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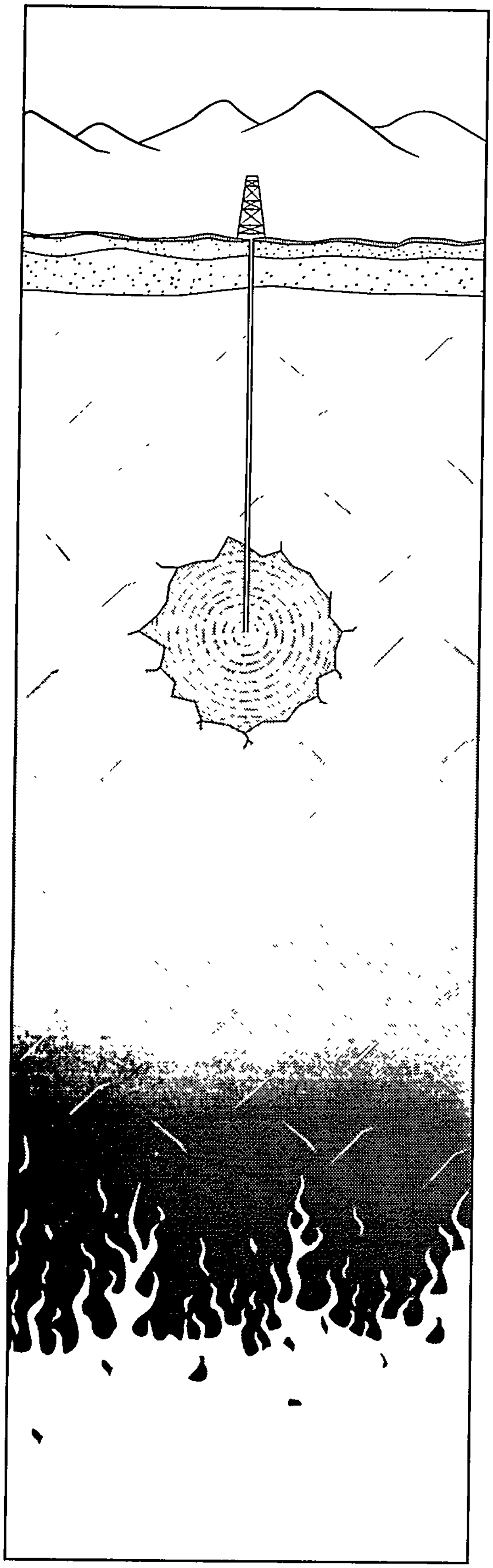

The Furnace in the Basement

Part I
The Early Days of the Hot Dry Rock Geothermal Energy Program, 1970–1973

Los Alamos
NATIONAL LABORATORY

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Cover illustration by Ruth Bigio, EES-4: An engineered geothermal reservoir is created by hydraulic fracturing through a wellbore drilled into hot dry rock (HDR). Magma (molten rock), which forms the core of the earth, is the ultimate source of the heat energy that is extracted from an HDR geothermal reservoir.

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The Furnace in the Basement

Part I
The Early Days of the Hot Dry Rock Geothermal Energy Program, 1970–1973

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PREFACE

In geology, the term "basement" generally refers to the crystalline rock that underlies the soils, gravels, and sedimentary formations that in most places constitute the uppermost layers of the earth's crust. Actually, however, the word is poorly defined since, with the exception of a few glassy materials such as obsidian, all rocks in fact are crystalline. Also, it covers a wide variety of rock types, including the metamorphic formations produced by the action of heat and pressure on such sedimentary rocks as sandstones and shales.

In the present context, the meaning of the word is specialized further. Here it is taken to mean the igneous, plutonic, and metamorphic formations that—unless they have been fractured by earth movements or thermal effects—in general have very low permeability and free-water content; are likely to contain significant concentrations of naturally occurring unstable isotopes whose spontaneous decay produces heat; and through which heat is also conducted upward from the lower crust and mantle toward the earth's relatively cool outer surface. It is this general kind of rock that is most likely to exist in nature at usefully high temperatures and accessible depths in the earth's upper crust and to have permeability low enough to contain a pressurized-water circulation loop that can extract heat from the rock and transport it to the surface. Accordingly, basement rock so described has received primary attention in the Hot Dry Rock Geothermal Energy Program that is the subject of this report. There are, of course, other rock types that qualify as natural sources of thermal energy, but these are the subjects of other reports.

This report describes the early history of the pioneer effort to develop a practical method of recovering useful energy from naturally heated rock in the earth's upper crust. It describes minor triumphs and disappointments and presents new evidence of the inherent perversity of nature, inanimate objects in general, much of mankind, and most bureaucracies. And it is undoubtedly biased by the fact that its author is one of the inventors of the hot dry rock energy system discussed here and has always been one of its most optimistic and vociferous advocates. During its turbulent early days he developed sensitivities and prejudices that will undoubtedly become apparent to the reader. Also, to a considerable degree, this is a personal as well as a technical narrative, and—for lack of perspective and objectivity—it therefore may not be very good history. It will, however, at least provide for better-qualified future historians a record of events during the initial development of an energy supply that is certain to be important in the world's energy future.

As far as possible, I have tried to present this record in chronological order. There are, however, so many subplots and relevant side issues that a simple diary of events would soon become as dull and confusing as the office records in which they were originally reported. I have, therefore, frequently resorted to presenting reasonably self-contained, stand-alone sections that describe a related series of events from beginning to end. One unfortunate result is that at the end of one section we might, for example, have completed drilling well GT-1, while at the beginning of the next section we might not have started drilling it yet. I hope, however, that this will be less distracting than interrupting the drilling record with paragraphs describing the many other things that were going on at the same time. One major anachronism is that I have, from the beginning of this report, identified this as the "Hot Dry Rock" (or HDR) Program. As will be explained, we did not call it that at first. There is also a major (and intentional) inconsistency in the units of measurement used throughout much of this history, to which all purists and most editors will take strong exception. It is written primarily for Americans interested in geothermal energy, to most of whom the units used will be familiar. Others, if so inclined, can make the conversions to a consistent set of units as easily (and tediously) as I could.

The reason for the inconsistency is that in the U.S. geothermal industry such things as temperature gradients and rates of heat flow are normally measured and reported by scientists who usually use metric units such as meters, calories, and degrees centigrade. However, field operations such as drilling and hydraulic fracturing are normally done by engineers who use English (or American) units such as feet and gallons and dollars and pounds per square inch. In our HDR Program, which includes both science and engineering, we have observed this mixture of traditions, and I have generally maintained it in what follows—as we normally did in collecting and reporting the information that is the real reason for this history.

This is just Part I of the history of a long-term program at Los Alamos to develop and demonstrate a practical and economical method of recovering useful thermal energy from the essentially inexhaustible energy resource represented by hot dry rock in the earth's upper crust. In Part II, that history will be extended through the construction and operation of the world's first hot dry rock energy production system, the very successful Phase I system at Fenton Hill, New Mexico.

THE FURNACE IN THE BASEMENT
PART I
THE EARLY DAYS OF THE HOT DRY ROCK GEOTHERMAL ENERGY PROGRAM, 1970–1973

by

Morton C. Smith

ABSTRACT

The earth's interior is very hot and, at depths that vary with the local geology and hydrology, crustal rock at temperatures high enough to be commercially useful exists everywhere beneath the earth's surface. The heat in that rock represents the largest and most broadly distributed supply of directly usable thermal energy that is accessible to man. Further, deep mines and drilled holes have shown that in many places usefully high temperatures are encountered at depths that are now routinely reached with modern drilling equipment. That is particularly common when a hole extends into the crystalline basement—the igneous, plutonic, and metamorphic rocks that underlie the surface sediments and typically have permeabilities so low that they have not been cooled significantly by groundwater circulation through them.

Considering all this, in 1970, a group at Los Alamos Scientific Laboratory (LASL) concluded that it should be possible to recover useful heat from that "hot dry rock" by drilling two holes into it, connecting them at depth by hydraulic fracturing, and circulating pressurized water through the connected underground heat-extraction loop so constructed. Background studies indicated that the equipment and techniques needed to construct and operate an energy system of this type were already available commercially. Accordingly, with financial support from the U.S. Atomic Energy Commission, a LASL Geothermal Energy Group was formed to investigate its possibilities and problems.

The first major steps in doing so were (1) to identify an area in which usefully hot, low-permeability rock could be expected to exist at a reasonable drilling depth and (2) to determine whether or not the properties and behavior of the hot rock were such that a contained heat-extraction loop could be constructed and operated successfully in it.

This report presents the history of the pioneering and generally successful attempt to accomplish those objectives. It includes descriptions of the background information collected initially and of the formation and development of the LASL Geothermal Energy Group. It discusses the organizational, financial, political, public-relations, geologic, hydrologic, physical, and mechanical problems encountered by the group during the period 1970 to 1973. And it reports the failures as well as the successes of this essential first stage in the development of hot dry rock geothermal energy systems.

1. INTRODUCTION

1.1. The discovery of hot rock in the earth's crust

Geothermal energy is natural heat in the earth's interior. It has existed as long as the earth itself, and its natural manifestations at the earth's surface have been observed, and in some cases used to advantage by mankind, for as long as intelligent humans have also existed.

The obvious evidence for the existence of geothermal energy includes hot springs, boiling mud pots, geysers, steam vents, and volcanic eruptions. Natural hot water was immediately useful for such things as bathing, washing, and boiling fish. The more spectacular high-temperature displays have always been major tourist attractions, and in some cultures came to have important religious significance.

It is not known when these geothermal phenomena were first attributed to heat in the earth's interior, but that may have been a very long time ago. The concept of a subterranean Hades of fire and brimstone is an ancient one, perhaps derived from the fiery plumes and sulfurous gases ejected by some types of active volcanos. However, the first direct observations of the existence of elevated temperature beneath the earth's surface apparently were made during the early sixteenth century when, with the development of pumps capable of dewatering them, the first deep metal mines were opened in eastern Europe. Georg Bauer, a remarkable Saxon physician who was also one of the world's first earth scientists, wrote about them in his book *De Ortu et Causis Subterraneorum;* published in 1546 under his latinized pen name, Georgius Agricola (Hoover and Hoover, 1950). He attributed the subterranean heat principally to the burning of "bitumen" with minor contributions from the combustion of sulfur and "the friction of internal winds." While this explanation leaves much to be further explained, it actually was not much improved upon for almost 400 years.

In a short history of the geothermal industry, Peter Smith cites what is probably the first published reference to a steady increase in temperature with increasing depth below the earth's surface (a "geothermal gradient")—published in Paris in 1619 by a French scientist named Morin (Smith, 1975). Morin reported that temperature increased continuously as he penetrated more and more deeply into a group of mines in Hungary. About 50 years later Robert Boyle, the distinguished English natural philosopher, reviewed the eyewitness accounts of others concerning this phenomenon. He concluded that the heat originated in chemical reactions deep within the earth and explained the temperature gradient observed nearer the surface in terms of the upward conduction of that heat—a subject on which he was one of the early experts.

By the nineteenth century, the existence of both a broadly distributed subterranean heat source and the resulting conductive geothermal gradient was well known in the scientific community. Between 1868 and 1883, a committee of the British Association for the Advancement of Science conducted an investigation of the rate at which thermal conduction delivered this heat to the earth's surface (Smith, 1975). The committee concluded that the average rate of terrestrial heat flow was 1.3×10^{-6} cal/cm^2 $\times$ sec (55 mW/m^2). This is somewhat less than a much more recent estimate of 67 mW/m^2 for the worldwide average (Luttig, 1985), but is probably representative of the area actually sampled.

Since then, a great deal of information has been collected on geothermal gradients, rates of heat flow, their worldwide distribution, and the heat sources responsible for them. However, no serious proposals for recovering useful heat from hot crustal rock appeared until deep-drilling technology was developed for the production of petroleum.

1.2. Nature and distribution of the hot dry rock thermal energy resource

When a deep hole is drilled downward from the earth's surface, it will usually pass first through some thickness of permeable soils, gravels, and porous or fragmented rock that has been kept at near-atmospheric temperatures by active groundwater circulation. At greater depths, the hole may encounter a fracture system or a confined aquifer that contains natural steam or hot water, heated either by conduction from hot rock at still greater depths or by deep circulation of groundwater through open faults and fractures. Unfortunately, since they can be convenient and economical sources of thermal energy, such "hydrothermal" systems are relatively rare and underlie only a very small fraction of the earth's surface (Sass and Lachenbruch, 1979).

Far more commonly, as temperatures of geothermal interest are approached, it is found that the combination of increasing temperature, overburden pressure, and chemical reactions has progressively reduced—to very low values—the permeability and free-water content of the formations penetrated. This is the typical "hot dry rock" (HDR) situation: naturally heated crustal rock that is too low in permeability and free-water content to be economically productive of natural steam or hot water.

The location, depth, volume, and useful heat content of an HDR geothermal reservoir are determined jointly by the local geology and the nature, location, and intensity of the heat sources affecting it. The principal heat source is usually heat from the earth's mantle and lower crust, conducted upward toward the earth's relatively cool surface. To this is added heat generated by the decay of naturally occurring unstable isotopes of uranium, thorium, and potassium in the upper crust. In some cases there are small contributions from chemical reactions involved in mineral alterations or generated by friction during large-scale earth movements, and in volcanic areas there may be large local contributions from near-surface magma bodies or their geologically recent solidification products.

It is usually impossible in any given location to estimate the magnitudes of these individual contributions with confidence, so that terrestrial heat flow must, in general, be measured experimentally. This involves measurements of both the temperature gradient within a particular formation and the thermal conductivity of that formation. Frequently however, only the gradient is measured and temperature is extrapolated downward by assuming typical values of conductivity for the various formations known or believed to exist at depths greater than those at which the geothermal gradients were measured. Since it is difficult to insure that gradient measurements are truly representative of *in situ* conditions and since the natures, thicknesses and conductivities of deeper formations are often unknown, there commonly are large uncertainties in such extrapolations. However, reliable information is now being collected on a worldwide basis from holes drilled deeply enough to penetrate reasonably homogeneous, low permeability rock at depths well below the deepest active groundwater circulation. Frequently the results indicate the existence of promising subterranean HDR geothermal reservoirs in unexpected places.

For example, Fig. 1 is a preliminary map of the conductive geothermal gradient in the conterminous United States. Since there are many large areas for which reliable data are still not available, the map is very incomplete. It does, however, demonstrate a broad distribution of large areas within which usefully high temperatures exist at economically accessible drilling depths, often at locations where they can be immediately useful when a mature technology for exploiting them has been developed. From such maps, estimates can be made of the resource base of HDR geothermal energy. While the U.S. is relatively fortunate in this regard, similar studies in other countries demonstrate that the natural heat in hot dry rock at accessible drilling depths is one of the largest supplies of usable energy that is available to man.

1.3. Early proposals for recovering geothermal heat

In 1904, Sir Charles Parsons, an eminent British engineer, proposed that the underground thermal regime be explored by sinking a shaft 12 miles deep (Parsons, 1904). He estimated that this would take about 85 years, cost 5 million 1904 pounds, and reach a temperature of the order of 600°C— representing an average geothermal gradient of 30°C/km, which is very close to the worldwide average. Since Sir Charles called this "The Hellfire Exploration Project," Christopher Armstead suggests that the proposal was made tongue-in-cheek (Armstead, 1978). However, Sir Charles did refer to it again in 1919 in his presidential address to the British Association. Unfortunately, the cost of sinking deep

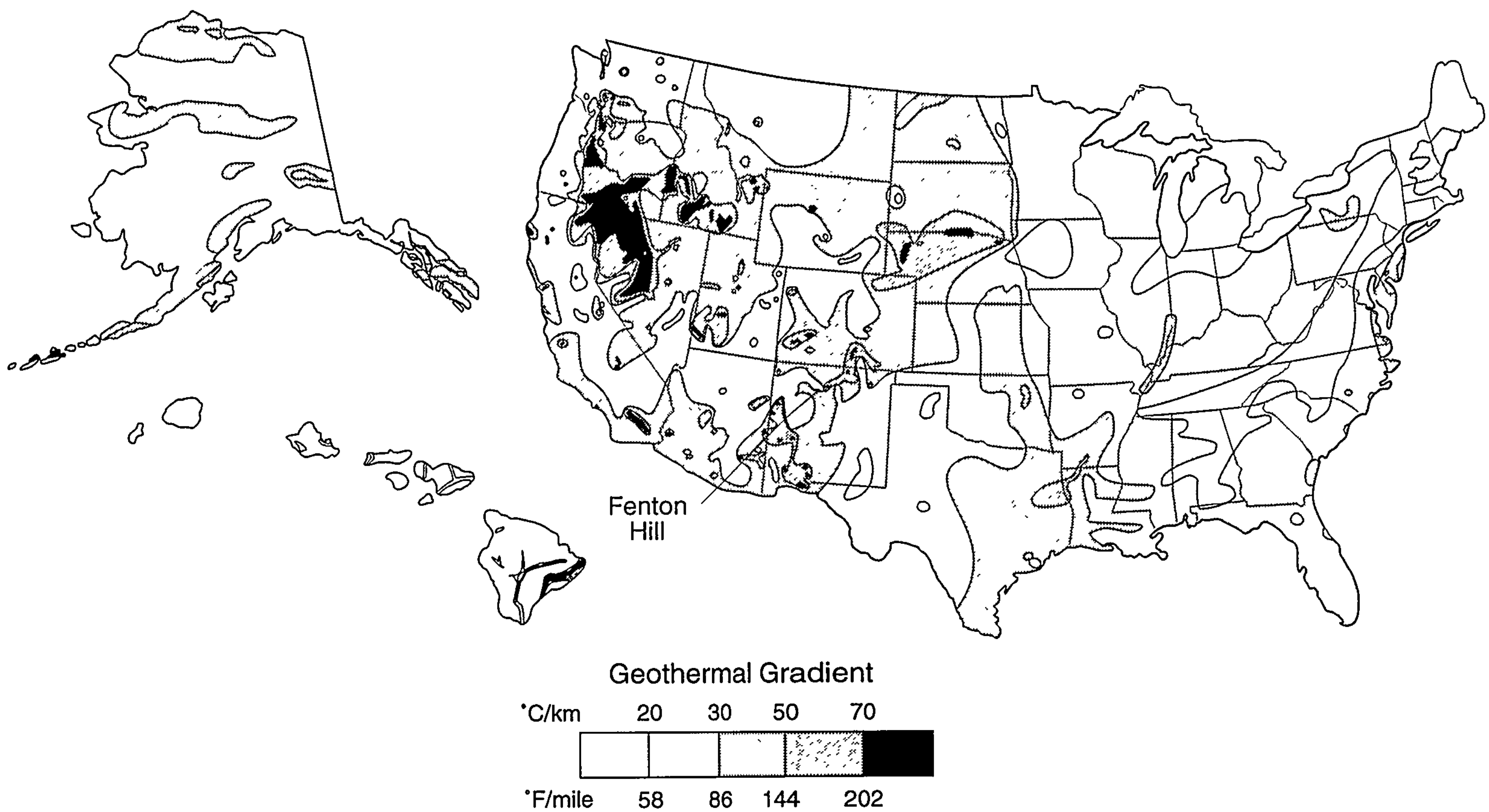

Fig. 1. Geothermal gradient map of the conterminous United States (Kron, Wohletz, and Tubb, 1991).

shafts is so high that none has ever been excavated for this purpose, although considerable information on the thermal regime at depth has since been collected in very deep and very hot metal and diamond mines on several continents.

By the 1920s, the needs of the petroleum industry had led to development of rotary drilling equipment capable of reaching depths of geothermal interest at costs much less than those of sinking a shaft of equal depth. Appreciating this, in an address to the British Association in 1927, John L. Hodson discussed the "vast potentialities" of geothermal heat (Smith, 1975). He proposed that heat be recovered from a drilled hole by inserting into it a simple downhole heat exchanger—water circulated through metal pipes. Recognizing one of the problems of heat exchange in such a system, Hodson suggested that it could be solved by simply increasing the length of the pipe in the lower part of the hole. Unfortunately, however, while this would increase the heat-transfer area of the heat exchanger, it would not increase the area of the borehole wall through which (in a purely conductive thermal environment) all of the heat must enter the hole. Since even crystalline rocks are relatively poor conductors of heat, the rate at which heat enters the hole decreases rapidly with time once heat removal through the heat exchanger has begun. For any hole size or depth, the value of the heat recovered is not sufficient to cover operating costs and amortize the cost of the hole and heat exchanger (Ernst, 1979). Downhole heat exchangers therefore have not been economical for heat recovery except where heat conducted into the wellbore is supplemented by heat brought into the well by hot water flowing continuously through it—as occurs, for example, at Klamath Falls, Oregon, where relatively shallow wells penetrate a hot aquifer.

It has often been suggested that in hot, low-permeability rock (the typical HDR situation) this deficiency could be overcome by simply enlarging the lower part of the hole—for example, by "springing" it with explosives, underreaming it, or mining out a large cavity. In fact, the amount of conventional explosives that can be contained in a unit length of borehole is relatively small and, being surrounded by solid rock, its fracturing efficiency is low. Repeated explosions can help a little but, at best, the effects of the explosive extend only a few feet outward from the hole. This, by itself, will not increase the rate of long-term heat production enough to make the system profitable, and the benefits of enlarging the lower part of the hole mechanically are even more limited. Mining out a usefully large cavity there is impractical, although solution-mining in a hot salt dome or bedded salt deposit may prove to be an economical method of developing a heat-recovery system.

Alternatively, if a large volume of rock around the bottom of the hole can be fragmented in such a way that water can circulate freely through it to extract heat from the rock, another type of heat-recovery system becomes possible. The use of nuclear explosives provides this possibility, since a very large amount of energy can be delivered from a downhole package a few feet long and slender enough to be lowered into a borehole of conventional diameter. Development of HDR energy systems by this means was studied extensively in both the U.S. and the Soviet Union, although it was not actually attempted in either country. In addition to a strong emotional reaction by the general public to any use of nuclear explosives, their potential environmental effects are so serious that an underground nuclear explosion would require a degree of long-term environmental surveillance that would be disastrous to the economics of such an energy system. An underground nuclear explosion produces fission products, leaves radioactive debris, and induces activity in some of the minerals in rock affected by it. Circulation of water through the fractured rock to extract heat from it would transport some radioactive material to the surface along with the heat. If the useful heat were recovered at the surface through heat exchangers and the cooled water recirculated underground, this activity could be safely contained (although the surface facilities would present a difficult disposal problem when the useful life of the system ended). However, a much more serious problem is presented by the possibility of migration of radioactive species by diffusion and in fluid escaping through cracks, natural joints, and any aquifers intersected by or adjacent to the well or the fractured volume. This would require that deep monitoring wells be drilled around the site and monitored continuously until the underground radioactivity had decayed to safely low levels. This would impose a large and essentially never-ending cost that could not be justified by even a large and otherwise economical energy system.

In 1970, an informal group of scientists and engineers at Los Alamos Scientific Laboratory (now called Los Alamos National Laboratory) proposed an alternative to the use of nuclear explosives to create the extensive underground fracture system required of an economical HDR energy system. They concluded that, in an operation called hydraulic fracturing, fluid pressure could be used to create the underground flow passages and heat-transfer surfaces required to recover heat efficiently from a body of hot rock at an accessible depth in the earth's crust. The background and initial investigations of this concept are described in the pages that follow.

1.4. Rationale for a hot dry rock energy development program

Heat is produced in the earth's interior by a variety of physical, chemical, and mechanical processes, a significant contributor being the slow, spontaneous, decay of naturally occurring unstable isotopes of uranium, thorium, and potassium. The decay reactions are similar to those that occur much more rapidly in man-made nuclear reactors, but in this case they occur underground, leaving energy behind as heat. Although the earth's surface is kept relatively cool by radiation of heat into outer space, the rocks, soils, and gravels that constitute the earth's upper crust are poor thermal conductors. Therefore, most of the internally generated heat remains stored within the earth. At accessible drilling depths, that heat represents the largest supply of directly usable energy that is available to man.

In an important number of fortunate locations, some of this geothermal heat is brought to or nearly to the earth's surface by the deep natural circulation of groundwater through permeable or fractured hot rock. Where this occurs, the hot water or (rarely) steam contained in aquifers or "hydrothermal reservoirs" can be a useful and economical source of heat for direct use or for conversion to electricity. However, at usefully high temperatures and with producible fluid volumes sufficient to support economic development, such occurrences are rare. Even at relatively shallow depths, more than 99% of the heat is contained in the rock rather than in any fluid contained in or moving through it (Muffler, 1979). This tremendous reservoir of thermal energy has a number of attractive characteristics.

- As heat in crustal rocks, geothermal energy is an indigenous energy supply in every country in the world. At depths that vary with the local geology, it exists at usefully high temperatures everywhere beneath the earth's surface.
- At accessible depths and useful temperatures, this natural supply of energy is so large that it can properly be considered "essentially inexhaustible" (ERDA, 1977). It is potentially capable of satisfying the world's total energy needs for thousands of years, during which time the resource base of geothermal energy will actually increase as technologies are developed to exploit it at greater depths, higher and lower temperatures, and in a greater variety of geologic environments.
- It is on permanent standby, available for use when needed and with no deterioration of quality or quantity with time.
- It is an inherently clean energy supply because the energy already exists as heat. It is not necessary to burn a fuel or operate a reactor to produce that heat, or to dispose of waste products from such chemical and physical reactions. In particular, an HDR geothermal energy system does not need a smokestack and does not contribute significantly to atmospheric pollution.
- Land use by an HDR system is minimal and unobtrusive. By use of directional drilling, wellheads can be concentrated in a limited area and surface facilities are small and relatively simple. Aside from distribution lines if the facility is used for district heating or power lines if it generates electricity, all operations occur at one site. There are no associated mines, railroad

spurs, coal piles, waste dumps, or spent-fuel storage areas. Given a suitable geological environment, a hot dry rock energy system could as well be built and operated within an urban area as at a very remote location—with only temporary, local inconvenience while the necessary wells were being drilled.

- The natural unit size of an HDR geothermal energy system is relatively small—of the order of perhaps 50 to a few hundred thermal megawatts, or 10 to 100 electrical megawatts. This is an advantage both when the market to be served is small and when incremental additions are needed to a large system. By addition of adjacent modular units, a geothermal system can of course be expanded, but there is little economy of scale. That is not true of fuel-fired or nuclear plants, where economics usually dictate construction of large plants with a very high initial investment and the probability that they will represent a great deal of unused capacity for a long time.
- In operation, an HDR geothermal energy system is secure against interruption by darkness, inclement weather, transportation problems, or civil or political upheavals away from the plant location.
- Once the HDR system is operating, the cost of producing energy from it is relatively stable, varying only with operating and maintenance costs. The "fuel cost" has been paid in the expense of drilling and completion of the underground system, manpower requirements for surveillance and maintenance are minimal, and the surface plant is relatively simple and should be nearly trouble-free.

There are, of course, also difficulties and disadvantages associated with the development and use of HDR geothermal energy systems that will become apparent as this report continues. Very broadly, these revolve around uncertainties concerning the geological characteristics of HDR thermal reservoirs, the engineering problems that remain in developing and using them, the economics of producing energy from them relative to utilization of more conventional energy sources, and the perceived risk of investing in their commercial development. However, these problems can certainly be solved in the foreseeable future, and the advantages of HDR systems are so great that it appears inevitable that they will eventually be constructed and used on a large scale around the world. They can make a major contribution to man's energy future, and the development of the scientific understanding and engineering technology needed to make them commercially successful deserves effort and support in some proportion to the importance of that potential contribution.

2. ROOTS OF THE LOS ALAMOS HOT DRY ROCK PROGRAM

The direct ancestry of the Los Alamos Hot Dry Rock Program can be traced back to the establishment of a highly secret weapons-development laboratory in northern New Mexico during World War II.

2.1. Nuclear energy programs

What came to be called Los Alamos Scientific Laboratory (LASL) was established in 1943 as Project Y of the Manhattan Engineer District. Its mission was to develop a massive new military weapon—the "atom bomb." To do so required extending the frontiers of knowledge in a wide variety of scientific, engineering, and military disciplines. One of the most important of these was what we would now consider a part of nuclear physics: collecting new information on the properties and behavior of fissionable materials (such as uranium) and on the fission process itself. Among many other things, this required experiments in a nuclear reactor, and so the world's third nuclear reactor—the first "Water Boiler"—was assembled in Los Alamos Canyon late in 1943. Since then, a long series of nuclear reactors of various kinds has been designed, built, and operated at LASL, and reactor expertise has always been a major strength of the Laboratory.

In the early 1950s, a military need was perceived for a rocket capable of delivering large explosive devices over very great distances. At the time, it was not clear that chemical propellants could produce the thrust needed by such a rocket. Los Alamos proposed that it could be done with a compact gas-cooled reactor as the rocket engine, and so a Nuclear Rocket Program called Project Rover was established at Los Alamos in 1955. Initially it was sponsored jointly by the U.S. Atomic Energy Commission (the AEC) and the U.S. Air Force—whose role was subsequently assumed by the National Aeronautics and Space Administration (NASA).

One reactor concept investigated for this application by LASL was called Dumbo. In such a reactor tungsten sheets loaded with uranium were to be assembled in a honeycomb pattern and used to heat the propellant, which was to be hydrogen. This approach was finally abandoned in favor of a graphite-core reactor, but not before an impressive amount of work had been done on core design, materials development and fabrication, and laboratory scale feasibility experiments and demonstrations. In particular, to demonstrate the heat-exchange characteristics of the metal core structure, a small electrically heated model core section was constructed from tungsten sheet. Hydrogen flowing through the metal honeycomb was raised to temperatures above 3000°C. It was an impressive demonstration and, while it did not lead to building a propulsion reactor, it was not forgotten.

The Dumbo concept originated and was largely developed in LASL Group CMF-4. That was an advanced inorganic chemistry group led by Eugene S. ("Robbie") Robinson and officially concerned with such matters as the separation of various fissionable isotopes. In his organization Robbie had assembled a very talented group of scientists with lots of imagination and energy. Its formal and informal meetings were notable for the eruption of far-out ideas among which—fortunately—Robbie had remarkable scientific and intuitive ability to discriminate the good from the bad. The Dumbo idea was one of the good ones, and in February 1957 a report entitled "A Metal Dumbo Rocket Reactor" (Knight et al., 1957) was distributed. Shortly thereafter, three of the authors of that report—Bruce W. Knight Jr., Berthus B. ("B.B.") McInteer, and Robert M. ("Bob") Potter—were transferred from Group CMF-4 to group N-l, in the new Propulsion Reactor division, to work on both the metal-core and the graphite-core concepts. However, in mid-1959 the Dumbo design was dropped from consideration, and the three returned to CMF-4 with instructions to take at least six months for innovative thinking—so long as it was not about nuclear rocketry. That, according to Robert D. ("Bob") Fowler, the CMF division leader, was to be strictly N Division's business.

2.2. A rock-melting penetrator

This innovative trio was joined in CMF-4 by James S. ("Jim") Coleman and Dale E. Armstrong, and serious thinking began. A number of weird and wonderful ideas emerged, one of which led finally to the HDR energy concept.

At one historic meeting of this group in early 1960, someone recalled the excellent performance of the tungsten heater used in the Dumbo heat-exchange demonstration. Bob Potter, who was always interested in everything, had recently reread Edgar Rice Burrough's book *At the Earth's Core,* and had decided that there must be a better way to make very large, very deep holes than by simply grinding up the rock. He suggested that melting the rock might be such a way, and that that tungsten heater certainly got hot enough to do it.

At that time there was great interest in such deep-drilling operations as the Mohole Project and offshore drilling for oil and gas. This idea therefore appeared to be timely as well as interesting. Accordingly, Bob Potter liberated a few pieces of the local basalt from a road cut on State Highway 4 a few miles east of Los Alamos and brought them back to a basement CMF-4 group shop to investigate the idea. The group rigged up a hairpin-shaped tungsten ribbon as an electrical-resistance heater, connected it to a small welding generator, and mounted a piece of the rock on a screw jack beneath the heater. When the tungsten reached a white heat they slowly raised the rock against it—and quickly melted a very neat slot in the basalt. An important observation was that the molten glass produced by melting the rock wetted the tungsten, creating an adherent surface layer that protected the metal against oxidation by air or by water vapor boiled out of the rock. The rock melter appeared to be another good idea.

While this was not the only innovation pursued by CMF-4, the group did proceed to develop a rock-melting penetrator. A series of small-diameter units was designed and tested successfully in the laboratory, the largest having an outside diameter of 2 inches (Fig. 2). The molten rock was removed from the hole by forcing it to flow into a central orifice in the melting head and blowing it to the surface with a high-velocity gas stream. Smooth, regular holes were produced in a variety of rock types, to depths up to about 8 inches. The groundwork was laid for development of larger penetrators to produce much deeper holes, but this happy state of affairs ended before that could be undertaken.

CMF Division was the Chemistry and Metallurgy Research Division, and Bob Fowler had strong opinions about what was research and what was not. In his opinion, development of a rocket-propulsion reactor was not research, and neither was development of a rock-melting penetrator. His orders to Robbie's group were to write a final report and get out of the drilling business. In 1962 they did, although the report (Armstrong et al., 1965) was not actually distributed for about three years. (The concept of the rock-melting penetrator was patented, with the patent assigned to the AEC.)

However, good ideas die hard. They are inclined to spring forth from the closet at some random future time, which may be opportune, or embarrassing, or both. In this case it was both.

Fig. 2. Jim Coleman with a laboratory scale rock-melting drill and scoria ejected from the melted hole (Robinson et al., 1971).

2.3. The nuclear penetrator

In those days, some of the senior staff of Group CMF-4 maintained the laudable custom of leaving work a little early on Friday afternoons in order to get a good table in the lounge at a local pub. There, in moderation, they consumed liquid restoratives and contemplated whichever of the universe's greater problems happened to come to mind. On one such Friday in early 1970 their discussion turned to the matter of what technologic developments since 1962 might contribute to further development of the rock-melting penetrator. By very late afternoon it had been decided that, using a compact nuclear reactor and heat pipes to transfer thermal energy to a refractory metal shell around it, very large melting penetrators could be built and operated. These could be used to bore the large, long shafts and tunnels needed for underground transportation of people and materials and for many other useful purposes.

It happened that on that particular day Manuel Lujan, Jr., then our district Representative in the United States Congress, was in Los Alamos on one of his periodic visits. Of course he knew Robbie who, among many other things, was a dedicated politician. Dropping into the lounge to greet any of his constituents who happened to be there, the Congressman spotted Robbie and his cheerful group of innovative thinkers, and came to their table to shake hands and ask what was new. Robbie, of course told him about a modern version of the rock-melting penetrator. Mr. Lujan was appropriately impressed by its possibilities as they were enthusiastically outlined to him by Robbie and the rest of the group. On leaving he announced that upon his return to Washington he would contact the Congressional Joint Committee on Atomic Energy and express his pleasure that the AEC was sponsoring such a worthwhile and farsighted program at LASL.

Of course the AEC wasn't doing any such thing, although those present in the lounge didn't want to tell our Congressman that, and his announcement gave pause even to Robbie. The predictable course of events was that the Joint Committee would contact Glenn T. Seaborg (then Chairman of the AEC) and inquire concerning this remarkable device. Chairman Seaborg would be irritated that LASL was developing such a thing without his knowledge, and then would call Norris E. Bradbury (then LASL's director) about it. Norris would be twice as mad, because he didn't know about it either. Robbie decided that, to minimize the repercussions that would immediately follow this chain of events, he had better explain things to Norris before word of it came to him from Washington. On the following Monday, after a restless weekend, he did.

Fortunately, Norris Bradbury was broad-minded, had a great sense of humor, and rather liked the nuclear-penetrator idea anyhow. He listened to Robbie with about as much sympathy as amusement, and the two decided that the best course of action was to make a quick paper-study of the postulated device. Then, when the occasion offered, Bradbury could tell Seaborg that we were indeed examining its potential but had not so far decided whether or not a program to develop it should be undertaken.

At that time my office was in the CMR (Chemistry and Metallurgy Research) building, three doors down the hall from Bob Fowler's office. On his way to break the good news to Fowler, Robbie stopped in to tell me (with considerable relief) about Bradbury's reaction, and (with great trepidation) to consider how he should explain it all to Fowler. The latter took him quite a while. Robbie was a pipesmoker and, after each of at least three pipefuls, he started for the door, had second thoughts, sat down, and filled his pipe again. Finally, however, he made it out the door and down the hall. Having myself had a few serious discussions with Bob Fowler, I knew that I would hear much more than half of the ensuing conversation, even that far away and with my office door closed. I did. What I heard was loud, long, and specific, leaving few details to the imagination. However, even Bob Fowler could not countermand the director's decision, and so the study of the nuclear penetrator was on.

Robbie promptly assembled an interdisciplinary ad hoc committee to undertake the study. It consisted of himself as chairman; Dale Armstrong, B. B. McInteer, and Bob Potter from Group CMF-4;

Robert L. ("Bob") Mills, a physicist expert in high pressures, from Group P-8; John C. Rowley from Group N-7, a mechanical engineer expert in theoretical and applied mechanics; Theodore P. ("Ted") Cotter from N-5, an expert on heat pipes: and me, Morton C. ("Mort") Smith, a metallurgist from CMF-13, as the materials man.

This committee met frequently during the spring, summer, and fall of 1970. Ted Cotter eventually withdrew from the committee, but he and other members of Group N-5 continued to advise us concerning heat pipes, compact fast nuclear reactors, and other matters. We also called in many other consultants from both within and outside the Laboratory. Among those whose information and advice were particularly helpful during that time were Orson L. Anderson, then of Columbia University, on rock physics and potential applications of very deep holes to research in the geological sciences; Sam Bradstreet, an independent consultant, on material selection; Richard C. Crook, of the Zia Company, concerning the geology of the Los Alamos area—where we hoped that field experiments might eventually be undertaken; Abe Rosenzweig, from the University of New Mexico, on minerals, rocks, geology in general, and geothermal reservoirs; and Johannes Weertman, from Northwestern University, on geophysics and the deformation and fracture of rocks.

2.4. The Subterrene proposal

One subject considered at length that summer was the desirability of a catchier name for the device than "rock-melting penetrator." We finally agreed on "Subterrene"—suggested, I believe, by John Rowley and (in analogy to a submarine) intended to signify an object that moved beneath the earth's surface. We still call it that.

However, the serious business of the spring and summer of 1970 was the study of how a Subterrene might be constructed, whether and how it would work, and what it would be good for if it did. The principal product of the committee was expected to be a written report to Norris Bradbury covering those subjects. I agreed to write the body of the report and to edit the more detailed appendices, which other committee members were to prepare. My first draft was written in April 1970 and, after six more subsequent drafts, the final version was completed in November. It had grown into "A Proposal for LASL Development of a Nuclear Subterrene," and was directed to Harold M. Agnew, who, on September 1, had replaced Norris Bradbury as LASL director. In the accompanying letter of transmittal, our committee reported: "We conclude that a major program should be established at Los Alamos Scientific Laboratory to perform the laboratory and field studies required to determine the feasibility of constructing and operating a Nuclear Subterrene and to undertake its orderly development and eventual full-scale demonstration.''

Recognizing the deteriorating energy situation of the nation, the U.S. Congress had recently directed the AEC to become involved also in the development of energy sources other than nuclear reactions. At that time, LASL funding was almost entirely from the AEC Division of Military Application (DMA), and this included a proportion of discretionary funds (called "Supplementary Research," or "SRI," funds) that could be used by the LASL director to support research and development (R&D) in almost any area consistent with AEC's interests. Drilling, which is basic to development of most energy supplies, had now become appropriate to AEC's interests, and therefore to LASL's. The Laboratory therefore was in a position to initiate work in that area if the director considered it worthwhile to do so. To assist him in making such decisions, Harold Agnew had established an Office of Special Projects, in the person of Austin D. ("Mac") McGuire. In early December 1970, Mac initiated a review of the Subterrene proposal by distributing copies of it for comment by senior Laboratory management and other LASL personnel with related interests. The resulting reviews were a mixed bag. One knowledgeable staff member considered it to represent one of the dumbest ideas in history. Others viewed it more favorably, although several suggested that, at least in its early stages, the program should not try to go beyond an electrically heated melting penetrator. The predominant opinion was that the idea was sufficiently promising that the Laboratory should pursue it, and the director authorized the ad hoc committee to initiate background work on the Subterrene and to explore the interest of the AEC and other federal agencies in supporting its development.

We promptly began to do so, although we were handicapped by the complete absence of a budget and by the fact that all members of the Subterrene Committee were already employed full-time in other, official, programs, to which each of us had commitments and responsibilities. A great many lunch hours, evenings, weekends, and holidays went into the Subterrene project, as they had previously and would in the future.

Appreciating the need for a slick-paper sales document, we spent December 1970 polishing up the proposal that we had addressed to Harold Agnew, in particular deleting the rather alarming estimates of personnel and funding requirements that made it a proposal rather than just a technical prospectus. The product was published in April 1971 as "A Preliminary Study of the Nuclear Subterrene" (Robinson et al., 1971).

An entertaining and instructive story could be written covering the subsequent history of the Subterrene program. We did sell the proposal in Washington, and the program initially was funded by the National Science Foundation (NSF) under its RANN Program (Research Applied to National Needs). Eventually it was transferred to ERDA, the Energy Research and Development Agency that succeeded the AEC, and it endured until 1976. With John Rowley as coordinator, it was quite successful—although, in fact, it never progressed beyond electrically heated melting penetrators and the production of relatively shallow, small-diameter holes. These, however, were sufficiently useful for a variety of applications that interest in the electric Subterrene has persisted both within the Laboratory and in a member of industrial organizations.

This report, however, is the history of the Hot Dry Rock Geothermal Energy Program, whose immediate ancestry is in the discussions that finally produced that first Subterrene proposal.

2.5. The hot dry rock idea

One matter that came to our attention during our discussions of the Subterrene concept was the existence of a natural gradient of increasing temperature in the earth's crust, of which initially we were only vaguely aware. In considering the probable usefulness of a melting penetrator, we concluded that one of its major advantages was that—unlike mechanical drilling devices—its efficiency and penetration rate should increase directly with rock temperature, and therefore with hole depth. To evaluate this, we explored the literature of geothermal energy and deep drilling for oil and gas, and incidentally learned quite a lot about subterranean geology. We were impressed by several of the facts that we discovered.

Since the earth's interior is very hot, there is a tremendous supply of potentially useful thermal energy stored in the crustal rocks beneath us everywhere. It is a truly international energy supply that, if it could be brought to the surface at useful temperatures and reasonable cost, would be a major source of energy for every country in the world—including those that lack all other indigenous energy supplies. Since it already exists as heat, this geothermal energy is inherently clean: no fossil or nuclear fuel need be burned to produce it, and there are few if any undesirable by-products or residues. In an environmentally concerned world, this should make it a very desirable energy source in industrialized as well as developing countries.

The rate of increase of temperature with depth—called the geothermal gradient—is about 25° to 30°C/km on a worldwide average. (It is, of course, considerably less than that in some areas and very much more than that in others.) Since hole depths of 6 to 7 km were becoming common in oil and gas drilling, it appeared to us that it should be possible almost anywhere to drill down to rock at usefully high temperatures—if not high enough for generating electricity, then at least useful for such applications as heating homes, drying crops, and processing foods, chemicals, and wood pulp.

Under something less than 1% of the earth's surface there are "hydrothermal reservoirs"—significant volumes of natural hot water or, rarely, of steam, confined within porous and permeable sedimentary rocks or faults and fractures in denser formations. By bringing the hot water or steam to the surface through drilled holes, some of these reservoirs can be developed into useful and economical energy sources. However, such occurrences are rare. Far more commonly it is found that, with increasing depth, the combination of increasing temperature and overburden pressure, mineral alterations, and deposition of secondary minerals, has caused the porosity, permeability, and free-water content of the formations encountered to diminish progressively. At depths and temperatures of interest for geothermal energy development, the normal geologic environment is hot rock with very low porosity, permeability, and free-water content—what we now call hot dry rock (HDR).

The Subterrene Committee concluded that the vast thermal reservoir represented by HDR at accessible depths in the earth's crust was an energy supply that could and should become extremely important in the world's energy future.

2.6. Hydraulic fracturing

When a Subterrene is operated in a very porous rock such as the volcanic tuff on which Los Alamos is built, the molten glass formed ahead of the penetrator is extruded into voids in the formation around it and freezes to form a useful glass lining in the hole. However, in denser rock, disposal of the melted material is a major problem. The early experiments with the melting penetrator had demonstrated that it was possible to extrude the melt up through the penetrator to form a solid rod; or to blow it out of the hole with a gas stream, as pellets or filaments; or to lift it out in an open bucket built into the penetrator. None of these operations was simple, mechanically or thermally, and the Subterrene Committee searched for a better idea. Bob Potter had one.

Bob had been reading up on deep drilling in the oil and gas business and had learned about an operation called hydraulic fracturing, which by then was quite commonly used to increase the flow of fluids into oil, gas, and water wells. In its simplest form this is done by using a high-pressure pump at the surface to pump water down into an isolated section of the wellbore until sufficient fluid pressure is developed there to split the wall of the hole—just as the pressure developed by water freezing within it can split a water pipe. The result is a thin crack, usually vertical, in the wall of the hole, which can be extended outward into the rock around it for hundreds and even thousands of feet simply by continuing to pump pressurized water into it. Bob suggested that something much like that could be done to dispose of the molten glass produced by the Subterrene. If a tight seal were maintained around the penetrator head by freezing some of the glass there, then enough force could be applied through the drill string to cause the remaining melt ahead of the penetrator to fracture the rock around it. The glass would be forced out into the resulting cracks where it would freeze and remain. This would eliminate the need to bring anything to the surface and also, by increasing the compressive stress field around the hole, would help to stabilize it.

To the committee, this seemed like a great idea. We called it lithofracturing, and discussed it at considerable length in both our initial proposal and the subsequent Subterrene report. Unfortunately, the Subterrene Program was terminated before the procedure could be investigated systematically. However, there were indications in several experiments that lithofracturing actually occurred.

