again Potter had a

suggestion. He had been reading about drilling in oil and gas fields and had learned about hydraulic fracturing—the use of fluid pressure to produce large cracks extending outward from the well to facilitate drainage of fluids into it. He proposed that sufficient pressure could be developed in the melt ahead of a penetrator to produce such cracks and force the glass into them, where it would freeze and remain. This idea was never actively pursued in the Subterrene program, but it appeared to the committee that hydraulic fracturing had many other possibilities. One of the most important of these, they concluded, was its use to create flow passages and heat-transfer surface in naturally heated crustal rock whose initial permeability was too low to be usefully productive of natural steam or hot water—"dry hot rock."

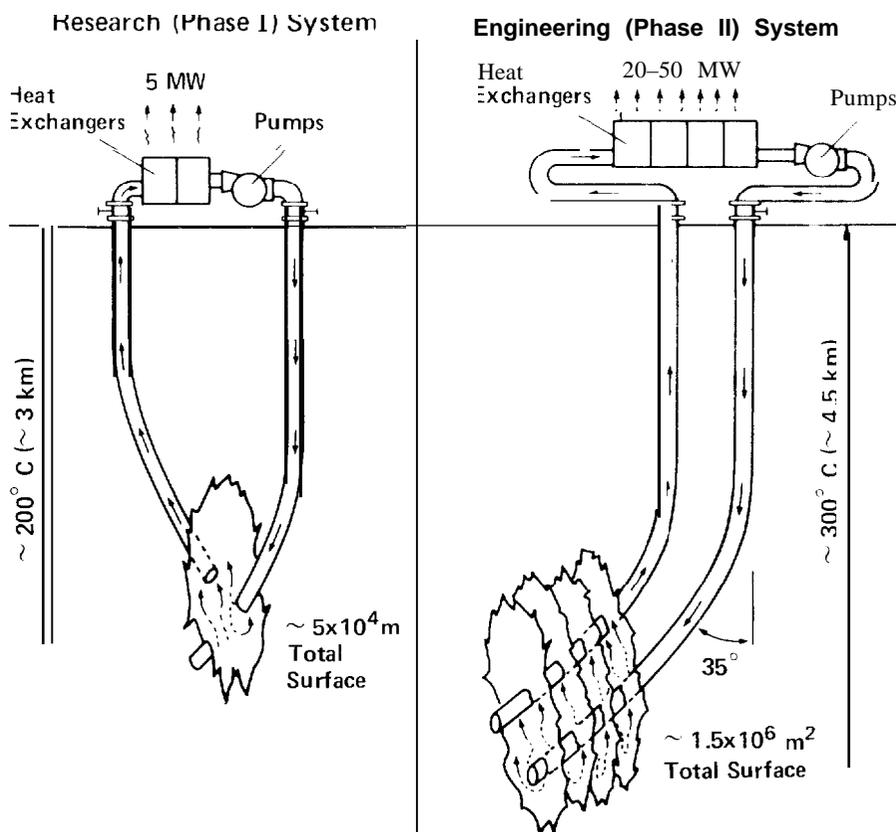
The method proposed by the committee was to drill a hole from the earth's surface to a sufficient depth to reach essentially impermeable rock at a usefully high temperature; to produce a large hydraulic fracture near the bottom of the hole; to drill a second hole from the surface to intersect that fracture; to pump water down the first hole to circulate through the fracture and extract heat from the rock around it; to recover the hot water through the second hole under sufficient pressure to prevent boiling; to extract its useful heat; to then return the water to the first hole to recirculate and extract more heat.