With regard to the present story, the important thing was that this brought to our attention the fact that fluid pressure in a wellbore could be used to produce large cracks in the wall of the hole and extend them outward for great distances into the surrounding formations. The committee envisioned many possible uses for large hydraulic fractures, including such things as development of systems for *in situ* leaching of mineral deposits, for extracting kerogen from oil shales and for underground storage of gases, liquids, and heat. However, with a serious energy supply problem developing in the United States and our new appreciation of the amount of thermal energy stored in the earth's crust, it seemed to us that the most important application of hydraulic fracturing would be to create man-made geothermal-energy systems. Large hydraulic fractures would provide the flow passages and heat-transfer surface required to circulate water through hot crustal rock, extract heat from it, and transport that heat to the earth's surface.

3. THE HDR GEOTHERMAL ENERGY CONCEPT

With this background, the Subterrene Committee gave some of its time to considering just how hydraulic fracturing might be used to recover useful thermal energy from the tremendous supply present in the earth's upper crust.

3.1. An HDR energy system

To extract useful heat from the earth's crust, we proposed that we should go wherever there was a need for energy in combination with a reasonably high geothermal gradient, then drill a hole there deep enough to reach a rock temperature sufficiently high to satisfy that need. If the hole should happen to encounter a productive reservoir of natural steam or usefully hot water, of course we would use that. In general, however, we would expect the hole to bottom in hot rock low in permeability and free-water content. In that case we would use hydraulic fracturing to produce a very large crack extending outward from near the bottom of the hole. Hydraulic-fracturing theory and limited evidence from oil- and gas-field experience indicated that, in reasonably homogeneous rock and a normal *in situ* stress field, the crack would be thin, roughly circular in outline, and approximately vertical. (This was commonly described in the literature as a penny-shaped fracture.)

Having created such a fracture, we would then intersect it at some point well above its center by means of a second hole drilled directionally from the surface—as is shown schematically in Fig. 3 (in which the crack thickness is badly overestimated). This would permit us to pump water down the first hole, from which it would circulate through the crack, extract heat from the rock, and return to the surface through the second hole as hot water under sufficient pressure to prevent boiling. At the surface, the hot water could be used directly for space-heating or other relatively low-temperature applications, flashed to steam to drive a turbine in an electrical-power plant, or passed through a heat-exchanger to transfer its heat to a second fluid that could then be used for either purpose. With its useful heat removed, the cooled water or condensate would be returned through the first hole to recirculate underground and recover more heat. (This is a closed, recirculating, pressurized-water, heat-extraction loop, similar in principle to the cooling system in an automobile engine.)

In low-permeability rock, the underground part of the system should be conservative of water and, being completely contained, it should present minimal hazard to the environment. Unlike a natural hydrothermal system, the chemistry of the fluid could be controlled by additions or special treatments at the surface, so that many of the corrosion and scaling problems common in conventional geothermal plants could be avoided. Further, we concluded that the density difference between cool water in the injection well and hot water in the production well would represent a "thermal siphon" that would assist in maintaining circulation around the loop and might—in deep wells with a large temperature difference—completely eliminate the need for mechanical pumping to maintain circulation. Finally, we believed that thermal contraction of the rock as heat was extracted from it would eventually create tensile stresses sufficient to open new cracks extending outward from the original hydraulic fracture and presenting additional rock surface for heat extraction. This "thermal-stress cracking" would cause the fracture system to grow with time and, if water circulated effectively through the new cracks, the useful life of the fractured thermal reservoir might be greatly extended.

These ideas seemed important to us, and we elaborated on them in our 1970 proposal to the Laboratory director. In that proposal we suggested that, once the Subterrene Project was under way, we should initiate a second major program to develop hot dry rock geothermal energy systems.

All of these concepts were also discussed in considerable detail in "Appendix F, Geothermal Energy," of the "Preliminary Study of the Nuclear Subterrene" (Robinson et al., 1971). That appendix was written principally by Bob Potter, and included a description of "A Geothermal-Energized Community" in which an HDR energy system would be used to generate electricity, heat buildings, distill water for domestic use, and provide energy for local

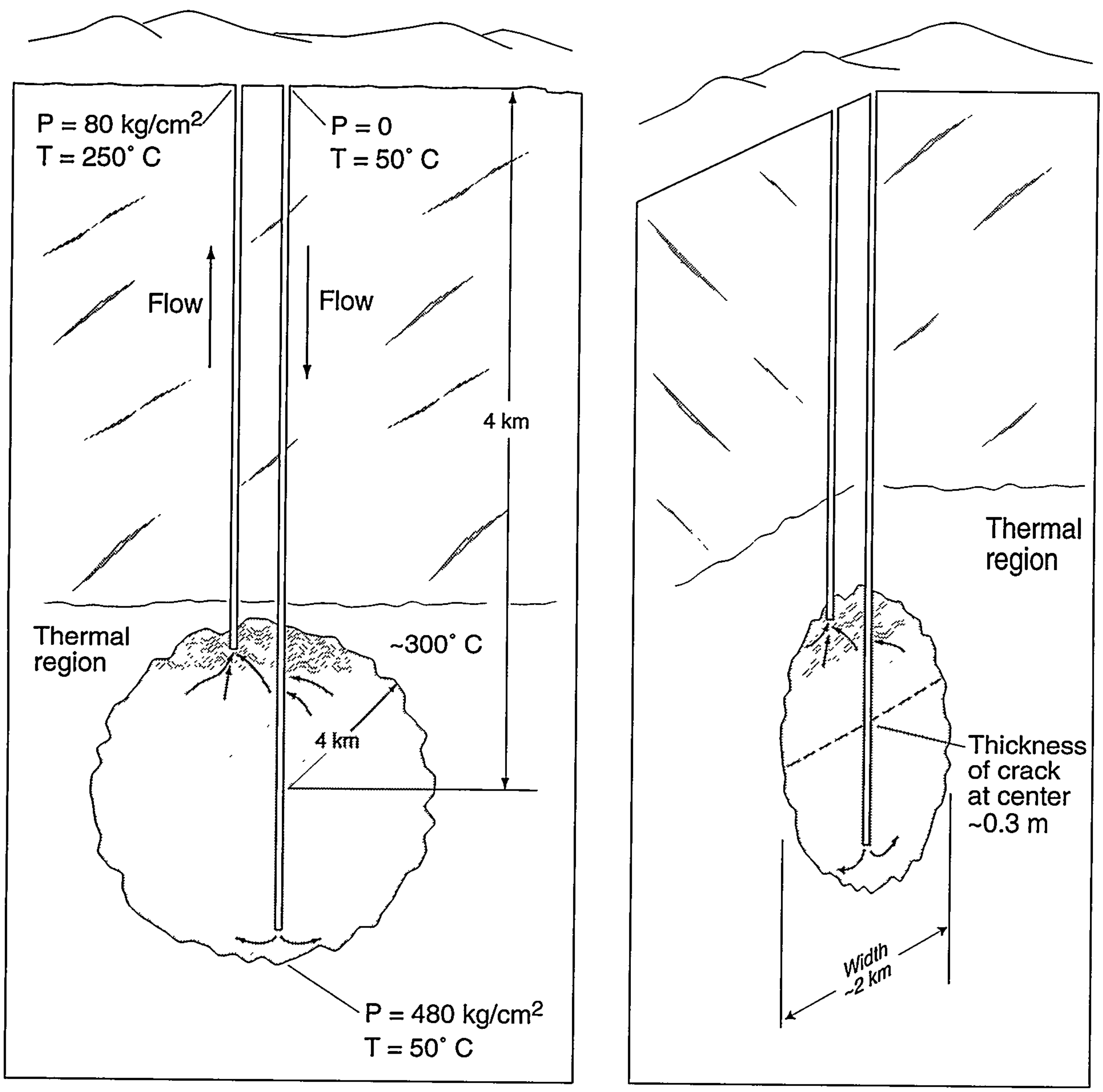

Fig. 3. Proposed system for developing the Los Alamos geothermal energy source (Robinson et al., 1971).

transportation in the form of either steam-powered or electrically driven vehicles. All of this was fairly visionary, but it did emphasize the idea that HDR energy systems could be broadly useful. The general idea deserved, and has since received, serious attention in communities with electrical-power, water-supply, pollution, and liquid-waste-disposal problems.

While of course it was not discussed in the above documents, the Subterrene Committee found some entertainment value in contemplating such things as the design of self-service stations to supply pressurized, superheated water to steam vehicles. Naturally there would also be an inexpensive car-wash. The discharge of steam and condensate during rush-hour traffic had interesting implications, especially during very cold weather. All this, however, does not mean that the idea should not receive further consideration. Efficient fuel-fired steam automobiles and trucks have been produced and used commercially, and the use of geothermally heated water would simplify them mechanically and thermally, avoid the use of hydrocarbon fuel, and exhaust only water vapor to the atmosphere.

3.2. Enlisting Laboratory support

This was a particularly difficult time for LASL. With development of efficient, reliable chemical propellants for rockets, the original mission of a nuclear-propulsion system had disappeared, and its possible use for space travel was considered to be very far in the future. In late 1970 there were rumors of a major cut in the Laboratory's Rover Program to develop such a system, and in late January 1971 the cutback materialized. As a result, it was necessary by the end of March either to find jobs in other laboratory programs for a large number of people or to lay them off. To minimize the layoffs, every effort was made to place the affected employees elsewhere in the Laboratory and to initiate new programs that could take up the slack. Although all of us on the Subterrene Committee were still supported by (and had obligations to) existing programs, we were encouraged by Laboratory management both to get limited initial work started on the Subterrene and geothermal-energy projects and to seek federal funding for them in Washington. We did both.

By the end of January 1971, the equivalent of about 15 full-time staff members from several Laboratory divisions had enlisted in the Subterrene Program, and background work had begun on further development of electrically heated melting penetrators. On February 25,1971, Richard P. ("Dick") Taschek, the Laboratory's newly appointed assistant director of research, announced the appointment of John Rowley as coordinator of the Subterrene Program.

Together with John, the other members of the original Subterrene Committee remained active both in its technical development and in selling it to funding agencies and to the public. However, most of us—and especially Bob Potter, B. B. McInteer and me—felt that the HDR geothermal-energy idea was at least as important as the Subterrene concept. We were anxious to pursue it and were encouraged to do so by the Laboratory's need for new programs. Further, the Subterrene report was about ready for publication and, in Appendix F, described the proposed HDR system in considerable detail. It was our idea, and we didn't want someone else to grab it and run before we could claim that LASL had already begun to develop it.

Among other things, selling a new program requires an advocate and lobbyist, and we had trouble finding an appropriate person willing to spearhead the effort. All of us on the Subterrene Committee were helping to get that program off (or into) the ground, and had other commitments to existing programs that were also important to us. Most of us were helping to retread other Lab employees caught in the Rover crunch, and none of us was anxious to bet his professional future on an untried idea in a strange new field. In particular, I was trying to keep alive an excellent carbon and graphite research and development program that had been badly wounded by the Rover cutback. However, I was either less reluctant or more adventurous than anyone else on the committee and so I agreed to take on the HDR assignment. Fortunately for me, Bob Potter was as enthusiastic about HDR as I was, and he agreed to work on it with me.

From 1970 to 1973, the HDR Program was

informal in every respect, including financial support and was carried on largely on a volunteer basis. All of us who were involved were hustling frantically on too many other things to keep good records of our HDR activities. As a result, in many cases I am unsure of dates and the exact sequence of events, and undoubtedly I have forgotten occurrences and interactions that should be part of this history. However, at about this time—probably in late February or early March of 1971—Dick Taschek did officially appoint Robbie coordinator of the HDR Program, with me as program manager (of a program that so far did not officially exist).

In the meantime, some important organizational changes had been made. Bob Fowler had retired from the Laboratory in June 1970, and most of his CMF Division had been absorbed into CMB Division under Richard D. ("Dick") Baker. However, several groups from both divisions had been spun off to form a new CNC Division (Chemistry and Nuclear Chemistry) under George A. Cowan. Both CMB and CNC Divisions had been badly stung by the Rover cutback, and both Baker and Cowan were sympathetic to the HDR proposal. So, of course, was Robbie, who was Bob Potter's group leader in what had become CNC-4, and so also was my own group leader, Robert B. ("Bob") Gibney in Group CMB-13. We also had the essential and continuing support of both Harold Agnew and Dick Taschek. Potter and I therefore were permitted to move into offices that Fowler had vacated (where Robbie had perspired so copiously in announcing resumption of work on the melting penetrator), to taper off our commitments to other programs and to give all the time that we could to cranking up what we then called a Dry Hot Rock Program. (That name was my fault. I insisted that all geothermal reservoirs contained hot rock and that the distinctive thing about the ones in which we were interested was that they were essentially dry—which deserved priority in the program title. Somewhat later, someone in Washington decided that "Hot Dry Rock" was more euphonious, and insisted that we change the title accordingly. We did, and perhaps for the better. In any case, that is now the familiar name, I have already used it in this history, and in general will continue to do so.)

Bob and I settled into our new offices, dug into the relevant literature, talked to anyone who would listen, wrote like crazy, and enlisted help wherever we could. One of our first and wisest moves was to recruit Donald W. ("Don") Brown to the HDR cause. He had been a mechanical engineer in N-7, one of the Rover Program groups, and came to us with the blessing of Roderick W. ("Rod") Spence, the N division leader (but rather emphatically, without the blessing of John Rowley—who had been Don's group leader in N Division and who could have used his help in the Subterrene Program).

Don moved in with Bob Potter, in the office adjoining mine. He became our field engineer, at which he excelled, and was gone from the office much of the time. Bob was a dedicated collector of "relevant information," and soon his collection overflowed onto Don's desk. When that also was obscured, the stuff began to accumulate on the small conference table in my office. I will not forget the day when, searching through the accumulation on the conference table, Bob dislodged a coffee cup that fell to the floor and shattered. It was one that he especially treasured and had given up for lost, which certainly it had been. Such disasters aside, Bob and Don and I enjoyed sharing those offices and, working that closely together, we also got a lot done.

After Don, our next valuable acquisition was R. Lee Aamodt, who supplied knowledge, capabilities, and contacts that we needed very badly. He was a physicist from the division office of J division—our Nuclear Test Division, then led by William E. ("Bill") Ogle. Among many other things, Lee had been directly involved in the Plowshare Program, which had included the first serious proposals for development of HDR geothermal energy—in that case, by the use of nuclear explosives. We found him an office just down the hall from us in the CMR building, and he set to work with the rest of us. It was an important bonus that Lee had many useful and influential acquaintances both within and beyond the Laboratory, and he as well as Don Brown turned out to be an excellent and discriminating recruiter for the HDR cause.

In an effort to drum up additional support from as many LASL organizations as possible, I invited representatives from several groups and divisions to attend our first HDR Information Meeting on April 8, 1971. Those outside our own small group who attended were

- Mac McGuire, from the Director's Special Projects Office;
- R. H. ("Bob") Campbell, Harry Otway, Robert R. ("Bob") Sharp, Jr., and James H. ("Jim") Hill from J Division (Weapons Testing);
- Harlow W. Russ and Fred Doremire from W Division (Weapons Development);
- George I. Bell from T Division (Theoretical); and
- L. P. ("Phil") Reinig and Robert I. ("Bob") Brasier from ENG Division (Engineering).

Robinson, Potter, Aamodt, and I gave them the HDR pitch, and most of them were interested and supportive. Thereafter we received a great deal of help from them and the people that worked with and for them—as well as from others from Laboratory organizations not represented at this particular meeting.

The HDR Information Meetings grew as the HDR Program expanded, and eventually became a weekly event—held on Tuesday mornings in the CMR building Conference Room. Their announced purpose was to exchange information on "our activities and progress as individuals and as a group." They were also useful in spreading the word on HDR throughout the Laboratory and in getting comments, criticism, new ideas, and fresh perspectives on what we were doing and planned to do. Invited to attend were everyone actively involved in HDR-related projects, their management, and everyone else that was interested. The meetings usually drew a good crowd. At them, I usually announced anything that was new with regard to program plans, our funding situation, other meetings of interest, and expected visitors. However, they were devoted principally to short presentations concerning what various individuals were doing at the time, and long, informal discussions of almost anything of interest to the group. They were useful not only in keeping everyone involved up to date concerning what was actually going on, but also in enlisting and maintaining support for the program by people who were not themselves directly involved in it.

That was important. In those days we did not have the programmatic constraints that have since been imposed in the form of day-by-day management of the activities, budgets, and plans of many laboratory programs by their several sponsors. Management by the AEC and the LASL administrations was relatively informal, and when any of us needed help we could usually walk down the hall or across the street and get it. If we needed a larger commitment of time or effort than an individual could conscientiously give us on his own, then his group or division leader had a great deal of freedom in authorizing him to work with us. Little or no red tape was involved, and we got a lot of help informally from a lot of people—from the Laboratory director on down. Without it and the atmosphere in which it occurred, the HDR Program could not have been started at Los Alamos.

3.3. More paper work

In early March 1971, I drafted a memo to Harold Agnew proposing that LASL should act promptly to establish a geothermal energy program based on our ideas for developing HDR energy systems. On March 9, I met with the Subterrene Committee and Don Brown to review and improve the draft. The resulting memorandum, dated April 8, 1971, was addressed to Harold Agnew, LASL director, entitled The Possibility of a LASL Geothermal Energy Program and signed by the ad hoc Committee on Rock-Melting Drills—Dale Armstrong, Don Brown, B. B. McInteer, Bob Mills, Bob Potter, Gene Robinson, John Rowley, and me (Fig. 4).

That memo described the importance and timeliness of a proposal for HDR development, and listed the major steps in a 7-year program to develop a prototype system. These included drilling the first hole to a depth of about 15,000 feet; hydraulically fracturing from it; then directionally drilling a second hole to intersect the fracture at about 10,000 feet. (This assumed a vertical fracture and implied a fracture radius of at least 2500 feet.) We noted that no engineering study of probable costs had been made, but gave "a very quick estimate" that perhaps $1.5 million would be needed for outside contracts in each of the first 2 years and $ 1.0 million per year for the next 5 years—with the cautioning note that these numbers might be low by a factor of 2 or 3. Of course they turned out to be worse than that and did not include salaries and overhead for the LASL staff. One reason that our guesses on costs were low was that, naively, we believed the claims of the drilling and service companies whom we had consulted that the drilling and fracturing which we proposed could be performed routinely with their existing capabilities. At least we were about right in believing that we could complete a prototype system (subsequently called the Phase I system) in about 7 years.

During the summer of 1971, I spent all of the time that I could in writing what amounted to a preproposal description of an HDR geothermal energy program to be established at Los Alamos and in enlisting the help that I needed to make a credible job of it. This was intended to be a sales document, to interest potential sponsors, and to be the main body of a subsequent formal proposal in which it would be supplemented by a series of technical appendices. My first draft was circulated on April 27, 1971, to what I called the ad hoc LASL GTE Committee, consisting of the original Subterrene Committee plus Don Brown and Lee Aamodt. (We were still Los Alamos Scientific Laboratory; GTE stands for geothermal energy; and we were about as ad hoc as any group could possibly be.) With input from the committee members and, in particular, a lot of help from Bob Potter and Don Brown, I prepared and circulated second and third drafts and finally what we called a Preliminary Study of the Development of Dry Geothermal Reservoirs. We sent this over to Harold Agnew in December 1971, signed by the members of what we decided was really the De Facto GTE Committee. During this time we also got to work on the technical appendices that would make the study a proposal, and on enlisting additional help in the many scientific and engineering areas in which we still needed it.

The history of that proposal and of our efforts to sell it comes later.

*Fig. 4. The Ad Hoc Committee on Rock-Melting Drills: (Clockwise from left) Don Brown, Bob Potter, Bob Mills, B.B. McInteer, John Rowley, Mort Smith (behind Rowley), and Dale Armstrong (*The Atom, *1971).*

4. SOME BACKGROUND STUDIES

Among those initially involved in the HDR Program, no one had any direct experience with geothermal energy. In some ways this was a distinct advantage, since we did not have to unlearn any of the existing misconceptions-which were plentiful. However, we did have an awful lot to learn. Therefore, in addition to literature surveys and our own analyses, we sought help wherever we might find it, both within and outside of our Laboratory.

4.1. A little scientific respectability

Lee Aamodt was well acquainted with the analytical and modeling capabilities of the Laboratory's T Division (our Theoretical Division). He interested Francis H. ("Frank") Harlow of Group T-3 in the problems of HDR energy systems, and Frank in turn enlisted William E. ("Bill") Pracht, also from T-3, in a study of them. On May 7, 1971, the two circulated a working paper entitled "A Theoretical Study of Geothermal Energy Extraction." It included analyses of fluid circulation through a large hydraulic fracture, rates of heat extraction from its surfaces, heat losses in bringing the produced hot water to the surface, and the possibility that thermal-stress cracking (secondary fracturing from cooling of the rock) would extend the fracture system and increase its life as a useful producer of thermal energy. They were optimistic about the technical feasibility and expected useful lifetime of such a system, and they went on from there in their study.

On June 30, 1971, Bill Pracht distributed "A Numerical Study of Early Time Energy Losses in Geothermal Energy Extraction." It examined heat losses from superheated water flowing up a production well in granite, basalt, or sand. Bill showed that the rate of heat loss would decrease rapidly with time, to less than 5% after only a day or two of operation.

Frank and Bill completed this phase of their work in 1972 and published its results in the Journal of Geophysical Research (Harlow and Pracht, 1972). Their major conclusion was that, if the thermal-stress fracturing occurred and underground water loss was not excessive, then "Under sufficiently favorable circumstances large amounts of geothermal energy can be extracted from dry wells in hot rock." We in the HDR group of course felt free to assume "sufficiently favorable circumstances," moderate rates of water loss, and system enlargement by additional hydraulic fracturing if thermal-stress cracking didn't do the job for us spontaneously. (However, as a materials man who had tried unsuccessfully to avoid thermal-stress cracking in a variety of brittle materials, I, for one, was sure that it would indeed occur in crystalline basement rocks.)

This paper gave some scientific respectability to our HDR concept, which it badly needed. There were plenty of skeptics.

In the meantime, Lee Aamodt had himself become our own applied theoretician (if indeed such a title is acceptable). For example, he analyzed for us such things as hydraulic-fracturing behavior in the light of the theories that existed at that time. In a memorandum dated May 11, 1971, he documented his concern that—because of the gradient of horizontal stress with depth—a vertical hydraulic fracture might tend to "run away" upward instead of extending more or less uniformly in the horizontal and vertical directions. Lee alerted us to many possibilities and potential problems that we would otherwise probably have overlooked.

In another memorandum, dated June 15, 1971, Lee presented tables showing water pressure in a well as functions of depth, temperature, and wellhead pressure. He then calculated the temperature change that would result from adiabatic cooling of water or steam as it flowed up the well and the pressure on it diminished. Assuming a downhole temperature of 340°C and a wellhead pressure of 200 bars (2900 psi) he calculated that water rising to the surface from a depth of 5 km (16,400 ft) would cool 16°C from this effect alone, while superheated steam would cool by 31°C. (This was an extreme example, but it points out one of several reasons that we preferred to produce superheated water from the underground loop instead of steam.)

On the same day, Lee sent a memo to Mac McGuire in the Laboratory's Special Projects Office discussing one of his many ideas for applications of

HDR energy systems. At that time the White Sands Missile Range in southern New Mexico was a serious candidate to become the main base for NASA's space-shuttle program. White Sands is within the very large Tularosa Basin, in which geothermal gradients are relatively high and the water table is quite near the surface. The water, however, is very brackish from dissolution of the gypsum that makes up much of the abundant white sand. Lee proposed an HDR development to generate electricity that would be used to electrolyze the water, producing hydrogen and oxygen to fuel a shuttle. Nothing has so far come of the idea, but there are variations of it that could be useful in this interesting local situation. In particular, desalination of the water (probably by distillation) could supply an obvious need for clean water in that desolate area, and the geothermal heat to distill it is evidently there at a reasonable depth.

One of Lee's most interesting early memos, dated June 23, 1972, addressed a subject that has always been important to us: "Alternative Ways of Developing Geothermal Reservoirs." As he suggested, a whole battery of techniques is available for use in the variety of geological environments in which an HDR thermal reservoir may exist. In this memo, Lee described several alternative methods for developing an HDR system in rock that is too permeable to contain pressurized water without excessive fluid loss. These were (1) use of a downhole pump in the production well to reduce fluid pressure locally in the fractured reservoir, so that water would flow toward that well instead of outward into the formation around the fracture system; (2) use of a circulating heat-extraction fluid less dense than water; (3) operating the system in the "huff-puff" mode, with alternate injection of cool water and production of hot water through the same well—an idea suggested by Robert W. ("Bob") Rex and discussed in later chapters of this report; and (4) drilling a circle of injection wells around a large central production well, so that only cool water would be lost to the surrounding formations—at a rate which Lee predicted would diminish as the reciprocal of the square root of elapsed time.

Lee Aamodt and Bob Potter were a talented and productive team. Both had lots of ideas, many of which were basic to HDR development, and they loved to argue them out with each other. If there was a major difference between the two in this regard it was that Bob's ideas often tended to be more blue-sky and (perhaps surprisingly, for a physicist) Lee's were inclined to be more practical. Also, Lee produced a barrage of documentation memos over the years while Bob, unless forcibly confined to his desk, was usually too busy having and discussing new ideas to do much writing. However, Bob was diligent, meticulous, and insightful in examining, analyzing, and interpreting data of almost any kind, and he has written and published a great many reports and papers that are important in the literature of geothermal energy.

Los Alamos has always sought the advice and direct assistance of consultants and visiting staff members in a wide variety of disciplines, and many of these have been very helpful to our HDR Program. One such consultant was Oleg D. ("Oley") Sherby, then from Stanford University, who at that time was a consultant to my old physical metallurgy research group. We had sought his advice on the materials problems of the Subterrene, and in July 1971 I asked him to read and comment on the third draft of our preliminary study of hot dry rock geothermal energy. He did, and in a memo to me dated August 4, 1971, he indicated that he thought our proposed HDR Program had an excellent chance for success. However, like many others at that time, he was concerned about the feasibility of fracturing granite hydraulically. Therefore he suggested an experiment that would have been fun to try, if a bit awkward to arrange.

What Oley proposed was that we freeze a block of ice about the size of my office (which wasn't very big) and surround it with liquid nitrogen. That would keep it at an absolute temperature which, with regard to its mechanical properties, was approximately equivalent to granite held at 100°C, so that its mechanical behavior would be similar to that of hot granite. A metallic penetrator held at 25°C would be forced into the ice and melting, cracking, and refreezing of the ice would be observed directly through the transparent ice, as an analog to granite (although the volume changes during melting and freezing would have the opposite sign). Such an experiment would, then, give us information about the behavior of a melting penetrator, about hydraulic fracturing, and about what we had called "lithofracturing," in which the fluid produced freezes and remains in the hydraulic fractures. So far as I know, nothing quite like this has ever been attempted—although we and others have done very small-scale hydraulic-fracturing experiments using drilled holes in clear plastic, whose properties unfortunately are quite unlike those of granite.

Oley also suggested some possible funding sources for both the Subterrene and the HDR Programs and listed good people for us to contact at several of them. He and other consultants and visitors gave us a great deal of help, ideas, and encouragement to get on with the jobs that we had undertaken.

4.2. Drilling-cost estimates

In one of his May 11, 1971, memos, Lee Aamodt included some cost estimates for drilling in hot granite that had been prepared by LASL Group J-6—the group in our Test Division primarily responsible for drilling operations at the Nevada Test Site. For a hole finished at 8-in. diameter at a depth of 15,000 ft in granite at 400°C their estimated total cost was $4.1 million. This included a large contingency factor for the expected problems of drilling hot, hard rock, and the probability that some equipment development would be necessary. Actually, in 1971 dollars and for the first such hole ever to be drilled, this turned out to be a very reasonable estimate. However, the amount was so much greater than our own partially educated guesses (based primarily on published oil-field experience in drilling hard sedimentary formations) that we were careful not to advertise it. Instead Don Brown sought a second opinion.

Fenix and Scisson Inc. ("F&S") had served as architect-engineers for LASL drilling operations in several parts of the world in connection with both weapons tests and Plowshare experiments. Don found out that they were willing to prepare time and cost estimates for drilling, casing, and hydraulic-fracturing operations for us at no cost to us (which was fortunate, since our budget was zero). John R. McLaughlin and Scott Houghton of F&S prepared for us not one but eventually two estimates for different drilling sites, on the basis of the best geologic information that Don could give them at the time. They also remained available to advise us on drilling-related programs whenever we needed help, which was frequently.

The F&S estimate of interest here was dated August 17, 1971, and was for drilling two holes to a depth of 15,000 ft, with 9 5/8-in. casing to 14,800 ft in one and to full depth in the other. Total cost was estimated to be approximately $3.3 million for the two holes, including two hydraulic-fracturing operations (through casing perforations) and a 20% contingency factor. Since this was less than the J-6 estimate for a single hole, it cheered us considerably. However, it shouldn't have. The major difference was in the much larger contingency factor included by J-6, which turned out to be realistic. Like us, the F&S engineers had consulted drilling contractors and service companies—who were very optimistic concerning their abilities to drill, complete, and fracture a type of hole that no one had ever before attempted, and with their existing equipment and techniques. They subsequently learned a great deal and so did we.

4.3. Disposing of the heat

Our original concept for a commercial-scale HDR heat-extraction loop demonstrated that indeed we were thinking big. As of June 30, 1971, we visualized two holes each about 15,000 ft deep, each lined with 13 3/8-in.-diameter casing. We assumed injection into one of them of 4175 gpm (gallons per minute) of recirculated water at 50°C and 2030 psi, and production from the other of superheated water at 250°C and 2100 psi. (The increment of pressure resulted from the "thermal siphon" represented by the density difference between hot water in the production well and cool water in the injection well. Once established, with these temperatures and hole depths, we believed that this should be more than sufficient to maintain circulation through the system without mechanical pumping. This, however, was based on a theoretical calculation of the width of a pressurized hydraulic fracture, and an estimate of flow impedance through a fracture of that width. (Here again, we had a lot to learn.) We assumed that the circulating water would be kept pressurized as it flowed through a heat-exchanger at the surface, where we estimated a 70-psi pressure drop (just enough to utilize the pressure differential between the two wells, produced by the thermal siphon). We warned that the recirculated water might be high enough in dissolved minerals to cause corrosion and scaling problems, and that the rate of water loss from the fractured reservoir might be as much as 400 gpm—roughly 10% of the rate of fluid injection.

With the temperature difference assumed between the injection and production wells, this self-pumping system would produce heat at the rate of 227 MWt (thermal megawatts). At the time we were not much concerned about what the heat would be used for, although the assumed temperature of the produced fluid would certainly be high enough for production of electricity. However, we asked the LASL Engineering Department to look at the problem of simply disposing of that much heat at the surface—which would be necessary if this were an experimental facility and also for testing a commercial system before a power plant was built. Hilton E. Jones did so, and in a report dated July 22, 1971, he concluded that only an evaporative cooling tower would be feasible. The volume and cost of the water required for this were obviously prohibitive, so we backed off and asked the Engineering Department to look instead at disposal of only 100 mwt.

In a memo dated September 15, 1971, Robert J. ("Bob") Donham of ENG-7 reported on several possibilities for disposing of 100 MWt, all of which were also pretty discouraging. He noted that the only way to cool the circulating fluid without consuming water was to do it with air, and that a 100 MWt mechanical-draft heat-exchanger for that purpose would cost about $3 million. He estimated that an evaporative cooling tower would cost about $2 million and would lose about 18 acre-feet (5.9 million gallons) of water to the atmosphere per day. A cooling pond would cost about $800,000 and lose about 3 acre-feet (1 million gallons) per day, and some form of cooling by steam-release might cost $500,000 and need 4 acre-feet (1.3 million gallons) of makeup water per day. Bob concluded that our best bet was a cooling pond, but warned that—in the area west of Los Alamos, which we were then considering for such a plant—we probably could not obtain water rights for the required 3 acre-feet per day. He thought that we might be able to get the necessary water out of the Rio Grande, which would probably require a $1.5 million pipeline to move it up to the area in which we were interested.

Again, however, Don Brown solicited another opinion, this time from The Marley Company, a cooling-tower manufacturer represented in Albuquerque by James & Cooke, Inc. The consensus of the Marley engineers was that water-cooling was impractical in our situation at Los Alamos, and that we should plan to use a forced-draft air-cooled unit. For such a unit capable of dissipating about 130 MWt, their price at that time was $221,085 FOB Los Alamos. For a 20 MWt unit, about what we were considering for our first experimental system, their price was $49,261. This of course did not include moving it from Los Alamos to our experimental site and installing it there, but these relatively low prices relieved us greatly. (We did, in fact, eventually buy the Marley 20 MWt unit for our experiments in the Jemez Mountains, and it performed very satisfactorily for us. The cool dry climate where it was finally installed is ideal for an air-cooled unit, and its use avoided the major problem of acquiring water rights for operation of a water-cooled unit of any type.)

4.4. Hydraulic fracturing

The use of fluid pressure in the well to create cracks in the borehole wall and extend them outward into the surrounding formation was developed in about 1947 to increase the rate at which petroleum would flow from the producing formation into the well. Called hydraulic fracturing, this technique was widely used by 1971 both to stimulate production of oil and natural gas and to increase the rate of fluid injection in secondary recovery of petroleum by waterflooding the producing formation. It was central to our HDR concept as the means of creating the flow passages and heat-transfer surfaces required to extract heat from the rock. However, since it had been developed and used in the oil and gas industries, hydraulic fracturing had, to that time, been done only in the sedimentary formations in which oil and gas occur in nature. Several of the "experts" that we consulted were convinced that it could not be done in the hard crystalline basement rocks in which we proposed to do it, and many of the rest were very doubtful about it. Particularly since hydraulic fracturing had been done routinely in such hard sedimentary rocks as competent dolomites, we saw no reason why it should be a problem in basement rocks, but of course we did worry about it.

Our initial contact with Halliburton Services, who had a great deal of field experience in hydraulic fracturing, occurred in the spring of 1971, probably initiated by John Rowley. Whatever the background, on June 8, 1971, Dick Taschek wrote to A. B. Waters, Manager of Halliburton's Production Improvements Department, welcoming their interest in our HDR concept and inviting him and members of his staff to visit Los Alamos and discuss our proposed fracturing approach with us. As a result, on September 9, 1971, we were visited by A. B. himself; John Tinsley—who was doing research on hydraulic fracturing; Bill Raabe from Halliburton's Midland Office; and Ray Tippit from their Farmington Office. They gave us a lot of good advice and well-considered opinions, among them the following:

- They felt that rock could be fractured hydraulically at temperatures up to at least 260°C.
- They noted that the fracturing fluid used was usually water, but that it could instead be an acid or a hydrocarbon, and that it would probably be advantageous to fracture from both wells simultaneously.
- In their experience, the pumping pressures required (at the surface) to extend hydraulic fractures were generally about 0.6 to 1.0 psi per foot of depth in oil and gas reservoirs, and were likely to be about 1 psi/ft in deep wells. The "breakdown pressure" required to initiate a hydraulic fracture might be the same as or considerably higher than the crack-extension pressure, but they guessed that the wellhead pressure required for crack initiation at a depth of 15,000 ft might be in the range of 12,000 to 15,000 psi. Since their pumping equipment could deliver up to 20,000 psi, they felt that the fractures we proposed could indeed be made.
- In tight sandstone, Halliburton had extended hydraulic fractures to estimated radial distances up to 3000 ft. The 1500-ft-radius crack that we were considering appeared reasonable to them, and they guessed that its width might be something like 1 inch.
- They made an off-the-cuff estimate that the cost of making such a 1500-ft fracture would be in the range of $200,000 to $500,000.

In a follow-up letter to John Rowley dated November 9, 1971, A. B. Waters enclosed a "Preliminary Feasibility Study for Fracturing Granite in Conjunction with Geothermal Energy Project," prepared especially for us by his staff. This refined some of their earlier estimates and reached the following conclusions:

- The properties of granite (which we assumed would be the usual HDR reservoir rock) made it a good candidate for hydraulic fracturing.
- Limestones with mechanical properties very similar to those of granite had been successfully fractured at depths of 12,000 to 15,000 ft.
- With a number of assumptions (made necessary by our lack of knowledge concerning details of the expected downhole environment) including a fracturing pressure of 14,000 psi at a depth of 14,000 ft, they presented a table of estimated pumping pressures and fracture dimensions as functions of the apparent viscosity of the fracturing fluid, its injection rate, and the volume of fluid injected. For example, using water (viscosity = 1 centipoise) with a friction reducer and an injection rate of 100 bbl/min (4200 gpm) they estimated that the pumping pressure for fracture extension would be about 9000 psi and that 1550 bbl (65,100 gal) of fluid would be required to open the fracture at the wellbore sufficiently to accept 20- to 40-mesh

propping particles. Further, they estimated that, under these pumping conditions, when the injected volume reached 12,000 bbl (504,000 gal) the fracture would be 1500 ft long, 3600 ft high, and—under pumping pressure—0.12 in. wide at the wellbore.

- Their study also included excellent and very useful discussions of fracturing fluids, proppants, well logs, and comments on the possibility of seismic mapping of fractures.

This document was very encouraging and extremely useful to us and was the beginning of a long and cordial relation with the Halliburton organization. It was followed by considerable correspondence between A. B. Waters and me, particularly with regard to the probable desirability of injecting particles to hold the hydraulic fractures open; by visits of a number of Halliburton personnel to Los Alamos and of our staff to their facilities; and by excellent results from contract work by their field crews in our subsequent HDR experiments in the Jemez Mountains and elsewhere.

4.5. The search for an experimental site begins

Since we had no official funding or immediate prospects for any, it was important to us to find a site for an HDR field experiment as close to home as possible. Fortunately the Jemez Mountains just west of Los Alamos have a long history of volcanic activity, the most recent extrusive volcanism there having occurred only about 50,000 to 100,000 years ago. That is very recent in geologic time and rocks in general are poor conductors of heat, implying that a large reservoir of subterranean heat should still exist there at fairly shallow depth. This conclusion was supported by the existence of hot springs and fumaroles in the region, and a report of the recent discovery of a promising hydrothermal system there.

That discovery was along a major fault structure within the Valles Caldera (Fig. 5)—a roughly circular subsidence feature about 16 miles in diameter formed as a result of major volcanic eruptions about 1.1 and 1.4 million years ago. According to a front-page story in the *Los Alamos Monitor* of October 22, 1970, drilling there had encountered high-quality natural steam on October 10 at a depth of 5000 feet. The consulting geologist on site at that time was Richard F. ("Dick") Dondanville, who explained to the *Monitor* reporter the difference between a noncommercial- and a commercial-quality steam well. According to Dick, with the former "you enjoy hanging around the well, taking pictures, and looking at it." With the latter, however, "you just want to grab a few pictures and get the hell out of there." This evidently looked like a commercial-quality well and of course demonstrated that a high-temperature thermal reservoir existed within the caldera at a reasonable drilling depth.

In fact, the major center of recent volcanism and the major terrestrial heat source in the Jemez Mountains are within and under the Valles Caldera. Almost all of the caldera is contained within the Baca Location No. 1, derived from an old Spanish land grant. With an area of about 150 square miles, it was owned by Dunigan Enterprises and the Baca Land and Cattle Company and operated as a working cattle ranch. Hydrothermal activity is particularly common in the western part of the caldera, where at one time there was a spa and resort hotel—the Sulfur Hot Springs Hotel—near the head of Sulfur Creek, which drains the northwestern part of the caldera.

Oddly enough, the first discovery of a high-temperature geothermal reservoir in the caldera occurred when, in 1960, Westates Petroleum Co. drilled a well there in search of oil. Reportedly this occurred because someone at the company's head office saw evidence of a domed structure—Redondo Peak, the "resurgent dome" near the center of the caldera—which he thought might represent a petroleum trap, so he sent out a drilling crew to investigate it. (At that time calderas were not yet well understood, and even a competent volcanologist might not have recognized that the structural high was the result of volcanic activity and so was very unlikely to represent a trap for petroleum) The exploratory well that they drilled may have been that identified as Bond No. 1, in Alamo Canyon—which feeds into Sulfur Canyon from the east. In any case, it was abandoned at a depth of about 3675

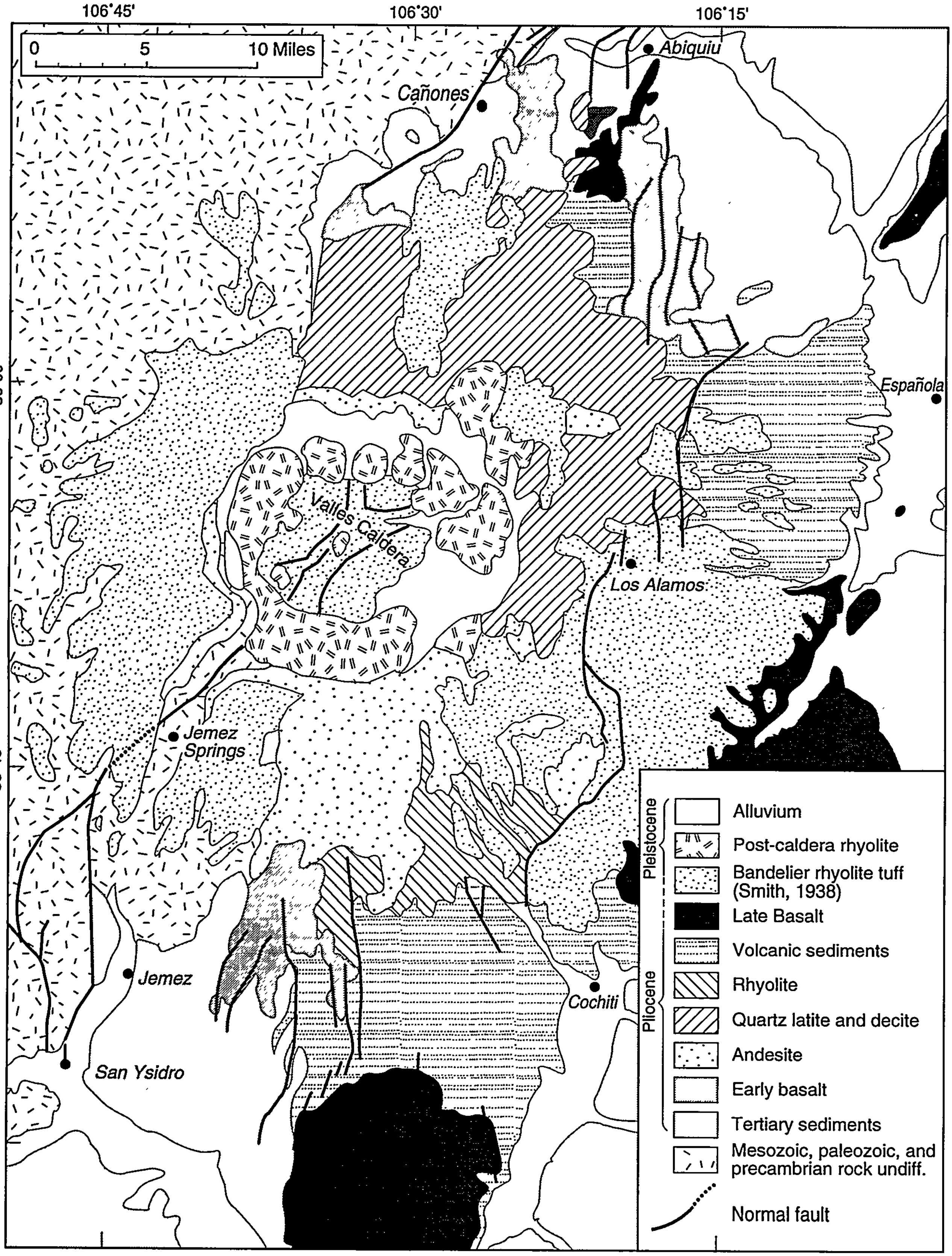

Fig. 5. Generalized geologic map of the Jemez Mountains (Ross, Smith, and Bailey, 1961).

ft because of a continuing flow of steam, said to have been energetic enough to blow the drilling mud out of the hole.

James P. ("Pat") Dunigan and his associates, the principals in Dunigan Enterprises and the Baca Land and Cattle Co., recognized the geothermal energy potential indicated by this unexpected occurrence. Accordingly, in May 1963 they spudded in a geothermal exploration well, Baca No. 1, about one-half mile up the canyon from the abandoned Sulfur Hot Springs Hotel. It is reported to have encountered "dry" (superheated) steam at 460 ft; reached what might have been commercial production of 85,000 lb/hr of 500°F steam in the interval 1300 to 1350 ft; and caved in when the well reached 2650 ft. Of course it was abandoned, but in July 1963 Baca No. 2 was drilled about a mile further up the canyon. It bottomed at 5600 ft in what was reported to be Precambrian granite and reached temperatures measured at up to 500°F. However, it did not produce steam at a rate sufficient to be of commercial interest and was capped. In the meantime, Baca No. 3 had been spudded in June 1963, about 100 ft east of Baca No. 1. It reached a final depth of about 2600 ft and produced mixed steam and hot water from two intervals below about 1800 ft. It was not nearly as good a steam producer as Baca No. 1, but during flow testing it produced enough hot water to overflow the storage pond constructed to contain it. Several earth dams along the creek, hurriedly erected with a bulldozer, were also washed out, and enough sediment was deposited in the hot springs area so that it had to be cleaned out with the bulldozer.

In July 1971, members of our geothermal group met in the morning with Pat Dunigan; Heinrich ("Heinie") Brauer, Pat's on-site ranch manager; and Kenneth E. ("Ken") Brunot, Brauer's brother-in-law, then associated with Robert W. ("Bob") Rex at the University of California at Riverside in developing a deep-geothermal-drilling proposal. In the afternoon, Don Brown and Bob Potter toured the Baca Location with Heinie Brauer and Ken Brunot (and much of the old drilling information summarized above is from Don's trip report on that tour, dated July 15, 1971). In a conversation with Pat Dunigan that afternoon, Don learned that Baca No. 2 was cased at 7 in. diameter to a depth of 3460 ft and that it could probably be made available to us if we were interested in using it in our experimental program. Of course we were interested, particularly since it was reported to have bottomed in granite—a type of basement rock that we thought would be very desirable to contain an HDR heat-extraction system.

However, in their search for a productive steam reservoir, the Dunigan organization had also drilled a number of 100-ft-deep heat-flow holes in Redondo Canyon, the next canyon east of Sulfur Creek. These indicated geothermal gradients high enough to justify drilling a deep exploration hole and, in October 1970, the successful steam well that was described so graphically by Dick Dondanville had been completed there. (Presumably this was well Baca No. 4, originally completed at 5048 ft and later deepened.) According to Joe B. Harrell, Dunigan's engineer and partner in the drilling program, this well was drilled "entirely in fractured volcanics," much to the surprise of their geologist.

The exploratory wells along Sulfur Creek and the productive steam well in Redondo Canyon apparently had attracted the serious interest of the Geothermal Division of Union Oil Co. of California, who were the principal operators of the geothermal steam field at The Geysers in northern California. While we were not aware of it at the time, negotiations between Union and the Dunigan interests must already have been under way at the time of our conversation with Pat Dunigan's group. In any case, later in 1971 Union entered into a "geothermal trade agreement," with Dunigan Enterprises and the Baca Land and Cattle Co. It gave Union exclusive rights to explore for and produce geothermal energy on the Baca Location, to sell it, and to construct and operate generating and transmission facilities for any electricity produced. Our subsequent discussions of possible HDR experiments within the Valles Caldera therefore were principally with Union Oil personnel.

We were also interested in the possibility of an HDR experiment at The Geysers where, in addition to a large number of very productive steam wells, there was a sizeable population of dry holes. Lee Aamodt visited The Geysers in the spring of 1971 and interested Chester F. Budd, Jr., then District Manager of Union's Geothermal Division, in the HDR concept. In a letter dated July 20, 1971, Budd described a Union well at The Geysers (LF State 4236-3) that appeared to meet our requirements for an HDR experiment. It was hot and had no apparent steam production. The well was cased at 9 5/8-in. to 5041 ft, with 8 3/4-in. open hole to 6734 ft, and had been plugged with cement in the interval 4990 to 5599 ft. Measured temperature in the hole increased to about 196°C at 4950 ft—the greatest depth at which a measurement was made—where the geothermal gradient was about 155°C/km and was increasing with depth.

In a letter dated August 10, 1971, Lee invited Carel Otte, Manager of Union Oil's Geothermal Division, to visit us in Los Alamos with some of his people, to discuss the possibility of a joint effort to investigate the HDR concept either at The Geysers or on the Baca Location. Lee enclosed the draft of a paper, "Geothermal Power from Hot, Dry Rock that he was preparing for John Lear, science editor of the *Saturday Review*, who was writing a series of articles on the potential of geothermal energy in the United States. Lee's paper described the geology and volcanic history of the Valles Caldera; a possible two-hole HDR system that might be developed there by hydraulic fracturing or, if necessary, by the use of explosives; and the concepts of buoyant circulation of the heat-extraction fluid and the growth of the system by thermal-stress cracking. He suggested that such a program might be carried out under AEC's Plowshare Program for joint government-industry development of engineering applications of nuclear explosives. As he explained, AEC was interested in investigating the growth of geothermal reservoirs even when no nuclear explosives were involved, in the interest of reducing the yield of the explosive device that might later be used to fracture the rock in a proposed Plowshare geothermal experiment. Lee pointed out that we hoped to eliminate completely the need for nuclear stimulation, but stated that "it may be desirable to use a small nuclear explosive in order to reach full output at an early time." (This probably made the Union people about as nervous as it would have made me if I had known that Lee was suggesting it.)

However, we also prepared a preliminary proposal (which did not mention the use of nuclear explosives) for a joint LASL-Union Oil experiment on the Baca Location. It proposed a hydraulically fractured one-hole heat-extraction experiment in a new well in Sulfur Canyon, adjacent to Baca No. 2. The well would be drilled to 6500 ft, which was expected to penetrate 1500 ft into the Precambrian granite, and cased at 9 5/8-in. to 5400 ft. Then a centrally located dual-string casing 7 in. outside diameter would be inserted through the cemented-in 9 5/8-in. casing, extending down to a point about 200 ft off bottom (Fig. 6). With the bottom 400 ft of the dual string cemented in place, a 500-ft-radius fracture would be made in the open hole below it. The fracture was expected to be vertical and was intended to intersect the open hole above the cement seal. Cool water pumped down the dual string would be heated as it circulated through the fracture and then rise to the surface through the annulus between the dual string and the 9 5/8-in. casing. To minimize heat-transfer between the ascending hot water and the descending cool water, the gap between the two concentric pipes composing the dual string would have been filled with a viscous oil. A one-year program was proposed, to begin in the early spring of 1972.

While there have been many subsequent visits to Los Alamos by Carel Otte and other Union Geothermal personnel, they did not immediately respond to this invitation from Lee or indicate any interest in a joint HDR experiment. Therefore we arranged to visit them in Los Angeles on January 6, 1972. Our party consisted of Gene Robinson, Lee Aamodt William D. ("Bill") Purtymun (a geohydrologist in Group H-8 of the LASL Health Division), Henry Heyman (representing our Legal Department), and me. We met with Carl Otte; Del Pyle, his Operations Manager; John Kilkenny, his Chief Geologist; Dick Dondanville, who had become Union's geologist on their Baca Project; and J. L. Wilson, from the Union legal staff.

As I commonly did in those days, I made the major pitch concerning our HDR Program. (I happen to have saved the outline of that talk which pretty well covered our thinking at the time.) After outlining our HDR concept and explaining our requirements for competent, low-permeability hot rock, I listed the major problems that we foresaw in completing and operating such a system. These were the following:

- Drilling in such rock, hopefully without encountering steam or hot water;
- Producing a hydraulic fracture with a radius of the order of 500 to 1500 ft for an energy extraction experiment, or perhaps 4000 ft for a commercial system;
- Completing the underground connection between two wells;
- The possible need to prop the fractures open with particles to permit fluid flow through them at a reasonably high rate;
- The possibility that repeated hydraulic fracturing might be required to reduce the impedance to fluid flow through the reservoir;
- Maintaining pressure in the reservoir without causing excessive fluid loss;
- Potential problems from corrosion and from dissolution and precipitation of minerals—with the concurrent possibility of selectively dissolving and recovering useful minerals;
- Projected useful lifetime of the system—with the possibility that thermal-stress cracking would extend it significantly.

As the optimum initial program, I proposed that we should drill 12-in. diameter holes and produce a hydraulic fracture with a 1500-ft radius, potentially capable of producing about 150 MWt. Our estimate of time requirements was 1 to 1.5 years for site preparation, drilling, and hydraulic fracturing; about 1 year of system operation to reach the point at which thermal-stress cracking might cause temperature of the produced fluid to begin to increase instead of decreasing; and about 1 more year of operation to investigate scaling and corrosion and permit an estimate to be made of useful system lifetime. I predicted a minimum of 3 years and a probable 5 years to complete this experiment if we were fully funded for it. However, if the funding were piecemeal, I proposed a step-wise program involving (1) small, then large hydraulic fracturing and propping experiments in an existing dry hole somewhere; (2) then a one-hole heat extraction experiment using double-wall casing as described above; and (3) finally, the deep, hot, two-hole experiment that I had outlined.

I assured the Union group that, necessarily, we would remain flexible, but that we intended to pursue the program on any scale that we could; that there was the possibility of federal funding for it; but that we hoped for cooperative programs with industrial organization such as Union Oil Co.

The general reaction of the Union Oil representatives present was that engineering development of a hydraulically fractured HDR system was probably possible but that, even in granite, water from a pressurized system would undoubtedly be lost into natural joints, fractures, and faults. They pointed out that the geology of the Valles Caldera was complex and full of surprises; that the groundwater circulation there was highly interconnected and probably extended down through the granite, which they assumed was highly fractured; and that fluid pressure at depth was subhydrostatic, indicating subsurface drainage out of the caldera into the Rio Grande and its tributaries. Considering all this, Carel was discouraging with regard to the desirability of any site within the Baca Location for an HDR experiment, concluding that our pressurized water would simply leak away into the groundwater. He said that Union would at least consider permitting a LASL experiment there, but that he would not recommend it to his management if it involved a pressurized underground loop—for fear that the water forced out of it would flood potentially valuable steam fields.

Carel pointed out that approval of a LASL experiment on the Baca Location also would be required from the landowners, who undoubtedly would require that LASL assume liability for any damage to the asset represented by potential steam production.

All of this made the Baca Location sound very unattractive as a possible site for our HDR experiment and, for the same reasons, Carel was equally negative about conducting such an experiment in the highly fractured reservoir formation in The Geysers area in California. Further, Union's interactions with the federal government (especially

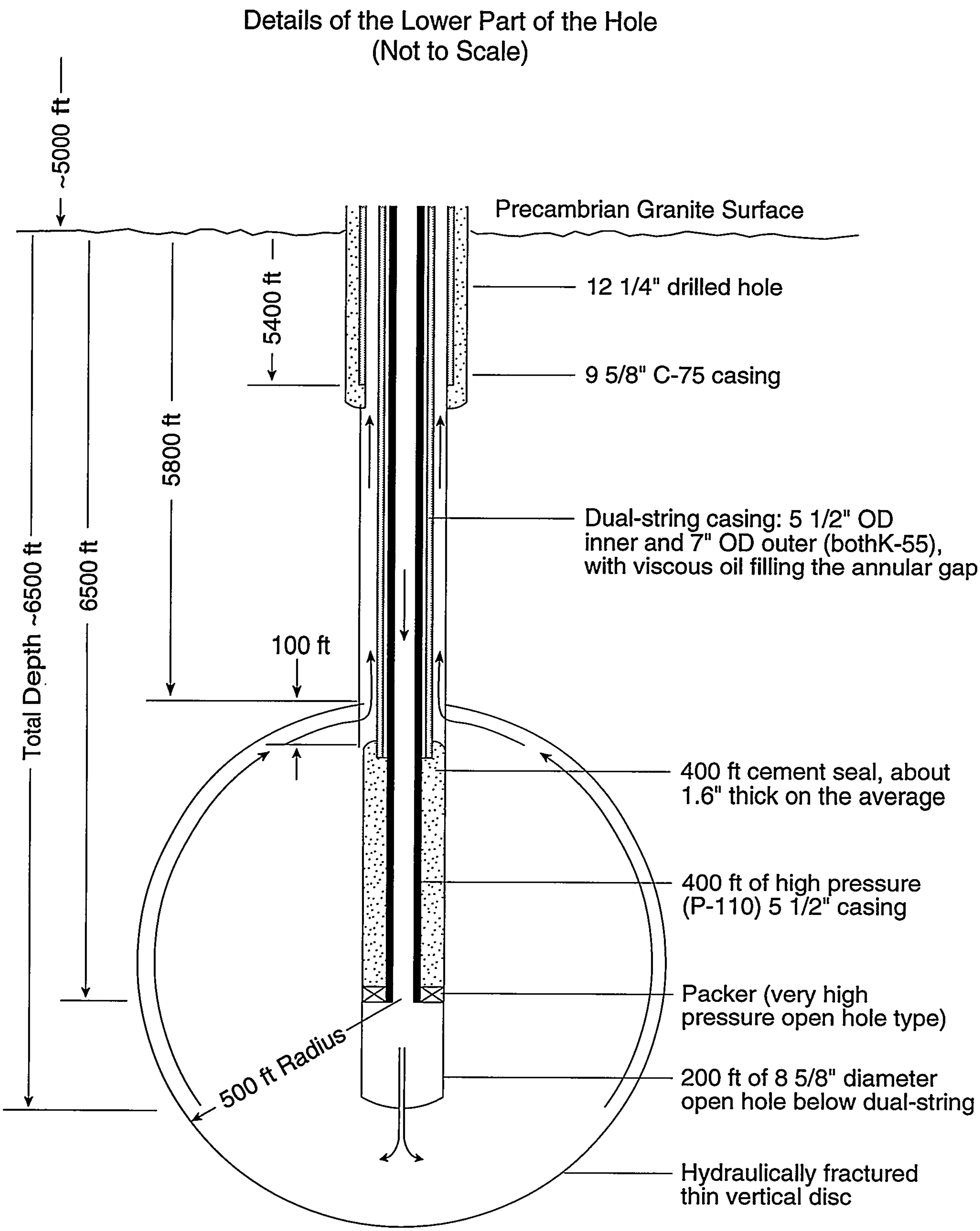

Fig. 6. One-hole circulation loop with concentric pipes proposed for joint LASL-Union Oil experiment on the Baca Location.

in connection with some relatively recent leakage of oil in the Santa Barbara Channel, off the coast of California) had not always been happy ones. As a result, Carel stated flatly that he and his company were not interested in working cooperatively with any U.S. Government Agency, which (although we were actually employed by the University of California, a contractor to the AEC) they considered us to be. That was a fairly emphatic closure to the discussion, but we parted friends.

On January 11, 1972, 1 wrote to Carel thanking him and his people for meeting with us and giving us a great deal of good advice. I noted that we considered the advantages of a pressurized-water loop so important that we would look elsewhere for a location at which we could try to develop one. However, I pointed out that if a pressurized system did prove to be impractical, we could instead draw off steam at subhydrostatic pressures. I also wrote to Pat Dunigan on January 20, described our January 6 meeting with the Union group and Carel's concerns regarding an HDR experiment on the Baca Location, and told him that—at least for a while—we would not be approaching him concerning any possible LASL program there.

Carel Otte, Del Pyle, Dick Dondanville, and others from Union Geothermal have been very helpful to us over the years as technical advisers and—together with Pat Dunigan and his people—in subsequently granting LASL groups access to the Baca Location for geologic, hydrologic, geochemical, geophysical, and environmental studies there. In return, our studies of the caldera region may have been useful to them, and LASL did eventually participate in an experiment designed to increase steam production from one of the Union wells by the use of hydraulic fracturing. In spite of the fact that, to them, we represented the hated Feds, this has been a friendly and useful association of a major industrial organization with a national laboratory.

4.6. The search continues

As an alternative to HDR experiments within the Valles Caldera or at The Geysers, the Union geologists suggested that a suitable location might be found in the granites that rim the Imperial Valley of California—which later was also suggested to us by Robert W. ("Bob") Rex, then at the University of California at Riverside. However, that was a long way from home, even for very preliminary field studies. Therefore, we continued to concentrate on the geothermal area centered in the Valles Caldera.

Fortunately for us, the Valles Caldera was a prime and accessible example of a volcanic caldera and, as such, had been studied intensively for several years by a number of very capable geologists and volcanologists—notably by a group from the U.S. Geological Survey (the USGS). Particularly important to us was an excellent USGS map of the region (Smith, Bailey, and Ross, 1970). Based on that map and the wells that had been drilled on the Baca Location, we initially assumed that the thermal anomaly surrounding this volcanic center would be fairly symmetrical. In that belief, in the Subterrene report we had suggested a possible site for an HDR experiment in a LASL Technical Area on the Pajarito Plateau about 3 miles east of the caldera rim (Fig. 7). However, since the cost and success of such an experiment depended critically on reaching a relatively high temperature in low-permeability rock at a reasonable drilling depth, we were aware that we needed more detailed information on terrestrial heat flow and subterranean geology before we actually picked a site. Don Brown set out to get it for us.

Don learned that Bill Purtymun in LASL Group H-8 had a small, portable, auger-type drilling rig that he used for soil-sampling and hydrologic studies, and Don thought that it could also be used for heat-flow measurements. Therefore he interested both Bill and Bill's group leader, Harry S. Jordan, in our project, and they were extremely helpful. Bill agreed to drill the holes for us, and he and Harry collected and evaluated the available geologic information about the area in and around the Valles Caldera. In a memo to me dated December 9, 1971, Harry described the general geology and hydrology of a particularly interesting area southwest of the Valles Caldera and offered estimates of the thicknesses of the volcanic and sedimentary formations overlying the Precambrian basement at two locations there. One, about 2 miles west of the ring fault bounding the caldera, was north of Fenton Hill (where our first heat-extraction experiments eventually were run). The other was southeast of Fenton Hill about 1 mile south of the ring fault. The descriptions and estimates were excellent and were very useful later when the time came actually to select a site for a heat-extraction experiment.

Aside from the LASL technical areas east of the caldera, most of the land around it was in the Jemez District of the Santa Fe National Forest. Through the AEC's Los Alamos Area Office, we sought and received permission from the Forest Service to drill a series of shallow heat-flow holes within the national forest around the rim of the caldera. In December 1971, with the help of Don Brown, Bill began drilling them. The surface elevation there is generally 8500 to 9000 feet above sea level, and it was usually necessary to shovel away two or three feet of snow before the holes could be spudded in. However, with minimum help from the rest of us and largely on weekends and holidays, during December 1971 Bill and Don managed to drill seven shallow heat-flow holes (described in Appendix A of this report) in the tuff apron around the outside of the caldera rim. Two were in Los Alamos County, east of the caldera; one was south of it near Jemez Falls, and four were west of it. These formed a rough semicircle around the southern rim of the caldera. The holes were kept open for temperature-gradient measurements by poking sections of plastic (PVC) tubing down them, with the sections held together with yellow plastic tape.

Under a series of LASL purchase orders, an arrangement was made for Harold Hartman—then a graduate student at New Mexico Institute of Mining and Technology—to run temperature logs in the holes. He did so during December and January, with good results. The tuff there is well-cemented volcanic ash whose properties were quite uniform at the places where these holes were drilled. Therefore, the temperature gradients near the bottoms of the holes (below the zone affected by summer heat and winter cold) were good indicators of the relative rates of terrestrial heat flow at those locations. In the nature of things, the gradients were least nearest Los Alamos, increased around the southern rim of the caldera, and were very high west of it.

Another useful source of information was the Zia Company, which at that time was responsible for providing and maintaining essentially all of the physical facilities required by both the Laboratory and the city of Los Alamos, including their water supplies. In its search for a domestic water supply, Zia had drilled a number of quite deep holes west of the townsite, and from them had collected a great deal of information concerning the subterranean geology and hydrology. We were, of course, interested in that information and hopeful that some of the holes might still be open so that we could measure temperature gradients in them at depths greater than those of our own shallow heat-flow holes—where the probability of any effect from groundwater circulation might be reduced and gradients might be higher. Accordingly, Don Brown visited Richard C. Crook, then manager of the Zia Company, to inquire about those holes. He and his associates were very helpful, and Don did get a great deal of useful information from them concerning the geology and hydrology of the area between Los Alamos and the eastern rim of the Valles Caldera.

In particular, the Zia folks remembered that one of those deep holes had been drilled in the northwest corner of Los Alamos County, on the slope of a small saddle in the eastern topographic rim of the caldera. They referred us to Edwin E. ("Bud") Wingfield, who then worked in the AEC's Los Alamos Area Office. Bud had been involved in siting the hole and, like so many others, he sprang to our assistance. He remembered about where the hole was and that a casing stub about 18 inches long had been left sticking up above ground, with a cover plate welded on so that people wouldn't drop rocks in it. That sounded promising, and Bud was sure that we could find it. One clear winter day he and Don Brown and I set off in my 1966 Jeep Wagoneer to do so.

The only way to get there was up a mountain road past the Pajarito Mountain ski area, on toward Camp May, then off on a trail that connected with the old Pipeline Road (another trail used during construction of a natural-gas pipeline from the San Juan Basin to Los Alamos). My Jeep had survived roads that were worse than that one, although not by

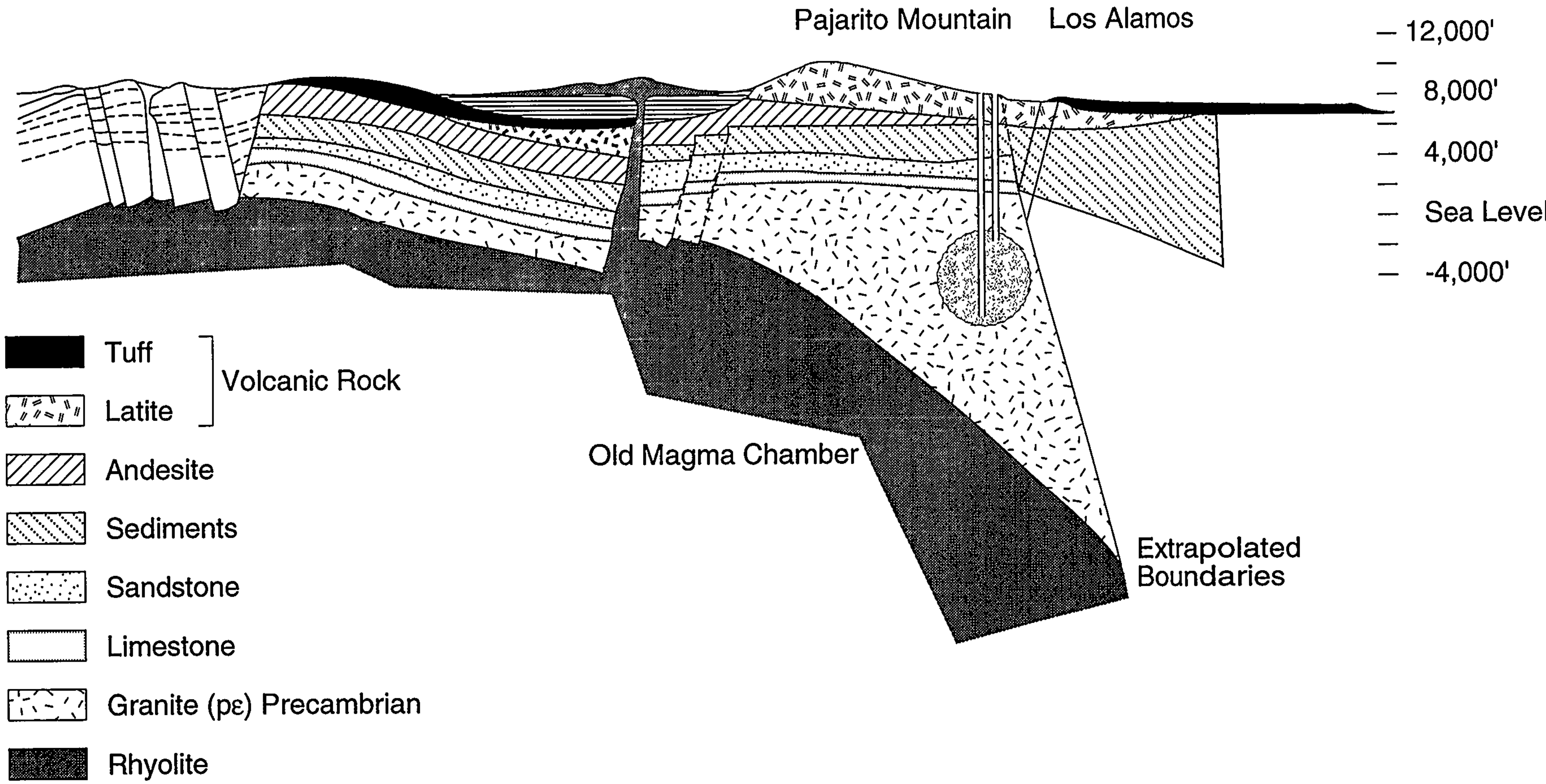

Fig. 7. Geologic section through the Valles Caldera and the surrounding area, indicating a possible site for an HDR geothermal energy development near the town of Los Alamos, New Mexico (Robinson et al., 1971).

much, and the deep snow added to the general interest of the expedition. However, in the low range of four-wheel drive, we were getting along fine until dropping down off a steep hillside, we came to a quiet stop in a little valley—with all four wheels spinning. The snow had drifted in there to a depth of about four feet, and we had tobogganed out on top of it. The weight of the vehicle had of course packed it down considerably, but not enough so that its wheels touched the ground. The majority decision was that (it being my Jeep and my shovel) I should stay and shovel out the Jeep while Bud and Don hiked on another half-mile or so and located that well head.

They hiked, I shovelled, and I got done first, so I turned the Jeep around and then hiked on after them. We kicked around in the snow until we found what must have been the drillers campsite and mudpit, and concluded that Bud had led us to the right place. However, we could not find the well-head. This being a popular area for trail-riding in the summer, Bud concluded that someone must have cut the pipe stub off at ground level so that horses wouldn't trip over it. We never did find that hole, or the others drilled for the same purpose elsewhere in the area. And that was the only time that I ever got that Jeep stuck. It was in a worthy if unsuccessful cause.

Continuing our heat-flow investigation in early February 1972 Bill and Don drilled three more shallow holes outside the western rim of the Valles Caldera, to explore further the area where heat-flow appeared to be especially high. We were also making plans to drill several deeper holes in that area, for better measurements and to help us select a specific location for a very deep hole that would actually penetrate the Precambrian basement. Under another purchase order to NMIMT, temperature logs in the three new holes were run for us by Marshall Reiter and C. L. Edwards.

Among other Zia employees attending Don Brown's conference with Mr. Crook were Roland A. Pettit and Daniel J. ("Dan") Miles. Roland was a geologist and civil engineer and Dan was a petroleum engineer. Both were interested by Don's description of the project and, as volunteers and on their own time, they turned out to help Don and Bill drill those last three shallow holes. Subsequently both joined the HDR Program and were involved with the rest of us in a lot more drilling—and many other project activities.

4.7. Initial involvement of the USGS

One of our greatest bits of good fortune was that the USGS had for several years been studying the geology and hydrology of the Valles Caldera, and had published extensively concerning the area since about 1961. In particular, Robert L. Smith, Roy A. Bailey, and Charles S. Ross had published an excellent geologic map of the area (referenced above). At the time that our HDR Program began, Frank W. Trainer was studying its hydrology and water resources. Lindrith ("Lin") Cordell was doing aeromagnetic and gravity studies of the region. In addition, the USGS had recently formed a "Geothermal Steam Division" based at Menlo Park, California, which included a number of real experts in a wide variety of geothermal studies. Bill Purtymun had been a USGS hydrologist before he came to work at LASL, and was an excellent contact for us with all of its branches.

It was probably Bill that arranged a visit to Los Alamos by Roy Bailey and Lin Cordell on January 11, 1972. We described to them our HDR concept and proposed experimental program, and asked their advice on selection of a site for our first field experiment. As background, they first reviewed for us the general and historical geology of the caldera region, including a great deal of their own still-unpublished data. With additional information on the local geology supplied by Bill and the results of temperature-gradient measurements so far made in our shallow heat-flow holes, we then discussed the relative merits of several possible locations for our HDR experiments.

- There was general agreement with the major conclusion reached in our discussions with Union Oil personnel: that the interior of the caldera was not a promising place for the type of experiments that we had in mind.

- The area near the privately owned Triple H ranch, which we had assumed was outside the south rim of the caldera structure, was probably actually on the southern edge of a ring-fracture zone that bounded the caldera and might be 2 or 3 miles wide. The fractures and breccias of that zone would be expected to extend down into the basement rock, adding permeability to the structural complexity at depth and making this also undesirable as an experimental site.
- The area in the northwestern corner of Los Alamos county that we had originally suggested for the experiments was east of the caldera rim and about one mile west of the Pajarito fault—a major north-trending fault zone on the western edge of the Rio Grande rift valley. We had assumed that a well drilled there would reach competent granite at a reasonable depth, as shown in Fig. 7. However, Roy and Lin pointed out that—as a result of the subsidence that created the Rio Grande Rift—the depth to basement there was actually quite uncertain; that the 25-million-year history of activity along the Pajarito fault zone would probably have affected the basement-rock structure to quite large distances from the main fault; and that there was also evidence from aeromagnetic and gravity data and from examinations of other faults in nearby canyons that a major northwest-trending lithologic discontinuity in the basement rock passed through that area. They concluded that a deep hole drilled there would be of great geologic interest, but that it was probably not a good place for an HDR experiment.
- It was their opinion that our best bet for a suitable location was northwest of the caldera, near Seven Springs. Although there were uncertainties concerning jointing, permeability, and fluid content of the basement rock, the stratigraphy and geologic structure there were relatively simple and the depth to basement was less than 3000 ft. This was excellent advice and was later confirmed by our own field data as we collected it. We subsequently drilled our first deep exploratory hole (well GT-1) in Barley Canyon, about 2 miles east of Seven Springs, and the subterranean geology was essentially as Roy and Lin had described it—and was just what we had hoped for.

In a letter to me dated February 23, 1972, Lin Cordell included a memo prepared jointly by him and Roy Bailey, summarizing our discussions on January 11. It confirmed the conclusions listed above and added more information about the geology and hydrology of the Seven Springs area. They commented favorably on our HDR and Subterrene Programs, and Lin offered to explore the possibility of a cooperative arrangement for seismic and deep-resistivity investigations in the area.

Since then we have had many contacts and interactions with USGS personnel, and this one was typical. They have been very generous in informing and advising and have contributed greatly to the successes of the LASL HDR Program.

4.8. Another drilling-cost estimate

By this time, with the advice given us by Bailey and Cordell and what we had learned from others and from our own investigations, we were quite sure of the general area in which our deep experimental holes should be drilled and of the general geology and stratigraphy we could expect to encounter there. Therefore, on January 20, 1972, Don Brown wrote to John McLaughlin at Fenix and Scisson requesting an estimate of drilling costs for a two-well heat-extraction loop west of the caldera. Don outlined drilling and casing programs for one well about 7500-ft-deep and a second well about 5500-ft-deep, the latter drilled directionally to intersect a vertical fracture with a radius of 1500 ft produced from the lower part of the first well. Don estimated that the holes would penetrate 300 ft of surface volcanics (the Bandelier tuff) and 2200 ft of Paleozoic and Cenozoic sedimentary formations, and would reach the granitic Precambrian basement at a depth of 2500 ft. (All of this turned out to be about right.)

Accompanied by Scott Houghton, John visited us in Los Alamos on February 8 and 9 to discuss the drilling plans. About a month later he sent us time and cost estimates, which we subsequently used in our proposals and budgeting. Total cost was estimated to be $2.3 million which, in 1972 dollars and for the proposed hole depths, was probably a good estimate.

4.9. Manpower estimates

In those days, instead of budgeting and reporting our manpower in "full-time equivalents" (FTEs) we did it in "ceiling points" (CPs)—in reference to employment ceilings established for the Laboratory as a whole and the individual organizations within it. We were charged 1.0 CP for each staff member and 0.5 CP for each hourly employee.

On January 24, 1972, at his request, I reported to Dick Taschek the manpower currently involved in the HDR program and a projection of our needs for the rest of Fiscal Year 1972 (FY72)—which would end on June 30, 1972. It showed, with the designation of the group in which each was employed and the fraction of his time devoted to HDR:

- Gene Robinson, CNC-4, 0.1 CP, and me, CMB-13, 0.5 CP, as program managers;
- Lee Aamodt, J-DO, 0.5 CP; Don Brown, N-7, 0.9 CP; Bob Potter, CNC-4, 0.25 CP; Bill Purtymun, H-8, 0.1 CP; and John Rowley, N-7, 0.1 CP—all as HDR staff members;
- Frank Harlow, T-3, 0.05 CP, and Bill Pracht, T-3, 1.0 CP, for computer modeling.

Thus, I listed a total of 9 staff members, all except Bill Pracht working only part-time on HDR, representing the equivalent of 3.5 full-time employees as our active HDR organization at that time.

For the balance of FY72, I indicated that Robbie's time would increase to 0.2 CP and Bill Purtymun's to 0.25 CP, while Bill Pracht's would decrease to 0. 10 CP—reducing the total to 2.85 CPs. However, I also asked for a half-time secretary (0.25 CP) for the rest of FY72, plus the equivalent of 3.5 CPs borrowed from other groups to help us in geologic investigations, seismic studies, geochemistry, engineering (surveys and plant design), an environmental-impact study, and a cost and economic analysis. This would have brought our total HDR staff to 7.10 CPs for the rest of the fiscal year, but of course we didn't come close to that. However, we really couldn't complain. Our program was still completely unofficial and our salaries were being paid largely by the official programs of the Laboratory groups to which we still belonged. At that time our management could still be very flexible with regard to such arrangements, and it is fortunate for the HDR Program that it could. Within a few years much of that flexibility was lost forever, and then we could probably not have been able to initiate what became an important and productive energy R&D program—which also led directly to development of a broad earth-sciences program at LASL.

5. THE GEOSCIENCES ADVISORY PANEL

In January 1971 the Rover Program cutback was announced, a decision was made to proceed with the Subterrene Program, and background studies and prototype development on it began. Officially, however, the program was unfunded, and an external funding source for it was urgently needed. Recognizing this and the fact that there were many areas in the geologic sciences that were important to Subterrene development and application in which, in 1971, LASL had little or no expertise, John Rowley approached Professor Orson L. Anderson concerning the desirability of forming an advisory panel for the program.

At that time Orson was working at LASL as a consultant, and we had become acquainted with him and enlisted his aid in preparing the original Subterrene report. (Among other things, we had prevailed on him to write an appendix for that report entitled "Research in the Geologic Sciences.") He felt that it was important that such a panel be formed, and he and John promptly began making plans to create one.

5.1. Organization

The direct function of the proposed advisory panel was to be to advise us in such areas as geology, geophysics, geochemistry, and rock mechanics. However, we felt that—with the right choice of panel members—it would also help publicize and develop interest in the Subterrene, lend a degree of scientific respectability to our program to develop it, and open the door to several potential sponsors of such a program. Orson, himself a distinguished member of the geoscience community, knew the right people. One of these was Robert E. ("Bob") Riecker.

Bob Riecker was a geologist-geophysicist at the Air Force Cambridge Research Laboratories, whose special expertise was in rock mechanics. At Orson's suggestion, he visited us at LASL in March 1971. He came to us skeptical but well prepared, having read our Subterrene report and been briefed by Orson. He was probably still skeptical when he returned to Cambridge, but at least he was interested in the program—particularly in the lithofracturing concept and the rock mechanics studies that such a program would need. He did subsequently join the advisory panel.

Through Bob Riecker, B. B. McInteer met Professor John W. Handin of Texas A&M University in September 1971, at a Penrose Conference on "Fracture Mechanics and Earthquake Source Mechanisms" held in Aspen, Colorado. Bob was enthusiastic about John's capabilities in rock mechanics, and B. B. soon came to agree with him. John, in turn, became interested in the Subterrene and HDR Programs as Bob and B. B. described them to him, and subsequently became both a consultant to LASL and another valuable member of our advisory panel

During the following months, Orson and John Rowley, working with Dick Taschek and others, proceeded to assemble a truly distinguished group of scientists willing to serve as members of the panel. Since the HDR Program by then was also informally under way and there were other LASL geoscience interests (particularly those relevant to activities at the Nevada Test Site) the group was called the Geosciences Advisory Panel (abbreviated GAP) instead of a Subterrene or geophysics panel. It was chartered as an advisory body to the LASL director, and its initial members were the following:

Professor Orson L. Anderson, Institute of Geophysics and Planetary Physics, University of California at Los Angeles—Chairman;

Professor Charles L. ("Chuck") Drake, Department of Earth Sciences, Dartmouth College;

Professor Priscilla Dudley, Boston College—Executive Secretary;

Professor John W. Handin, College of Geosciences, Texas A&M University;

Dr. Arthur H. ("Art") Lachenbruch, U.S. Geological Survey;

Dr. Robert E. ("Bob") Riecker, Air Force Cambridge Research Laboratories;

Professor David B. ("Burt") Slemmons, Department of Geology, University of Nevada;

Dr. Richard F. ("Dick") Taschek, Assistant Director for Research, LASL; and

Dr. Hatton S. ("Hat") Yoder, Jr., Geophysical Laboratory, Carnegie Institute of Washington.

5.2. First meeting

The first GAP meeting was held in Los Alamos on February 17–19, 1972, hosted principally by Dick Taschek. Laboratory personnel made presentations on the Subterrene and HDR Programs and, more briefly, on certain other LASL programs involving the geosciences.

In the HDR initial part of the meeting, I made the presentation, outlining the magnitude of the HDR resource base, our proposed method of recovering heat from it, and the problems that we foresaw in such areas as fracture mapping, water loss, corrosion, scaling, and the possibility of triggering earthquakes. By then we had investigated the probable difficulties of drilling hot igneous and metamorphic rocks (through literature surveys and consultations with drilling experts), and we had received the feasibility studies and cost and time analyses prepared by Fenix and Scisson. As a result, we anticipated no major difficulty or unusual expense in producing the hole depths and diameters required for commercial HDR systems, to depths of at least 15,000 ft. Similarly, through literature studies, mathematical analyses, and consultations with rock-mechanics experts and commercial firms (particularly, Halliburton Services and The Western Co., who were experienced in performing the operation), we were convinced that the required hydraulic fractures could be produced with existing equipment and techniques—and at any depth that could currently be reached by conventional drilling methods. (Both we and the drilling and service companies still had a lot to learn about all this.)

I also reported on the unofficial organization and nonexistent funding of the HDR Program and the help that we were getting from individuals in other LASL programs and from persons and organizations outside the Laboratory. Finally, I presented our estimate that, if competent hot granite were found at moderate depth, we could complete our initial HDR field experiment in 5 to 7 years at a cost of about $10 million (in 1972 dollars). Thereafter we hoped for follow-on programs in such areas as construction of larger, deeper systems in a variety of geologic environments and construction of demonstration power and desalination plants. (To anticipate: our time estimate was quite good, but our cost estimate was very low, partly because this was a period of rapid inflation but chiefly because—as will be discussed—we were very naive about the actual state of the art in drilling, well completion, hydraulic fracturing, and downhole instrumentation.)

Frank Harlow then described the computer modeling of an HDR system that he and Bill Pracht were doing. This considered fluid circulation, heat transfer, and the possibility of fracture-system growth by thermal-stress cracking. (As a materials man who had been generally unsuccessful in preventing it in brittle solids, I assured the panel that thermal-stress cracking was certain to occur in the cooled regions of an operating HDR system.)

Bob Potter followed with a discussion of an economic study (really an engineering cost analysis) that he and Don Brown were doing in conjunction with Ken Brunot—then with the Western Projects Office of the National Science Foundation. They had concluded that HDR energy systems could be profitable almost anywhere, specifically including the Conway granites in New Hampshire.

Finally, Don Brown described the prototype two-hole heat-extraction system that we would like to create and operate in order to demonstrate the usefulness of such systems and explore their problems.

In response to our presentations, the panel made a number of recommendation to us, including the following. We should

- secure aerial photographs of our proposed experimental area, particularly for fault-mapping. (Burt Slemmons volunteered to examine and interpret the resulting aerial photographs.)
- arrange for a seismic-reflection survey of the

area, primarily for information on the basement-rock structure.

- at lower priority, arrange for an electrical-resistivity survey and, at very low priority, for an infrared scan. (The panel considered a magnetic survey unnecessary.)
- set up at least three permanent, widely dispersed, conventional seismic stations around the experimental area, and later add additional stations closer-in and on the site itself.
- arrange for hydrologic studies of the area.
- begin looking at environmental issues: liquid, solid, and thermal wastes; seismic hazard; mineral by-products; subsidence; and the general hydrologic cycle. (Drake, Handin, and Slemmons volunteered to look into the seismic hazards.)
- measure *in situ* stress in our first deep hole (presumably by hydraulic fracturing, since no other method was then available).
- consider tagging the water in the circulation loop with a tracer to assess its interaction with the local hydrology.
- set up some kind of an experimental system, such as a laboratory heat-extraction loop, to investigate thermal-stress cracking.
- as soon as possible, hire a fracture-dynamics expert who specialized in rock mechanics.
- hire an experimental petrologist.

The panel's recommendations were excellent and much appreciated. Many of the items were already in our long-range planning, and most of them were subsequently implemented.

In addition to many useful discussions, the meeting included excellent lunches, cocktail parties, dinners, and—on the last day—a tour of the Valles Caldera area. The tour was conducted by Don Brown and Bill Purtymun and attended by all of the panel members except Chuck Drake, who had to get back to Dartmouth.

5.3. Subsequent meetings

With occasional changes in its membership, the Geosciences Advisory Panel continued to meet throughout the years covered by this history, and beyond. Officially, the panel reported to the Laboratory's director, evaluating our activities and making recommendations concerning them. Unfortunately, my records of what they reported to the director are incomplete, although I do have copies of some of their reports. However, as individuals and as a group, they also interacted directly with all of us in the HDR Program, and some of their reports and many of those interactions will be described in the pages that follow.

As individuals, every member of the panel has been extremely helpful to us. As a group, they were usually a blessing, but occasionally they were not. They were a group of very distinguished earth scientists, each with his own special area of expertise, and in general they were much more interested in the geoscience aspects and research possibilities of the HDR Program than in its engineering problems (or those of the Subterrence). Since almost all of us involved in the program also had backgrounds in research, it was often difficult to resist the pressure of the panel to concentrate on science instead of the engineering development to which we were committed and for which we eventually were funded.

6. SPREADING THE WORD

To create an official, funded, Hot Dry Rock Geothermal-Energy Development Program it obviously was necessary for its proponents to gain moral and financial support wherever they could, both within and outside the Laboratory. The first step in this was simply to spread the word as widely as we could concerning what we were convinced was an important new energy concept.

6.1. Within the Laboratory

On November 9, 1971, we made our first presentation to a group of LASL administrators concerning establishment of a Los Alamos HDR Program. As spokesman for what we then called the De-Facto Committee on Geothermal Energy, and accompanied by other members of that committee to help answer questions, I described the HDR concept and its probable advantages and problems. I then listed the major items that we thought would concern an industrial organization that might consider developing a commercial HDR system and which we therefore would certainly have to investigate in field experiments. These were

- the feasibility of drilling and hydraulic fracturing in hot igneous and metamorphic rock;
- the rates of energy extraction that would be practical with a commercial-scale system;
- the economic life of such a system;
- the possible hazards associated with it, such as blowouts, aquifer contamination, surface subsidence, and triggering earthquakes;
- corrosion problems in system components; and
- the rates at which basement-rock minerals dissolve in hot, pressurized water; the plugging and scaling problems that might result from their reprecipitation; and the possibility that some mineral values might be recovered profitably.

We proposed an experimental program that would address such questions and suggested that, when we had the answers, any interested commercial organization would certainly make its own economic analysis. The program centered on constructing and operating a relatively small HDR system so that, in a year or less of heat extraction, we could expect to observe, first, a decrease in temperature of the produced fluid, and thereafter could determine whether or not thermal-stress cracking was causing the system to grow and cause the fluid temperature to begin to increase.

At that time we were still considering a possible experimental site in the northwestern corner of Los Alamos County, north of the Laboratory's S-Site. It was a flat, burned-over area on Forest Service land, near a spring that could be a source of water, accessible by an old forest road, and (we thought at the time) safely distant from any major fault. There we expected to reach granite at about 300°C at a depth of 15,000 ft or less, although we thought that we should drill at least one slim preliminary exploration hole to about that depth to check the geology, hydrology, and our temperature estimate. If these were suitable for our experiment, we proposed to drill an injection well to about 15,000 ft, pump through casing perforations at 13,800 ft to produce a hydraulic fracture with a radius of about 1500 ft, and—if possible—map the crack acoustically. Then we would directionally drill a production well to intersect the fracture at a depth near 12,500 ft. We proposed to circulate water through the system for perhaps 5 years to study its thermal, hydraulic, corrosion, and plugging and scaling behavior.

For construction of this system Fenix and Scisson had estimated a total drilling, casing, and hydraulic-fracturing cost of $3.25 million, including 20% for contingencies, and a total time requirement of just over one year. Halliburton Services foresaw no major difficulty in creating a 1500-ft-radius crack in hot granite by hydraulic fracturing. The Marley Co. had estimated that a forced-draft air-cooled heat-exchanger to dissipate 150 MWt would cost $250,000 FOB Los Alamos. Including road improvements, power and water lines, erection of the heat-exchanger, and a shack and instrument trailer at the site, we estimated that setting up the experiment would cost $4 million and take about 2 years. For this and 5 years of system operation, we assumed a payroll of about 20 LASL personnel at an annual cost of about $1 million. We therefore estimated a total program cost of about $11 million (in 1971 dollars).

At that time we were completing a preproposal summary of the proposed program, and I announced that we would begin circulating it for comment within about two weeks. We were also working on the technical appendices that would make it a full-fledged proposal. Our plea to the Laboratory brass was that LASL support the proposed program and begin doing so by giving it official status and the internal organization needed to develop the program vigorously and sell it to one or more funding agencies.

We had an interested and sympathetic audience, and there were many questions and much discussion. Of course there was no immediate decision with regard to organizing and supporting such a program, but our impression was that that might happen before very long. In the meantime, we continued to took elsewhere within the Laboratory for immediate help.

At that time, the Laboratory's major field operations (principally underground testing of nuclear explosives at the Nevada Test Site) were the responsibility of J Division, under Bill Ogle. The body of our "Preliminary Study" was essentially complete by then, but it was quite brief and nontechnical (i.e., it was a sales pitch). We wanted to beef it up with technical appendices on individual aspects of the proposed method of HDR development and, particularly in the areas of geology, drilling, and seismology, most of LASL's limited expertise existed in J Division. Encouraged by Lee Aamodt (who, officially was still a member of Bill's division-office staff) we decided to try to enlist some of the J division experts both in preparing those appendices and, later, in our field operations. Accordingly, we invited everyone in J Division that was interested to meet with us in the CMR building on February 27, 1972. Those who attended the meeting were broadly representative of the J Division interests and capabilities. They were

- Buford C. ("Carl") Lyon from Group J-1;
- Robert ("Bob") Bradshaw from J-6;
- James H. ("Jim"') Hill from J-7;
- Theodore ("Ted") Crawford, Robert S. Fitshugh, William Frye, and Andrew M. ("Andy") Koonce, all from J-8;
- Robert R. ("Bob") Brownlee, Kenneth H. ("Ken") Olsen, and Robert R. ("Bob") Sharp, Jr., all from J-9; and
- Fred Young from J-14

The entertainment committee included essentially everyone then actively involved in the HDR investigation: Lee Aamodt, Don Brown, Bob Potter, Bill Purtymun, Gene Robinson, and me. We reviewed the HDR concept for our visitors and offered them copies of the draft version of our "Preliminary Study." I explained our need for help in correcting and improving that version and in preparing documented technical appendices for the final version. I pointed out that we had Dick Taschek's blessing to recruit people to help us, although—since we had no money to pay for their support—it would, initially, have to be volunteer help by interested people, probably on a part-time basis, and preferably by individuals who might stick with us when we had money and a big program to run. In particular, at that moment we needed help on sections of the report concerned with geothermal sources in general, the competence of *in situ* basement rock to contain pressurized fluids, drilling experience in hot rock, geochemistry, thermal-stress cracking, heat-exchangers and other surface facilities for an experimental system, power plants for commercial systems, and the direct (nonelectric) uses of geothermal heat. We had nothing started on seismology or environmental concerns, and we were going to need help in such areas as obtaining permits, doing site surveys, geophysical studies, improving access roads, preparing an environmental statement, and drilling and logging deep holes.