When the Subterrene program had been launched, Bob Potter and I assembled a group of volunteers and initiated a "Dry Hot Rock Geothermal Energy program" to investigate this concept. (The name was subsequently changed by someone in Washington who thought that "Hot Dry Rock" was more euphonious.) Initially the program was unofficial, unfunded, and supported largely by faith and the tolerance of Laboratory management. Most of the first year's work was done on weekends and holidays, and much of it in snow up to there. However, in 1971 the group managed to digest much of the

existing information on geothermal areas and the equipment and techniques needed to create a dry hot rock energy system, and to begin a terrestrial heat flow study in the Jemez Mountains west of Los Alamos. In 1972 that study was concluded and, with discretionary research and development funds provided to the Laboratory by the Division of Military Application of the AEC, an exploratory hole was drilled in Barley Canyon—about 30 kilometers west of the Laboratory. The hole reached a final depth of 785 meters, penetrated about 143 meters of granitic basement rock, and had a bottomhole temperature of 100.4 degrees Celsius. With additional funding from the Division of Physical Research of the AEC, hydraulic-fracturing and pressurization tests were run in the lower part of the hole, and it was concluded that the basement rock was well suited to creation and containment of a pressurized-water heat-extraction loop.

With this encouragement and the prospect of substantial funding from the newly formed Division of Applied Technology of the AEC, an official 'Los Alamos Geothermal Energy Group' was formed early in 1973, with myself as Group Leader. The anticipated funds materialized, and in 1974 a deeper exploratory hole was drilled at a more accessible and convenient location—on Fenton Hill, about 2.5 kilometers south of Barley Canyon. This hole reached a depth of 2930 meters and a rock temperature of 197 degrees Celsius. Experiments in it confirmed the observations previously made in Barley Canyon, but at greater depth and higher temperature.

In 1975 a second hole was drilled at Fenton Hill (photograph and figure) to a final depth of 3064 meters and a rock temperature of 205 degrees Celsius. A poor connection was made between hydraulic fractures produced from the two holes. After considerable experimentation and much development of new equipment and instruments, the connection was improved in 1977 by redrilling one of the holes, and in 1979 the



The photograph shows the hot dry rock geothermal site at Fenton Hill, looking southwest. Phase I of the project, shown schematically on the left side of the figure and with its two wells and heat exchangers labeled in the photograph, was completed in 1979 and has been producing heat at rates high enough that several hundred homes could be heated. It is hoped that when Phase II (labeled in photograph and right side of figure) is completed, heat production will be sufficient to demonstrate that a commercial electric power plant could be supported.

underground loop was enlarged by additional hydraulic fracturing (Phase I). With an air-cooled heat exchanger at the surface to dissipate the heat, this pioneering hot dry rock energy system has been operated intermittently since 1978 as a closed, recirculating pressurized-water loop. Heat has been produced at rates up to 5 megawatts (thermal), which would heat several hundred homes if there were that many nearby. The longest continuous run lasted nine months and had no detectable environmental effect. Some of the heat has been used to generate electricity in a 60-kilowatt binary cycle plant, but neither the temperature nor the rate of heat production was sufficient to support a commercial power plant. Therefore, a larger, deeper, hotter system (Phase II) designed to demonstrate that capability is now being constructed at Fenton Hill.

While the objective of the Hot Dry Rock program has always been the very practical one of making a vast, indigenous energy supply useful to man, the effort to do so has necessarily included a wide variety of supporting research and development activities—many of them done cooperatively with industrial organizations, university groups, and complementary programs at other laboratories and in other countries. To justify existence of the program, the very large resource base of thermal energy at accessible depths across the entire United States had to be evaluated. To implement the field program, it was necessary to develop drilling, well-completion, and hydraulic-fracturing equipment and techniques usable in very hot, inclined geothermal wells and also downhole instruments to log such wells and collect data in them. And to analyze and understand the information collected in the field has required both theoretical and laboratory studies of rock-water interactions, fluid and rock mechanics, heat transfer and transport, acoustic emissions, and other subjects. The program is broadly interdisciplinary and covers the entire spectrum from basic research to engineering application.

Since its inception, the Hot Dry Rock program has been supported primarily by the AEC and its successor agencies, ERDA and DOE, with supplementary support since 1980 by agencies of the governments of West Germany and Japan. However, the most important support has come from people like Harold Agnew, Director of the Laboratory during most of the history of the Hot Dry Rock program and always its most personable, articulate, and effective advocate. ■