This was a particularly stimulating and productive meeting with a group of very helpful experts in a variety of disciplines. It included penetrating questions, many of which we couldn't answer; good suggestions and advice; occasional skepticism, and some direct and undoubtedly wholesome criticism. Sooner or later, several of the people present became directly involved in the HDR Program. Ken Olsen was immediately helpful in seismology. Bob Bradshaw permitted Francis G. West, from his group, to help us in geology, hydrology, and geophysics, and Francis subsequently wrote the appendix on "Regional Geology and Geophysics" for our HDR report. Jim Hill soon joined our group; Bob Brownlee later became our Division leader, Fred Young got us involved with the Navajo Nation; and over the years we received a great deal of help from other J Division personnel. Bill Ogle ran a very tight ship and was not anxious to get deeply involved in an unfunded program. However, he had allowed Lee Aamodt to work with us and those mentioned above to help us get the program started. Bill always felt that he was directly responsible for making the HDR Program possible, and—together with a few others like Dick Taschek, Dick Baker and Rod Spence—he certainly played an important part in it. However, when the program finally became official and received serious funding, Bill decided that it really belonged in J Division. I resisted, successfully. It was about the only battle that I ever won from him and if I had known him as well then as I came to a few years later, I might instead have jumped at the chance. His division had a great deal of the expertise that we needed plus extensive experience in large field experiments, in which we were entirely lacking. However I was a little afraid of both Bill and his management style and was very comfortable in CMB Division under my old friend Dick Baker. Therefore, we stayed in CMB Division for a while, until a new LASL energy division was formed around our Hot Dry Rock Program.

6.2. Outside the Laboratory

In December 1971, Don Brown and Bill Purtymun were out in the snow on the western flank of the Valles Caldera, drilling shallow heat-flow holes to determine the trend of geothermal gradients there and as a basis for locating a few deeper heat-flow holes. We were waiting for their results, for the Fennix and Scisson cost and time estimates for constructing a deep circulation loop in that area, for some preliminary reservoir modeling, and for several technical appendices for our initial HDR proposal. We hoped to get all that together by the end of March have the proposal ready to circulate by April 1, and then begin a hard sell for funding on May 1. In the meantime we undertook a preliminary soft sell—spreading the word on our concept and plans as widely as we could.

One prompt and useful result of our presentation to the LASL administrators in November 1971 was an article in the December 1971 issue of *The Atom,* a slick-paper general-interest publication by the Laboratory that was distributed quite widely outside as well as within the Lab. Titled "Geothermal Energy for Electric Power?" It summarized the information that we had presented at that November meeting, and included the following very appropriate paragraph.

> "The merits of a geothermal energy system of this type are uncertain at this point. The advantages of such systems for production of electrical power are more easily predicted than the disadvantages, and only an experiment such as proposed by the LASL scientists can provide the information necessary to weigh one against the other."

This characterized our situation quite accurately. We were sure that the HDR idea was important and optimistic that we could make it work. However, we were also aware that it offered many potential problems about which we knew very little.

This *Atom* article was the first real publication concerning our HDR Program and it aroused a great deal of interest. One of the interested people was a Mr. Kornegay, who apparently reported to the local press that we were preparing to build a geothermal power plant in western Los Alamos County, and suggested that the Los Alamos County Council formally take a position in favor of doing so. I don't have a copy of the newspaper that reported this but apparently someone on the council had read it, and the Council invited me to tell them about it at their meeting on the evening of December 13, 1971.

It happened that I had already been invited to talk about HDR at a meeting of the LASL Scientific Smorgasbord on the afternoon of December 13. This was an informal assembly of interested LASL staff members that met to hear about and discuss whatever was new around the lab. I outlined for them the HDR concept and our proposed field experiment to test and develop it, and pointed out that—with regard to actually building an HDR power plant—Mr. Kornegay had jumped the gun by at least 5 years.

That evening I addressed the County Council in the same vein, of course with fewer technical details. I assured them that we welcomed their interest and moral support and that of Mr. Kornegay and everyone else concerned with the problems of energy supply and environmental pollution.

However, I said that—"like motherhood and apple pie,"—our objectives seemed not to require that a formal position be taken by the Council. I suggested that, if our experiments were successful, we might be back in five years or so to talk about roads and power lines. That was obviously soon enough for the County Council who thanked me and went on about their more immediate business.

Among the many ways in which they helped us launch our HDR Program, Harold Agnew, Dick Taschek, and others often arranged opportunities for us to brainwash visitors to the Laboratory who were here for quite different purposes. An example was a visit on January 28, 1972, by Richard L ("Dick") Garwin, from the Thomas J. Watson Research Center, who was a consultant to our national defense programs, a distinguished scientist, and a widely known public figure. He had prepared himself by reading the geothermal appendix of our Subterrene report, and he kept six of us very busy for an hour trying to answer some very discerning questions about the HDR concept and its potential usefulness. (Our panel consisted of Lee Aamodt, Don Brown, Frank Harlow, Bob Potter, John Rowley, and me.) Garwin's concerns were much like our own: the technical uncertainties concerning the creation and operation of a pressurized underground heat-extraction loop; the geochemistry of the system, including possible scaling and corrosion problems; end uses, including direct-heat applications, generating electricity, and combinations of the two; and the economics of such systems. He found Frank Harlow's computer simulations reassuring, as we all did, and reacted favorably both to the HDR concept and to our proposed experimental approach to investigating and developing it. He concluded the session by telling us "That is a very exciting idea." Of course we found this heartening, particularly because the parting comment occasionally heard from others was something like, "You'll never make it work."

Another example was an opportunity to brief Representative Mike McCormick, from the State of Washington, on HDR during his visit to LASL in June 1972. He was an influential member of energy-related committees in the U.S. House of Representatives. Fortunately he liked our ideas and, together with our own New Mexico Congressional delegation, he became an important contact for us in Washington.

All of us involved in the HDR Program took every possible opportunity to beat the drum, and there were others around the Laboratory who did the same for us. A particularly enthusiastic and exciting drumbeater was the late James ("Jim") Tuck. Jim had been a British member of the original Manhattan Project team. He later returned to LASL and, among many other things, initiated the fusion-reactor program (appropriately, "The Sherwood Project") here. However, he was interested in and a contributor to almost everything novel that appeared around the Laboratory, and of course we got him interested in the Subterrene and HDR Programs. He spread the word for us in many important places. For instance, on July 21, 1971, LASL was visited by three members of the New Mexico Public Service Commission: Richard P. Montoya, its chairman, Morris H. Yashvin, and J. C. Hester. Together with John Rowley, Jim (who was really there to talk about fusion energy) seized the opportunity to educate them concerning HDR and its potential importance to New Mexico. This was the first attempt to interest a state agency and the electrical-power industry in the idea. It was a rather typical performance. Whenever someone associated with HDR got a chance to do so, he would buttonhole a visitor and deliver the HDR prospectus, no matter who the visitor was or why he was here. LASL has always had a great many visitors from almost everywhere, and many of them left Los Alamos with badly frayed buttonholes.

Ever since it was opened to the public, Los Alamos has been a mecca for newspaper, magazine, radio, and television reporters and science writers in general. One of the science writers, John Petty from the Reuter news agency, visited the Laboratory in early December 1971, was intrigued by our description of the HDR idea, and wrote an excellent article about it that was distributed worldwide. The first publication of it that I saw was in the December 25, 1971, issue of the Des Moines, Iowa, *Register*—which I considered a fine Christmas gift for everyone. A second copy, clipped from the January 25, 1972, Djakarta, Indonesia, *Times*, was mailed to me later by Dallas Dale Fowler, who was then Project Manager for a geothermal exploration project on the Dieng Plateau in central Java, sponsored by USAID (the United States Agency for International Development). Dallas had been a classmate of mine in college and we had been out of touch with each other for years until he saw my name in Mr. Petty's article. It was an interesting coincidence that he was already an expert in a field that I was just entering, and encouraging that he liked our HDR idea.

Another excellent article on HDR, by John Noble Wilford, appeared in the *New York Times* on June 21, 1972. It was based on the first technical paper on HDR that we presented at a national meeting (described below) and a subsequent interview with me in Los Alamos. Mr. Wilford's article was titled "New Plan Is Outlined for Tapping Geothermal Energy," and a condensed version of it was also circulated widely by the *New York Times* News Service. I first saw the abbreviated version in the *Grand Rapids Press* of June 21, 1972, headlined "Earth's Heat May Yield Cheap Power."

Two short but quite similar articles, also based on our presentation at that meeting, appeared in the *Engineering News Record* of June 29, 1972, one headlined "Power from the Earth" and the other "Dry geothermal areas have huge power potential."

The word was getting around.

6.3. The Navajos

Our first direct "international" contact concerning HDR was with the Navajo Nation on January 27, 1971 (a couple of months before we had even formally proposed an HDR Program to the LASL Director). It was arranged by Fred Young (who subsequently resumed his Navajo name of Fred Begay)—a Ph.D. theoretical physicist in LASL group J-14. Fred has always been much concerned about the many problems facing his people on the Navajo Reservation, and he has tried, repeatedly and with some success, to bring LASL's expertise and resources to bear on several of them. At that time he had arranged for representatives of the Navajo Tribal Council to visit Los Alamos and explore that possibility.

Among the urgent needs of many Navajo communities are heat, electrical power, and potable water—all of which could be provided by successful HDR developments (the last probably by distillation of brackish water, which is plentiful in some populated areas on the Reservation). Fred had probably first learned of our HDR Program by reading the article in the December 1971 issue of *The Atom* and evidently he recognized the possible usefulness of such systems on the Reservation. In any case, he included them in the subjects to be discussed at this meeting.

As I recall, Bob Potter and I met with Fred and two tribal council representatives, whose names, unfortunately, I recorded only as "Hubbard" and "Shorty." We outlined the HDR concept to them, but about all we could say about it was that we hoped within a few years to demonstrate that the idea worked, and that there many areas on the Reservation that appeared promising for its application. Of particular interest was an area northeast of Flagstaff, Arizona, where volcanic activity at Sunset Crater had occurred only about 900 years ago. However, we foresaw the possibility of small HDR developments scattered around the reservation, each providing electrical power locally—with the waste heat from the power plant used for heating, air conditioning, and distillation of brackish water. All of this, of course, was at best just a possibility for the future and offered no immediate help to the Navajos.

Largely because of Fred's efforts, interactions between the Laboratory and the Navajo Nation have continued and increased through the years, and an interest in development of HDR systems has been maintained. (For example, on August 27, 1973, Bob Potter briefed Peter McDonald, the chairman of the Navajo Nation, on HDR.) At this writing there still are no HDR energy systems on the Navajo Reservation, although some day they may be there. In the meantime, the geological, geophysical, and hydrologic studies subsequently done on Navajo lands in the course of our resource assessments may have been of some value to the nation.

6.4. The Rex connection

In 1971, Robert W. ("Bob") Rex was assistant director of the Institute of Geophysics and Planetary Physics at the University of California at Riverside (UCR), and director of its Geothermal Resources Program. I am not sure how our first meeting with him was arranged, but suppose that it resulted from a commitment of LASL funds to UCR, concerning which I know very little. Some light is thrown on the subject by a memo dated August 18, 1972, from

Robert J. ("Bob") Van Gemert—then head of the LASL Supply and Property Department—to H. Jack Blackwell, at that time manager of the AEC's Los Alamos Area Office (LAAO). According to that memo, on September 8, 1971, LAAO had authorized LASL to expend approximately $38,500 for salaries, fringe benefits, subsistence, and travel by UCR personnel, for work not described in the memorandum. It was probably the availability of those funds that made Bob Rex's visit to LASL possible, and it was probably Ken Brunot—then working with Bob and his staff on a deep-geothermal-drilling proposal—who interested Bob in our HDR Program

In any case, Bob Rex visited us in Los Alamos on November 18, 1971. He impressed me as a very bright, knowledgeable, articulate, earth-scientist, and he gave us a lot of useful information about geothermal energy in general. He recognized the potential of our approach to HDR energy development, thought it would work, and indicated that he would like to be associated with it—all of which added to our opinion of his wisdom. However, he was also somewhat glib and did try to brainwash us in a few areas in which we knew more than he did, so we came to treat him with both respect and caution.

Bob was (and continued to be) an entrepreneur, and it became apparent that he would be happy to direct our program—although there was no obvious way in which he could arrange to do so. Therefore, he suggested instead a joint UCR-LASL proposal naming him principal investigator for a study of HDR prospects in the granites that rim the Imperial Valley of California. The LASL principal investigator would manage the development of HDR technology at Los Alamos, which eventually would be applied at a site that Bob would select. (Of course, no such arrangement materialized, although some interesting related possibilities have appeared from time to time.) In the meantime, Bob was willing to work with us as a consultant and to accompany us on our anticipated visits to Washington, when he was available to do so. I was sure that he would be very useful in talking to potential sponsors in Washington or anywhere else. He came on a little strong, but he knew a lot and made a fine impression.

It was at this meeting that Bob suggested the "huff-puff" method of recovering heat from an HDR thermal reservoir, which would involve alternate injection of cool water and production of hot water from the same well. He also suggested that dissolved minerals in our recirculated heat extraction fluid could probably be removed by adding to it a very fine clay mineral, on which silica and most other dissolved solids would be expected to adsorb; then removing the particles in a centrifugal separator. Finally, he opined that the principal corrosive agents in an HDR system would be hydrogen sulfide and ammonia; that corrosion would be mild; and that existing oil-field types of equipment could handle the conditions in the system that we foresaw.

Evidently, Bob was genuinely interested in the HDR concept. He discussed it at some length in two subsequent publications of the UCR Institute of Geophysics and Planetary Physics: "Geothermal Energy in the United States," Contribution 72-9, March 1972; and "Geothermal Energy—Its Potential Role in the National Energy Picture," Contribution 72-10. April 1972. I don't know how widely these documents were circulated, but they were the first technical reports from an authoritative source outside our own Laboratory to discuss the magnitude of the HDR resource and the probable technical and economic feasibility of recovering useful thermal energy from it. In them Bob pointed out that "Almost all of the enormous heat content of the earth's interior is present in dry rock." He described our concept for creating and operating an HDR energy system and his own preference for a pulsed "huff-puff" system. Among other things, he recommended "Expansion of the hot rock concept tests at the Los Alamos Scientific Laboratory. " Of course we were pleased by all this, although we still preferred the idea of a two hole heat-extraction system with continuous fluid circulation.

After his visit to Los Alamos in November 1971, Bob somehow arranged to use part of the LASL funding to support a study of the economics of HDR energy by himself and David J. ("Dave") Howell, a young economist in his group who specialized in forecasting payoffs from geothermal energy developments. That study began with a visit to Los Alamos on April 20, 1972, by Bob and Dave, during which we reviewed for them our progress to that point and gave them such cost information as we so far had collected. Bob, of course, gave us his view on the direction that our continuing efforts should follow.

In the meantime, however much of the LASL funding had gone to the UCR Geothermal Resources Program, it evidently wasn't enough. In a telephone conversation on April 24, Bob told me that three of his top scientists were available for employment elsewhere. He had, in fact, organized a very strong geothermal-energy group, and we certainly could have used some of its members if we had been able to pay their salaries. At that time we had no money to do so, as I told Bob. However, among many other things, Bob was an aggressive politician. He indicated that he had been in touch with Gerald W. ("Jerry") Johnson, who was soon to become director of a new Division of Applied Technology at AEC Headquarters. Jerry had told him that we were not asking for enough money to support our Geothermal and Subterrene Programs; we should be requesting $5 million for each of them for the first year, with an increase of $1 million per year thereafter for each program. (Jerry never told me that!) Presumably it was the prospect of funding of that magnitude that made Bob think that we could afford to add more people to our nonexistent payroll.

Evidently Bob kept in close and profitable touch with Jerry. In a letter to John Rowley dated July 3, 1972, Bob reported that his group's work on their geothermal and Subterrene studies for us was nearly complete; and that limited additional support for July and August would permit its final completion; and that Jerry had agreed to make $8000 available to LASL for that purpose. This resulted in some confusion. According to the August 18 memo from Bob Van Gemert to Jack Blackwell, John Rowley had written to Van Gemert on June 19 stating that no further expenditures would be required in connection with that contract with UCR. Nevertheless, on July 7 someone at AEC Headquarters called Dick Taschek and, among other things, told him that $8000 of Plowshare funds were indeed available to continue support of Bob Rex's work. Of course, LANL had to spend it as directed, so, on July 10, Glen A. Graves (then Dick Taschek's assistant director for research) sent a memo to Edward R. ("Ed") Laymen in our Supply and Property Department requesting that $8000 of Plowshare funds be committed as soon as possible to support Bob's work during the period July and August 1972. (Among the many things about this arrangement that I will never understand is the fact that this $8000 somehow brought our commitment to UCR up to only $37,000 compared to the $38,500 that had already been authorized.) However, UCR requested a new purchase order to cover that additional time, and I was directed on July 12 to write the necessary purchase request. I did, and was belatedly in the game—although I didn't know either the rules or the score.

Donald W. ("Don") Bryson telegraphed the good news of this additional funding to UCR on July 13, and the actual purchase order was written and sent to LAAO for approval on July 31. That was when and where things hit the fan. The work described was obviously covered by the original purchase order for $38,500; John Rowley had reported that the work had already been completed; and now LASL was attempting to spend another $8000 on it! Fortunately, Bob Van Gemert was a great negotiator and justifier, who bailed us out repeatedly, and somehow he did it again on this occasion. The order was finally approved and issued on September 6. Since it covered the period July and August, that was a little late. In fact, however, we had to modify it on September 29 to extend it through September, and again on November 3 to extend it through November and December (fortunately, at no additional cost to LASL). We received the final reports on both the HDR economic study and the Subterrene work (with which I am not familiar) in January 1973.

The final UCR report by David J. Howell and Robert W. Rex, was titled "The Economics of Hot Dry Rock Geothermal Energy Development," dated December 30, 1972, and identified as Institute of Geophysics and Planetary Physics, University of California, Riverside, Contribution 72-41. It was primarily an essay on where the costs of a geothermal development occurred and how, for a high-risk venture, they were treated for tax purposes and on a corporation's books. Apparently the main body of the text had been prepared originally as a treatise on the economics of natural steam and hot-water geothermal systems and was based largely on experience at The Geysers development in northern California—then the only commercial geothermal operation in the United States that was producing electrical power. With regard to HDR, it considered only the development of high-temperature systems for generating electricity, using a binary cycle and either continuous-flow or pulsed fluid circulation. It concluded that in the western and midcontinent (but not the eastern) United States, the busbar cost of generating electricity by such HDR systems would be slightly less than that from burning coal. Considering our—and their—state of knowledge of the techniques and actual costs of developing and operating an HDR system, it was a good report, and of course we were pleased by its conclusions.

To anticipate by about one year: by December 1973, Bob Rex—having left UCR and served briefly as exploration manager for Pacific Energy Corporation—had formed and become president of Republic Geothermal, Inc. He telephoned me on December 3 to say that he had talked to James C. ("Jim") Bresee at AEC Headquarters about updating and extending that economic study, and Jim had referred him to me. Bob felt that the original study had been done too hastily [!] and that new and better cost information was then available so that an improved version of it was possible. He proposed a one-year study to cost us $60,000. I told him that we recognized the desirability of such a study although I was not sure that we could afford to pay for it at that time, and that I would welcome a letter from him outlining what he had in mind. His letter to me, dated December 20, 1973, proposed a $115,450 study lasting 12 to 14 months, and included the following noteworthy statement: "Furthermore, it is my contention that the economic and political incentives for hot, dry rock are so large that a national crash program in this area is warranted. We envision economic justification for a $200 million per year ten-year program as being a sound economic investment for the U.S." (While I have occasionally been accused of thinking too big concerning HDR, I was not in the same league as Bob Rex!)

By that time we did have a budget, but there was nothing in it to support another economic study; we were already overcommitted for that fiscal year, we were pessimistic about funding prospects for the next one; and we were not sure that Republic Geothermal was the best organization available to do such a study in the first place. Accordingly, with the concurrence of LASL management, I wrote a noncommittal letter to Bob explaining our situation and postponing any such study indefinitely—with a copy to Jim Bresee. That particular study was never undertaken, although of course we were to see more of Bob Rex in the future—and usually much to our benefit.

Although Bob Rex had left UCR, we did see more of it and others of its faculty. Dean Stahrl Edmunds of the UCR Graduate School of Administration visited LASL in early March 1973 and discussed with Dick Taschek and others the possibility of joint research efforts with LASL on energy problems, including geothermal energy. Edmunds suggested a visit by LASL personnel to discuss this possibility with UCR staff and determine where there were common interests. On April 23 and 24 several of us from LASL did visit UCR, and were cordially received. I made short presentations on our various geothermal activities at LASL, toured the UCR geology laboratories and geothermal research facilities, and enjoyed discussions with Wilfred Elders, Seymour Schlanger, James Combs, and Tyler Coplen—all of whom were involved in geothermal studies in the Imperial Valley. Nothing specific came of this meeting immediately except for plans for future discussions of cooperative projects. However, for me, this was a first and valuable contact with another group of real experts on geothermal energy.

6.5. The AEC

In the meantime, the United States Congress had directed the Atomic Energy Commission to assume new responsibility for R&D related to all aspects of nonnuclear as well as nuclear energy supply, conversion, distribution, and storage, and the AEC was preparing to do so. A reorganization of AEC operating functions to accomplish this was announced on December 7, 1971, which was another bit of good fortune for us. Recognizing the opportunity for support of our HDR Program that this new AEC mission presented, Dick Taschek included a two-page section on "Exploitation of Dry Geothermal Energy Reservoirs" in a report to the AEC on LASL Research and Development Activities, dated November 23, 1971 (thus anticipating the public announcement by a couple of weeks). That section included short discussions of the nature, magnitude, and availability of the HDR geothermal resource, a description of the LASL concept for developing and utilizing it, and a fairly optimistic comparison (prepared by Bob Potter) of the estimated plant and fuel costs for coal-fired, nuclear, and HDR power plants. The cost comparison concluded that a relatively small (40-MW electrical) HDR power plant could produce electricity at lower cost than either coal-fired or nuclear plants of much larger capacity (e.g., 940 MWe).

Dick's report suggested that the AEC could now appropriately undertake an investigation of the HDR concept. He proposed a 7-year program in which the first 2 years and $4 million would be spent on drilling, hydraulic fracturing, and construction of surface facilities at the first HDR experimental site; and the next 5 years and $5 million on calculational and experimental studies of the nature and behavior of the underground system and on contract monitoring, application of HDR systems to uses other than generating electricity, and selection of other promising sites for development of HDR energy systems.

So far as we were concerned, the timing of all this couldn't have been better. The U.S. was becoming aware of the limitations of its conventional energy supplies and the environmental problems that resulted from their exploitation; the AEC had been directed to do something to overcome those limitations; and the AEC was now informed that one of its major laboratories was ready to undertake the development of a new, essentially inexhaustible, environmentally benign, domestic energy supply. We were probably the first in line for AEC support of an alternative energy project, but we knew that we couldn't just sit still and wait for good things to happen. We had to help make them happen, and we proceeded to do so.

7. POSSIBLE EXPERIMENTS AND SOME CONCLUSIONS

Throughout the history of the HDR Program, many suggestions have been made concerning experiments that we should run and alternative methods that we should try in developing and operating HDR energy systems. Some of these have already been mentioned, and more will be in the pages that follow. However, we have always been constrained by the realities of funding, manpower, time, programmatic directives, and the perversities of nature, inanimate objects, and much of mankind. There have always been many things that we wanted to try but couldn't, for one reason or another.

7.1. The original concept

In an appendix to the original Subterrene report (Robinson et al., 1971), we considered only one type of heat-extraction system: a recirculating pressurized-water loop between two boreholes connected through low-permeability hot rock by hydraulic fractures and cracks subsequently opened to fluid circulation by thermal-stress cracking. This system geometry remained the principal focus of our attention. For example, in my talk to the LASL Scientific Smorgasbord on December 13, 1971, I described the experimental system that we were then just preparing to propose formally. It involved a single hydraulic fracture with a radius of about 1500 ft, which we believed would be small enough so that, in about 10 months, there would be sufficient cooling of the fracture surfaces so that a significant amount of thermal-stress cracking would have occurred.

As the materials man on the original Subterrene Committee, I was very familiar with thermal-stress cracking, and was convinced that it was certain to occur during prolonged heat extraction from an HDR reservoir. This type of failure results when a brittle solid is cooled locally, as would occur when a cool fluid is circulated through an opening, in hot rock. Thermal contraction of the cooled surface layers of the rock would be restrained by the uncooled rock around it, developing tensile stress in the cooled material which, as cooling continued, would increase in intensity and slowly overcome the compressive stress field typical of the subterranean environment. With sufficient cooling the induced stress would finally become great enough to break the cooled layer in tension, creating cracks radiating outward from the original cooled surface. This would increase the area of rock in contact with the heat-extraction fluid circulated through the reservoir, increasing the useful lifetime of the reservoir. The greater the amount of cooling, the greater the extent of thermal-stress cracking, so that the fracture system would tend to grow preferentially into regions where the initial rock temperature was highest—outward, but especially downward, in the direction of increasing temperature. In modeling this behavior, Frank Harlow and Bill Pracht concluded that, during operation of a heat-extraction loop, the temperature of the produced fluid would at first increase while the surroundings of the production well warmed up and the rate of radial heat loss from it diminished. It would then decrease as a result of cooling of the surfaces of the original hydraulic fracture. Eventually, however, it would begin to increase again as fluid circulation was established through the new thermal-stress cracks extending into rock that had been cooled less extensively. Finally it would rise above the temperature maximum observed in the earliest stage of fluid circulation as the crack system continued to grow downward into rock at still higher temperatures (Fig. 8).

It was this transition, from decreasing to increasing temperature of the produced fluid that we hoped would occur after about 10 months of circulation through a relatively small system.

7.2. Drilling by cracking

Lee Aamodt, in particular, was intrigued by the possibilities of thermal-stress cracking, and suggested a novel application of it: "drilling by cracking." This concept was based on the conclusion that, if a hole drilled in hot rock was cooled at the bottom, thermal-stress cracking would extend it preferentially downward into progressively hotter rock.

In a memorandum to Frank Harlow dated July 28, 1971, Lee inquired concerning the possibility of simulating mathematically a type of one-hole heat-extraction experiment that might be attempted at

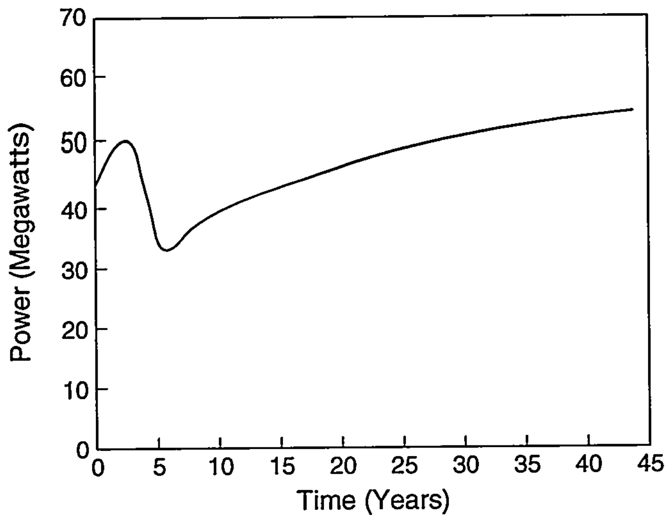

Fig. 8. Thermal power vs time for a large HDR system at constant fluid-circulation rate (Harlow and Pracht, 1972).

The Geysers in the dry well described by Chester Budd. Lee's suggestion was to drill out the existing cement plug in the casing, clean out the hole, leave some water in the open hole section below the casing, then seal off the open-hole section by means of a packer installed just above the bottom of the casing. Refluxing in the steam-water system below the casing would then transport heat from the bottom of the open-hole section upward to the packer, where the temperature would be measured by a transducer penetrating the packer. An increase in steam temperature just below the packer would indicate that thermal-stress cracking had made it possible for the water to circulate through hotter rock at greater depth. Lee thought that it might be possible to remove heat from the reflux system, perhaps by means of heat pipes penetrating the packer and then transporting the heat to a condenser at the earth's surface. He also suggested adding a pressure transducer to supplement the temperature information, and the possibility of later extending the thermal-stress cracks by hydraulic fracturing.

So far as I know, no system of this type was ever modeled by Frank Harlow or anyone else, and such an experiment was not actually proposed to Union Oil. However, something like it would be interesting to try, perhaps with some modifications and add-ons. Since no rock is literally impermeable, it would probably require provision for adding makeup water to the uncased region below the packer—to compensate for permeation loss to the surrounding rock. Heat-removal from the top of the packer would certainly be necessary, and a stack of heat pipes to bring the heat to the surface is not inconceivable. In a very hot hole of moderate depth, such a system would offer the possibility of experimenting with gas-fracturing by the steam pressure developed below the packer and, at greater depth, with circulation of a supercritical fluid.

While Lee did not push this idea as a practical heat-production system, it has interesting possibilities

7.3. Huff-puff operation

As has been mentioned, in his meeting with us in Los Alamos in November 1971, Bob Rex suggested "huff-puff," or "pulsed," operation of a one-hole HDR heat-extraction system. He continued to emphasize it in subsequent discussions and publications, and we have always been interested in it, although probably in modified form and primarily in a somewhat different application than Bob was considering.

As Bob described it, a huff-puff system would be constructed by drilling a single hole, casing it nearly to bottom, and making a large set of hydraulic fractures extending into the rock surrounding the open-hole section below the casing. In operation, cool water would be pumped down into the reservoir under sufficient pressure to inflate the fractures; it would be held in the reservoir for a sufficient period to reach essentially the temperature of the rock around it; and it would then be permitted to return spontaneously to the surface through the same hole, driven by the compressive stresses stored in the rock and the water during pumping plus the increment of pressure developed by the fluid as it was warmed and tried to expand. From a single well, hot water or steam would of course be produced only intermittently. However, by using perhaps three wells in different stages of the injection-storage-venting cycle, continuous production of heat could be achieved. This could probably be managed in such a way that, after its useful heat had been removed, water produced from one well would be injected directly into another well, reducing pumping costs and the need for water storage at the surface and conserving whatever low-temperature heat remained in the fluid.

Such a system would have several advantages. Drilling costs would of course be less for a one-hole than for a two-hole system, and the uncertainty concerning making a good hydraulic connection at depth between the two wells would be avoided. So also would be the possibility of short-circuiting of cool fluid through preferred flow paths between the two wells; huff-puff operation would inflate and extract heat from the entire fracture system. Aside from the fact that heat production would be intermittent if only one well were used, the principal disadvantage of such a system is that alternate injection of cool fluid and production of hot fluid through the same well would create cyclic thermal stresses that could result in premature failure of production tubing, well casing, cement, and any downhole seals or hardware. Further, cooling of the well and its immediate surroundings while cool water was being injected would result in a significant reduction in both temperature and heat content of the produced fluid as it flowed back up and rewarmed the well.

To avoid these difficulties, our Los Alamos group concluded that a huff-puff operation would better be conducted in a two-hole system such as those previously described. In this geometry, cool fluid would always be injected through the same well, with the production well shut in; and hot fluid would always be recovered through the production well, with the injection well shut in. The heat produced could of course be used for any purpose that its temperature would satisfy, but a particularly interesting possibility is the use of such a system for storage of energy. For example, when customer demand fell below the base-load generating capacity of a power plant, the relatively inexpensive surplus electrical power would be used to pump water into the fractured reservoir. Then during peak-demand periods, the pressurized fluid would be vented through the production well, to drive hydraulic turbines. This would recover much of the energy previously expended in inflating the reservoir, with at least some of the system's inefficiencies compensated by the pressure increase that would result from warming of the fluid while it was stored in the reservoir. Further, if the heat in the produced fluid could also be used beneficially—for example, in preheating boiler water or in generating electricity in an associated binary-cycle power plant—it is quite possible that more useful energy would be produced than was originally used to inflate the reservoir. (This is not perpetual motion, since the increment of energy would be the heat extracted from the reservoir rock.)

<u>7.4. Other system designs</u>

The hydraulically fractured two-hole continuous-circulation loop discussed in earlier sections was designed specifically for development and operation in hot rock of very low initial permeability, in which loss of the pressurized heat-extraction fluid (and of the heat it had extracted from the thermal reservoir) would be minimal. For geologic environments in which that is not the case, a wide variety of other system designs and operating modes should be useful.

For example, if there is excessive loss of fluid from a pressurized-water loop, pressure in the reservoir can be reduced to less than the natural pore pressure of fluid in the surrounding rock by installing and operating a downhole pump in the production well. By reducing fluid pressure at the bottom of that well, this would create a pressure gradient through the fractured reservoir that would cause water to flow to the production well instead of outward into the rock around the reservoir. Alternatively, fluid loss could be reduced by drilling one or more additional production wells appropriately located relative to the geometry of the fracture system. Hydraulic fractures grow preferentially in a plane normal to the least compressive earth stress, which, at depth, is generally horizontal. The fracture system created therefore tends to be approximately vertical and, in its horizontal dimensions, long and narrow. Two production wells drilled into the fracture system, along its strike and on opposite sides of the injection well, would reduce overall flow-impedance and fluid loss and would also make available for heat-extraction a much larger fraction of the fracture surfaces. In the extreme case, where the reservoir is naturally permeable in two or three dimensions, hydraulic fracturing might not be necessary. Each injection well could be surrounded by four production wells, in the "five-spot" pattern commonly used in water-drive systems for recovery of petroleum. Finally, of course, steam instead of pressurized water could be produced, as is commonly done in exploiting natural hydrothermal systems.

These and other possible system geometries, construction methods, and operating modes should make it possible to recover thermal energy from HDR reservoirs at almost any location in which there is a reasonably high geothermal gradient. We hoped eventually to investigate many of them in field experiments in a wide variety of geological environments.

7.5. Some conclusions as of December 1971

Before we printed and circulated our initial sales document (a "Preliminary Study of the Development of Dry Geothermal Reservoirs") we had spent a great deal of time discussing and deciding what we should propose with regard to an initial HDR field experiment. Our conclusions at that time were summarized in a memorandum to Dick Taschek dated December 28, 1971, from "The De Facto GTE Committee." It considered the following points.

7.5.1. System geometry. For several reasons, we had concluded that the major HDR thermal reservoirs at usefully high temperatures would be found principally in the crystalline basement. Any overlying loose sediments and sedimentary or volcanic formations would, in many cases, be sufficiently permeable to permit active groundwater circulation, which would tend to keep them cool. Their presence, however, would not necessarily be a disadvantage. Since such formations in general have thermal conductivities significantly lower than those of typical basement rocks, they would (if not cooled too efficiently by abnormally rapid groundwater circulation) serve as a useful insulating blanket over the basement. This would reduce heat loss from the top of the basement formation and increase temperature there, reducing the depth to which it would be necessary to drill in order to reach any required rock temperature.

Having decided that, regardless of the presence or absence of a significant cover of sediments or volcanics, future commercial HDR energy systems would probably be developed principally in low-permeability basement rock, we felt that our initial field experiments should be conducted in that type of rock. Since most commercial applications would require continuous production of heat at a fairly consistent temperature, we felt that continuous circulation through hydraulic fractures between two wells was the most promising type of system for development in the basement rock. We did mention in our memo to Taschek that, if funds for our initial experiment were very limited, a useful and relatively inexpensive initial experiment could be done in a single hole by simply pumping cold water down an insulated pipe in the center of the hole and recovering hot water through the annulus around it. [By creating hydraulic fractures around the lower part of the hole to increase heat-transfer area and closing the annulus with a packer near the bottom of the injection tubing, this system could also be used for a huff-puff experiment.] However, we concluded that the advantages of a two-hole circulation loop were such that only it should be proposed for our initial field experiment.

7.5.2. Site location. Because the Jemez Mountains just west of Los Alamos were now known to contain a large, relatively shallow geothermal area with high geothermal gradients and moderate depth to basement, we had decided that there was no need to look elsewhere for our initial experimental site. By this time we had explored the possibility of using old wells or drilling new ones within the Valles Caldera and had abandoned that idea both because of the disturbed geology there and because the current geothermal leaseholder would not permit us to try our preferred type of experiment there. Our best bet appeared to be near but outside of the caldera. Accordingly, at that time we were still proposing a possible location near the Laboratory's S-Site, in northwestern Los Alamos County and just outside the eastern topographic rim of the caldera. It had the advantages of convenience, accessibility, and available services. However, this area was within the Rio Grande Rift, where depth to basement was relatively great (estimated there to be 10,000 to 15,000 ft—which, however, we noted might be typical of other parts of the western U.S.). Further, although the location was between the edge of the caldera and a major north-south trending fault (the Pajarito fault), we at that time believed that it was sufficiently remote from both so that the basement rock there would be competent granite. On the other hand, we had just completed temperature—gradient measurements in five shallow heat-flow holes around the southern rim of the caldera and had found that the gradient increased with increasing distance from Los Alamos. The available stratigraphic information indicated that depth to basement decreased continuously along the same semicircular path. The most remote area so far investigated, just outside the western rim of the caldera, might then be the most desirable place for our initial experiment. However, we did not yet know enough about it to be sure of that, so we merely qualified our selection of the location in Los Alamos County by calling it a proposed site and noting that more-detailed geological studies and heat-flow measurements were needed to confirm its suitability for an HDR experiment.

__7.5.3. Seismic studies.__ We were interested in seismic activity at the experimental site on two scales of intensity. First, we were aware that the fluid injections needed to create large hydraulic fractures might trigger moderate-to-large earthquakes. (This had occurred in the Denver area when, at the Rocky Mountain Arsenal, liquid wastes were disposed of by pumping them at high pressure down a deep well into an active fault system [Evans, 1966].) Second, we were also aware that hydraulic fractures had been mapped in waste-disposal studies at Oak Ridge National Laboratory by determining the source locations of microseismic signals (acoustic emissions) generated by the series of small fracturing events by means of which the fractures grew (McClain, 1971).

Fortunately for us, LASL Group H-8 and the LASL Engineering Department had already undertaken development of a conventional seismic observation system, primarily to monitor any large earth movements that might damage Laboratory structures. Ordinarily, such events occur along existing fault systems where, since the last time stress was relieved by a shearing movement (an earthquake), stress has again built up to the point at which an additional displacement occurs spontaneously. Our discussions with Harry Jordan of H-8 and Bob Brasier of ENG-7, found a common interest that helped in the collection of good data on the seismic background and fault structure of the Jemez area before HDR experiments were to begin there.

Among other things, our memo to Taschek suggested that, with this LASL seismic facility as a nucleus, our hydraulic-fracturing experiments "could grow into a very important program directed toward earthquake control." Here the idea was that small fluid injections at intervals along a dangerous fault would trigger a series of harmless small earthquakes that would reduce the stress sufficiently so that a damaging large event would not occur.

Acoustic mapping at Oak Ridge had been done with surface instruments over a relatively shallow fracture that was produced by injecting a cement slurry. Only a small number of microseismic events were recorded whose source locations could be determined, so that the resulting fracture map left much to be desired However, it demonstrated that the technique was promising, although we recognized the necessity for much further development if it were to satisfy our need for accurate maps of hydraulic fractures created in hot rock at the much greater depths that would be reached in developing HDR energy systems.

7.5.4. Financial and other support. In this memorandum to Dick Taschek, we noted that, to that point in time, the salaries and expenses of our HDR activity had largely been absorbed by other, existing Laboratory programs principally in LASL Groups CMB-13, CNC-4, N-7, T-3, H-8, and J-DO. However, we promised that the serious pursuit of external funding would begin in January 1972.

We also acknowledged the direct assistance that we had received from the AEC's Los Alamos Area Office, the Zia Company, Los Alamos County, the U.S. Forest Service, New Mexico Institute of Mining and Technology, Fenix and Scisson, Halliburton Services, and the Marley Co.

8. LOOKING FOR AN ANGEL

As advertised, our serious pursuit of external funding for the LASL HDR Program began in January 1972, initially with a media blitz of sorts.

8.1 Distribution of the promotional literature

By that time we had completed our initial sales document, a "Preliminary Study of the Development of Dry Geothermal Reservoirs." In it we suggested no alternative to development of a two-hole, hydraulically fractured circulation loop. For our initial field experiment, we proposed a "relatively small" hydraulic-fracture radius of 1500 ft, so that the expected positive effects of thermal-stress cracking could be observed after only a few months of system operation. We assumed that the fracture would be created by injecting fluid through perforations in the well casing, and that it would then be propped open with particles. And we continued to describe the location in northwestern Los Alamos County as a possible site for our first field experiment, although we cautioned that a more detailed investigation of it was needed before a final site selection could be made.

We sent copies of this document to almost everyone we could think of who might be influential or directly helpful in launching an HDR Program at Los Alamos, among them the following individuals—most of whom will reappear later in this narrative.

- Senators Clinton P. Anderson and Joseph M. Montoya of New Mexico, Alan Bible of Nevada, and Mike Gravel of Alaska; and Representatives Manuel Lujan and Harold L. Runnels of New Mexico. (The New Mexico Congressional delegation, in particular, has always been very supportive of the HDR Program and, at least once, Manuel Lujan saved it from extinction.)
- William M. Magruder, Special Consultant to the President.
- Edward E. David, Jr., science advisor to the President and director of OST, the White House Office of Science and Technology. (He later wrote to us to say that our idea and report were timely, since OST was currently heading a study of energy R&D goals. He had provided copies of our report to Dallas Peck of the USGS, who was responsible for the geothermal-energy part of the study—and who later was very helpful to us in a variety of ways.)
- William L. (Bill") Butcher of OST. (Together with his very efficient secretary, Bill had made it possible for Gene Robinson, Bob Potter, John Rowley, and me to prepare and submit a Subterrene proposal for the President's Technical Initiatives Program in less than one day. The four of us from Los Alamos were in Washington trying to sell the Subterrene project when Bill told us about the Technical Initiatives Program, and that the final date for submission of proposals for it was the next day. We wrote a Subterrene proposal overnight in a hotel room, Bill's secretary typed it for us the next morning in his office in the Old Executive Office Building, and we submitted it to Bill. It actually turned out pretty well, and we've always been grateful to him and to his cheerful and extremely competent secretary.)
- John J. Flaherty, the AEC's assistant general manager for energy and development programs (who later was instrumental in arranging for the first direct funding of our HDR Program by the AEC).
- Spofford G. English, the AEC's assistant general manager for research.
- George A. Kolstad, assistant director of the AEC's Division of Physical Research. (George was an old friend of many of us at LASL who, with John Flaherty, later arranged for our first official funding. George asked us for 25 or 30 copies of our "Preliminary Study" for distribution to an Interagency Geophysics Discussion Group and the AEC Geophysics Working Group. I happily sent him 30 copies, and George introduced our HDR concept and the geophysical studies that its development would involve to many members of the scientific community within the AEC and other federal

agencies.)

- Donald I. ("Don") Gale of the Division of Military Application of AEC (who had helped us arrange our hasty submission of a Subterrene proposal to Bill Butcher at OST).
- Richard ("Dick") Hamburger, then with the Plowshare Program in the AEC's Division of Peaceful Nuclear Explosives. (Dick had arranged for us to present the Subterrene pitch to the Interagency Committee on Excavation Technology and, with Don Gale, had helped arrange for our Subterrene proposal to OST. In addition to sending him a copy of our HDR study, John Rowley had written to him in the hope that some Plowshare funds might be siphoned off to support HDR. In his reply to John, Dick said that at that time [February 2, 1972] there were no uncommitted Plowshare funds. He also relayed some legitimate Headquarters concerns about the HDR concept. Principally, these were "uncertainty that hydraulic fracturing will produce the extent or orientation of cracks as postulated," and "uncertainty that thermal fracturing will propagate or expose additional heat transfer surface to the extent postulated." However, Dick said that the Headquarters people with whom he had discussed it felt that "the concept does offer sufficient possibility of success that it should be pursued somewhat further." This, of course was encouraging, particularly from a representative of the Plowshare Program—whose own HDR concept was still being pursued actively. In fact, Dick also later joined a new AEC division that became our primary sponsor, and helped us in many ways in developing and promoting the HDR Program.)
- Marcy Williamson (referred to me by Dick Hamburger as the man at AEC Headquarters principally involved in keeping track of geothermal activities and developments).
- Anthony H. ("Tony") Ewing, also in the AEC's Division of Peaceful Nuclear Explosives. (Tony was deeply involved in the Plowshare geothermal program but he was also very helpful to us. Among other things, he helped us to arrange a meeting with Dr. Denton at NSF, and he later transferred to the AEC Division that became our major source of funding.)
- Jesse C. Denton of the Office of Interdisciplinary Research of NSF, the National Science Foundation (whom we later visited in Washington).
- Roy Bailey and Lin Cordell of the USGS.
- Donald E. ("Don") White, also of the USGS. (Don was a pioneer in the study of geothermal energy in the United States and, like Roy and Lin, was a continuing source of information and good advice to our HDR Program.)
- George C. Kennedy of the Institute of Geophysics of the University of California at Los Angeles. (Kennedy was one of the early proponents of the Plowshare geothermal concept. In responding to our study, he stated frankly that he didn't think that our HDR scheme would work. He did not believe that rocks would "spall and fracture" as we suggested. Further, he saw no merit in the heat-exchanger loop that we described; the hot water produced could instead be flashed directly to steam to drive a turbine. Finally, he did not believe that we could recover any minerals of commercial value with such a circulation loop. These comments from an acknowledged expert of course added to our already considerable worries.)
- Bob Rex at the University of California, Riverside.
- Orson L. Anderson and all other members of our Geosciences Advisory Panel.
- Carel Otte at Union Oil Company.
- Pat Dunigan of the Baca Land and Cattle Co.
- John C. McLaughlin and Scott Houghton at Fenix and Scisson, Inc.
- A. B. Waters and John Tinsley of Halliburton Services.

This initial distribution list was, of course, only the beginning. As we encountered or heard of other people that were (or we thought should be) interested in HDR energy systems, we sent them copies too. This included additional AEC and USGS personnel and representatives of other federal and state agencies, oil-field service companies, the faculties of several additional universities, power companies, potential industrial users, and a wide variety of casually or seriously interested companies and individuals.

8.2 Making the rounds in Washington

Since financial support of LASL came almost entirely from the AEC and we already had many contacts (and a few real friends) at AEC Headquarters, we of course hoped that we could persuade it to sponsor our HDR Program. However, there were other federal agencies that we thought should be interested in such a program and, as the opportunities arose, we also touched base with them.

8.2.1. First the AEC. With reorganization of the AEC announced on December 7, 1971, a new Division of Applied Technology (DAT) was formed, whose principal mission was to investigate and encourage development of nonnuclear energy sources other than the fossil fuels. John J. Flaherty, assistant general manager for energy and development programs, served as its acting director until Jerry Johnson was appointed director in May 1972.

I don't how our initial direct contact with John Flaherty occurred, although it may well have been through Harold Agnew or Dick Taschek. In any case, the timing was perfect. John was looking for activities in the energy area appropriate to the new DAT mission, and we were trying to sell one that we were convinced would be a winner. Fortunately, from the information that he had already received, John liked our HDR idea. Unfortunately, at that time DAT had no budget to support our proposed program—or any other. However, John of course knew people at AEC Headquarters who might have some slack in their budgets and, as an assistant general manager, he was in an excellent position to encourage them to help us out. His first step was to have Jack Vanderryn, his technical assistant, arrange a meeting in Washington at which we could present the HDR story to a select group of Headquarters personnel. The meeting occurred on February 28, 1972, and our audience included

- John Flaherty;
- Jack Vanderryn;
- George Kolstad from the Division of Physical Research (DPR) who was already well informed concerning our HDR concept and our initial efforts to investigate it;
- Jim Coleman, also from DPR, who had worked at LASL with Robbie and his group in the early 1960s on the initial development of the rock-melting penetrator;
- John Kelly, director of the Division of Peaceful Nuclear Explosives (the Plowshare Program) to whom we had previously sent a copy of our "Preliminary Study;"
- Several members of the AEC Geophysics Working Group, who had been informed about our concept by George Kolstad and who may already have read our "Preliminary Study."

As usual on such occasions, I led off with a discussion of the HDR geothermal energy resource and its nature, distribution, and magnitude; our concept for developing and utilizing it; and some problems that we foresaw in doing so and which required experimental investigation in the field. Lee Aamodt then described the modeling done by Frank Harlow and Bill Pracht, which was encouraging with regard to the probability of effective heat recovery by circulation of pressurized water through large hydraulic fractures. Don Brown discussed the experimental program that we were proposing to investigate and develop HDR heat-extraction systems and explore their problems. Finally, Bob Potter presented a preliminary engineering estimate of the economics of a commercial-scale HDR system, which suggested that in many places it could be competitive with conventional energy sources.

Our audience was interested, and there were many questions and good discussions. We were encouraged concerning the possibility that the AEC might support the HDR Program financially, but of course at that time were given no assurance that that would actually occur.

8.2.2 Then some other federal agencies.

Since we were already in Washington and were primed to brainwash anyone that would listen, we spent the next day celebrating leap year (the date was February 29, 1972) by visiting some other agencies that we thought should be interested in our HDR Program.

First we visited ARPA, the Advanced Research Projects Agency of the Department of Defense. There we presented our ideas to Donald H. ("Don") Clements, emphasizing our belief that HDR systems would be ideal as energy supplies for military bases, particularly those in remote areas. Don gave us very little encouragement, although, very indirectly, we learned later that (probably through his effort) the HDR concept was subsequently considered in detail by the military at at least one rather large technical meeting (to which we were not invited).

Then we visited the Division of Advanced Technology Applications of the National Science Foundation, where we briefed Jesse C. Denton and some of his staff. Although they were interested, our reception was rather cool—perhaps because (as we learned later) NSF felt that it rather than the AEC should be the lead agency in geothermal energy R&D. In fact, for a while it was the lead agency.

Finally we visited the USGS, where we were received cordially by some real and very helpful experts on geothermal energy. There we met with Dallas L. Peck, L. J. Patrick ("Pat") Muffler, and Richard S. ("Dick") Fiske. Dallas and Pat, in particular, later joined panels organized to review our HDR Program as it developed, and they were among our most valued advisors.

8.3 We propose to DPR.

Just what happened at AEC Headquarters after our visit there on February 28, 1972, I do not know. However, fortunately for us, it happened very quickly, and undoubtedly it was instigated by John Flaherty.

In those days the federal government's fiscal year began on July 1, so that by the end of February all federal agencies had already submitted final detailed budget requests for the next fiscal year—in this case, FY73. As of February 28, HDR was not mentioned in any of them. Nevertheless, on March 3, 1972, George Kolstad telephoned from Washington and invited us to submit three proposals to DPR (the AEC Division of Physical Research) for FY73 funding of about $50,000 per proposal. They were to be in the areas of geophysics research, chemistry research, and metallurgy and materials research, each to be related to, but not part of our geothermal energy program. The proposals were to be discussed with him over the telephone on March 6, and later submitted formally on AEC Form 189 (about which, more later). We hastily outlined three proposals that we hoped would fill the bill: one each in the areas of terrestrial heat flow (geophysics), geochemistry (chemistry), and rock mechanics (materials). As directed, Dick Taschek called George on March 6 and discussed these proposals with him. George felt that the heat-flow and rock-mechanics studies were appropriate for support by his Physics and Mathematics Branch of DPR, and the geochemistry study by A. R. Van Dyken's Chemistry Branch. At Dick's request, on March 8 we supplied him with formal titles for the proposals, which he telephoned to George. Then we put the proposals themselves into what we naively supposed would be their final form.

Much of our support during that period—including my own time, that of Georgia Courtney, our excellent CMB-13 group secretary, and our office space and furnishings—was made available to us by Dick Baker, the CMB division leader. Accordingly, our proposals were submitted on April 5, 1972, by me to Dick Baker, by him to Dick Taschek, by Taschek to Harold Agnew, and by Harold on April 17 to Paul W. McDaniel, then director of DPR. They were later revised repeatedly, but in their original form they proposed the following activities.

"Heat Flow Study of a Potential Geothermal Energy Source." Dated March 28, 1972, this proposal listed R. D. Baker as the person in charge, with M. C. Smith and D. W. Brown as principal investigators. Manpower costs were "to be funded by LASL," but $52,000 were requested for materials, services, and subcontracts. The proposed research included heat-flow measurements in 100-

ft-deep holes in the Bandelier tuff to delineate the geothermal area west of the Valles Caldera, then in 600-ft holes into the Paleozoic sediments, for better measurements in the more promising parts of the areas, and finally in one hole about 3000-ft-deep drilled into the Precambrian basement and cored at intervals to investigate stratigraphy and hydrology as well as heat flow. Temperature logging and thermal-conductivity measurements on the cores were to be made under contract from us by Marshall Reiter's heat-flow group at New Mexico Institute of Mining and Technology (NMIMT).

"Chemical Equilibrium and Materials Transport in Systems Involving Pressurized Superheated Water and Silicate Minerals." Dated March 28, 1972, this again listed R. D. Baker as the person in charge, with M. C. Smith and John P. Balagna as principal investigators. It proposed preliminary static studies ("bomb tests") of the interactions of superheated water with the complex mineral systems represented by crystalline igneous rocks, as functions of elevated temperature and pressure. These were intended both to produce data on the reactions that occurred and their rates and to provide information for subsequent design of a dynamic system that would involve fluid circulation. The total funding sought was $49,700, most of which was to be spent on high-pressure bomb tests at other laboratories already equipped for such experiments and on petrographic studies of tested and untested rock samples by a LASL consultant.

"Fracture Dynamics of Hydraulic Fracturing and Thermal Stress Cracking in Crystalline Igneous Rocks." Also dated March 28, 1972, this again listed R. D. Baker as the person in charge, with M. C. Smith and J. C. Rowley as principal investigators. Total funding of $49,100 was proposed, to be used principally to support consultants specializing in the fracture dynamics of rocks—who were to prepare a summary of the state of knowledge of rock mechanics as it applied to hydraulic fracturing and thermal-stress cracking. A hydraulic-fracturing experiment in the hot granitic section of the deep hole produced in the heat-flow study was also included in this proposal together with an assessment of the type and magnitude of the rock-mechanics capability that should eventually be developed at LASL.

An obvious problem in developing an underground heat-extraction loop by the proposed method is that of locating the hydraulic fractures made from one borehole so that a second hole can in fact be directionally drilled to intersect them. Fortunately, in 1970, William C. McClain at Oak Ridge National Laboratory had demonstrated that seismic methods could be used to locate the sources of acoustic signals generated by fracturing events underground, and thus to provide a means of mapping the fractures themselves. However, this was done with a surface array of four seismometers, the fractures produced were in shale at a depth of only about 1000 ft, and only 11 fracturing events were recorded with sufficient information so that their source locations could be mapped in three dimensions. Nevertheless, this general technique looked like our best bet for acquiring the information needed to design a drilling trajectory for a second well that would indeed intersect the hydraulic fractures. However, it was evident that, to produce an accurate map of hydraulic fractures created in hot basement rock at depths of several thousand feet, much further development of the method would be needed. Accordingly, we prepared and submitted to DPR the following additional unsolicited fourth proposal.

"Seismic Studies Related to Artificial Geothermal Energy Sources." Dated May 10, 1972, this proposal listed R. D. Baker as the person in charge, with M. C. Smith and R. M. Potter as principal investigators. It proposed that salaries for this research be funded by LASL, but $60,000 were requested for operating costs plus $20,000 for capital equipment. It was intended in FY73 to develop both surface and downhole seismometer arrays and to use them first to determine the seismic background of the site selected for the initial field experiments; then to detect and record the seismic signals generated by hydraulic-fracturing experiments in the deep borehole drilled there; and finally to detect immediately any significant earthquakes that might be triggered by the fluid injections.

These four proposals were intended to support work (and to some extent the people who would do the work) that would provide background information needed for a major HDR heat-extraction experiment, but without addressing that experiment directly. They were prepared in haste and without the opportunity for adequate discussion with DPR, and there were obvious deficiencies in all of them. For example, they all showed Dick Baker as the person in charge, no matter where in the Laboratory the work would actually be done, and they all showed me as one of the principal investigators, whether I knew anything about the subject of the proposal or not. (This was because I did most of the proposal writing, had to put down somebody's name, and also wanted to be personally involved in whatever finally materialized.) Obviously, revisions were in order, and over time there were plenty of them. As is described later, this took a lot of our time and had some embarrassing fallout. However, on March 3, 1972, we were assured by George Kolstad that we would indeed get about $150,000 to fund the three initial proposals to DPR. We mistakenly assumed that that money would be made available to us quite promptly after FY73 began, and we planned accordingly.

8.4 A nibble from DAT

However, we didn't forget about the Division of Applied Technology, which we hoped could be persuaded to fund our major HDR field experiment. We were encouraged in this by a telephone call to Dick Taschek on March 24, 1972, followed by a confirming letter of the same date from Jack Vanderryn, John Flaherty's technical assistant in DAT. Jack requested an update on our HDR Program and our plans for the next few months. At Dick's request, I responded in a letter to Jack dated March 30, 1972. By then, in addition to a lot of background studies and consultations and some field work of our own, we had reached a few decisions and initiated several important activities. I reported these to Jack as follows:

- We had decided to concentrate on the area just west of the Valles Caldera, in order to evaluate its suitability for a major HDR field experiment.
- Through Paul R. Guthals, who was involved in the Laboratory's air-sampling programs and worked directly with the Air Force in collecting samples for them, we had arranged (at no cost to us) for an Air Force orientation and photographic reconnaissance flight over that area. Paul and other LASL personnel had gone along on that flight, together with Burt Slemmons—who by then was serving as a consultant to LASL as well as a member of our Geoscience Panel. Burt had undertaken a study of faults in the area, which we wanted to avoid in selecting an experimental site in order to minimize the possibility that our hydraulic-fracturing experiments would trigger significant earthquakes. An important part of that study was low-sun-angle aerial photography. (The shadows cast when the sun is low often reveal surface relief—including small displacements along faults—that otherwise escapes detection.) At the time of this letter to Jack, Burt was examining the photographs taken on that flight, and he had already discovered a previously unmapped fault. Fortunately, it was a safe distance south of the area of our immediate interest. He was also working out favorable times and procedures for future photographic flights.
- In consultation with Allan R. Sanford at New Mexico Institute of Mining and Technology, we were planning a seismometer array to monitor earth movements in the entire caldera so that, if an earthquake occurred there, we could locate its epicenter, know whether or not we had caused it by injecting pressurized fluids in our drilled holes and be able to document it.
- We were out for bids on drilling about five deeper heat-flow holes in the area, then planned to be about 600 ft deep. Locations for them had already been selected with the assistance of Forest Service personnel and consideration of access and environmental concerns as well as technical requirements.
- We were keeping in touch with the USGS and the University of New Mexico, both of whom were considering geophysical studies in the caldera area during the next summer.
- At a low level we had undertaken a geochemical study of the caldera region and, for it, were collecting water samples from hot springs and steam condensates in the vicinity.

- We had arranged a one-week visit to Los Alamos by Dave Howell of the University of California at Riverside, in connection with his economic analysis of HDR systems.
- We were busy preparing the proposals to DPR described above.

My letter to Jack noted that we were also committed to preparing an HDR proposal for support by the National Science Foundation, although so far we really hadn't done much about it. (In fact, we never did.)

8.5 Cranking up the big proposal

In the meantime, we were working hard on the proposal to DAT for our first major HDR field experiment. We had a chance to try the proposal out on a group of visitors to LASL from AEC Headquarters on April 10, 1972. I don't have an attendance list from that meeting, but Jerry Johnson was there and by that time we knew that he would eventually be in charge of DAT. Therefore the meeting obviously was important to the future of our proposed program.

Fortuitously, I have found my notes for the talk that I gave on that occasion, and they are very familiar. The talk was a bit more elaborate than usual, but it was pretty much the same old stuff: the nature, magnitude, and distribution of the HDR energy source, and our proposed method of creating a heat-extraction loop by hydraulic fracturing.

On the day of that meeting (and far in advance even of any DPR funding) the first of our 600-ft-deep heat-flow holes was being drilled west of the Valles Caldera, but we were already quite sure that was a better area for our field experiments than sites closer to Los Alamos. I explained why, and went on from there. In view of the early date of the meeting, some of the statements that I made at that time are of historical interest relative to what we actually found out later.

- On the basis of our preliminary temperature-gradient measurements, we believed that in that area we would reach rock temperatures above 300°C at depths less than 20,000 ft. However, we didn't think that we would want temperatures that high because of the relatively high system pressures that would be required to prevent boiling of the water circulated through the system. (We were convinced that there were important advantages in circulating and producing superheated liquid water instead of steam.) We assumed, therefore, that hole depths for our initial system would be significantly less than 20,000 ft.
- For our first experimental system, we proposed to create a hydraulic with a radius of "only 1500 ft"—which, at that time, we considered relatively small. We wanted the fracture to be small enough so that its surfaces would cool significantly in a matter of months instead of years in order that, in a reasonably short period of fluid circulation, we could determine whether or not thermal-stress cracking would have a significant effect on the system's useful life.
- We proposed to use an array of microseismic instruments to map the hydraulic fracture as we created it, and then to watch it grow.
- However, we recognized that microseismic fracture-mapping techniques were not yet highly developed. We believed that they could give us the information needed to define a target that we could hit with a directionally drilled second well. If that didn't work, we assumed that we could intersect the fracture by directionally redrilling the lower part of the second well two or three times in various directions; or we could intersect a fracture made from the first hole either by fracturing hydraulically at the appropriate depth from the second well or by detonating conventional high explosives at that depth.
- We assumed that studies of the potential problems of corrosion, plugging, and scaling, would require operation of the completed heat-extraction loop for not less than two or three years.
- We hoped eventually not only for a small electrical-power plant on our experimental site but also for a multiple-use system. As an example of the potential usefulness of such a system, I cited the need of the Navajos in Arizona for both small electrical-power plants

and for large water-distillation units—to produce potable domestic water from the abundance of brackish groundwater found in many areas on their reservation.

- While we recognized the need for better economic analyses, I reported the results of some of our own preliminary engineering cost studies, which predicted favorable economics for HDR energy systems.
- Our preliminary resource-assessment studies indicated that in much of the western U.S. we could expect to reach a rock temperature of 300°C at depths of 15,000 ft or less, representing an energy supply suitable for generating electricity. There were a few places in the eastern U.S.—for example, around Corning, in southern New York—where the geothermal gradients appeared to be high enough so that HDR systems might also be useful there for generating electricity. The Conway granite in New Hampshire appeared to be marginal in this respect, but in most of the eastern and central U.S. we expected HDR systems to be useful primarily to supply heat for relatively low-temperature direct uses such as space heating, crop drying, and processing foods and chemicals.
- As follow-ons to our initial experiment, we suggested investigation of reduced-pressure or multiple-well systems for heat recovery from permeable formations; of shallow systems applicable to the lava beds in Hawaii and the Pacific Northwest; and of hydraulically fractured two-hole systems as leaching-in-place mining methods.
- In my talk, I also commented on the Union Oil geothermal development in the Valles Caldera, on the Baca Location west of Los Alamos. My notes say: "It is privately owned by the Baca Land and Cattle Co., and it is now leased by Union Oil Co., who are prospecting for natural steam. They have found some, at depths less than 5000 feet, and it is hot, dry steam. But they haven't found much, and they have had some very nasty geologic surprises. I will be very surprised if this develops into a commercial steam field." In fact, I later become more optimistic about the success of the venture as Union developed some hydrothermal wells—producing superheated water instead of steam—and the Public Service Co. of New Mexico began to build a power plant to use the hot water. However, the project was eventually abandoned for lack of a sufficient supply of that superheated water.

Not long after this, in a letter to E. A. ("Al") Bacon of the LASL Business Office, dated April 26, 1972, Daniel E. Pollock—a staff assistant to John Flaherty—gave us some guidance on preparation of a proposal to DAT. We were instructed to use the very elaborate AEC Form 189a, a blank copy of which he enclosed. Since that form was designed specifically for nuclear projects, it gave us a variety of problems in proposing a geothermal-energy program, and we had to leave many blanks and add a lot of supplements. (This in spite of the fact that, at least in part, geothermal heat is produced by the decay of naturally occurring unstable isotopes of uranium, thorium, and potassium in the earth's crust. As Bob Rex once remarked, it is, in fact, "fossil nuclear energy.") However, before the rest of us had even seen Form 189a, we received some additional guidance—from John Flaherty himself.

John visited us in Los Alamos on May 1, 1972, accompanied by Jerry Johnson and Jack Vanderryn. We reviewed much of the same material that we had presented to Jerry and others on April 10, discussed with them our proposal for a "LASL Geothermal Energy Program" and I offered some preliminary time and cost estimates. These estimates, for the period FY73 through FY75 were somewhat premature and had been prepared in too much of a hurry. Among other things they did not include LASL salaries and overhead, in part because we had not yet worked out what and how much would be done by consultants and contractors and what we should try to do ourselves. Our guess at the time was that the major HDR field experiment (which we hoped that DAT would fund) would cost $2,993,000 plus contingencies and LASL salaries and overhead. The time-chart showed circulation experiments in the completed system beginning in FY74 and continuing through and beyond the first half of

FY75. John suggested that, for the major experiment, we should

- increase our cost estimates, especially for the surface plant—a total of $5 million being reasonable;
- add 25% for contingencies and round off individual numbers
- extend the time scale realistically, breaking it down by fiscal years; and
- in the proposal writeup, mention Nevada specifically to arouse the interest of Alan Bible of Nevada (who was and continued to be a strong proponent in the U.S. Congress for development of geothermal energy).

In a private discussion with me later that day, Jack Vanderryn agreed to try for FY73 funding of $3 million to begin the major experiment, including $0.5 million for LASL personnel, and FY74 funding of $3.5 million, including $1.0 million for LASL personnel.

During the first week in June, Jack called and asked me for a summary of the HDR concept, outlining to me the general content and approximate length that he wanted. Presumably this was to be a sort of "executive summary" of our proposal, for circulation at AEC Headquarters. I responded on June 9, 1971, with a four-pager entitled "The Development of Dry Geothermal Sources." It was, of course, a condensed version of what had become our standard HDR sales pitch.

Then on June 20 Jerry Johnson sent out a memo to a number of individuals at AEC offices around the country and at LASL, Lawrence Berkeley Laboratory (LBL), Battelle-Northwest Laboratories (BNW), the USGS, and the China Lake Naval Weapons Center (NWC). It described the charge of the newly formed Division of Applied Technology and invited proposals for demonstration-oriented projects designed to demonstrate the ability of new geothermal resources to produce power. Jerry pointed out that the AEC laboratories at Los Alamos, Livermore, and Battelle-Northwest had already expressed interest and had demonstrated capabilities to participate in such projects and that DAT had already received such proposals from LASL and LBL. The LASL proposal was of course our preliminary HDR sales document. The Berkeley proposal was for a study of the geothermal energy potential of the Coso Hot Springs area in California, to be coordinated by Alan D. K. Laird of LBL. Since the Coso area is contained largely in the Naval Weapons Center reservation, this was to be a cooperative study with NWC—at that time represented by G. W. ("Bill") Leonard.

In a draft memo dated June 23, 1972, Tony Ewing elaborated on DAT's proposed "Geothermal Energy Development Program." The first paragraph of the memo stated that

> "The objectives of the geothermal energy development program are to demonstrate through engineering field projects, concepts which will broaden the recovery of geothermal energy for the production of electricity, process heat, space heating and air conditioning applications. Research, engineering, development and design efforts will be directed toward construction and operation of geothermal test facilities to demonstrate the economic and technical feasibility of utilizing the energy recoverable from (1) dry geothermal formations void of mobile ground water, (2) nonproductive wet and dry steam systems, and (3) hot water or brine systems."

Tony went on to describe briefly five-year programs and estimated budgets for the types of projects that DAT intended to support. By fiscal year and in millions of dollars these were

This of course did not represent an official AEC budget for geothermal energy development, and the $1.0 million for HDR in FY73 was only one-third of what Jack Vanderryn had agreed to try for. However, $13 million for a five-year HDR Program looked awfully good to us at the time.

In his memo, Tony briefly described a joint industry-AEC study and demonstration of binary cycle electricity generation, which apparently is item 3 in the above table. He also included the following statement: "In addition, coordination of supporting research and development effort common to the general engineering field demonstration projects will be monitored by a working level basic studies task force composed of individuals actively engaged in project activities." This was the first indication of an intention to form the advisory panel that came to be known as "The Ogle Committee," some of whose activities are discussed later in this narrative.

AEC Announcement No. 152, dated June 30, 1972, outlined the organization and listed the staff of the new Division of Applied Technology. As expected, it listed Jerry Johnson as its director, with Jack Vanderryn as his technical assistant. James C. ("Jim") Bresee, whom I had met previously when I visited Oak Ridge National Laboratory, now appeared as assistant director for General Energy Development, responsible for R&D related to all phases of energy supply, conversion, distribution and storage, with the exception of nuclear energy. Jim's domain of course included our HDR Program, and he soon became our principal contact at AEC Headquarters. The other two branches of DAT were Isotope Development and Peaceful Nuclear Explosives, existence of the latter indicating that the Plowshare Program had been moved into DAT. Dick Hamburger, Marcy Williamson, and Tony Ewing were now all listed as members of the DAT staff, and DPNE had disappeared.

The funding needed to initiate our major HDR field experiment was not yet committed, but it appeared hopeful that it soon would be.

Table 8-1.

	FY73	FY74	FY75	FY76	FY77
1) Dry Rock Energy Recovery System, LASL	$1.0M	$3.0M	$3.5M	$4.0M	$1.5M
2) Utilization of Wet Geothermal Systems	0.4M	1.0M	1.5M	3.5M	2.0M
3) Joint Industrial Demonstration Project	0.2M	1.0M	0.5M	0.5M	0.5M
4) Cost-Effectiveness Analysis	0.25M	0.3M	0.3M	0.3M	0.3M
5) Geochemical Support Research	0. 1M	1.2M	0.25M	0.25M	0.25M
Annual Totals:	$1.95M	$6.5M	$6.05M	$8.55M	$4.55M

9. THE BIG PROPOSAL

Preparing the elaborate proposal required to get financial support for a new and potentially quite large program was a new game for me, and I needed a lot of help—particularly in dealing with AEC Form 189a. Fortunately, Mac McGuire in the LASL Office of Special Projects was an old hand at the business and was willing to help me. Together we put together the initial proposal in a hurry. It was prepared for the Division of Applied Technology (DAT) of the AEC, and was submitted to DAT even before DPR had acted on the four smaller research proposals described in Section 8.

In the 189a format, the HDR proposal was for a "Dry Geothermal Source Demonstration." (In the course of my discussions with Mac, I insisted that all geothermal sources were, by definition, hot, and that the distinctive features of the ones in which we were interested was that they were dry—in the sense that they did not contain enough free water to be commercial producers of natural steam or hot water. Hence the emphasis in the title on the word "Dry." Subsequently this became "Dry Hot Rock," and later, by directive from Washington, "Hot Dry Rock,") The proposal was dated May 12, 1972, and listed R. F. Taschek as the person in charge and M. C. Smith as principal investigator. It requested funding of $3.838 million for FY73, including $187 thousand for equipment, and it listed and was coordinated with our proposals to DPR. Projected total costs for FY74 and FY75 were $2.549 million and $4.156 million respectively. There were some in LASL management who—perhaps justifiably—felt that these dollar figures were so high that they would surely cause DAT to reject our proposal, and urged me to reduce them to some "more reasonable" values. However, I considered them realistic for the program that we were proposing, and they didn't scare Mac (who was seasoned in the costs of weapons activities). We went ahead with them.

In this 189a, we proposed that in FY73 (beginning July 1, 1972) we would

- select a site on the Jemez Plateau (west of the Valles Caldera) for the world's first HDR heat-extraction field experiment;
- drill a 10-in.-diameter hole there to a depth of 7500 ft, where we expected the rock temperature to be about 300°C;
- hydraulically fracture from that hole;
- drill a second hole to intersect the fracture zone at a depth of about 5500 ft; and
- begin construction of the surface facilities required to operate a recirculating pressurized-water loop that would extract heat from the fractured rock at depth, dissipate it at the surface, and return the cooled water to the underground loop to extract additional heat.

The surface facilities would then be completed in FY74, the loop operated for a year or more, and a second location selected for a similar field experiment in a different geologic environment.

This proposal was sent to John Flaherty at DAT by Dick Taschek on May 12, 1972. At about that time Jerry Johnson succeeded John as acting director of DAT, and it was Jerry who responded to our proposal. In a letter to Dick dated May 26, 1972, he acknowledged receipt of our 189a but pointed out that the AEC's FY73 budget did not specifically identify any funds for geothermal energy R&D (a fact of which we already were painfully aware). Therefore, he was uncertain how much money could be provided to LASL in FY73 for our HDR Program. However, he stated that they were "very much interested in providing support for the LASL concept and will be discussing specific plans with you in coming weeks." Jerry asked that, in the meantime, we provide him with a revised, up-to-date, technical description of the LASL HDR concept that clearly identified the scientific and technical aspects of the project—including identification of the state of the art of the technology and the major areas that required investigation.
That, of course, was just the type of promotional literature that we had been working on, and Taschek leaned on us to get it done by June 23. He offered us top Laboratory priority for such things as the preparation of illustrations, which helped us considerably since we had no illustrators of our own.

The June 23 deadline was set primarily so that we could get the finished document to AEC headquarters before the new fiscal year began on

July 1. We barely made it. Dated June 27, 1972, and titled "Dry Geothermal Energy Sources," it was an elaboration of the "Preliminary Study" that we had circulated within LASL during 1971 and submitted to Harold Agnew in December 1971. And it was fairly elaborate, all right, as witness the table of contents reproduced in Fig. 9. It was, of course, a sales pitch, and it was pretty optimistic in spots— but on the whole it was about as honest as our state of knowledge permitted at the time. I wrote the body of the report and among us we persuaded experts from all over the Lab to supplement it with the eleven appendices listed—which I edited lightly for consistency. Of the authors of those appendices

- John Rowley and Bob Potter were members of the original ad hoc Subterrene Group and the de facto GTE Committee;
- Don Brown and Lee Aamodt were early volunteers to the HDR cause;
- John Balagna was from the LASL Radiochemistry Group, temporarily turned geochemist;
- Bill Purtymun, a geologist-hydrologist, was the custodian and principal operator of the sampling rig used to drill our initial heat-flow holes;
- Francis West was a geologist-geophysicist from the LASL Nuclear Testing Division;
- Bill Sedlacek was an environmentalist from our Health Division;
- Bob Hendron and Dick Foster were from our Engineering Division.

This particular document was never published and had only limited circulation within LASL and the AEC, which was both good and bad. It actually was quite a sound exposition of the characteristics and advantages of HDR energy systems and also of the uncertainties and potential problems involved in their construction and operation. However, while a number of cautionary notes were sounded and we proposed an experimental system that was much less ambitious than the commercial systems we visualized, we did go overboard in some areas. For example, we were confident that hydraulic fractures made at the depths of useful HDR reservoirs would necessarily be vertical; that a single vertical fracture with a radius of 4000 ft could be produced in granitic rock at 300°C; and that from such a fracture we could produce 260 MWt (thermal megawatts) for at least 10 years. Also, reflecting the sales pitches of the drilling and service-company experts that we had consulted and much of the technical literature that we had read, we were overly optimistic about drilling times and costs for our proposed experimental system, the ease with which we could produce very large hydraulic fractures and circulate water through them, and the cost per installed kilowatt of generating capacity for completed HDR systems designed to produce electrical power.

However, considering when this document was prepared and the actual state of knowledge and value of the U.S. dollar at the time, it is a pleasant surprise to look back and see how much of this document was correct!

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by Morton C. Smith

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K. Alternative Methods of Developing Geothermal Reservoirs, by R. Lee Aamodt

Fig. 9. Table of contents from "Dry Geothermal Energy Sources" (Smith, 1972).

10. BACK TO THE BREADLINE

All of that nice money—and even the proposals that eventually made it available to us—were still far in the future in January 1972, although at that time we didn't realize quite how far in the future they really were. However, in preparation for what we hoped would develop into a well-funded HDR program, all of us who were concerned with it were trying to do a sound technical job in a lot of related areas that, at least initially, we knew very little about. That wasn't easy, since we were also doing the jobs for which the Laboratory was officially paying us. In my own case, I was trying to keep alive a challenging carbon and graphite R&D program that I had managed for several years and funding for which had been greatly reduced by termination of the Rover Program. Several of us were also trying to locate jobs and funding support for those of our employees individually affected by the demise of Rover, and it was a fairly frantic time. However, we got a lot of help and support from elsewhere in the Laboratory (often from people who had problems similar to our own), and we did manage to get quite a lot done—much of it after hours and on weekends and holidays.

During January, Bill Purtymun and Don Brown completed the last four of our shallow heat-flow holes, west of the Valles Caldera, and we began to consider where deeper holes should be drilled for better heat-flow measurements and to explore the subsurface geology and hydrology of the area. We hoped to go out for bids at the end of February for drilling of several such holes and to get them drilled and logged during March.

In a memo to me dated January 26, 1972, Bill Purtymun discussed the geology and hydrology of the area just west of the caldera. There were no existing deep wells in that area, but he predicted that a deep hole would pass through about 150 ft of Bandelier Tuff and then 1400 ft of Abo Formation, with considerable variations in thickness from place to place because of erosion of the Abo before the tuff was deposited. He felt that the tuff would not contain significant groundwater but that the Abo would be saturated—although its permeability was thought to be low. He predicted that there would be no particular difficulty in rotary drilling through the tuff and Abo, although there might be some circulation loss in the tuff, and he estimated that penetration rates would be 15 to 20 ft/hr. All of these turned out to be good estimates.

As a result, of our discussions with Lin Cordell, we hoped that he would be doing a fine-grained magnetic study of the area west of the caldera during March, and that we might be able to arrange for Adel A. R. Zohdy—also from the USGS—to do a deep electrical-resistivity survey there at that time. This didn't work out. In a telephone conversation on March 10, Lin told me that he would be working in the Taos area that summer, although he could be available to work with us late in the summer if that were desirable (probably on fine-grained gravity studies in the Calaveras Canyon area north of Seven Springs, where he had detected an intriguing gravity and magnetic anomaly). He had talked to Adel Zohdy who felt that, in the area west of the caldera, there was so much topographic relief both at the surface and on top of the basement rock, and so much complexity in the intervening strata, that resistivity measurements would probably not give us useful information about the basement. Lin thought that seismic reflection would show us very little beyond how deep it was to basement, about which we already had a good idea. He had recently talked to George R. Jiracek, a geophysicist at the University of New Mexico, who expected to have a graduate student doing a magnetic study in the Jemez Mountains that summer. (However, Lin felt that ground-based magnetic measurements would show so much detail that they would be fairly unenlightening.) We did later involve George in the HDR Program and Lin continued to be a valuable source of help and advice.

Another source of the types of expertise that we needed was the New Mexico Institute of Mining and Technology (where I had taught metallurgy briefly some years before, while it was still the New Mexico School of Mines). Some time in the early spring of 1972, Don Brown, Bob Potter, and I visited the campus in Socorro, New Mexico, and discussed our program with Merle E. Hanson, an expert in rock mechanics; Allen R. ("Al") Sanford, known worldwide particularly for his seismic studies in the Rio Grande Rift Valley; and Marshall A. Reiter, whose heat-flow studies covered much of New Mexico and extended into the surrounding states. We were impressed by their backgrounds and capabilities, and expressed the hope that DPR funding would soon appear so that we could arrange agreements with each of them for part-time work with us during the rest of their academic year and full-time assistance during the coming summer.

At that time we understood that DPR funding would be available to us by early March, and we proceeded accordingly. Our proposals to DPR therefore included funding for dynamics studies of hydraulic fracturing and thermal-stress cracking of crystalline rock, in which we intended to depend on Merle Hanson; seismic studies, where we mentioned Al Sanford by name; and heat-flow studies by the NMIMT group under Marshall Reiter. Unfortunately, it was not until the next winter that we actually got the DPR money, and we had to cancel the whole deal with the NMIMT group—who had to change their personal plans accordingly. It was a letdown for them, a worse one for us, and is still a source of embarrassment to me. We did manage to siphon off a little money from other sources to arrange for Marshall and his students to log temperatures in our heat-flow holes and make thermal-conductivity measurements on some of our drill cores and cuttings; and we took full advantage of Al Sanford's knowledge and helpfulness and of some of his seismic studies related to other LASL interests. However, I have always been sorry that the arrangement that we initially proposed didn't work out.

During early 1972, Bob Potter was doing some small-scale experiments on thermal-cracking with positive results. Also, following up on discussions with our Geosciences Panel, he undertook an analysis of fluid loss from a pressurized HDR reservoir in basement rock. He reported this in a memorandum entitled, "Reservoir Leakage," dated February 23, 1972. He estimated that a fractured reservoir in granitic basement rock, pressurized at 1500 psi and producing 100 MWt, would, during the first year of heat extraction, lose 6% of the water flowed through it. Particularly considering the state of knowledge of *in situ* permeability at that time, this was a remarkably good estimate.

A lot of us were working on various parts of our report, "Dry Geothermal Energy Sources." John Rowley was preparing a write-up on rock mechanics and hydraulic fracturing, assisted by Lee Aamodt and Halliburton Services. Frank Harlow and Bill Pracht were continuing their computer studies of the mechanical and thermal behavior of HDR reservoirs. Bob Potter and Don Brown were working on an economic study of HDR energy systems, assisted by Ken Brunot (then with the University of California at Riverside). Bill Purtymun and Don Brown were extending Bill's write-up on the geology and heat flow in the Valles Caldera region, with input from Roy Bailey, Lin Cordell, Marshall Reiter, and others. Don was also preparing a preliminary plan for our first major field experiment. Harry Otway from J Division had given us some background information on environmental studies, which we were sure that we would need, and I was looking for someone to write a section on that. I was also trying to find help in other parts of the Laboratory in several additional areas, and in particular had approached John Balagna about helping us in geochemistry. All of this, of course, was going a lot slower than we had hoped, but, considering the circumstances, we actually were making quite good progress.

11. OUR FIRST OFFICIAL FUNDING

To this point, the HDR Program had survived on LASL internal funding that was generally traceable to the discretionary research funds provided by the AEC Division of Military Application; on salaries and facilities provided by the Laboratory groups in which individuals in the program were still officially employed; and on the unpaid efforts of those individuals during their lunch hours, evenings, weekends, and holidays. We had been disappointed with regard to prompt funding by DPR and DAT but still hoped that that financial support soon would materialize.

11.1. Promises, promises

In early June 1972, Dick Taschek was instructed by DPR to resubmit our Form 189 proposals with a stronger slant toward basic research in the geosciences. This was intended to indicate initiation at LASL of a strong geoscience research activity aimed at future funding of about $250,000 annually, supporting but not dependent on our geothermal and Subterrene programs. Then, on July 7, Dick was informed by telephone from Washington that $50,000 had been included in the DPR budget for general support of our geothermal energy program, and an additional $100,000 was being reserved by DPR for later inclusion when our 189s had been approved. He was told that about 10% of that funding should be spent on equipment. (However, even the $50,000 did not actually appear in our financial plan until the following December.)

In this same phone call, Dick was informed that an additional $8000 from Plowshare funds was available to support Bob Rex's work at the University of California at Riverside. That $8000 was discussed in Section 6.4 of this report, and I still don't know quite how it was arranged.

The "approved 189s" took a while.

11.1.1. The heat-flow study. This proposal was amended and resubmitted to DPR on July 17, 1972, for FY73 funding. It now included $8700 to cover some LASL salaries and indirect costs. By that time four 600-ft heat-flow holes and one deep exploration hole (well GT-1, 2575 ft deep) had been drilled west of the caldera, during FY72. Accordingly, drilling proposed for FY73 was reduced to 7 to 10 additional heat-flow holes. Development of a LASL capability for thermal-conductivity measurements was added. "Operating costs" were reduced to $48,700, but $5000 were added for capital equipment—raising the total FY73 funding request to $53,700. This version of the proposal showed continuation of the research through FY74 with an estimated cost for that year of $488,800, including an ambitious drilling program. The letter of transmittal accompanying the revised proposal indicated that this (and the other revisions described below) represented the first step in the development at LASL of a program of fundamental research in the geological sciences that would support not only the geothermal-energy project but also many other AEC activities.

The heat-flow proposal was amended again on October 1, 1972, and sent by Dick Taschek to Daniel R. Miller who, as acting director of DPR had replaced Paul McDaniel. It now listed Roderick W. ("Rod") Spence as the person in charge, with three principal investigators: Don Brown, in charge of drilling and field measurements; William L. ("Bill") Sibbitt, a Rover Program alumnus, in charge of thermal-conductivity measurements and development of downhole equipment for measuring conductivity *in situ*; and Bob Potter, in charge of local and national heat-flow analysis and interpretation. The number of additional heat-flow holes to be drilled in FY73 was reduced to 5 to 7, and greater emphasis was given to analysis, computer modeling, and evaluation of the resource base of HDR thermal energy throughout the entire United States. Locally, the heat-flow study was broadened to cover not only the Valles Caldera region but also the nearby Rio Grande Rift. Funding requests were rounded to $53,000 for FY73 and $490,000 for FY74.

11.1.2. The geochemistry proposal. This was amended on June 27, 1972, increasing the funding request to $56,500 for FY73 and proposing that, instead of farming out the high-pressure, high-temperature chemical testing, LASL modify some of its existing facilities to do the work. The next amendment, dated July 10, 1972, listed, George Cowan as the person in charge with John Balagna and John W. ("Jack") Barnes as principal investigators. It proposed extending the study through FY74, with funding of $183,800 for that year, to provide for considerable expansions of both laboratory and field work. Another amendment on August 1, 1972, was directed to A. R. Van Dyken, assistant director of DPR for Chemistry Programs, and increased the emphasis on fundamental studies and applications to natural hydrothermal systems.

11.1.3. The rock-mechanics proposal. An amendment dated July 11, 1972, added a proposal for continued funding of $259,200 for FY74, primarily for development of an in-house LASL capability in rock mechanics and fracture dynamics. A second amendment dated October 1, 1972, listed Rod Spence as the person in charge with John Rowley and Bob Potter as principal investigators, pending recruitment of a rock-mechanics expert. It rounded the FY73 funding request to $50,000 and the FY74 request to $260,000, and elaborated particularly on field experiments in hydraulic fracturing to be conducted in existing wells.

11.1.4. Seismic studies. An amended proposal dated August 2, 1972, proposed continuation of the project through FY74 at a cost for that year of $313,800 and expanded on the scientific interest of studies relative to natural earthquakes and volcanic activity. Another amendment dated October 1, 1972, listed Charles I. Browne (then J division leader) as the person in charge, with Kenneth H. ("Ken") Olsen—a J Division seismologist—and Bob Potter as principal investigators. It rounded the FY74 funding request to $315,000 and elaborated on expected expenditures for equipment and consultants.

11.2. More promises and finally some cash

Funds appropriated by Congress were apportioned among federal agencies by the Office of Management and Budget (OMB) and, in this case, its various field and operations offices and laboratories by the AEC. The LASL funds could be committed by Los Alamos only after they appeared in the AEC's formal financial plan for the Laboratory. An important fact that we in the HDR Program finally learned the hard way was that there were usually long delays between Congressional approval even of line-item funding and actual release of the money by OMB to AEC, and again between AEC approval and the appearance of spendable money in the Laboratory's financial plan. We got caught up in these delays repeatedly and they were a frequent source of frustration—and occasional embarrassment, as was the case in our attempt to arrange for part-time and summer help from NMIMT faculty members.

In a letter to Harold Agnew dated October 31, 1972, D. R. Miller reported that the LASL financial plan did (finally) include $45,000 "for a program of research in geochemistry"—the chemistry program described above, which was separately funded as one of DPR's chemistry programs. The $50,000 ostensibly committed on July 7 for general support of our other three proposals to DPR now actually appeared, under the heading of "geophysics," and was listed in this version of the financial plan under "Low Energy Physics." (In fact, at that level of funding, not much energy could have been committed to those activities.)

On December 11, 1972, Enloe Ritter of DPR called me from AEC Headquarters to say that "with some confidence" we could expect to get a total of $100,000 in FY73 operating funds, plus $20,000 for equipment, in support of our three geophysics proposals. (This, however, included the $50,000 that now was actually in our financial plan.) He said that the AEC would leave to LASL the distribution of these funds among those programs, but he suggested special emphasis on the seismic studies. However, in spite of the efforts of our friends at AEC Headquarters, that financial package didn't materialize intact. We eventually were allocated $43,000 in operating funds (in addition to the original $50,000) for the geophysics work, with no additional money for capital equipment. That brought our total FY73 funding from DPR to $138,00, of which $45,000 was for geochemistry and $93,000 for geophysics. Although we had expected to receive a somewhat larger amount some months earlier, the money was of course very welcome when it finally came. It represented the first official sponsorship of our HDR Program and the initiation of what grew into a broad geoscience program at LASL.

George Kolstad, who was instrumental in arranging all this at AEC Headquarters, has remarked that—because of it—he was the real father of our HDR Program. In that sense, he was at least one of quite a large number of the program's fathers, and a very important one.

With regard to the missing equipment money: in another phone call to me on January 12, 1973, Enloe Ritter told me that the $20,000 promised for capital equipment did not, for some reason, appear in our funding request as it reached him. He asked me if we still wanted it, and of course I told him that we certainly did. At his request, I prepared a breakdown of what we would spend it on, which I mailed to him the next day. (That being a Saturday and our Laboratory mailing channels being closed for the weekend, I mailed it to him unofficially through the local post office—and cleared it with my LASL bosses the following Monday. While not officially sanctioned, such impromptu actions were generally tolerated when the occasion seemed to justify them.) I proposed that we spend $5000 on downhole temperature-logging equipment, including an armored temperature-sensor, a high-temperature cable, a winch, and a recorder, and $15,000 on microseismic equipment, including an armored three-component downhole seismometer, a cable, and a recorder. It turned out, of course, that all this was a waste of time for both Enloe and me. We didn't get the $20,000. Incidentally, as usual, I signed this message as "Project Manager, Geothermal Energy," a job to which I had already been appointed at LASL even though we still had no official project.

11.3. Greater things to come

On July 14, 1972, Lee Aamodt gave me a copy of a "white paper" called "Energy Production from Geothermal Sources." According to Lee, this reported a presentation that AEC representatives had made to an Office of Science and Technology (OST) panel in Washington during the last week in June. The paper was divided into three sections:

A. Energy Potentials of Geothermal Systems. This section emphasized the magnitude of the U.S. resource base of HDR geothermal energy, pointing out that it had the potential to supply total U.S. energy needs for a few hundred thousand years.

B. Possibilities for Development of Geothermal Systems. This section was divided into the following four subsections.
   1. Improvements in Drilling Technology. Again HDR was emphasized. The advantages of being able to drill economically to depths of 30,000 to 40,000 feet were discussed, together with the potential of the Subterrene to do it.
   2. Improvements in Low Temperature Fluid Utilization. Here the advantages of direct use of low-temperature geothermal heat—for example, for space-heating—were described, together with its probable availability at usefully high temperatures and depths of 20,000 feet or less throughout most of the United States.
   3. Recovery of Energy from Hot Dry Rock. Our proposed method of developing a hydraulically fractured HDR heat-extraction loop was described in considerable detail.
   4. Improvements in Wet Geothermal Systems. Experiments were proposed to determine appropriate stimulation techniques (e.g., hydraulic fracturing) for "wet" geothermal systems that were marginal or uneconomic for commercial production.

C. Opportunities for Productive Experimental Projects. In summary, the paper concluded that three paths should be followed toward the goal of plentiful geothermal power, as outlined in the budget breakdown below.. It stated that "The largest stakes are in power from dry rock," but that stimulation of marginal hydrothermal systems could give a sizeable increase and that binary generating systems could also increase available energy by extending the usable temperature range down to about 150°C. (In a binary power plant the geothermal heat is used to vaporize an organic liquid whose boiling point is lower than that of water. The heavy organic vapor is then used instead of steam to drive a turbine.)

The proposed time scale and year-by-year costs of this geothermal-energy development program were presented on three slides that accompanied the white paper. They are summarized below, with the dollar amounts in millions.

While this represented a substantial and (for a government agency) relatively long-range program, it did not specify times in terms of either calendar or fiscal years. However, Lee's note to me accompanying this paper stated that the AEC would actually get only $1.5 million for its geothermal energy programs in FY73, but that they still planned to give us $1.0 million for "Dry Geothermal Formation"—our HDR Program. From the table here, it appeared that FY73— the current fiscal year—was actually year zero in the AEC program, and that we were going to get a head start on the schedule outlined there.

By this time we were becoming very skeptical of projected funding amounts and time frames. However, we were encouraged by the strong AEC support for HDR represented by this presentation to OST. In part, of course, this support came because we had been very lucky with regard to timing. The AEC was new to the alternative-energy game, and we were in line to help them spend whatever funds they had available for it.

Table 11-1. Program	**Year 1**	**2**	**3**	**4**	**5**	**Total Estimated Cost**
Dry Geothermal Formation: hydraulic-thermal fracture engineering experiment	$2.5M	$3.5M	$4.0M	$10M	$ 2.0M	$22M
Binary Fluid System: engineering experiment	0.5M	2.0M	2.5M	2M	2.0M	9M
Wet Geothermal Systems: engineering experts	1.5M	2.5M	3.0M	4M	12.0M	23M
Totals:	4.5M	8.0M	9.5M	16M	16.0M	54M

12. DRILLING OPERATIONS

Between December 1971 and July 1972 we drilled or had drilled for us a series of holes for the following three principal purposes:

- Shallow heat-flow holes, generally 50 to 100 ft deep, to sample temperature gradients in a rough semicircle outside the southern half of the Valles Caldera;
- Intermediate-depth heat-flow holes, 500 to 750 ft deep, for better measurements in the area west of the caldera where shallow measurements indicated that heat-flow was highest and geological information indicated that the depth to basement was moderate and the subterranean geology was least complicated;
- A deep exploratory hole (GT-1, 2575 ft deep) to investigate temperature gradients, stratigraphy, and hydrology through the overlying sediments and volcanics and into the granitic Precambrian basement.

Locations of these holes are shown in Fig. 10 and details of drilling, well-completion, stratigraphy, and temperature-gradient and heat-flow data are summarized in Appendix A of this report. All holes were drilled in the Jemez District of the Santa Fe National Forest under special-use permits issued by the U.S. Forest Service.

12.1. Shallow heat-flow holes

These should probably be called "temperature-gradient holes" since, because of large variations in the thermal conductivities of the tuff in which they were drilled, calculated heat-flow values were very uncertain. However, as at least qualitative indicators of the relative values of heat flow in different locations, the measured temperature gradients were, in fact, very useful.

These holes were drilled by Bill Purtymun and Don Brown using H Division's auger-type soil-sampling rig. They were kept open for temperature-logging by inserting 2-in.-diameter plastic tubing, surrounded by cuttings from drilling the holes. Seven holes were drilled in December 1971 and three more in February 1972. All were at elevations above 8000 ft and, in general, the surface stratum was two or three feet of snow and surface temperatures were usually well below freezing. Dry drilling was necessary for removal of cuttings from the hole, and this type of drill was well suited for the job—although in several of the holes very hard formations were encountered that it could not penetrate. This was a truly heroic effort by Bill and Don, who in general were out in the snow drilling entirely on their own. I did make an occasional trip to their current drilling site when they were unusually late getting back, to see if they had frozen to the rig or fallen in the hole. They never had and almost always got back home before I did.

Of the 10 holes attempted, 7 were deep enough to be well below the zone affected by daily and annual temperature cycles and dry enough so that they were probably not significantly affected by active groundwater circulation. When sufficient time had passed since the holes were drilled so that substantial temperature equilibrium had been reestablished in them, they were logged for us under contract by Marshall Reiter's NMIMT heat-flow crew. Temperature gradients were measured in sections of tuff (consolidated volcanic ash) near the bottom of each hole and, since tuff is generally very porous and a poor conductor of heat, the gradients were high. They ranged from 84°C/km east of the caldera to 99°C/km south of the caldera and up to 235°C/km west of it, and fell off quite rapidly with radial distance away from the edge of the caldera. They demonstrated the existence of a large, high-grade geothermal area surrounding the caldera, with the most promising location for a relatively shallow HDR development on the Jemez Plateau west of the caldera.

12.2. Intermediate-depth heat-flow holes

On the basis of results from our shallow heat-flow holes and in consultation with the U.S. Forest Service concerning access and environmental concerns, we selected six possible locations for deeper heat-flow holes west of the Valles Caldera. Three of them were about 3 to 4 miles apart along an arc approximately 2 miles west of the main ring fault

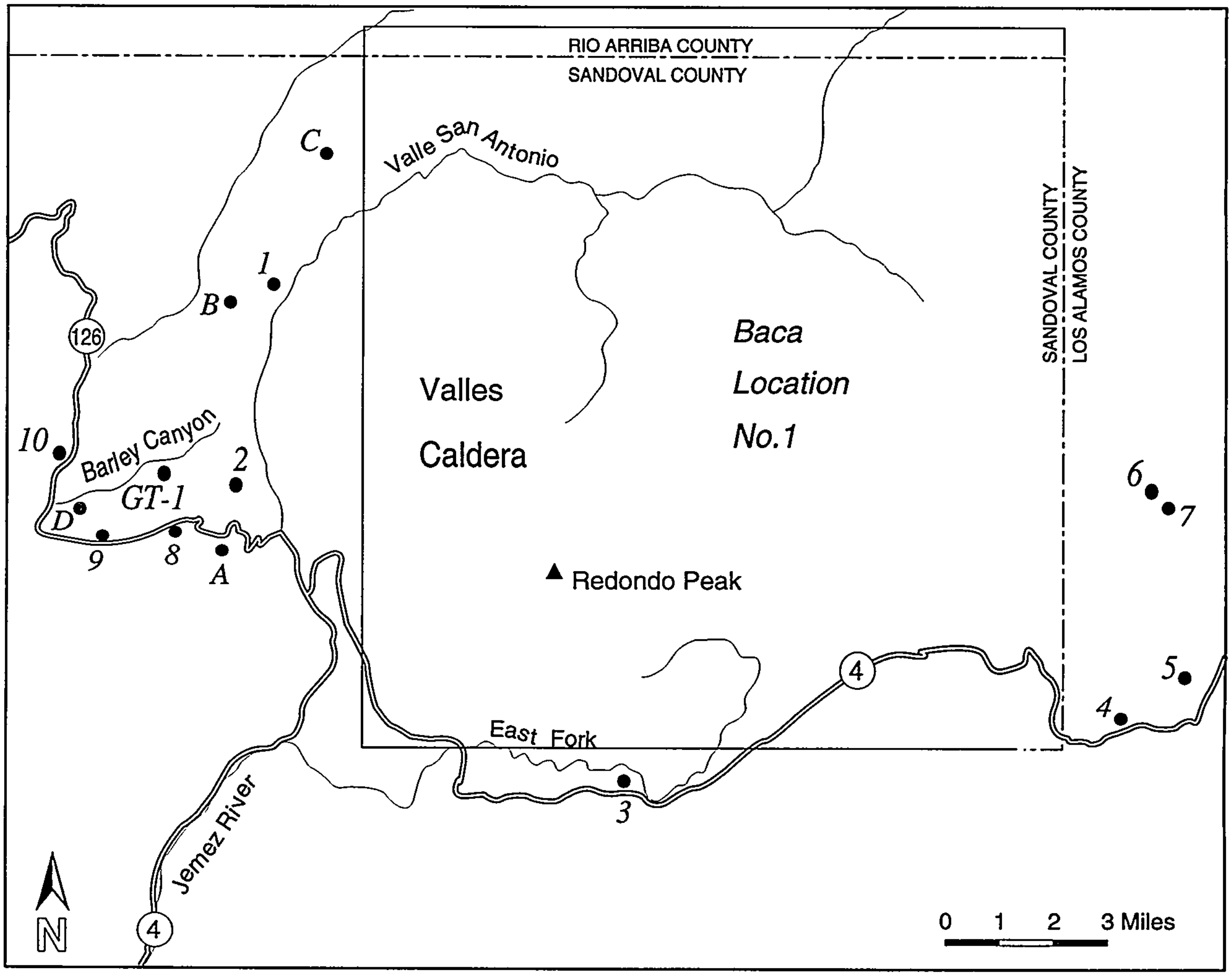

Fig. 10. Approximate hole locations. Numbered circles indicate shallow heat-flow holes. Letters represent intermediate-depth heat-flow holes. GT-1 is our first deep exploratory hole.

bounding the caldera—about the minimum distance from that fault at which we believed that the basement rock would not have been seriously disturbed by caldera subsidence. The other three were about 2 to 3 miles farther from the ring fault, in the same general directions as the first three, and were intended to sample the rate at which heat flow diminished with distance from the caldera. A request for quotations was issued on March 24, 1972, for drilling 4 to 6 of those holes, nominally 6- to 6 1/4-in. in diameter, to depths of approximately 600 ft. The RFQ was accompanied by a memo written by Bill Purtymun describing the geology and hydrology of the area. The successful bidder was Stewart Brothers Drilling Co. of Grants, New Mexico, who proposed to drill with a truck-mounted drilling rig normally used for uranium prospecting.

On April 12, 1972, a purchase order to Stewart Brothers was issued by James H. ("Jim") Sahling of the LASL Supply and Property Department—who was invaluable to us in initiating and expediting our various procurements and preserving our very shaky credit rating. This particular order provided for rig mobilization and demobilization, bits, facilities, and manpower to drill just four holes (which, it turned out, was all that we could afford) to 96-ft depth for setting 9 7/8-in.-diameter surface casing, and then to continue the holes at 6 1/4-in.-diameter to a depth of approximately 600 ft. Total cost was to be $13,944.00, although an additional $600 was made available for lost-circulation materials should they be needed, plus another $800 maximum for shutting off water inflow if that should occur.

These four holes were drilled successfully by Stewart Brothers during April 1972, using a bentonite mud as the drilling fluid.

- Hole A was completed on April 10 at a depth of 590 ft in Lake Fork Canyon, west of the village of La Cueva, about 0.7 mile southeast of what was to become our Fenton Hill experimental site and about 1.9 miles southwest of the major ring fault bounding the Valles Caldera.
- Hole B was completed on April 13 at a depth of 650 ft in Oat Canyon, about 4 1/2 miles north of hole A and about 2 miles west of the ring fault.
- Hole C was completed on April 16 at 750-ft depth just south of Road Canyon, about 2.7 miles northeast of hole B and about 2.3 miles from the ring fault.

These three holes were intended to sample heat flow along an arc outside the western edge of the caldera in the general area in which our shallow heat-flow holes had shown that geothermal gradients were very high.

- Hole D was completed on April 18 in Barley Canyon, northeast of Fenton Lake, about 5 miles southwest of hole B and about 4 miles west of the ring fault. It was intended to determine the rate at which heat flow decreased with increasing distance from the caldera.

Depending on location—whether on top of a mesa or in a stream valley eroded into the plateau—these holes penetrated from 120 to 580 ft of Cenozoic tuff and other volcanics, and continued in the underlying Paleozoic Abo Formation (the "Permian red beds") —consisting of alternating layers of shale, siltstone, sandstone, and clay. Cores were saved for petrographic examination and thermal-conductivity measurements. Some previously unknown sedimentary stratigraphy was discovered, but there were no significant drilling, lost-circulation, or water-inflow problems.

By the end of April preliminary temperature logs in the four holes had been run (gratis) by a very helpful USGS crew from Albuquerque. Temperature equilibrium in the holes, disturbed by the drilling operation, had of course not yet been reestablished. However, these measurements indicated that temperature gradients were very high in the red beds (of the order of 100° to 150°C per kilometer, and more in some low-conductivity strata) and so was the rate of heat flow.

During May 1972 we arranged for a workover rig to clean out the four holes and install 4 1/2-in. steel casing in them to keep them open for a series of temperature logs. The contractor for this was Dan Laughlin of Shamrock Drilling Co., and my purchase request, dated May 4, 1972, committed $2160 for the work.

We also arranged, under another purchase order, for Marshall Reiter's NMIMT logging crew to run repeated temperature logs over a period of time to observe the approach of the holes to temperature equilibrium; and for his laboratory staff to measure thermal conductivities on the cores we had taken—to permit calculation of heat flow. My cost estimate for hole-logging was $1075, and for 40 conductivity measurements it was $400.

Details of hole locations, drilling and well-completion, geologic sections, and temperature gradient and heat-flow values, are given in Appendix A. Heat flow in the general area about 2 miles west of the ring fault was found to be 5×10^{-6} to 6×10^{-6} $cal/cm^2 \bullet sec$, which is three to four times the world-wide average, and to increase slightly from south to north. At a location 4 miles west of the ring fault it was only 2.2×10^{-6} $cal/cm^2 \bullet sec$ (still about half-again the world average), indicating a strong dependence on distance from the heat source under the caldera. Measured temperature gradients in each hole of course varied widely according to the thermal conductivities of the formations in which the measurements were made—tuff, latite, sandstone, conglomerate, clay, and shale.

We had expected these holes simply to help us pinpoint a location west of the Valles Caldera where the geothermal gradient was high enough so that we would reach a usefully high rock temperature without drilling very far into the basement rock. Instead, the results from the 600-ft holes showed that that should occur no matter where we drilled over an area of several square miles—which was even better. In addition to identifying a very large and high-grade geothermal area, they demonstrated the flexibility with regard to hole location that is a major advantage of HDR over natural hydrothermal systems.

12.3. Geothermal Test Well No. 1. (GT-1)

In view of our condition of ignorance and optimism, it would have been appropriate for us to operate in Las Vegas, Nevada, instead of Los Alamos, New Mexico (although our simultaneous condition of poverty made the odds very long against a successful Las Vegas venture). However, people like Dick Taschek, Dick Baker, and Rod Spence had beaten the odds many times before, liked what we were trying to do, and somehow managed to arrange for the funding that we needed to keep the HDR Project moving. They did it again with regard to the drilling of our first deep exploratory hole.

12.3.1. Preparations. On the basis simply of geologic information and preliminary temperature-gradient data from our shallow heat-flow holes, in the early spring of 1972 we began to plan for a deep (about 3000-ft) exploratory hole west of the caldera—long before we had any results from the 600-ft holes. Our principal reasons for wanting such a hole were

- to sample the complete geologic section of the Jemez Plateau from the surface down into unaltered, crystalline basement rock;
- to measure temperature gradients through the section and better evaluate the rate of terrestrial heat flow within what was evidently a very large geothermal area;
- to recover samples of the basement rock for petrographic analysis, structural study, and physical and mechanical-properties measurements;
- to determine the *in situ* permeability of the basement rock, the fluid pressure required to fracture it hydraulically, and—to the degree possible—the characteristics and orientation of the fractures produced; and
- to investigate drilling problems and the hydrology in the area.

Fortunately for us, Rod Spence found a pocket of money that could be applied to the cost of drilling the deep exploratory hole. I do not know the source of these funds but assume that their availability was somehow associated with the demise of the Rover Program. It was "operational" funding that, within limits, could be transferred within the Laboratory, but it had to be spent by the end of FY72 (on June 30) or it would revert to the AEC. Accordingly, we hurried.

On April 30, 1972, the day that heat-flow hole B was completed, I submitted a purchase request (PR) for "Special Project: Geothermal Energy." It was to "Drill one hole approximately 3000-ft deep according to specifications provided by LASL with regard to location, casing schedule, etc." Our estimated cost for this was $25,000 to cover rig mobilization and demobilization, bits, actual drilling (including a limited amount of drilling with air), and control of lost circulation. This did not include the costs of casing (estimated at $10,000), cementing ($2100), or the services of a drilling superintendent ($2000)—whom we thought we might obtain from Fenix and Scisson. All of this amounted to $39,100, not accounting for LASL salaries or providing for possible contingencies—of which, it turned out, there were plenty.

This PR was originally approved by Dick Baker, my division leader. Then it was redone on a different form and approved by Rod Spence, the N division leader, who headed the Rover Program. It was subsequently manipulated by Roger Westcott, the N Division financial wizard, to specify how costs should be charged and to include John Rowley, the N-7 group leader, as my corequester so that—to avoid the complexities of an interdivisional fund transfer—the charges could be routed through John's N Division group. Finally, as LASL PR 622166, it was signed by Franklin P. Durham (Rod's alternate division leader) and sent over to our Supply and Property department. This sounds like a lot of messing around, which it was, but in those days we got a lot of free help when we needed it—and fast!

In fact, it was on April 14 that Jim Sahling short-circuited the system and sent out requests for quotations on this drilling operation to several drilling companies. The RFQ specified drilling at 12 1/4-in.-diameter to 260 ft for setting 10 3/4-in. surface casing through the volcanics and into the Abo Formation; drilling at 9 7/8-in.-diameter to about 1600 ft for setting 7 5/8-in. intermediate casing through the Abo into the Magdalena Group; drilling at 6 3/4-in. to a depth between 2800 and 3800 ft, through the Magdalena Group and into the Precambrian basement, this section to be cased at 4 1/4 in.; and finally drilling at 3 7/8-in.-diameter for 200 ft below the final casing depth. Below the surface casing, 1-ft-long cores were to be taken at 250-ft intervals section, at all significant formation changes, at 100-ft intervals through the first 600 ft of the Precambrian, and at 200-ft intervals thereafter.

The RFQ requested quotations on a cost-per-hour basis and inclusion of mobilization and demobilization charges. However, it provided for reimbursement not to exceed $1500 for lost circulation materials, reimbursement not to exceed $800 for materials and services required for water shut-off; reimbursement not to exceed $5000 for drill bits; and expenditure of any of these funds not used for the purposes listed to deepen the hole further. Casing, a surface flange connection, and cementing services were to be supplied by LASL. The LASL drilling supervisors were to be Don Brown (N-7), Bill Purtymun (H-8), Francis West (J-6), and Scott Houghton (Fenix and Scisson).

Data from our heat-flow holes indicated that there was a slow increase in heat-flow rate as we moved northward along the western edge of the Valles Caldera. However, the USGS had reported that in a relatively inaccessible area still farther north, there was evidence of magnetic and gravity anomalies, fault structures, and minor seismic activity. To play it safe, for easier access, and because heat flow in the area that we had already investigated was certainly high enough for our purposes, we decided that our deep exploratory hole should be drilled in the general vicinity of our intermediate-depth heat-flow holes. A suitable location in that area had already been found, beside a forest road in Barley Canyon. The area was nearly flat, contained no standing timber except a few small aspen, and was large enough to contain a drilling rig, other necessary equipment, a mud pit, a small trailer, and a few vehicles. Accordingly, on a Forest Service map attached to the RFQ, the probable drilling site was shown in Barley Canyon—at the approximate center of a triangle formed by heat-flow holes A, B, and D, and about 2 1/2 miles west of the main ring fault bounding the Valles Caldera. With the help of Bud Wingfield and others at LAAO, we were arranging with the U.S. Forest Service for this special use of the land, which is in the Jemez District of the

Santa Fe National Forest.

By May 1, 1972, we had received and evaluated bids from several drilling companies and, on the basis of capabilities and cost, had selected Roberts Drilling Corporation of Farmington, New Mexico, as the contractor to drill Geothermal Test Well No. 1 (well GT-1) in Barley Canyon. We prepared a purchase request and on that day submitted it to our Supply and Property Department, accompanied by a memorandum from me to Bob Van Gemert explaining the purpose of GT-1 and indicating the urgency of getting it drilled before FY72 ended on June 30. The memo did not explain that the money for drilling the hole would disappear if it were not spent by that date, but of course Bob already knew that. The memo did explain that most of our summer's work and that planned for FY73 depended on having GT-1 completed during the spring of 1972; that the dry season in the Jemez Mountains was approaching, during which the danger of forest fires might prevent us from drilling in the National Forest; and that available drilling rigs were scarce and getting scarcer, so that we had better get one committed to the job while we still could.

There were, of course, other arrangements to make, and somehow they got made. Among them were the following:

- All of our heat-flow holes in the Santa Fe National Forest had been drilled under a Special Use Permit issued by the U.S. Forest Service. Working largely with Fred R. Swetnam, the District Forest Ranger based in Jemez Springs, the original permit was amended on May 5, 1972, to cover the drilling of the deep exploratory hole in Barley Canyon. The amendment covered improvement of the forest road leading to the site, removal of six aspen trees, leveling the site, construction of a mud pit, restoration of the site when drilling was completed, and such incidentals as provision of chemical toilets and an on-site fire truck. It stated that "The only adverse environmental impact anticipated is temporary soil disturbance resulting from leveling the site and digging the pit."
- We rented a 16-ft camping trailer with sleeping and cooking facilities to house our on-site manager and staff in Barley Canyon and arranged for the Zia Co. to pick it up, clean it, deliver it to the site when the site itself had been prepared, and eventually to clean it again and return it to its owner.
- We also arranged with the Zia Co. to haul water to the site for drilling and domestic use and eventually to retrieve any unused pipe and return it to Los Alamos.
- We arranged with Orson Anderson at UCLA for thin-sectioning and petrographic analysis of core samples from GT-1 and from our earlier heat-flow holes. (More about this arrangement later.)

12.3.2. Drilling. Unfortunately, my records of the actual drilling of well GT-1 are almost nonexistent, and no formal report on it was ever written. Such correspondence and memos concerning it as I have found in my old files deal more with financial problems than with technical details of drilling and well-completion operations. We did, however, manage to accomplish most of our major objectives, although with considerable pain—both technical and financial.

Well GT-1 was spudded on May 9, 1972, by Roberts Drilling Corporation, at the selected site in Barley Canyon. The first 160 feet of drilling were in the surface volcanics (Bandelier and Abiquiu Tuff), which drilled easily. From 160 to 1070 feet, it was through the Abo Formation (the "Permian red beds") consisting primarily of shales and sandstones, which frequently are poorly consolidated and contain some clay lenses and a few thin beds of limestone. Because of caving of the hole walls, swelling clays, normal equipment problems, and coring requirements, there were a number of drilling delays. With each delay, much of the hole filled with water from several aquifers in the Abo Formation. We were drilling with air for cuttings removal and the necessity of blowing that water out of the hole before drilling could be resumed added considerably to those delays. (In this situation, drilling with air was obviously a mistake. In later drilling campaigns we instead drilled with water for cuttings removal and then with mud.)

Between 1070 and 1815 feet, drilling was in the Magdalena Group, and from 1815 to 2015 feet in the Sandia Formation. Both consist largely of cavernous limestones, with some interbedded layers and lenses of clay, shale, and sandstone. Here again there were serious drilling problems, principally because of swelling clays and the difficulty of removing cuttings from the cavernous formations when using air as the circulating medium.

We had hoped to have GT-1 drilled well into the crystalline basement by the time that our Geosciences Advisory Panel (the GAP) assembled on May 18, 1972. However, on that date we were still a few hundred feet above the Precambrian surface (we didn't know quite how far) and we were having problems with stuck drilling tools because of swelling clays and caving of poorly consolidated formations higher in the hole. Therefore, when Don Brown appeared to report to the panel on the morning of May 18, he did not bring along the hoped-for piece of hot granite (which we had intended, if necessary, to warm up on a hot plate before displaying it to the panel). Instead he reported that we still had a few hundred feet to go to reach the crystalline basement, and that we were entirely out of drilling money so that we would have to dismiss the drilling rig at midnight that night.

The panel reacted appropriately and emphatically, and insisted that Dick Taschek take some sort of emergency action so that we could continue drilling. Dick responded nobly. He disappeared from the meeting for a couple of hours, during which he apparently twisted the arm of Duncan P. MacDougall, our assistant director for weapons programs. Duncan had a very tough arm but a fairly soft heart. Accordingly, Dick came back to the panel meeting with the promise of an additional $50,000 for our drilling operation, and in those days an oral commitment from Duncan to Dick, from Dick to us, and from us to the tool pusher was all that was needed. Don hurried back to Barley Canyon and told the driller to keep on drilling, which he did. (Of course I don't know where Duncan's $50,000 came from. He told me later that he was "bending things considerably" when he committed it to us, but that he certainly didn't regret having done it. Of course we didn't regret it either; we were and still are extremely grateful.)

Because no deep holes had ever been drilled in the area in which we were drilling GT-1, nobody knew exactly how far down it was to the top of the Precambrian basement. As the end of the fiscal year approached, we had reached a depth of 2410 ft and we were sure that we were close to it. The common guess was that at that depth we were in the lower member of the Sandia Formation, a dense, siliceous limestone. However, some chips that appeared to be granite were found in the drill cuttings from the last few feet of drilling, so another guess was that we had reached a "granite wash" on top of the weathered basement surface. Don Brown, on the other hand, thought that we had already reached the basement. Therefore, after the final 5-in. casing had been cemented in to a depth of 2400 ft and the hardened cement left in it had been drilled out, Don insisted on one more bit run to confirm his conclusion. This was done with a 4-in., three-cone carbide-insert, roller bit. The circulating medium was water and the final depth reached was 2430 ft. The rock was very hard and drilling was slow. With the 5-in. casing in place to prevent caving down of material from higher formations the cuttings recovered were granitic rock mixed only with traces of cement—confirming Don's conclusion. We were, in fact, well into the basement rock, although not nearly as far as we had hoped to be.

Subsequent examination of cuttings samples in the laboratory showed that we had in fact reached the Precambrian at a depth of 2105 ft and had drilled to our final depth primarily through gneiss, entering a reddish-brown granite at the very bottom of the hole. The gneiss had drilled very much like the dense, siliceous limestone just above it, and our previous uncertainties were due to the fact that, in the field, the very fine gneiss cuttings were hard to see among the coarser and much more common particles of sedimentary rocks that had caved down from higher in the hole.

On June 1, 1972, we had reached a depth of 2430 ft, penetrated 325 ft into the Precambrian basement, committed all of the available drilling money (including Duncan's $50,000), cased and cemented the hole to 2400 ft, dismissed the drilling rig, and left GT-1 in a condition such that we could come back later to deepen it further. There was then a short delay while we scrambled for some more money so that we could drill ahead.

We had an agreement with the Forest Service that, aside from the samples saved for laboratory study, drill cuttings and drilling fluids collected in settling ponds on the site could be removed and distributed along nearby roads—to improve the roadbed and disperse any drilling additives sufficiently so that they would not damage any of the vegetation. The access roads to the drilling site certainly needed improvement, this method of cuttings disposal was a great convenience for us, and there has since been no indication of environmental damage. Otherwise, the arrangement was a fiasco. When it rained, the fine drill cuttings turned to a thin mush, making already marginal roads in the vicinity nearly impassable. Fortunately, the cuttings have since disappeared in the roadbed and surrounding terrain, again with no indication of environmental damage.

When the weather is good, Barley Canyon is beautiful (Fig. 11). However, during much of the time that we were working at the GT-1 site, we were up to about there in either mud or snow—and we certainly contributed to the surplus of mud.

12.3.3. The need to deepen GT-1 further.

Because the upper surface of the Precambrian had been eroded for some millions of years before the overlying sedimentary formations were deposited and because the uppermost section drilled into the basement rock was an augen gneiss overlying a true granite, we were not sure either that we had reached unweathered material or that the rock we had penetrated was truly representative of the Precambrian basement beneath the Jemez Plateau. Further, we felt that we needed a much longer section of open hole in the basement rock to permit good temperature-gradient measurements, a series of hydraulic-fracturing experiments, and a more comprehensive study of the physical and mechanical properties of a representative section of that rock. Accordingly, in a memo to Bob Van Gemert on "The object and urgency of extending the existing geothermal exploration hole," dated June 14, 1972, I stated that we were "under increasing pressure to reenter the hole promptly and extend it as quickly as we can into unaltered granite." (The pressure, I suppose, was largely self-imposed, but there also was some from our advisory panel and from interested people elsewhere in the Laboratory. We were working hard to promote FY73 funding from DAT for a major HDR experiment, and we badly needed assurance from GT-1 that the basement rock under the Jemez Plateau represented a suitable subterranean environment for such an experiment.)

In my memo to Bob, I listed estimated costs for using diamond core drilling to deepen GT-1 by 500 ft in order to penetrate the granite sufficiently to insure that it had entered unweathered rock that was representative of what we might expect to encounter at still greater depths. Including rig time, bits, core barrels, water hauling, well logging, associated equipment, site preparation and cleanup, and 20% for contingencies, the estimated total cost was $56,400. However, I pointed out that—as in any other wildcat drilling operation—we might encounter some expensive surprises, as had occurred in abundance in getting GT-1 down to 2430 ft. In the absence of major surprises, I predicted a drilling time of 20 to 30 days, and noted that an appropriate drilling rig was available that could be on site within two days. (I assume that it was Don Brown who provided me with these estimates and that it was he who had located that drilling rig.)

12.3.4. Drilling ahead. Nothing in my records describes in detail the deepening of well GT-1 or where the money to do it came from. However, within days after my memo to Bob Van Gemert was written, a lighter, less-expensive rotary drilling rig was on site. Drilling with a 4 1/4-in. diameter diamond coring bit and circulating water to cool the bit and remove cuttings, the hole was deepened from 2430 ft to its final depth of 2575 ft. We stopped drilling on June 30, 1972, the last day on which we could spend FY72 operating funds. We were far short of the additional 500 ft of hole that we had

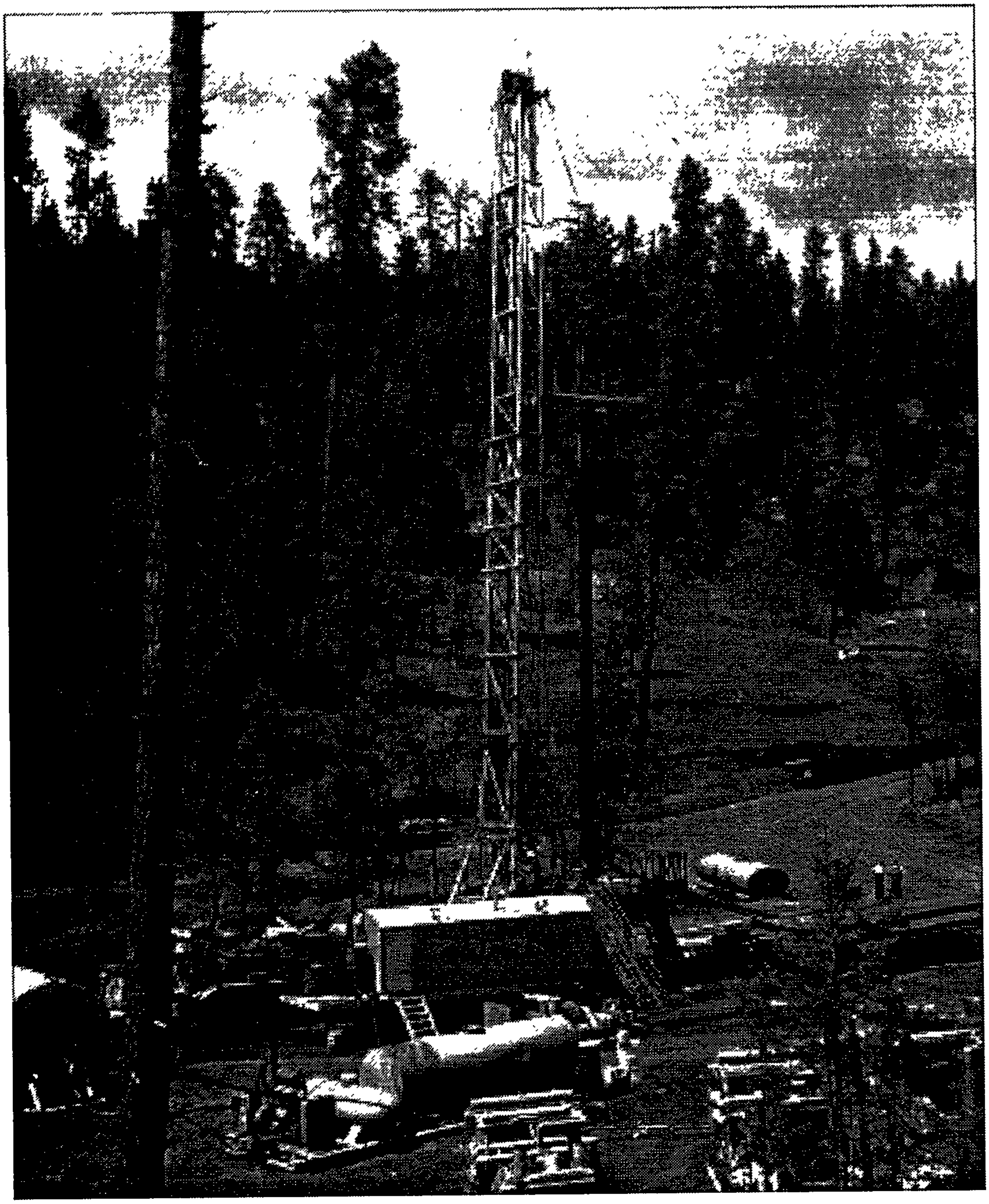

Fig. 11. Roberts Drilling Corporation rig drilling well GT-1 in Barley Canyon.

hoped for, but the extra 145 ft and the cores recovered while drilling it were subsequently very useful for our experiments. We also were surprised by the lithology; it was not just granite that we encountered as the hole was deepened.

From the top of the Precambrian basement at 2105-ft depth, altogether we had drilled through 325 ft of augen gneiss, then 50 ft of true granite, another 40 ft of gneiss, and finally 55 ft of amphibolite. The rock temperature at the bottom of GT-1 (after temperature equilibrium had been restored) was 100.4°C, and from 2400 to 2575 ft the hole was left uncased. We had penetrated 470 ft into the crystalline basement, although it obviously was not all the uniform pink granite that, in our innocence, we had expected. However, it was an excellent hole for some preliminary experiments.

At this point FY72 had ended, and we could only plan experiments to be conducted in our first deep exploratory hole—and pursue the funding that would make it possible for us to conduct them.

<u>12.3.5. Some financial mysteries.</u> Here the dates, order, and details of the financial events related to the drilling of GT-1 get a little fuzzy, and I hope that the statute of limitations has run out on that aspect of the HDR Program.

After GT-1 had been completed initially at 2430 ft and the drilling rig had been dismissed, the time came to settle the account with the drilling contractor. In a TWX to Jack Roberts of Roberts Drilling Corporation dated June 16, 1972, Donald N. ("Don") Bryson—another good and enduring friend in our Supply and Property Department—confirmed "previous authorizations to expend up to $72,500 for drilling services in Barley Canyon," and authorized expenditure of "an additional $50,000 for further services there." This brought the maximum authorized expenditure for drilling GT-1 to that depth to $122,500. (The additional $50,000 was of course that made available by Duncan MacDougall. However, Don's TWX noted that there had already been a cost overrun of approximately $6000—perhaps for that final run which showed that we had indeed reached the basement—so that the additional amount actually made available was only $44,000.) At the bottom of my copy of Don's TWX is an unsigned, handwritten note saying "verbal approval to send this TWX rec'd in telecon Blackwell/Van Gemert 6/16/72~3:30 PM." That would be H. Jack Blackwell, then manager of AEC's Los Alamos Area Office—another good, helpful and broad-minded friend of HDR—and Bob Van Gemert, then head of LASL's Supply and Property Department. At least, whatever its source, spending the $50,000 had been officially sanctioned, and in fact it had indeed been spent.

Our original cost estimate for drilling services on April 13, 1972, was $25,000. (The additional costs of casing, $10,000; cementing, $2100; and the services of a drilling superintendent, $2000, were to be covered separately by LASL—a total of $14,100.) The RFQ for drilling services, dated April 14, 1972, further provided for reimbursements to the drilling contractor not to exceed $1500 for lost circulation materials, $800 for water shut-off, and $5000 for drill bits—a total of $7300—with the provision that any of this money not spent for the designated purpose was to be used to further deepen the hole. However, in reviewing the problems encountered by Stewart Brothers in drilling our 600-ft heat-flow holes, we concluded that the drilling rig for GT-1 would need greater hoisting capacity than would be provided by the relatively small rig that we originally specified. Therefore, we later modified the PR to obtain a larger rig. Further, based on what we had learned about geothermal drilling experience elsewhere and on the fact that Stewart Brothers had encountered severe lost-circulation problems when drilling with mud, we also decided that GT-1 should be drilled using air instead of mud to remove cuttings from the hole. Accordingly, we increased the authorized expenditure for drilling services from the $25,000 stated in the original PR to $37,500. (I do not remember where we expected the additional $12,500 to come from.)

By May 15, after many problems and delays in penetrating the Permian red beds, well GT-1 had reached a depth of about 1200 ft in the cavernous limestones of the Magdalena Formation, where there were additional drilling problems, including caving down of material from the overlying red beds. Our contract with Roberts Drilling Corporation specified an hourly charge for drilling operations and, because of caving problems, swelling clays, coring requirements, our mistaken decision to drill with air, and normal equipment breakdowns, we were far behind schedule and about out of money. Accordingly, on that day John Rowley wrote a memo to Bob Van Gemert requesting that the authorization of $37,500 be increased to $47,500 to cover four additional days of drilling. (Presumably, this $10,000 was operating money transferred from the Rover Program.) On May 17, John confirmed this in a memo to R. O. Whitson, also of our Supply and Property Department, citing the need for prompt action to save the hole by drilling on into the basement granite and casing the well. Of course four days wasn't enough, and on May 18 Rod Spence wrote a memo to Bob Van Gemert authorizing addition of another $25,000 to the contract. (Again, presumably this was from Rover operating funds.) The total was now $72,500. Then when the promise of an additional $50,000 from Duncan MacDougall's weapons budget (also on May 18) brought the total to $122,500, it was possible for us to drill on, case the hole to 1600 ft—shutting off the worst of the trouble—and finally to extend it down into the granitic basement. The initial completion of well GT-1 occurred on June 1, 1972, at a depth of 2430 ft.

Major uncertainties remain with regard to the total cost of drilling and completing well GT-1. In a so-called "Preliminary Proposal" dated July 14, 1972, submitted to Jack Blackwell at the Los Alamos Area Office by J. B. Weldon of the LASL Engineering Department, the total cost was given as $145,250, broken down as detailed in Table 12-1. Rounded to $145,000 (in 1972 dollars) this figure has since been published as the total cost of well GT-1. The "Drilling" cost listed, $128,350, appears to be too high for the Roberts Drilling Corporation contract, under which we reached the depth of 2430 ft; for that we had available only $122,500, and presumably we spent it all on that contract. The difference of $5850 may, then, represent the cost of deepening the well by 145 ft from 2430 to 2575 ft. Unfortunately, I have no records concerning that operation. Obviously, however, because we were forced to dismiss the rig at the end of the fiscal year, we stopped far short of the 500 ft of additional drilling that I had proposed in my June 14 memo to Bob Van Gemert, and thus spent far less on deepening the hole than I had estimated in that memo. We were also some 425 ft short of the 3000-ft depth that we had hoped for, but we did finally have a hole well-suited to the field experiments that we intended to run.

The above listing does not include such things as the costs of hauling water to the Barley Canyon site, rental of the camping trailer, cementing services, and the salaries of LASL personnel involved in the operation. Therefore, the total project cost listed above undoubtedly is somewhat low. However, I have no records that permit me to improve on it, and will let $145,000 stand as the estimated cost of drilling and completing well GT-1—all of which, since the facility was considered experimental and temporary, was paid for with "operating expense" money eventually attributed to the supplementary research funds provided to the Laboratory by the AEC.

Table 12-1. Breakdown of Costs for Drilling Well GT-1

Engineering, design, and inspection	= $ 2,150	(Includes drilling superintendent)
Improvements to land	= 3,000	(Site preparation and cleanup)
Well casing	= 11,750	(LASL purchase)
Drilling	= 128,350	(including mobilization and demob.)
Total Project	= $145,250	

13. SOME MAJOR MEETINGS

As the AEC's and LASL's interest in developing geothermal energy sources expanded and became generally known, a number of meetings important to our HDR Program occurred—among them the following.

13.1. The second GAP meeting

In preparation for the second meeting of the Director's Advisory Panel on the Geosciences (the "GAP") I wrote to Orson Anderson, its chairman, on April 25, 1972. In that letter I summarized the current situation of the HDR Program, the problems that we foresaw in continuing it, some ideas that we had for solving those problems and broadening the program and, of course, soliciting the advice of the panel in all of those areas. Among things that I discussed, a few are of particular interest with regard to later developments in the HDR Program.

- Development of a microseismic system for fracture mapping. I suggested that this could develop into a rock-physics program that would study the generation and transmission of the acoustic signals emitted by local fracturing events as a hydraulic fracture grows. Since each event is a tiny earthquake occurring in an otherwise quiet environment, with a known energy release in a known geologic setting and at a predictable location and time, this could also develop into a novel and important earthquake study.
- Since both hydraulic fracturing and thermal-stress cracking result in local stress relief they might eventually be developed into a method of locally dissipating the strain energy accumulated along active fault systems—and therefore as a means of preventing subsequent major earthquakes along those faults.
- Broadening of the geochemistry activity to include such things as in-situ leaching of natural ore bodies, recovery of heavy isotopes from bomb-testing debris, and disposal of radioactive wastes.
- The importance of strong interactions with other organizations in such areas as hydrology studies, volcanology, and geothermal-prospecting methods.

The second GAP meeting was held at LASL on May 18-20, 1972. The morning and early afternoon of the 18th were filled with reports by LASL personnel involved in the HDR Program, followed by a report by Burt Slemmons and John Handin on their analysis of the aerial photos of the Valles Caldera region. The morning of May 19 was devoted to the Subterrene Program and to waste management and disposal problems at LASL. On the morning of May 20 the panel visited the Valles Caldera and our drilling sites west of it. General discussions with the panel occupied all three afternoons as well as our coffee breaks and luncheon periods.

Dick Taschek introduced the HDR presentations on May 18 which, in order, were the following:

1. Don Brown reported on the progress of our internally funded drilling and well-logging activities to that point in FY72 and on our proposal for further exploratory drilling in FY73. On that day GT- 1 had reached a depth of 2420 ft, and we were proposing in FY73 to drill a second deep exploratory hole, GT-2, at Fenton Hill—about 1.5 miles south of the Barley Canyon site of GT-1.
2. In preparation for discussion of our proposals to DPR and DAT, I explained to the panel what a "Form 189" and a "Form 189a" were. (A 189 was a fairly simple request for funding, and a 189a was a much more detailed description and justification of the scope of the proposal together with explanations of funding and manpower requirements, etc.) Then I briefly described the three 189s already submitted to DPR and the fourth (on seismic studies) still in preparation, and also our 189a proposal to DAT for a "Dry Geothermal Source Demonstration."

Finally, I discussed our funding prospects and uncertainties and the tentative schedule of our proposed future experiments—which, of course, was contingent on AEC funding. By then we were confident of financial support from DPR for our geophysics and geochemistry activities and optimistic about major funding from DAT for our first big field experiment. I noted that there was ARPA and NSF interest in HDR, although we had not so far submitted proposals to either of them.

I also mentioned that Dave Howell would visit LASL during the following week in connection with his economic study of HDR; that we were certainly gambling in some areas (such as the use of air-drilling in GT-1 and site selection for GT-2); and—to the obvious dismay of most of the GAP members—that, if necessary, we would sacrifice some science in order to accomplish an engineering demonstration of the viability of HDR as a major new energy resource.

3. Bob Potter discussed our seismology proposal and our plans for work in developing and applying seismic equipment and techniques.
4. John Balagna discussed the geochemistry proposal and our plans for implementing it.
5. John Rowley discussed our need for fracture-dynamics studies and what might be possible in satisfying it.

Burt Slemmons and John Handin then presented their analysis of the aerial photographs of the area on the Jemez Plateau, west of the Valles Caldera, which we believed was our best choice as the general location for our first major HDR field experiment. They found that in that area there were faults outlining blocks about 1 by 3 kilometers in their horizontal dimensions. They felt that granite blocks in the Precambrian basement would have similar dimensions, although they noted that Burt's field investigations later in the summer might find evidence of a smaller block size. For the time being, however, they believed that deep holes within 1000 ft of GT-1 would all be in the same fault block so that information gained from GT-1 would be useful in planning experiments to be run in those additional holes.

During these presentations, in their subsequent discussions with us, and in memoranda to Harold Agnew, the LASL director, the panel offered many comments and recommendations concerning our past, present, and future activities. Among the major ones were the following:

1. The panel was pleased with our general progress on the substance of the recommendations that they had made at their first meeting.
2. They also praised our progress in drilling GT-1, which exceeded their most optimistic expectations. (We were less pleased by it, having been even more optimistic.)
3. They recommended that, if possible, core should be taken every 100 ft as GT- 1 was deepened and that cores should be characterized with regard to

    - identity and proportions of all phases present, by petrographic analysis (to be done by Priscilla Dudley under the direction of Hat Yoder, Burt Slemmons, and Orson Anderson);
    - orientation and distribution of microfractures and mineral alterations (also to be done by Priscilla);
    - compositions of grain boundaries in areas likely to be leached, by microprobe analysis (again by Priscilla);
    - determination of whether or not there was preferred orientation of minerals and fractures (by Priscilla); and
    - measurements of compressibility, sound velocity, permeability, and thermal conductivity (by LASL materials groups).

4. Hole GT-1 should be cored again after one year to observe changes in the granite.
5. When GT-1 has been completed to its final depth, the following studies should be made in it.

    - Downhole photography of the bore hole walls in the uncased bottom section to observe the naturally occurring cracks in the granite and, later, those produced by hydraulic fracturing;
    - Resistivity, gamma response, continuous sound velocity, and caliper logs of the

uncased section;
- Pump-up and pump-out tests to measure *in situ* permeability of the granite;
- Measure pore-pressure before fracturing;
- Measure pressure decay before as well as after hydraulic fracturing;
- Repeated temperature logs over the entire length of the hole;
- Small-scale hydraulic-fracturing experiment, for stress measurement as well as feasibility demonstration;
- Attempt seismic monitoring of the fracturing experiment to determine the extent and orientation of the fracture and the character of any microearthquakes produced—with the seismometers as close to the fracture as possible, both vertically and horizontally.

6. As a visiting staff member at LASL during the summer of 1972, Burt Slemmons should make a detailed field reconnaissance of the area west of the caldera to confirm the conclusions from aerial photography and help interpret the petrography of core samples.
7. An effort should be made to investigate other potential HDR test sites, for example, in central Nevada.
8. An expert in rock mechanics should be added to the LASL staff very soon.
9. The panel should recruit a seismologist and a hydrologist to its membership to help in planning experiments in the development of HDR energy systems. (In part, this recommendation was satisfied in July 1972 by recruitment to the panel of John ["Jack"] Healy, a distinguished seismologist from the USGS at Menlo Park, California.)

Of course we could not accomplish all of these things immediately but, on the whole, the recommendations of the panel were sound and useful—and in most cases consistent with our own thinking and intentions. We managed eventually to satisfy most of them.

13.2. The Ogle Committee

In his memorandum of June 23, 1972, Tony Ewing had announced that "coordination of supporting research and development effort common to the general engineering field demonstration projects will be monitored by a working level basic studies task force composed of individuals actively engaged in project activities." Organization of this task force began within the next month.

Some time in late June or early July 1972, soon after he became director of the AEC's Division of Applied Technology, Jerry Johnson was seated on an airplane with Bill Ogle—probably, as usual, travelling from one AEC meeting to another. By then, Jerry had received geothermal-energy proposals from both LASL and LBL. It was a subject about which at that time he knew very little, but he thought that it was an appropriate area for DAT involvement and support. He felt that it would be wise to call together representatives of the various AEC laboratories to consider what DAT should do about it. Bill didn't know a lot about geothermal energy either (which he freely admitted) but, from his involvement in Plowshare programs and his dealings with us at LASL, he was interested in it and optimistic about the energy contribution that it could make. He intended to retire from LASL later in 1972 but was arranging to continue his association with AEC as an independent consultant. In that capacity and at Jerry's request, he agreed to convene and chair what officially became the AEC Coordinating Group on Geothermal Energy. However, during Bill's long tenure, the group was always known to those involved in it as the "Ogle Committee." Bill learned fast, worked hard at doing so, and was an excellent chairman for the committee. In particular, he rarely disagreed directly with its members; instead he would discuss things quietly with us, and we would usually find that we were soon agreeing with him. Fortunately, he was usually right.

The first meeting of the Ogle Committee was held at the AEC's San Francisco Operations Office

(SFOO) on July 18 and 19, 1972. Those attending were

- Bill Ogle (Chairman), Mort Smith, and Lee Aamodt, from LASL
- Tony Ewing, from AEC Headquarters
- Alan Laird and Jack M. Hollander, from LBL
- A. E. ("Al") Sherwood and Milo Nordyke from LLL (Lawrence Livermore Laboratory)
- Donald H. ("Don") Stewart from BNW
- Gordon Eaton from the USGS and Bill Leonard from NWC, both as "observers," although both became very much part of the committee.

In a letter to Jerry Johnson dated July 21, 1972 (and distributed to all of us who had attended), Bill summarized and commented quite honestly on the meeting. I did the same in a memo to Dick Taschek. Bill and I agreed on most aspects of the meeting, although of course his viewpoint and mine differed in some particulars.

As Bill's letter to Jerry pointed out, the committee really didn't know until the second day of the two-day meeting what we were supposed to be doing, and by then there wasn't enough time left to do very much or to do that very well. (I, for one, went to the meeting not knowing the name, composition, status, or purpose of the committee or that it would ever meet again. I was simply invited by Bill to be there on a certain date, and I think that was also true of most of the other attendees.) At first we believed that we were supposed to outline a national geothermal energy program and then to detail the AEC's piece of it—including an AEC geothermal budget for FY74. Fortunately (primarily, I think, as a result of Bill's phone calls to Jerry Johnson) we finally learned that we were not asked to lay out another national program; several already existed, all more carefully considered than would have been possible for us in a single day. Instead we were asked to recommend to DAT a 10-year program to begin in FY74, including the year-by-year funding needed to support it. Of course we couldn't do even that intelligently in a single day and, as Bill reported in his letter to Jerry, "We simply added up what the four AEC organizations wanted to do," together with their first guesses as to how much each would cost. Of course the result left a lot to be desired.

Early in the meeting, Tony Ewing announced firmly that AEC would spend $1.5 to $2.0 million on geothermal energy studies in FY73 (the current fiscal year) and that LASL would receive $1.0 to $1.5 million of it for our HDR Program. I believed him and hastily adjusted our activity and cost schedules accordingly. At Tony's insistence, I extrapolated the schedules over 10 years; added demonstration power plants; and provided for investigations of HDR energy possibilities in lava beds, of water availability, and of joint demonstration projects with industry. At Bill Ogle's request, I also included special investigations of environmental problems and of direct (nonelectrical) uses of HDR geothermal energy. Of course I was not prepared for all this, had no reference material with me, and had only a few hours in which to do it. With Lee Aamodt's help and a certain amount of imagination, I made some good guesses—and some very bad ones. For FY73, we at LASL had already asked DAT for operational funding of $3.651 million for operating expense plus $0.517 million for equipment, and this did not include the add-ons requested by Tony and Bill. With only the $1.0 to $1.5 million that Tony said we would receive, I had to reschedule much of the work originally proposed for FY73, shifting it into FY74. This included drilling and fracturing the second deep hole at Fenton Hill and providing the surface facilities needed there. That brought my estimate of our funding requirement for FY74 to about $4.7 million.

As Bill stated in his letter to Jerry Johnson, "The LASL hot rock program was clearly endorsed" by the committee. However, the committee recommended only $2.127 million for HDR in FY74 (which, incidentally, included $200 thousand under "Magma" to investigate energy extraction from lava pools and underground reservoirs of molten rock. The committee also recommended $2 million to support the LASL Subterrene Program in FY74.) The total FY74 AEC funding for geothermal energy studies recommended by the committee was

$11.022 million. We didn't really try to be specific about funding for subsequent years; we had already run out of time in putting together just an FY74 estimate.

Allen Laird was the only one present besides Lee and me who had come to the meeting with a firm proposal for AEC funding of geothermal energy work. Essentially, his proposal was that LBL should plan and manage the nation's geothermal energy program (and farm out to the rest of us only such small things as LBL could not conveniently set up to do itself). To my amazement, the rest of the committee took this proposal seriously and recommended FY74 funding of $1.4 million for it!

It turned out that Don Stewart had previously suggested and Tony Ewing had previously approved a $250 thousand BNW study of the cost-effectiveness of geothermal energy, and this was not open to discussion. However, neither Don nor anyone else had come to the meeting with a Plowshare geothermal proposal, and Don became increasingly aggrieved by this omission. In this he was supported by Ewing, Nordyke, and Sherwood, and to some degree by Ogle and Aamodt—all of whom had worked together on other aspects of the Plowshare Program for peaceful applications of nuclear energy. Finally, in the last minutes of the meeting, Don announced that, under "Dry rock," Plowshare should be assigned year-by-year funding equal to that listed for the LASL HDR Program. Since the majority of the committee were Plowshare alumni substantially that was done—with no serious discussion and no consideration of how that money would or could be spent. Presumably the cost would be covered by separate AEC funding from the existing Plowshare Program. (This did eventually result in an HDR project at Marysville, Montana, managed by BNW. It turned out to be non-nuclear, funded by NSF instead of the AEC, and not "dry rock" after all.)

Nordyke and Sherwood had also come to the meeting with no LLL proposal for geothermal work, but they put one together overnight. It proposed that LLL find an industrial partner with an interesting geothermal energy prospect and then figure out some clever way to develop it. Even this was taken seriously and approved by the committee.

Bill's letter to Jerry listed some uncertainties that we felt needed further study and some suggestions for actions beyond those included in actual or anticipated proposals. These were the following:

- What are the real problems of hydrothermal energy development? We need to know what difficulties industry is really having and why it is not moving faster on its own.
- Natural steam problems (at The Geysers) should probably be left for industry to solve.
- Should DAT support development of binary cycle electrical power plants for geothermal energy applications? (Bill and I felt strongly that it should.)
- Industrial partners with appropriate prospects should be sought for R&D on hot-water geothermal energy. However, since such partners might not be found, the AEC should also seek a dedicated site for that purpose.
- Complete information services concerning geothermal energy are badly needed. The USGS may provide them but, if they do not, then the AEC should.

This was a strange, disorganized, and unsatisfying meeting, which left a great deal to be desired. However, it was the birth of what Bill Ogle developed into a strong and useful committee, advisory to the Division of Applied Technology of AEC and to the divisions that replaced it in the federal agencies that succeeded the AEC.

13.3. More Ogle Committee Meetings

The Ogle Committee met regularly until Bill died suddenly from a massive heart attack at Stanford University, on May 16, 1984, while he was chairing the 37th meeting of the committee.

In part because I was a charter member of the committee and was always there with an axe to grind, there were discussions of or related to HDR at most of its meetings, and these were too numerous to summarize here. They were, however, particularly useful during the first few meetings of the group, while we were trying to establish a formal, funded HDR Program at LASL and while the AEC was deciding just what its geothermal interests should be and where its money should be spent.

13.3.1. Second Ogle Committee Meeting. This was held on October 26 and 27, 1972, again at SFOO. Except for Al Sherwood and Gordon Eaton, all those present at the first meeting of the committee also attended this one. New members added to the committee were Jim Bresee and Ron Stearns from the AEC, Pat Muffler from the USGS, Richard G. Stone from LLL, Kenneth F. ("Ken") Mirk and Frank Morrison from LBL, and Richard N. ("Dick") Lyon from ORNL.

Most of October 26 was spent discussing a draft of the Hickel Committee Report, which is discussed below. The Ogle Committee agreed that much of what was listed was appropriate for AEC support, some for USGS action, some for NSF, and a great deal for joint government-industry support if that could be arranged. Disagreeing with a major assumption of the Hickel Report (that generation of electricity was the important goal of geothermal-energy development) most members of the Ogle Committee felt that direct uses of geothermal heat (in such applications as space-heating, production of chemicals, and water desalination) were probably even more important.

The Ogle Committee strongly supported a major program of HDR development, with AEC or NSF support, or both. It considered HDR much more promising than development of geopressured geothermal systems, which was another contender for major federal funding. Don Stewart made another pitch for investigation of what he and others believed was an excellent HDR prospect at Marysville, Montana.

There was considerable discussion of existing impediments to commercial geothermal development, and what should and could be done to overcome them. These included such things as multiple licensing requirements, problems in leasing federal lands, and the lack of such governmental financial incentives as depletion allowances. While the Ogle Committee saw no way in which we could help much with such problems, we did feel that government action in such areas as underground resource evaluation and joint demonstration projects with industry could help to accelerate commercial development.

On the morning of the second day of the meeting we were joined by Fred Klemach, Warren D. McBee, Hugh Mathews, and Harvey C. Jacobs of Sperry Rand Vickers, who discussed with us their concept for a downhole turbine pump for geothermal wells. That afternoon we were joined by Herb Rogers and James T. ("Jim") Kuwada of Rogers Engineering who discussed binary power plants and participated in our discussions of what the federal government could do to accelerate the commercial development of geothermal energy

13.3.2. Third Ogle Committee Meeting. The Ogle Committee met again on February 8 and 9, 1973, at the AEC's Nevada Operations Office in Las Vegas. Present were

- Bill Ogle, Jim Bresee, Ken Mirk, Dick Lyon, Don Stewart, Mort Smith, Lee Aamodt, Bill Leonard, and Pat Muffler, all of whom had attended the second meeting of the committee; and four new members:
- Harold Wollenberg, from LBL and John H. ("Jack") Howard, Gary Higgins, and Roy Austin, all from LLL.

Several representatives of various industrial organizations who also attended parts of the two-day meeting, included

- David J. Goerz and Larry O. Beaulaurier, Bechtel Corporation;
- Art Kohl, Atomics International;
- B. C. McCabe and Joseph W. Aidlin, Magma Power;
- Leonard J. Keller, Keller Corporation; and
- Michael C. Sodano, Asian Economic and Technical Services Co.

Reports were presented on the prospects of federal funding of geothermal-energy R&D, on the problems and status of leasing federal lands for geothermal-energy development, and on the geothermal interests and activities of LBL, LLL, ORNL, BNW, Bechtel, Magma Power, Atomics International, and the Keller Corporation.

Of special interest was Don Stewart's report on the Marysville, Montana, geothermal anomaly, which he said was 4 or 5 miles in diameter and presumably resulted from the presence of a buried granitic pluton less than 80,000 years old. The USGS had been studying the area, and Pat Muffler suggested that there was a narrow, elongated, north-south-trending pluton whose upper surface was at a depth of about 1.5 km (4900 ft) at the north end and 2.5 km (8200 ft) at the south end. Battelle Northwest had mailed a proposal to NSF the previous week for an HDR experiment at Marysville, and Don said that the RANN Program had money available to fund it. They were still considering the use of nuclear explosives to create a fractured reservoir in the pluton, from which to extract heat. The BNW plan was to begin a geophysical investigation of the anomaly in June 1973 and continue it through the summer; to drill an 8000-ft hole into the pluton in the spring of 1974; and then to evaluate the HDR thermal reservoir and complete detailed planning for the recovery of heat from it.

Finally, the committee prepared a prioritized list of geothermal programs that it recommended for support by DAT. In order of priority, the recommended programs were

1. power-plant development, jointly with industry, using geothermal hot water or two-phase water-steam production;
2. "Development of one or more hot dry rock energy removal and conversion systems. While early development of 'wet' systems is a high priority aim, real impact on the national energy picture will probably not come about without the development of systems to extract energy from deep hot dry rock. Demonstration of hydrofracturing, thermal fracturing, and, if necessary, nuclear fracturing, should be followed by joint demonstration power plant construction and operation as soon as the source is evaluated. The extraction of energy from hot magma (Alaska, Hawaii) should be studied as part of this item. The appropriate environmental studies (gaseous and particulate emission, noise, removal of dissolved salts, etc.) would be part of this work." (Mention of 'nuclear fracturing,' of course, was not my idea. Otherwise I was delighted with this recommendation and with the priority that it was given.)
3. direct, nonelectrical applications of low-temperature geothermal heat;
4. development and demonstration of methods of conversion of relatively low-temperature (150° to 350°F) geothermal heat to electricity, and of high-temperature downhole pumps; and
5. development of high-temperature drilling tools and downhole sensing devices and of more economical drilling techniques for deep holes—including new drilling concepts such as the Subterrene and water-jet drills.

13.3.3. Fourth Ogle Committee Meeting.

This meeting was held at the AEC's San Francisco Operations Office on May 24 and 25, 1973. Present were

- Bill Ogle, Mort Smith, Lee Aamodt, Don Stewart, Bill Leonard, Jim Bresee, Pat Muffler, Ken Mirk, Dick Lyon, Harold Wollenberg, Jack Howard, and Gary Higgins all of whom had attended the third meeting of the committee;
- Alan Laird and Jack Hollander, who had attended its first two meetings;
- Dana Kilgore and John Phillip from the San Francisco Operations Office, Richard Wood from the Idaho Operations Office, Paul Witherspoon and Paul Hernandez from LBL, Andy Lundberg and Fred Fulton from LLL, Stan Milora from ORNL and Barry Raleigh from the USGS, all of whom were new representatives to the committee.

Also present for parts of the meeting to discuss specific programs were

- David D. ("Dave") Blackwell from Southern Methodist University;
- James ("Jim") Combs from the University of California at Riverside;
- Jay Kunze from Aerojet Nuclear Co.;
- Jack Barnett from the Raft River Electric Cooperative; and
- Edward Schlender from the Snake River Power Association.

Jim Bresee presented a discouraging report on funding prospects for AEC geothermal energy projects. The Department of Interior had received $25 million for energy R&D and had established a new Energy Office to split up the money among various federal agencies. Of this apparently only $4.1 million would be available for geothermal—unless OMB released other R&D funds for it. The AEC still hoped to spend $4 million on geothermal energy, but there was no reason to hope for it before October 1973—i.e., during the second quarter of FY74. (However, Jim later asked me, by one week from the following Monday, to update our five-year plan for HDR development, this to go to Ray Zahradnik—who was both the geothermal coordinator at NSF and a member of an energy committee advisory to the federal administration.)

Representatives of most of the AEC laboratories reported on their geothermal programs and interests. I summarized our observations in our exploratory well GT-1, where various experiments were in progress at that time, and also described our Subterrene Program (the committee expressed its opinion that, like our HDR Program, the Subterrene Program "should be pushed by AEC if necessary.") Don Stewart discussed the Marysville Project. He expected that BNW would get an NSF contract to pursue it within the next day or two.

Dave Blackwell and Jim Combs discussed ARPA's interest in geothermal energy, which was primarily in relatively small (e.g., 10 MWe) electrical power plants and multiple-use systems at military installations. They were looking primarily at relatively low-temperature hydrothermal systems, assuming that the AEC would support the development of HDR energy systems. China Lake, California—which was also discussed by Bill Leonard—appeared to be a good candidate for an initial pilot development, to serve the China Lake Naval Weapons Center.

Jay Kunze, Ed Schlender, and Jack Barnett reported on their new proposal for a low-temperature hydrothermal development at Raft River, Idaho, to provide electrical power to the Raft River Electric Cooperative.

At the third Ogle Committee Meeting, Roy Austin of LLL had reported that the LASL estimates of the energy extraction capability of an HDR system (by Harlow and Pracht) were too high by a factor of ten. At this meeting, Jack Howard reported that Roy had since redone his calculations and now had results closely similar to those of Harlow and Pracht. Accordingly, Roy had recently written a letter to the editor of the *Journal of Geophysical Research* (where, I believe, his earlier results—as well as the referenced paper by Harlow and Pracht—had appeared in a similar letter) reporting his new results. However, in the new letter he had expressed caution concerning the effect on power generation of temperature drawdown with time—which, of course, was also one of our major concerns.

13.3.4 Subsequent Ogle Committee Meetings. The Ogle Committee continued to meet regularly through the transition from AEC to ERDA (the Energy Research and Development Administration) and from ERDA to DOE (the Department of Energy) until Bill's death at the committee's 37th meeting. It was then reorganized as the Division of Geothermal Energy (DGE) Coordinating Group on Geothermal Energy, and continued to serve as a useful advisory panel to the Department of Energy.

13.4. The Hickel Committee

In a letter to Dick Taschek dated March 24, 1972, Jack Vanderryn alerted us to an "assessment of geothermal energy" being prepared for the Office of Science and Technology (OST) and to a "Geothermal Resources Research Workshop" that was to be held later in the year and was expected to contribute to the OST assessment. He suggested that we keep in close touch with Don Stewart who would participate in that workshop, was manager of BNW's part of the AEC Plowshare study, and was therefore familiar with the AEC's interests in geothermal energy. (In fact, we saw quite a lot of Don over the years, particularly at meetings of the Ogle Committee and in connection with the Marysville geothermal project.)

Walter J. Hickel who had been both governor of Alaska and U.S. Secretary of the Interior, at that time was serving as an adjunct professor at the University of Alaska. He had submitted a proposal to NSF to hold a conference "To develop an assessment of the state of the art and to recommend a research program to provide the requisite knowledge for establishing the proper role of geothermal resources in providing (1) Additional energy to alleviate the nation's shortage; (2) Water to supplement present supplies; and (3) Mineral resources."

This proposal was approved and funded by NSF's RANN Program (Research Applied to National Needs). A planning meeting for the conference was held in Anchorage, Alaska, on May 8 and 9, 1972. It was chaired by Walter Hickel with Donald D. Dunlop as executive secretary, and attended by ten experts who had agreed to chair various panels at the conference, plus Jesse Denton of NSF and Dallas Peck of the USGS as "observers." (Los Alamos was not represented, which was probably appropriate. At that time we were certainly not "experts.") In addition to planning the geothermal conference—to be held in Seattle in September—this group made some preliminary recommendations for geothermal R&D, which were not formally published but apparently did contribute to what later became known as "The Peck Report."

13.4.1. The Peck Report. In his energy message on June 14, 1971, the president of the United States had called for a comprehensive assessment of energy technologies, to be conducted by OST in cooperation with the Federal Council on Science and Technology (FCST) and "appropriate Federal Agencies." In December 1971 OST asked the Department of Interior to assess the technology of geothermal energy and recommend a program of geothermal research and development. This was done by the USGS in collaboration with an informal Interagency Geothermal Coordinating Committee that included representatives of the Bureau of Reclamation, the Office of Saline Water, the USGS, the Bureau of Mines, NSF, the AEC, NASA, ARPA, the Bureau of Land Management, and the Environmental Protection Agency. The committee began meeting in September 1971 and so was well prepared to assist the USGS in its study. Later, there was also input from the Hickel planning committee described above.

The product of this USGS study was an informal report by the Panel on Geothermal Resources, Department of the Interior, Dallas L. Peck, Coordinator, entitled "Assessment of Geothermal Resources," dated June 26, 1972. Usually called "The Peck Report," it was the first comprehensive assessment of geothermal energy by a U.S. Federal agency and, the USGS having had very competent people studying geothermal energy for some years, it was excellent. It reviewed the state of knowledge, the existing technology, and the history of geothermal energy development in the United States; discussed the R&D needed to encourage and support growth of the geothermal industry; and described the ongoing and proposed activities of all agencies in some way involved with geothermal energy.

Appropriately for its time, the emphasis of the Peck Report was on hydrothermal resources—natural steam and hot water. However, it recognized "the enormous geothermal resource base" represented by thermal energy contained in hot but "relatively dry" geothermal reservoirs and among other things recommended first-year funding of $3 million "to investigate the artificial stimulation of geothermal reservoirs and the feasibility of tapping the thermal energy stored in hot, relatively dry rocks." Quite properly, the USGS has always been very careful in its terminology, and—because no rock is totally devoid of moisture—it has been annoyed by our use of the term "hot dry rock," which we don't even punctuate properly. When we happen to think of it and there are purists in the audience, we explain that we use "dry" in the sense common in the petroleum industry, where a well that is not an economical producer is called a "dry hole" even if there is a show of oil or gas or it is actually full of water. Usually it is only a few of our Laboratory editors that seem to notice the absence of punctuation.) In its description of the various federal programs, the Peck report noted that, to that time, the AEC had not had a formal geothermal resources program but that—in testimony before the Senate Appropriations Committee—it had indicated its intention to initiate one in FY73, with primary emphasis on "hot relatively dry rocks."

While the Peck Report of course preferred the USGS estimates of the magnitude of the U.S. geothermal energy resource, it did also report some less-conservative estimates—in particular those of Bob Rex, which were a lot less conservative. In some of his resource estimates, Bob included the thermal energy in HDR reservoirs, resulting in a very large increase over the USGS estimates (which considered only hydrothermal reservoirs). This was important in bringing the potential of HDR to the attention of some very influential readers of the Peck Report.

Finally, the Peck Report recommended an expanded five-year federal geothermal-energy program with first-year funding of $25 million, this increasing steadily to $44.5 million in the fifth year and totalling $209 million over the five-year period.

13.4.2. The Geothermal Resource Research Conference. This conference of "The Hickel Committee" was held at the Battelle Seattle Research Center in Seattle, Washington, on September 18–20, 1972. Bob Potter and I were invited to attend the conference which was a workshop involving about 60 people from energy companies, federal and state agencies, the United Nations, universities, independent laboratories, power companies, equipment manufacturers, architect-engineering firms, environmental groups, and legal advisors. Everyone present was involved in some way with geothermal energy, several were already prominent in that field, and many of the rest eventually became so. It was a rather remarkable group, and it met at a time when the United States was beginning to realize that it had a very serious energy supply problem. All of us believed that the federal government was about to embark on a broad and massive program to develop other domestic energy sources as alternatives to imported oil, and nearly all of us believed that geothermal energy could and should be an important part of the solution to that problem.

The workshop was to be divided into six panels instructed to review and make recommendations concerning R&D requirements in the following areas:

Resource exploration,
Resource assessment,
Resource development and production,
Utilization technology and economics,
Environmental effects, and
Institutional considerations.

In advance of the Seattle meeting, each of us was asked to submit a position paper on the subject of the panel on which he was expected to serve. Bob Potter was to be on the Resource Assessment Panel, and he submitted a paper called, "Geothermal Resources Created by Hydraulic Fracturing in Hot Dry Rock." Using the scanty data available in the literature at that time, he derived average conductive and radiogenic contributions to heat flow, average thermal conductivities of sedimentary and crystalline rocks, and—on a state-by-state basis—average depth to basement rock. Then, assuming that 10% of the land area was available for development and that 25% of the heat in HDR reservoirs could be recovered, he calculated for each state the heat above 50°C available from hot rock at depths between 3 and 9 km. Necessarily, a great many estimates and assumptions were involved, and Bob's results for individual states were pretty uncertain. His calculated value of the total thermal energy available in the United States from this source was 203,000 quads (where 1 quad = 10^{15} Btu). This was a brave and difficult effort, and the first of its kind. However, taking into account the information then available and his assumed minimum temperature, depth range, availability, and recovery factors, his estimate of the total U.S. resource base represented by hot dry rock was amazingly good.

I was to be a member of the Reservoir Development and Production Panel, and submitted a position paper called "The Development of 'Dry' Geothermal Reservoirs." It was much less elaborate than Bob's. In it I described a variety of methods that might be used to recover geothermal heat from crustal rock in a variety of geologic situations, including circulation through permeability created by fracturing with conventional explosives, with nuclear explosives, hydraulically, or by thermal-stress cracking; one-hole systems using continuous countercurrent circulation through concentric pipes or operating in the huff-puff mode; and waterflooding of permeable formations. However, my emphasis was on R&D needs for the geothermal industry as a whole, particularly in the areas of drilling and well-completion, creation of heat-transfer surface in the geothermal reservoir, fluid flow and heat extraction, geochemistry, rock- mechanics and fracture dynamics, seismology, geological and geophysical exploration and evaluation, and energy-conversion and direct-use systems. This was a very broad list of what I considered to be necessary R&D activities, and I urged that they be "carefully integrated, undertaken in parallel, and started promptly."

With Bob Potter and Bob Rex on the Resource Assessment Panel and Don Stewart, Tony Ewing, and me on the Reservoir Development and Production Panel, the Hickel Committee was made aware of the existence and energy potential of HDR geothermal reservoirs—including the concept of developing HDR energy systems by hydraulic fracturing. Don and Tony of course made the case for fracturing the rock by using nuclear explosives—a concept with which Bob Potter and I were careful to dissociate ourselves.

Each panel prepared and submitted a report which the editor of the final committee report interpreted, summarized, and frequently supplemented.

13.4.3. The Hickel Report. The final report of the workshop, titled "Geothermal Energy" and subtitled "A National Proposal for Geothermal Resources Research," was a slick-paper document published by the University of Alaska. The cover identified it as "A Special Report by Walter J. Hickel," although the title page listed him only as chairman of the Geothermal Resources Research Conference, with Donald D. Dunlop as secretary and Jesse C. Denton as editor.

In its published form, the Hickel Report was much less technical and detailed than the Peck Report, but it put even greater emphasis on R&D needs. It recommended a 10-year federal "Resource Assessment Research Budget, with first-year funding of $15.6 million, increasing steadily to $31.3 million in the eighth year and then diminishing to $24.0 million in the tenth year—for a 10-year total of $273.7 million. For R&D on "hot impermeable rock" (which it identified elsewhere as "hot dry rock") it included a total of $41.5 million, covering both the Plowshare approach and our hydraulic-fracturing method.

The Hickel Report has since been referred to in other geothermal literature principally as the source of outrageously high predictions of the amounts of electrical power, generated from geothermal heat, that would be on line by 1985 and by 2000. There is some justice in this, although there is more misunderstanding than justice. As an attention-getter, Table I of the report, as "The Geothermal Energy Resources Potential," listed 132,000 MWe (megawatts electrical) of power by 1985 and 395,000 MWe by 2000. However, the accompanying text made it clear that these were not projections of what would occur but rather were estimates of what would be possible if several assumptions regarding resource assessment, technology development, economic competitiveness, and R&D funding were justified. In fact, with inclusion of HDR, the resource base to support that much generating capacity did exist (i.e., the "Resources Potential" was there), although the time-frame listed for its development was impossibly short for anything less than a truly massive and very high priority national effort.

However, the text of the Hickel report was misleading in stating that these estimates came from the Peck Report, and were the ones that had "withstood the scrutiny of the largest range of expertise." In fact, the Peck Report preferred estimates of 19,000 MWe of installed generating capacity by 1985 and 75,000 MWe by 2000, although it did discuss several estimates made by others. The highest of these (by Bob Rex) was 400,000 MWt (thermal, not electrical, megawatts) that could be developed in the western U.S. within 20 years, much of which was from HDR resources. Apparently the editor of the Hickel report misunderstood and manipulated the most optimistic estimates mentioned in the Peck Report to get the numbers that appeared in his Table I—which, to repeat, was the attention-getter at the beginning of the Hickel Report and represented a "potential" not a prediction. It really had nothing to do with the body of the report except to indicate to the reader that the energy supply being considered was large enough to justify reading the rest of it. (That purpose could probably have been served as well by quoting the much more conservative preferred estimates of the Peck Report.)

The Hickel Report was much shorter and easier to read than the Peck Report (which was prepared inexpensively for a limited scientific audience). It was printed on slick paper instead of photocopied, had a fancy cover, was less technical and put even more emphasis on the R&D needed to make the geothermal industry grow. It was, in fact, the sort of document that important people in industry and government might actually take time to read—which, of course, was what was hoped for. It got the word out to a broad audience, in what was really quite a good report on an excellent workshop. Further, although its emphasis of course was on hydrothermal resources, it did give appropriate attention to HDR—giving about equal time to Plowshare and to our hydraulic-fracturing concept, which probably was fair at the time.

13.5. An American Nuclear Society Meeting and the fifth HDR book chapter

As a result primarily of the existence of the AEC's Plowshare Program, the American Nuclear Society (ANS) had organized an Explosives Division, which sponsored a special session on "Geothermal Energy Stimulation" at the Annual Meeting of the ANS in Las Vegas, Nevada, on June 19 and 20, 1972.

The Plowshare Program was dedicated to development of peaceful uses of nuclear explosives, for example, to excavate canals and harbors and to stimulate production of natural gas from low-permeability geologic formations. In 1959, R. H. Carlson suggested that nuclear explosives could also be used to increase the flow of hot water into marginally productive geothermal wells (Carlson, 1959). The concept was further developed by George Kennedy, who in 1964 discussed the economics of using nuclear explosives to stimulate unproductive hydrothermal systems in rock at 10,000-ft depth and 500°C (Kennedy, 1964). J. B. Burnham and D. H. Stewart considered the feasibility of using nuclear explosives to fracture a subterranean formation in which there was no mobile free water and then to recover the heat generated by the explosion by injecting water into the fractured rock and recovering it as steam. They concluded that the amount of heat left in the rock by the nuclear explosion was not sufficient to result in economical energy production, but that it could be remedied by creating such a system in rock that was already hot (Burnham and Stewart, 1970). That was the first serious HDR proposal and—sponsored by the AEC's Division of Peaceful Nuclear Explosive—it was studied intensively for several years. The ANS Annual Meeting provided an appropriate forum for discussion of the idea.

Lee Aamodt, who was a member of ANS, prepared and presented a paper on the "Induction and Growth of Fractures in Hot Rock" at that meeting (which generously listed me as a coauthor). In it he described our idea for creating an HDR system by hydraulic fracturing instead of using explosives, and discussed the work that we were doing to develop the concept and our ideas and plans for future experiments to investigate its feasibility.

Our ideas were also featured prominently in several other papers. Jesse Denton of NSF (the editor of the Hickel Report) outlined a very broad program of geothermal research for discussion at the meeting and emphasized the potential importance of HDR. Bob Rex described the heat sources and general distribution of high-grade HDR areas. Tony Ewing's paper on the "Stimulation of Geothermal Systems" gave top research priority to HDR development, and discussed both the nuclear and the hydraulic-fracturing options. John Burnham and Don Stewart described the results of the Plowshare geothermal study and mentioned the existence of the HDR prospect at Marysville as a candidate site for a Plowshare HDR field experiment. (They noted that the rock there might be at 700°C at a depth of 3 km.)

This meeting was well attended, particularly by members of the geothermal community, and our HDR Program received a great deal of attention. Lee and I had the opportunity to discuss it informally with a number of experts in a variety of areas related to our own interests, and Jerry Johnson arranged for us to meet with representatives of several other government agencies who were present. Several of those contacts later proved very useful to our programs, among others a group of earth scientists from the Menlo Park branch of the USGS whom we had not met before.

After technical review and editing, the papers presented at the ANS meeting were published in a volume called "Geothermal Energy: Resources, Production, Stimulation" (Kruger and Otte, 1973). Lee's paper was included under its original title, although it had been revised and broadened quite extensively. Bob Potter, Don Brown, and I worked with Lee on the revision, and I am still embarrassed by one result of that effort. This being the first major technical publication concerning our HDR Program, we suggested to Lee that it would be nice if all three of us were listed as his coauthors. As was often the case, our approach on this was probably not particularly tactful and, somewhat ruffled, Lee submitted the list of authors as "Morton Smith, R. Potter, D. Brown, and R. L. Aamodt," in that order. As the original and principal author, Lee should of course have been listed first, and subsequent references to that paper should have been to "Aamodt et al." instead of "Smith et al."

In any case, it was an important first paper of our HDR Program, and a very creditable one. It is of some interest that, in addition to the usual discussion of a two-hole circulation loop, the paper also suggested a one-hole system with flow down an insulated central pipe, out through a set of hydraulic fractures, and back to the surface through the annulus between the central pipe and the well casing. That idea has since been patented by others.

14. THE SUMMER OF 1972

In June of 1972, we and the rest of the Laboratory suffered a major loss with the death of Eugene S. Robinson, who—among many other things—had played such an important part in initiating both the HDR and the Subterrene Programs. Of course, as Robbie would have expected, those of us who remained continued those programs as diligently and capably as we could.

In particular, during the spring, summer, and fall of 1972, we in the HDR Program were busy with a variety of background studies and field investigations, both on our own and with the cooperation and assistance of others both from within our own Laboratory and from other organizations and agencies. We arranged a number of consulting and visiting staff-member contracts, purchase orders, and informal cooperative agreements in areas in which we needed outside help. We also used to great advantage the special expertise of members of our Geoscience Advisory Panel. Among the major activities during that period were the following.

14.1. The Project Directive for GT-1

Among other things, the AEC required that there be an approved, formal project directive for any LASL project that involved construction. The format for such a directive was outlined in AEC manual AECM 6101, which specified certain special procedures for such projects. Somewhat belatedly, the Los Alamos Area Office (LAAO) asked us to prepare such a directive for our operations in Barley Canyon. In a series of meetings between LASL and LAAO personnel, it had previously been agreed that—since our Barley Canyon facilities were experimental and temporary—our work there did not involve construction as (in accordance with the Congressional Davis-Bacon Act) the word "construction" was defined by the AEC. Accordingly, a project directive for it appeared to be unnecessary, and I suspect that LAAO requested it primarily to complete its own documentation of what, even for them, was a pretty non-routine operation. However, a request from them was not to be ignored.

Our Engineering Department (ENG), then headed by L. Phillip ("Phil") Reinig, was another LASL organization that was essential to our survival. Among other things it handled many of our requests for permits and approvals as well as land surveys, engineering designs, and cost estimates. Fortunately for us, it also was familiar with AECM 6101, knew how to write project directives, and was willing to write one for us.

On July 14, 1972, J. B. Weldon of ENG sent to H. Jack Blackwell (then manager of LAAO) a project directive for well GT-1 in Barley Canyon, together with a "preliminary proposal" for it and a water-pollution-control annex. It was a very well-prepared document and, since GT-1 had already been drilled and completed, it was of course correct in almost every detail. (In his accompanying memorandum, Weldon did point out that, in this case, he did not think that a project directive was really necessary.)

In response to Weldon's submission, Jack Blackwell sent a memorandum (dated August 29, 1972) to Harold Agnew, listing four LAAO requirements on our work.

1. Any operation on Forest Land "not consistent with normal public use" required a formal "Memorandum of Understanding" between the AEC and the Forest Service. (Such a memorandum was subsequently prepared by LAAO and agreed to by the Forest Service replacing our original Special Use Permit. It covered not only the Barley Canyon site but also the Fenton Hill location at which our later experiments were conducted.)
2. "Substantially firm plans" for future work were to be submitted to the LAAO Work Review Committee for a determination of whether or not the proposed work or any part of it was actually construction and therefore was covered under the Davis-Bacon Act. If it was found to be covered, LAAO would arrange for its performance by prime construction contract. (This requirement resulted in a recurring headache for us. Essentially, it meant that any operation determined to be "construction" was to be contracted out by LAAO and required wage

determinations and payment of local union-scale wages to all workmen involved—which drilling contractors, in particular, absolutely refused to do. We had a lot of "Davis-Bacon meetings" with LAAO personnel and a great many disagreements about what was construction and what was not. We, LAAO, and the HDR Program managed to survive them.)

3. Support by the Zia Company, who handled most minor construction and maintenance at LASL, was not to be considered for the HDR Project. (This was a mixed blessing. Zia was immediately and—usually—promptly available, with a minimum of red tape. However, it was not always equipped or staffed for the things that we needed done, and we could often get them done more quickly and less expensively by using outside contractors. Fortunately, it turned out that we were able to arrange for Zia to perform a variety of relatively minor tasks, such as hauling water to our drilling site.)
4. Field contract administrative responsibilities were to be delegated formally to a specific "qualified individual." (That was usually Don Brown, who therefore "qualified" for an amazing variety of responsibilities—almost all of which he assumed cheerfully and managed successfully.)

14.2. Logging Well GT-1

While GT-1 was being drilled, Schlumberger, Ltd., logged the well through the surface volcanics and the upper part of the sedimentary section for temperature, density, and electrical resistivity. When the hole had been completed, the Birdwell Division of Seismograph Service Corporation logged the entire hole for temperature and natural gamma, and the uncased bottom section for density, caliper, resistivity, three-dimensional acoustic velocity and—to look for pre-existing natural fractures—with a borehole televiewer. The USGS also ran a caliper log and made water-flow measurements in the well.

Throughout the summer of 1972, as the effects of drilling dissipated and the formations along the wellbores slowly approached temperature equilibrium with their surroundings, repeated temperature logs were run in the 600-ft heat-flow holes and in GT-1 by a USGS crew under James Hudson, a NMIMT crew under Marshall Reiter, and a Los Alamos crew under Bob Potter.

14.3. Temperature gradients and heat flow

A major concern with regard to development of an HDR system is the depth to which it will be necessary to drill in order to reach the rock temperature required to satisfy the energy need for which the system is being considered. In the absence of a nearby hole deep enough to have reached that temperature already (and such holes are rare in most geothermal prospect areas) it is generally necessary—because of the high cost of drilling deep holes—to make measurements in relatively shallow holes, and then extrapolate the results downward.

This procedure begins with a temperature log (measurements of temperature as a function of depth) in existing wells or in holes drilled specifically for that purpose, and deep enough to be unaffected by daily or yearly temperature cycles at the surface or by active groundwater circulation—which usually reduces the temperature of the formations through which the water circulates, although in a geothermal area it may instead increase it. If the hole is sufficiently deep to avoid those disturbing effects, then the undisturbed temperature gradient in the lower part of the hole is determined jointly by the rate at which heat is flowing upward toward the earth's relatively cool surface and the thermal conductivity of the rock:

heat flow = temperature gradient x thermal conductivity, or

temperature gradient = heat flow / thermal conductivity.

The rate of heat flow may change somewhat with depth if the formations penetrated contain significant amounts of unstable isotopes (e.g., of uranium), whose slow decay produces heat that increases the rock temperature locally. This temperature rise increases the rate of heat flow above the region in which the heat was deposited. However, in the formations usually encountered and over the depths likely to be reached in geothermal drilling, this is not likely to result in a significant change in heat flow with depth. Accordingly, it is generally assumed that heat flow is constant and independent of depth at any given hole location. Heat flow is then calculated as the product of the temperature gradient and the thermal conductivity of the formation in which it was measured, and—assuming constant heat flow—the temperature gradient in any other formation encountered in the same hole is inversely proportional to the thermal conductivity of that formation.

As has been described, in the early days of the HDR Program we were not equipped to run temperature logs ourselves and had arranged for Marshall Reiter and his students at NMIMT to run them for us. By the spring of 1972, Marshall Reiter and one of his graduate students, Charles Weidman, were also developing equipment to measure thermal conductivities on rock cores and drill cuttings. Through a series of purchase orders, we arranged for them to measure conductivities on samples of cuttings from our 600-ft heat-flow holes and subsequently on 15 core samples of basement rock from well GT-1. As might be expected with new equipment and a rather difficult measuring technique, the initial results of those measurements were somewhat uncertain.

Some time in April 1972, Art Lachenbruch encountered Marshall in Washington and discussed the problems of heat-flow measurements with him at considerable length. Undoubtedly this helped Marshall in improving his apparatus and techniques. In the meantime, Art was anxious to have reliable heat-flow results from GT-1 for discussion at the GAP meeting in May. Therefore, in a letter to John Rowley dated April 25, 1972, Art offered to have a few conductivity measurements made for us in his USGS laboratory at Menlo Park. Since Art is a world-renowned expert on heat flow and had one of the world's best thermal-properties laboratories, we were delighted—and promptly sent him 13 samples of the cores cut from the basement rock in the bottom section of GT-1. The actual conductivity measurements were made by Robert Munroe in Art's laboratory. These results, in which we had great confidence, showed thermal conductivities significantly higher than those reported to us by NMIMT from their measurements on similar samples. Therefore, when Art had returned his samples to us, we sent them down to Marshall and I prepared another purchase order to cover the cost of remeasuring both Art's samples and those that Marshall already had. By this time the group at Tech had improved its equipment and techniques sufficiently so that they got quite good results on all of the samples.

In a worthwhile sample exchange, Marshall subsequently sent Art seven of the samples originally sent to NMIMT. The USGS measurements on them confirmed the results that they had obtained on similar samples that we had originally sent to them and were in good agreement with Marshall's new results.

14.4. Seismometry

Direct monitoring of natural earthquakes in the area and close-in monitoring of microearthquakes produced by our hydraulic-fracturing experiments were both essential to the HDR Program, the former to document the fact that they were not initiated by our experiments and the latter as a means of mapping the fractures that we did create. Fortunately, Ken Olsen of LASL Group J-9 was already involved in an earthquake-risk analysis for such LASL facilities as the meson-physics and plutonium-research areas, and he was also interested in our HDR Program. During the summer of 1972 he was developing a four-station areal seismic array (which later was extended over much of northern New Mexico) and he also began work on a surface array of microseismometers for use in connection with our hydraulic-fracturing operations. Together with Bob Potter, it was Ken Olsen who got us started in surface seismometry—at no cost to our as-yet unfunded HDR Program.

However, no one was sure that the tiny acoustic signals generated by hydraulic fracturing at depths of thousands of feet could, in fact, be detected at the surface. Therefore Bert R. Dennis of Group WX-7, another early volunteer to the program, undertook development of a three-component seismometer for use downhole, where it would be in competent rock, close to the signal sources, and well shielded from cultural noise at the surface. At that time we assumed that the seismometer would be suspended in the well immediately below the section being pressurized. However, we recognized that a single instrument might not be capable of locating signal sources unambiguously, so we also anticipated the future development of a linear array of such seismometers suspended vertically in the well during fracturing operations.

Bert became our instrumentation and data-acquisition expert, and these were the first steps in a major project to develop and use very sensitive seismometers for acoustic mapping of fractures.

14.5. Petrography

Precambrian granites that we believed would prove to be quite similar to those underlying the Jemez Plateau west of the Valles Caldera are exposed at the surface both in the Sandia Mountains east of Albuquerque and in the San Pedro Parks area a few miles north of Barley Canyon. Bob Potter collected samples of granites from both of these exposures in order to gain an early understanding of the probable nature of the basement rock under the area in which we hoped to develop an experimental HDR heat-extraction loop.

During May 1972 we sent four samples of the Sandia granite to Burt Slemmons for geological examination. When basement-rock cores became available from GT-1, we cut 25 samples from them and sent those to Orson Anderson, together with 9 granite samples from San Pedro Parks. Burt also sent him three of the Sandia samples. These 37 samples were thin-sectioned in Orson's laboratory at UCLA and passed on to Priscilla Dudley for petrographic examination.

At that time Priscilla was a professor at Boston College, where she taught petrography. However, she was spending the summer at the Museum of Northern Arizona, in Flagstaff. As the executive secretary of our Geosciences Panel she was already a consultant to LASL, but—so that they could work at LASL as well as at Flagstaff and UCLA—she and Orson were also made visiting staff members at LASL.

In the summer of 1972, Priscilla (who got married that summer and became Priscilla C. Perkins) examined these 37 samples and reported her observations and conclusions informally and then in an excellent Los Alamos report (Perkins, 1973). She found that the upper approximately 94 ft of the basement rock sampled in GT-1 was granite and granodiorite, partly gneissic in texture, underlain by about 61 ft of biotite-amphibolite veined by tonalite-aplite. She observed chlorite-lined fractures in hand specimens but found that all cracks observed in thin sections followed narrow calcite veins. She also observed that the foliation, when present, dipped on the average at 45° to the axis of the core and that the calcite veins had two dominant orientations: parallel to the foliation and parallel to the core axis (which was approximately vertical).

Priscilla concluded that the GT-1 cores were very similar in composition to the basement-rock samples collected in the Sandia and San Pedro Parks areas and predicted that the predominant basement-rock type under the Jemez Plateau would be found to be granitic. (In this she was certainly correct. In deeper holes subsequently drilled at Fenton Hill—the deepest of which bottomed at 14,400 ft—all of the basement rock penetrated was granitic. Incidentally, the only amphibolite encountered in all of the deep drilling done in that area was that found in the bottom section of GT- 1.)

14.6. Some internecine skirmishes

It was evident that if, as we hoped, we were to drill more deep holes in the future, then we would need good petrography done on the cores from them—and Priscilla was doing an excellent job for us on the GT-1 cores and the granite samples from other locations. In the meantime, there were entertaining aspects to the arrangements for her to do so (although, at the time, I was not much entertained by some of them).

As chairman of our Geosciences Advisory Panel, it was Orson Anderson who introduced Priscilla to us as its executive secretary—another job that she did very well. At the second GAP meeting on May 18-20, 1972 (described in Section 13.1 of this report) many subjects were discussed, including—briefly—the obvious need for petrographic examination of the GT-1 cores. Surprisingly, in the report of the meeting that Orson submitted to Harold Agnew, about one-fourth was devoted to a strong pitch for arranging to have Priscilla do the petrography on those cores during that summer. That arrangement was made, although I am not sure who made it. (I suppose it was Dick Taschek, who must also have arranged to pay for it.)

In about the middle of August, Orson telephoned me and said that Priscilla was leaving Boston College and needed a job. I replied that we were very happy with what she was doing on our cores but that our HDR Program was still unfunded and we could not offer anyone regular employment in it. He then suggested that he could provide 80% support for her and proposed that we provide the other 20%. I told him that we could not even do that; we had no money even to support the LASL staff already working on HDR and didn't know when we would get any. Further, I informed him that Priscilla's investigation of the GT-1 cores would pretty well satisfy our current need for petrography; that we were uncertain when or if we would be able to do any more deep drilling; and that, in the meantime, our urgent needs to keep the program alive were such that additional petrographic work would necessarily have a relatively low priority for some time to come. Orson was not pleased.

The third GAP meeting began on August 28, 1972, and, at its first coffee break, Orson took me aside and twisted my arm vigorously with regard to us hiring Priscilla. I gave him the same answers that I had given him previously on the telephone, and he didn't like them any better the second time. (Later that day Priscilla gave an excellent progress report on her petrographic studies, and the panel concluded that the GT-1 core samples represented very well the basement rocks in the area in which we were most interested.)

That, of course, was not the end of it. In a memo to Dick Taschek dated August 31, 1972, Priscilla proposed a "Petrographic Investigation, Granite Test Hole One," this to last 9 months and cost $600 per month. (In those days, that was probably about the 20% support that Orson had proposed to me. Presumably this proposal was to legitimize the work that Priscilla was doing.) Handwritten at the bottom of the memo was "Approved, O. L. Anderson, Aug. 31, 1972." Below that, also handwritten, was "9/1/72. Mort—This seems to be very important to fill out the information need for geochemistry & will eventually be an unavoidable prerequisite for environmental impact statements. Dick." (The Dick, of course, was Dick Taschek.)

Even that was not the end, which is why I have included this rather personal narrative here. It was probably not a coincidence that on the morning of September 1, 1972, Dick Taschek telephoned and told me that he was not satisfied with my management of the LASL geothermal energy program and that this function had therefore been assumed by the GAP (i.e., by Orson, although Dick didn't say that). Having to that time had the major responsibility for the program (which, in the circumstances, I thought had gone surprisingly well) it was my turn not to be pleased. In fact, I drafted a very hot memo on the subject but decided not to send it but instead to wait and see what happened next. Not much did. Orson and the other panel members were not around enough to get much involved in the day-by-day activities of the program except in a few special areas described below. They did not really try to manage it, so I simply continued to do so.

To complete the story (finally): On September 7, 1972, Dick Taschek wrote a memo to Bob Van Gemert asking him to arrange a purchase request for Priscilla's continuing petrographic services, but restricting it initially to three months work at $600 per month plus travel expenses for a couple of visits to Los Alamos. Here, since no member of the GAP (including Orson) had the authority to sign a LASL purchase request, it was necessary for Dick—at least implicitly—to reinstate me. At his request, I was pleased to write and sign the purchase request for Priscilla's petrographic services and later to approve the bills that she submitted. She did an excellent job for us, and the job that was done on me was only temporary (although, obviously, it has not been forgotten).

14.7. Seismic risk

Large, high-pressure fluid injections into recently active fault zones were known to have triggered small to moderate earthquakes both in the Denver area (Evans, 1966) and in an oil field near Rangely, Colorado (Raleigh et al., 1971). These localized, man-caused events occurred because of the following facts and actions:

- An earthquake occurs when one part of the earth's crust slides across another part—described as a "shearing displacement."
- In general, this occurs along a preexisting fault (a large fracture in the earth's crust) because the relative motion occurs more easily along an existing fracture than when the movement requires that new rock must be broken.
- It occurs when shearing stress across the fault overcomes the frictional resistance to slip along it. The frictional resistance may be supplemented by secondary minerals deposited in the fault zone, but such minerals are often relatively weak or poorly bonded to the fault surfaces, or both, so that a region of weakness still exists.
- In nature, the shearing displacement results from a buildup of shearing stress, usually as a result of large-scale movements of the earth's tectonic plates—although local effects such as intrusion of a magma body into the crust or collapse of a caldera may also create very large shearing stresses.
- However, it may also occur when such stresses are not high enough to cause spontaneous movement but, instead, the frictional resistance that initially prevents movement is somehow reduced.
- Injecting pressurized fluid into the fault tends to force its opposite surfaces apart, reducing the frictional resistance and thus permitting relative motion to occur at reduced stress. The seismic events observed at Denver and Rangely resulted from such fluid injections into fault zones.
- The presence of a fault provides a location at which this sequence of events may occur, and evidence of geologically recent motion along it suggests that shearing stress there may be high enough so that fluid injection (hydraulic fracturing) could trigger an earthquake.

In the oil and gas business, many hydraulic fractures are made every day, all over the world, without triggering earthquakes. However, the events at Denver and Rangely demonstrate that the seismic risk of hydraulic fracturing is not zero—at least if it is done in the wrong geologic environment. We believed that we could reduce that risk very nearly to zero by avoiding locations near fault zones.

As a first step in evaluating and minimizing the possibility that our proposed hydraulic-fracturing experiments on the Jemez Plateau might trigger an earthquake, we invited Burt Slemmons to spend the period from about July 4 to about August 11, 1972, as a visiting staff member at LASL. He was an expert on earthquakes, and we asked him to investigate the fault structure and earthquake history of the area.

Actually, this investigation had begun well in advance of Burt's summer visit. Northern New Mexico and the Valles Caldera region, in particular, had been studied for many years by a large number of geologists and geophysicists. The information that they had collected was compiled and reviewed. This included some aerial and satellite photographs and earthquake records as well as field studies on the ground.

As has been mentioned, in mid-March 1972—under an arrangement made by Paul R. Guthals of LASL's J Division with the U.S. Air Force Base at Kirtland Field in Albuquerque—additional aerial photography of the area west of the Valles Caldera was done at altitudes 1000 to 20,000 ft above ground level. This was primarily low-sun-angle photography during early mornings and late afternoons, with stereographic coverage, to locate faults on the basis of the shadows cast by small changes in surface elevation. However, it also included conventional high-sun-angle photography, supplementing previous coverage by the U.S. Forest Service.

During the spring and summer of 1972, Burt analyzed these photographs and followed up with field studies on the ground. He confirmed the presence of known faults in the area and discovered a previously unmapped minor fault in Virgin Canyon—about 4 miles southeast of the GT-1 site (and 2.5 miles southeast of Fenton Hill). Burt found that the Virgin Canyon fault displaced the Bandelier tuff by 50 to 75 ft, showing that movement along it had occurred since that formation was deposited about 1.1 million years ago; that it had a very low average rate of movement; that it trended away from Fenton Hill; and that it appeared unlikely to be activated by our proposed experiments either in Barley Canyon or at Fenton Hill. There also appeared to be no earthquake hazard from other faults within a radius of 15 miles from Fenton Hill. Except for the Virgin Canyon fault, none was found that displaced the geologically young surface formations.

Burt also collected and analyzed all available earthquake data for New Mexico and concluded that the level of seismic activity was very low in the region surrounding Barley Canyon and Fenton Hill.

This work was summarized in an excellent LASL report (Slemmons, 1975) in which Burt concluded that our proposed experiments in the area involved very little seismic risk from natural fault activity or local earthquakes, and that those experiments were not likely to activate any of the known faults in the area—including the closest and most recent one in Virgin Canyon.

Burt's results greatly increased our confidence that we could proceed safely with our proposed experiments and were important to our evaluations of their environmental as well as their experimental risk.

14.8. Hydrology

Although we were quite confident that our experiment would not contaminate the surface waters or groundwater in the area, it was obviously important that, if somehow they did, we should detect it promptly and immediately turn off whatever activity was causing it. It was also important that if such contamination should originate in some other source—such as a change in hot-springs activity or Union Oil's hydrothermal development in the Valles Caldera—we should be able to document the fact that it wasn't our fault. This required a detailed initial hydrologic study and continued hydrologic surveillance of the area.

Fortunately, one of the earliest volunteers in the HDR Program was Bill Purtymun. In addition to being custodian and principal operator of the soil-sampling rig used to drill our shallow heat-flow holes, Bill was a talented and experienced hydrologist. Before coming to LASL (primarily to work on the water supply for the Laboratory and the city of Los Alamos), Bill had worked in the Water Resources Division of the USGS in Albuquerque. One of his colleagues there was Frank W. Trainer who, when our HDR Program began, was conducting a long-range hydrologic study of the area that included both the Valles Caldera and the Jemez Plateau west and southwest of the caldera—where our own interests lay. Frank was a constant source of help and information and worked closely with Bill and his associates in setting up our own water-quality-monitoring system (which was largely an extension of his own). In return, of course, we promptly shared with Frank the information that we collected on hydrology, water chemistry, and subsurface geology.

Frank was also very helpful in arranging for other USGS personnel in Albuquerque and elsewhere to help us with information, advice, and some of our field studies. During the summer of 1972 this included well-logging and, using a USGS downhole water-sampler, obtaining water samples at various depths in well GT-1.

14.9. Rock mechanics

John Handin visited us in Los Alamos on June 28 and 29, 1972, primarily to examine with us the cores taken during the drilling of GT-1. He was very knowledgeable in many areas important to the HDR Program, particularly in rock mechanics and engineering geology, and was always helpful, patient, and tactful in advising us. Although we had no money at that time to start one, we knew that we needed a strong program on the mechanics of hydraulic fracturing, thermal-stress cracking, and the behavior of a pressurized, fractured, HDR reservoir. John shared with us his insights into what was needed, what was known, and what was possible, and he continued to do so over the years.

14.10. Visitors, briefings, and presentations

Orson Anderson also visited Los Alamos several times during the summerof 1972 (as a visiting staff member at the expense of my old metallurgy research group, CMB-13). He participated in a number of geologic field trips and in many discussions of both our geothermal energy and Subterrene programs. He too was both very knowledgeable and very helpful, although rather inclined to manage—as, in fact, he was later authorized by Dick Taschek to do—rather than simply to advise.

Los Alamos has always had a great many visitors with a wide variety of interests. Whenever we had the opportunity—whether they were really interested or not—we attempted to brainwash them on the merits of HDR energy systems and how we hoped to develop them. Fortunately for our cause, Harold Agnew and others in LASL management liked to show them that we were more than just a weapons laboratory, and that we were concerned about such things as the energy crisis, depletion of our natural resources, and pollution of the environment. Therefore we were given many opportunities to brief visitors who were at LASL for other reasons and we were always ready to do so at the drop of a hat. Further, we and others were spreading the word elsewhere as widely as we could, and we were invited to make presentations to a wide variety of individual groups, societies, and conferences.

Of course by this time we had a fairly standard pitch on the subject of HDR and, with minor variations according to the audience being addressed and the individual making the presentation, we delivered it repeatedly during the summer and fall of 1972. Sometimes members of the group did it by themselves and sometimes we appeared as a panel. A partial listing of our presentations during this period includes the following:

- Representative Mike McCormack, May 26, 1972. He was influential on energy matters in the U.S. House of Representatives, where he represented the State of Washington. Since BNW, based in Seattle, was still pushing the Plowshare concept of HDR development, I included that in my briefing, but of course I emphasized hydraulic fracturing as a desirable alternative to the use of nuclear explosives.
- General Frank A. Camm, June 26, 1972. In this case I emphasized the flexibility of HDR systems with regard to location and therefore their potential usefulness at military bases—particularly those at remote and isolated locations.
- American Physical Society (APS) Symposium of the Forum on Physics and Society, Regional Meeting, Albuquerque, June 5, 1972. Here I presented an invited paper on "Clean Energy from the Earth" in which of course I was discussing HDR energy systems. This was one of our first public presentations to a scientific audience on the HDR concept and our program to investigate and develop it.
- Oak Ridge Associated Universities Summer Institute on "Energy Sources for the Future," Oak Ridge, Tennessee, August 8, 1972. At this seminar I gave an updated version of the talk that I had given to the APS in Albuquerque, to about 20 participants in the Institute (college professors from various universities) plus about 30 ORNL scientists and engineers. In particular, I acknowledged the pioneering work on acoustic mapping of hydraulic fractures done by Bill McClain and his associates at ORNL
- Dixie Lee Ray, August 22,1972. Dr. Ray was the newly appointed chairperson of the AEC, and this was her first visit to Los Alamos. Of course we were delighted at the opportunity to try to enlist her support for the HDR Program, and—judging from subsequent events—apparently we were successful in doing so.
- University of California Review Committee meeting at LASL, September 15, 1972. While nearly all LASL funding came from the AEC and in most matters we negotiated directly with them, the University of California held the contract to manage LASL for the AEC, and all

of us at LASL were actually employees of the University. It was a very comfortable arrangement and we hoped to help keep it that way.

- American Nuclear Society, Trinity Section, meeting in Santa Fe, September 29, 1972.
- Sandia Research Colloquium, Sandia Laboratories, Albuquerque, October 4, 1972.
- Institute of Electrical and Electronic Engineers, Los Alamos-Santa Fe Subsection, meeting in Los Alamos, October 17, 1972.
- Refractory Composites Working Group, Cleveland, Ohio, October 20, 1972.
- New Mexico Electrical Cooperatives, meeting in Los Alamos, October 24, 1972. This group consisted of representatives of the managements of Plains Electric Generation and Transmission Cooperative based in Albuquerque, Socorro Electric Cooperative, and Springer Electric Cooperative. Plains Electric, in particular, was interested in possible applications of the HDR concept in its service area, and subsequently contributed to our progress in a number of ways.
- LASL Colloquium, November 7, 1972. This was the first discussion of the HDR Program before a Laboratory-wide audience. Among other things, I described the development of the HDR concept pretty much as it is outlined in Section 2 of this report.
- Advanced Development and Production Conference, at LASL, November 10, 1972.
- McDonnell Douglas Astronautics personnel visiting Los Alamos, November 16, 1972.
- Sierra Club, Rio Grande Chapter, Santa Fe, November 18, 1972. We have always felt that the environmental advantages of HDR are so important that such organizations should be among the principal proponents of HDR energy systems.
- AEC Office of Planning and Analysis, at Los Alamos, November 21, 1972.

In addition to the small group of people actively involved in the HDR Program, many other LASL personnel dropped a kind word about it when the occasion offered. In fact, Harold Agnew, our Laboratory director, was probably our most important and effective salesman. In addition to giving us the opportunity to brief many visitors to the Laboratory, he himself made the pitch to many high-level audiences and gained a great deal of valuable support for the HDR Program. Periodically he asked me for a new set of briefing materials for such presentations, and typically I gave him about a two-page summary and about a half-dozen transparencies showing the area west of Los Alamos where we were working, our heat-extraction concept, recent photographs of our experimental area (preferably showing a drilling rig and, later, some steam) and whatever recent experimental results seemed appropriate. I don't know what he did with the stuff after he had used it, but I did accuse him of having a chute beside his desk leading to a basement storeroom full of my transparencies. He didn't deny it.

15. PLANNING FOR GT-2

Although we had yet to begin our major experiments in well GT-1, we were sufficiently optimistic about what their results would be so that, in the summer and fall of 1972, we were already making plans to drill a second, deeper exploratory hole at another location on the Jemez Plateau. This was intended to confirm and extend the results of the GT-1 experiments; to demonstrate that the subterranean environment characterized there was typical of the large geothermal area west of the Valles Caldera; and to identify and investigate a location that would be suitable for subsequent development of a two-hole pressurized-water heat-extraction loop.

The GT-1 location was not well suited to such a development. It was in a narrow canyon with no level areas large enough to accommodate the surface facilities that would be needed for the proposed system. Access to it was difficult. Electrical power was not available. And direct communication to the outside world, even by radio, was nearly impossible. We hoped to find a more convenient location whose development would be less expensive and less damaging to the environment. This would also demonstrate the flexibility with regard to location that is a major advantage of HDR energy systems.

15.1. The proposed well

Our second exploratory well, GT-2, at that time was planned to be 4000 ft deep, cased at 10 3/4 in. to a depth of 2600 ft (about 350 ft into the granitic basement), then drilled at 9 1/2 in. to its final depth—with this bottom section left uncased. During drilling, particular attention was to be given to observation of changes in rock structure and permeability with depth. In the completed well, the series of hydrology, pressurization, and fracturing experiments planned for GT- 1 was to be repeated, at greater depths and higher temperatures.

It was proposed that, if the nature of the basement rock and the available equipment permitted, a hydraulic fracture would be made from the open-hole section of GT-2 and extended outward to a radius of about 1000 ft. Further, it was proposed that a one-hole heat-extraction experiment be conducted in the new well. The method outlined was to install 6-in. fiberglass tubing extending from the surface to near the bottom of the hole, where the rock temperature was expected to be about 140°C. Cool water would be pumped down the tubing, returning to the surface through the annulus around it. Heat extracted from the hot rock surrounding the lower part of the hole—perhaps supplemented by fluid circulation through whatever hydraulic fractures we had created—would be dissipated at the surface through a simple heat exchanger, and the cooled water returned through the tubing for recirculation downhole and recovery of more heat. Heat extraction would be maintained while the first of the larger, still-deeper holes of a two-hole heat-extraction system was drilled nearby. Eventually GT-2 would be used for emplacement of instruments to monitor the construction and operation of that more-ambitious two-hole system.

In addition to the first-ever demonstration of heat-extraction from hot, dry, crustal rock, the one-hole experiment would provide preliminary information on the geochemistry of a recirculating, pressurized-water loop and an opportunity to determine whether or not thermal-stress cracking of the cooling rock produced detectable microseismic signals. Further, since we believed that the hot water would return to the surface at a temperature above its atmospheric-pressure boiling point, in a memo to Dick Taschek dated October 11, 1972, I pointed out that "With appropriate valving, it would be possible to flash and vent steam when desirable for public-relations purposes." (We knew that we had a selling job to do.)

15.2. A site location for well GT-2

With this very preliminary plan in mind, Don Brown went exploring for a suitable and convenient location for our second exploratory well and subsequently for development of the world's first two-hole heat-extraction system. He found it at Fenton Hill, about 1.5 miles south of our Barley Canyon site and about 21 air miles west of Los Alamos. It was on a relatively flat mesa in the Jemez District of the Santa Fe National Forest, at an elevation of 8700 ft above sea level. It had been burned over in a major forest fire a year or two earlier and therefore was almost free of live timber. It was adjacent to a surfaced, all-weather road, New Mexico State Highway 126, and to both telephone and power lines.

Possible use of this area by our HDR Program was discussed with local and regional Forest Service officials at the Jemez Springs Ranger District and the Santa Fe National Forest Office in Santa Fe. They all reacted favorably and, so long as it was neat and well planned, they favored a highly visible experiment there as a demonstration of multiple use of federal land. They were particularly enthusiastic about having a pond available as a source of water for fighting forest fires and a hard-surfaced landing area nearby for their helicopters—both of which our site development would provide. Therefore, with their permission, our LASL Engineering Department mapped the Fenton Hill area topographically during the summer of 1972, in preparation for detailed site planning, environmental impact statements, and a formal request to the Department of Agriculture for temporary occupancy of several acres of Forest Service land on Fenton Hill.

15.3. Fenton Hill—to be continued

Our proposed experimental development at the Fenton Hill site was based primarily on four assumptions:

- that our experiments in GT-1 would demonstrate that the basement rock underlying Barley Canyon was well suited to the development of an HDR heat-extraction system by hydraulic fracturing;
- that the basement rock at Fenton Hill was quite similar to that at Barley Canyon;
- that, in fact, our proposed method of developing a subterranean circulation loop by drilling and hydraulic fracturing was feasible; and
- that adequate funding would be provided to support development of the Fenton Hill site as well as our preliminary experiments in well GT-1.

By that time we had repeatedly been assured that our FY73 funding from DAT would be at least $1.0 million, with much larger funding in FY74 to complete development of the experimental two-hole circulation system. Accordingly, in my October 11, 1972, memo to Dick Taschek, I pointed out that the optimum funding level for the rest of FY73 would actually be $1.3 million, which we thought would permit us to complete our experiments in GT-1 and to drill both GT-2 and the first hole of a two-hole circulation loop. In fact, our total official funding for FY73 turned out to be only $164 thousand, from DPR and DMA (although there was a great deal of unpaid volunteer work and internally funded Laboratory support that was not included in that amount).

Necessarily, our major development at Fenton Hill was postponed to a time beyond that covered in this report, although we did what we could to prepare the site in the meantime. Further, because of funding limitations, our plans for well GT-2 finally were changed quite drastically; in the end it was deepened and made one leg of a successful two-hole system. And we never did attempt that one-hole heat-extraction experiment. If we had followed the original plan outlined above, heat loss to the relatively cool near-surface formations and, through the fiberglass tubing, to cool water being pumped down the hole, would probably have caused the temperature of the produced fluid to be disappointingly low. A considerably more sophisticated and expensive well-completion would undoubtedly have been necessary to make the experiment worthwhile.

16. THE FALL AND WINTER, 1972–73

During the fall and winter of 1972-73, we were, as usual, acutely deficient in the funding department and were therefore unable to proceed with most of the field work and laboratory experiments that we were anxious to undertake. There were, however, plenty of things to keep the small HDR staff busy, including meetings, budget exercises, various background studies, and planning for the happy day when the money spigot would finally open wide.

16.1. Third GAP meeting

The Geosciences Advisory Panel met again at LASL on August 28-30, 1972. In addition to the panel members and Laboratory staff, Dr. Charles Theis, a distinguished hydrologist, attended as a guest participant. The agenda included discussions not only of the HDR and Subterrene Programs, but also of siting for a new LASL plutonium research facility, radioactive waste disposal, the general seismology program, and other long-range geoscience problems at the Laboratory. At the end of the meeting the panel met in a closed executive session to review those things and decide on recommendations to be made to the Laboratory director, Harold Agnew.

In introducing the HDR section of the program, I emphasized that—while we still had the usual promises—we were not yet funded to undertake our proposed major field experiments and that our plans for them necessarily would remain very flexible until we knew how much money we would actually get and when we would get it.

Don Brown reported on the final completion of well GT-1 at 2575 ft depth. Then he and Bob Potter discussed the temperature and other logs that had been run in it and the cores taken in the basement-rock section. Finally, Bill Purtymun related those things to the general geology of the Jemez Plateau and discussed some preliminary hydrology experiments run in GT-1. We hoped to do more sophisticated hydrology and pressurization experiments in GT-1 in October and, if funding for it materialized, to begin drilling our second exploratory well (GT-2) at Fenton Hill in May 1973. We also hoped to drill the first leg of a relatively deep two-hole heat-extraction loop there late in FY73, that system to be completed in FY74. I noted that, if funding were insufficient for all that, we hoped at least to complete well GT-2 before the end of FY73 and that we might then attempt a one-hole heat-extraction experiment in it.

Burt Slemmons discussed the fault and earthquake study that he had done during the summer and Priscilla Dudley reported on her continuing petrographic study of the GT-1 cores. Since Burt, Orson Anderson, John Handin, Bob Potter, Don Brown, and most of the rest of us in the HDR group had all also examined those cores, they were discussed in considerable detail.

As usual, the panel reported its comments and recommendations in a memorandum to Harold Agnew, with copies to Dick Taschek and some of the rest of us. This memo was exceptionally long and detailed. The panel's comments and recommendations concerning the HDR Program are summarized as follows:

1. The panel was pleased with the work done so far but urged that several of our activities be strengthened, especially in order to collect information for the environmental-impact statement that would be needed for the deep two-hole experiment at Fenton Hill. They recommended that

    - temperature logging and analysis for GT-1 should be stepped up, in order to understand the thermal regime. Additional thermal-conductivity measurements should also be made.
    - geochemistry efforts should be strengthened immediately. A contract should be let to a university for leaching experiments on core samples at realistic temperatures and pressures, including both static and flow-through tests. LASL chemists should undertake bulk-chemical, trace-element and isotopic analyses of rocks and fluids and, eventually, age dating of the Precambrian rocks.

- petrography should be continued on core samples and samples of granites exposed in surface outcrops near our experimental sites.
- preliminary hydrology experiments, both before and after hydraulic fracturing, should be done in GT-1 before winter weather makes them impractical. These should include slug tests, constant-head tests, pump-up and pump-down tests, and permeability measurements both in GT-1 and on core samples.
- all available information, should be collected on wells, springs, and stream flows in the vicinity, including water quality, so that baseline conditions will be known before hydraulic fracturing might alter them.
- a preliminary evaluation should be made of the seismic risk of the two-hole experiment, including an estimate of the largest earthquake that could be tolerated at the site of the experiment. Experience at the Nevada Test Site with ground-motion produced by firing nuclear devices was assumed by the panel to be applicable.

2. A systematic study should be made of joint patterns in exposed Precambrian rocks close to Fenton Hill, including the frequency, continuity, and preferred orientation of the joints.
3. The hydraulic-fracturing experiments should be designed to measure regional stresses.
4. Downhole microseismic instruments will be needed to attempt to detect seismic signals from the propagating cracks during hydraulic-fracturing operations. These and portable instruments at the surface should be planned to determine accurately the locations and magnitudes of the microearthquakes that produced the signals. A few strong-motion instruments should also be installed at selected critical locations to monitor and locate any larger seismic events.
5. Laboratory measurements should be made on representative GT-1 core samples to determine Poisson's ratio, bulk modulus, shear modulus, and Young's modulus, wet and dry; compressive and tensile strength; and thermal conductivity.
6. All available data from GT-1 and laboratory measurements should be assembled and correlated, including stratigraphy, petrography, temperature, drilling history, casing history, downhole logs, and other aspects of drilling, in advance of any changes in GT-1 produced by hydrologic testing and hydraulic fracturing.

This was a useful list of things that needed to be done. Of course, some of them had already been started and more were already in our plans for the future (which the GAP knew, but always failed to mention in their reports to our director). However, we lacked the money and manpower to proceed with many of them at the rate that we and the panel would have liked.

The panel requested that, by November 1, 1972, we send them a report outlining various alternative strategies for drilling the proposed deep holes at Fenton Hill, considering results of our experiments to that time as well as our funding prospects. They also requested, by November 25, a report on a credible range of hydraulic-fracturing conditions, considering uncertainties concerning natural fracture-permeability and the dimensions of artificially produced fractures. This was a fairly large order for our small HDR organization, but that didn't bother the panel—who preferred to believe that the resources of a laboratory such as LASL were limitless.

16.2. A National Geothermal Energy Program Budget

On September 12, 1972, Tony Ewing at AEC Headquarters mailed me a report with the above title. It had been prepared by a federal Interagency Working Group made up of representatives from the USGS, the AEC, the NSF, the Bureau of Reclamation, the Office of Saline Water, and the Environmental Protection Agency. It outlined a 10-year program of geothermal energy R&D broken down into the following general categories:

- Resource Appraisal,
- Exploration Methods,
- Reservoir Development and Production,
- Utilization Technology and Economics,
- Environmental Effects, and
- Institutional and Legal Aspects.

The report proposed total funding of $652.5 million for the 10-year program, beginning at $25.0 million in FY74, peaking at $101.0 million in FY77 and again at $91.0 million in FY81, then falling off to $36.5 million in FY83. The working group visualized the program as a cooperative effort of federal agencies and private industry but recognized that the major share of its cost would necessarily be borne by the federal government until technical and economic feasibility of the new technologies had been established. While their outline of activities and funding extended only through FY83, the report assumed continuation of the program to at least the year 2000—with progressively reduced federal involvement.

While HDR activities presumably were included in all of the R&D categories shown below, breakdowns by resource type were listed only for Reservoir Development and Production and for Utilization Technology and Economics, where a few specific milestones were identified. For HDR—still called "Dry Hot Rock" by this working group—the proposed funding for hydraulic fracturing (our LASL program) and explosive fracturing (the BNW Plowshare nuclear program) appeared separately as shown below, with the costs in millions of 1972 dollars.

It was encouraging to us that representatives of this group of federal agencies took HDR seriously and proposed serious funding for its investigation and development. Of course it was not clear which agency, if any, would actually provide the proposed funds, although we were hopeful that in our case the AEC was preparing to do so. If there was some additional HDR funding for activity categories other than the two tabulated above, then the dollar amounts listed there did not appear unreasonable at the time. However, our program was already under way, and we also still needed funding for the balance of FY73.

Table 16-1.

	Reservoir Development & Production		Utilization Technology & Economics		
Fiscal Year	Hydraulic Fracturing	Explosive Fracturing	Hyraulic Fracturing	Explosive Fracturing	Proposed LASL Milestones
74	$2.00	$0.25	$0.50	$0.00	First hydraulic-fracturing tests
75	4.50	0.50	0.50	0.50	Demonstrate prototype generating plant
76	1.50	1.50	1.00	2.00	
77	4.50	5.50	8.00	6.00	Large heat source developed
78	3.50	5.50	2.00	1.00	
79	6.50	2.50	0.50	3.00	
80	11.50	4.50	1.00	4.00	Large prototype generating plant
81	4.00	2.00	16.00	1.00	completed
82	4.00	2.00	5.00	1.00	Prototype HDR power source developed
83	2.00	3.50	1.00	0.00	and tested
Totals	$44.00	$27.75	$35.50	$18.50	

16.3. Our own proposed budget and organization

Of course we had our own ideas concerning future activities and funding requirements for the LASL HDR program. Among other things, we were concerned that even a completely successful demonstration of the hydraulic-fracturing method of developing an HDR energy system might be assumed by others to result from the fortuitous existence at Fenton Hill of a uniquely favorable geologic environment. Therefore, to achieve commercial acceptance of the general technique, we felt that it would be necessary to conduct similar experiments at locations remote from Fenton Hill significantly different from Fenton Hill in those respects and to develop HDR energy systems at those locations as soon as possible.

In discussions with Dick Taschek on September 13, 1972 (before I received the Interagency Working Group report described above), I suggested the following annual budgets and major activities for our HDR Program. In addition, and not shown in the Optimum Funding column, I proposed separate annual funding of about $1 million (perhaps provided by DPR) for background research in support of the engineering development that was our primary programmatic function. The money amounts listed are in millions of 1972 dollars.

Table 16-2.

Fiscal Year	Optimum Funding	Major Activities
73	$1.3	Complete experiments in GT-1 and drill GT-2 and first leg of two-hole system at Fenton Hill
74	4.7	Two-hole system completed; circulation experiment begins.
75	4.0	Occurrence and result of thermal-stress cracking determined. Search for a second site in different geology begins.
76	2.5	Steady state chemistry of recirculated heat-extraction fluid determined. Start exploratory drilling at second site.
77	3.3	Complete experiment at Fenton Hill and evaluation of second site. (If a power plant is to be built at Fenton Hill, add $7 million for its design and equipment.)
78	3.5	Circulation loop completed at second site. (If power plant has been built at Fenton Hill, add $1 million to complete and begin operating it.)
79	4.0	Circulation experiment conducted at second site. Begin exploration for Site 3. (If power plant operating at Fenton Hill, add $1 million. If power plant is to be planned for Site 2, add another $1 million.)
80	3.5	Continue circulation experiment at Site 2. Begin development at Site 3. (If power plant is constructed at Site 2, add $6 million.)
81	3.5	Complete circulation experiment at Site 2. (Add $2 million if power plant at Site 2 has been started—to complete and begin operating it.) At Site 3, begin circulation experiment.
82	2.5	Complete circulation experiment at Site 3. (Add $1 million for planning if a power plant is to be built there.)
Total:	$32.8	(or $51.8, if power plants are included)

that offered the widest possible variety of geologic conditions, depths, and temperatures. Accordingly, even while work at Fenton Hill was still in progress, we proposed to begin a search for other sites

In hindsight, all of this of course was outrageously optimistic with regard to both schedule and costs, but it does represent our wishful thinking and guesswork at the time. It also indicates the importance that we attached, to exploring and developing HDR prospects significantly different from that at Fenton Hill as a demonstration of the versatility and potential widespread usefulness of HDR energy systems.

At this same meeting, Dick and I also discussed formation of an official LASL Geothermal Energy Group, presumably to be formed during that fiscal year (FY73). We agreed on the following initial organization.

group full-time when that became possible. Darrell Sims, a widely recognized expert in designing drilling equipment and techniques for unusual situations, had been recruited by John Rowley to work in the Subterrene Program but was available to work part-time with us on directional drilling of very deep holes in very hot, very hard rock. Lee Aamodt was still an essential member of our unofficial group. He does not appear in the above organizational listing, probably because we thought at the time that he would prefer to remain on Bill Ogle's staff in the J Division Office.

Table 16-3.

Person or Group	Function	Fractional Time	Remarks
M. C. Smith	Group Leader, Project Mgr.	1.0	Diminishing commitment to CMB-13
R. M. Potter	Alternate Group Leader, Assistant Project Mgr.	1.0	Diminishing commitment to CNC-4
G. P. Courtney	Secretary	0.5	Shared with CMB-13 and CMB-8
D. W. Brown	Project Engr., Field Mgr.	1.0	
ENG-6	Eng.Design, R. H. Hendron	1.0	Will remain in ENG-6
	assisted by R. D. Foster	1.0	Will remain in ENG-6
H-8	Geology, Hydrology, Drilling, W. D. Purtymun	0.5	Will remain in H-8
D. L. Sims	Drilling, Hole Design	0.5	Shared with Subterrene Program
F. G. West	Geology, Hydrology	1.0	Currently in J-6
J-9	Seismology, K. H. Olsen	0.5	Will remain in J-9
CNC-11	Geochemistry, J. P. Balagna	0.5	Will remain in CNC-11
B. R. Dennis	Instrumentation	1.0	Currently in WX-7
———	Rock Mechanics	0.5	To be recruited and shared with the Subterrene Program
———	Technician	1.0	To be recruited
	Total Full-Time Equivalents	**11.0**	

Bob Potter, Don Brown, and I were already fully committed to the HDR Program, and Georgia Courtney was already serving as our part-time secretary. Bill Purtymun, Ken Olsen, and John Balagna were deeply involved in some of our DPR-supported activities, and Bob Hendron and Dick Foster had undertaken some pre-engineering design work for us. Francis West and Bert Dennis had volunteered to help us in their technical specialties, and both had expressed interest in joining the HDR group full-time when that became possible.

All of this, of course, represented only Dick's and my thinking on that particular day. However, most of it eventually materialized, with a couple of major exceptions. After talking things over with Bob Potter and others, I met with Allen G. Blair, then an assistant to Dick Taschek, on November 27, and we made a few changes. In particular, Bob—

my partner in starting the whole thing—emphatically refused to become a manager, preferring instead to remain an untitled staff member so that he could work and think instead of shuffling paper. Don Brown was an excellent alternative choice for a management job, and we listed him as both assistant group leader and project engineer—while showing Bob Potter as a staff member responsible for "analysis and advanced concepts." Al and I agreed that we would need a secretary more than part-time and listed Georgia Courtney as full-time in that position. By this time Lee Aamodt had agreed that he would like to transfer to the new group when it was formed. Harry Jordan had approved hiring a technician in H-8 to assist Bill Purtymun; Charles Browne had approved hiring a technician in J-9 to assist Ken Olsen; Jim Sattizahn had approved assigning a half-time technician to assist John Balagna; and John Rowley had approved sharing with us both Darrell Sims and a new rock-mechanics staff member when we found one.

We were all set except for funding and actual formation of such a group.

16.4. Expansion of the group begins

On October 3, 1972, Dick Taschek told me that funds would be available within a few days to support Francis West and Bert Dennis full-time in the geothermal-energy program. This came as a very pleasant surprise to me, since the program was still unofficial and not officially funded except for the DPR contribution—which was already overcommitted. Obviously Dick knew something that I didn't, including where the money was to come from. In any case, he instructed me to start through regular channels to arrange the transfer of Francis and Bert from J-6 and WX-7 to our nonexistent geothermal-energy group. Accordingly, at my request, Dick Baker (my CMB division leader) agreed to call the J and WX division leaders and get their approval for me to talk to Francis and Bert about such a transfer. Dick called, the other division leaders approved, and I talked to Francis and Bert—this time officially. They were soon full-time participants in the HDR operation and important contributors to its progress.

16.5. We become somewhat official

In a letter dated October 13, 1972, to Harold Agnew, Jerry Johnson (then director of DAT at AEC Headquarters) outlined the general practices and procedures to be followed by DAT and, among other things, officially recognized our plans for an HDR Program. This letter authorized LASL "to initiate work in general energy development areas" with the broad goal of "turning over to industry viable technologies, or as mutually agreed to, the final development of technologies initially supported jointly or solely by the Government." It continued to say that, "It is our intention to provide funds for your proposed geothermal energy development effort as soon as it can be arranged." Jerry went on to suggest that any questions be directed either to him or to Jim Bresee, his assistant director for General Energy Development, who became our principal contact at AEC Headquarters.

At that time the AEC's and LASL's fiscal years began on July 1, so that the Laboratory's funding and financial plan for FY73 (ending June 30, 1973) presumably had been established some months previously—although we were still expecting a special allocation of FY73 funds for our HDR Program. As indicated by the federal Interagency Working Group's proposals described above, FY74 federal funding for geothermal energy R&D was already being considered in Washington. Because of this, in a memo dated November 6, 1972, Dick Taschek requested that, by December 1, I provide him with long-range budget projections for the period FY75 through FY79. (For some reason, I did not receive the memo until December 8, so I had to hurry.) Dick asked me to include in the budget both operating costs and capital equipment obligations and, year by year, the average number of full-time equivalents on the payroll with staff members (salaried employees) and graded series (hourly employees) listed separately. A similar request was included in a letter dated November 9, 1972, to Harold Agnew from S. G. English and Frank A. Camm for the AEC Divisions of Research and of Military Application, respectively. (It was probably a telephoned preview of this letter that inspired Dick's memo to me.)

It was, of course, important that the engineering development to be funded by DAT be coordinated with the background research supported by DPR. Therefore it was useful that Enloe Ritter telephoned me from AEC Headquarters on December 11, 1972, to tell me what we could expect from DPR. As has been mentioned above, he said that "with some confidence" we could expect $100,000 in operating funds plus $20,000 for equipment in FY73. He also thought that $200,000 to $400,000 would be provided to support our geophysics research programs in FY74, and he stated that all this would be firmed up during January.

Upon receiving Dick Taschek's memo, I began an intensive period of head scratching, estimating, and guessing to prepare detailed breakdowns of schedules, manpower requirements, and operational equipment costs. Since each successive year's activities depended directly on what had been accomplished during the previous year, I felt it necessary to include some comments concerning the rest of FY73 as well as breakdowns for FY74, neither of which was asked for. The results, submitted later in December to Dick and by him to the AEC, were summarized in a short document titled "Dry Geothermal Source Demonstration." The first paragraph of the text of that document summarizes our situation at that time quite clearly, and is repeated here.

> "Through midyear of FY73, no funding commitment has been made in support of this program, no LASL organization has been established to conduct it, and no LASL manpower has been formally assigned to implement it. No relief from this situation is in sight, and program plans for the balance of FY73 are therefore in abeyance. It is still hoped, however, that funding and formalization of the program will occur soon enough so that, during FY73: (1) A second deep exploratory hole ("GT-2," about 4000 feet deep) can be drilled at the proposed site of the first major energy extraction experiment; (2) Enough hydrology, pressurization, hydraulic-fracturing, and tectonic stress measurements can be made in this hole, and with sufficiently positive results, to demonstrate that the site is indeed suitable for a major circulation experiment; (3) At least the funding commitment can be made for drilling of the first large hole for that experiment. The above manpower and cost breakdowns and the project schedule outlined above assume that this will all be possible in FY73."

The manpower and cost breakdowns are repeated below, and the proposed project schedule was generally similar to that discussed with Dick Taschek on September 13 and reported above on page 122.

Table 16-4.

	FY74	FY75	FY76	FY77	FY78	FY79
Man-Years (Full-Time Equivalents)						
(a) Scientific	21	20	26	26	26	26
(b) Other Technical	11	14	16	16	16	16
Total	32	34	42	42	42	42
Funding (in Thousands)						
(a) Direct Salaries	734	915	1,064	1,128	1,196	1,268
(b) Materials, Services, Subcontr's.	2,063	1,681	4,000	3,500	4,000	3,500
(c) Indirect Expenses	374	467	543	575	610	647
Total Operating Costs	3,171	3,063	5,607	5,203	5,806	5,415
Obligations for Capital Equipment Not Related to Construction	539	352	6,600	500	500	7,500

There were many efforts to follow up and support the general schedule that this represents. One of the most interesting of these was a series of letters (with supporting documents) from Glen A. Graves, then Dick Taschek's assistant for research, to James E. Akins, President Nixon's science advisor at the White House. Glen made a detailed and very strong case for presidential support of the HDR Program as an important contributor to the solution of what was becoming a national energy crisis. Among other things, he pointed out several possibilities for HDR technology beyond thermal and electrical energy production, emphasizing the possibilities of using hydraulic fracturing and thermal-stress cracking for increasing oil and gas production, for producing underground storage cavities, and for regasifying liquefied natural gas. We had always hoped for presidential as well as congressional support for HDR, and this was a particularly good try.

16.6. Fourth meeting of the Geosciences Advisory Panel

The fourth GAP meeting was held at LASL on November 30 and December 1, 1972. It began with a closed session of the panel chaired by Dick Taschek, followed by our presentations on the HDR Program.

In introducing the HDR session, I emphasized the technical frame of reference in which we were, and would be, working. I explained the dual nature of the Laboratory's activities: it was both a scientific institution doing a great deal of fundamental and applied research, and an engineering-development laboratory, specializing in large, difficult projects. The HDR Program had the same dual nature, with separation of its research and engineering-development functions emphasized by our two funding sources. We expected our major funding to come from the AEC Division of Applied Technology, for the fastest possible development and demonstration of a new kind of geothermal energy system. Necessarily, there would be a high degree of pragmatism in such a program and, in principle, our HDR group would be involved only in that activity. However, it would also require a great deal of background information and special expertise from research projects related to, but clearly separate from, the main line engineering development program. With separate and much smaller funding from the AEC Division of Physical Research, we expected this relatively fundamental long-range research to be done by others. (In practice, of course, this bifurcation was pretty fuzzy, and necessarily we did much more research with DAT funding than was contemplated in our original charter. However, at this point I felt it necessary to make the panel aware that this distinction officially existed. Its members were primarily research scientists and, while they were very helpful in many important ways, most of them expected a research emphasis in our field projects that we simply could not provide. Most of us in the HDR group were research-oriented too, so we were sympathetic with their viewpoint. However, our job was to produce a commercially viable engineering system and, when we couldn't explain something quickly, let science do that later.)

My harangue to the panel was followed by presentations by

- Bob Potter, on temperature logging and heat-flow;
- Francis West, on the hydrology of the test area and hydrologic experiments in GT-1;
- Bill Purtymun, on water sampling in the area and the analyses of the samples collected;
- John Balagna, on his geochemistry studies, including planned leaching experiments on basement-rock samples;
- Don Brown, on the next set of field experiments, which were to be pressurization and hydraulic-fracturing experiments in GT-1; and
- Lee Aamodt, on the prospects for thermal-energy extraction from fractured HDR reservoirs.

There was a closed session of the panel at the end of the first day of the meeting. The second day began with presentations on the Subterrene Program and later that day we again reviewed the HDR

Program—primarily for the benefit of Allen V. Kneese, an economist from Resources for the Future, Inc., who was a guest of the panel on that day only. This time I reviewed the entire HDR Program briefly; Bob Potter discussed his preliminary economic analysis of HDR energy systems; and Don Brown discussed our strategy for drilling our second, deeper exploratory hole at Fenton Hill. This was followed by an open session to discuss both the HDR and the Subterrene Programs and finally by a closed session of the panel to prepare its comments and recommendations.

In its subsequent report to Harold Agnew, the panel focussed almost entirely on the HDR Program. Its comments and recommendations concerning HDR can be summarized as follows:

1. The panel was concerned that we had not provided them with hard scientific data on the thermal regime in GT-1 and our various heat-flow holes, which they considered essential to selection of a site for a deeper exploratory hole. They urged that we resolve the existing uncertainty in the magnitude of the heat-flow in GT-1 without delay and recommended that we try to make good heat-flow determinations in holes A, B, C, and D—our 600-ft holes west of the caldera.
2. They recommended that we actively develop plans for deep HDR experiments at alternative sites, so that we could move to a different location if insurmountable difficulties arose at Fenton Hill.
3. They recommended that we map the streams, springs, and wells in the vicinity of our experimental sites and, as soon as possible, initiate a program to measure flow rates and monitor water chemistry in those areas most likely to be affected by our experiments. They proposed that this should include monitoring the pH and chemistry of precipitation falling on the area.
4. As usual the panel's report emphasized geochemistry and petrography, recommending the following with regard to core samples from GT-1.
   a. A curator should continue to be in charge of all samples, and sample records should be kept of all investigations made on each.
   b. Chemical analyses of the three major rock types encountered should be augmented by determining FeO as well as Fe_2O_3.
   c. Electron microprobe analyses should be made of individual minerals to correlate modes of occurrence with bulk chemical analyses and investigate the possible presence of two feldspars.
   d. Analyses should be made of trace elements, especially the rare earths; of Sr^{87}/Sr^{86}, O^{18}/O^{16}, and D/H ratios; and total oxygen content. Some samples should be mapped for hydrogen content to reveal the distribution of water.
   e. Lead-uranium ages should be determined on zircon and compared to ages of other Precambrian rocks in northern New Mexico.
   f. Leaching experiments should be initiated as soon as possible.
   g. The identity of carbonate present as vein material should be established and investigations of opaque minerals continued.
   h. Skills should be developed for identification of alteration products of feldspar in the granite.
   i. Petrofabric analyses should be made to determine preferred orientations of grains —which affect structural properties and waterflow through the rock.
   j. Petrographic and petrofabric data should be correlated with physical properties such as permeability and resistivity.
5. At earlier meetings we had discussed the possibility of initiating significant earthquakes with the fluid injections required to produce large hydraulic fractures, and the panel had agreed that the methods developed for predicting the ground motion and structural damage produced by nuclear explosions at the Nevada Test Site would be useful in estimating the possible seismic effects of such injections.

Using two of those methods, Lee Aamodt had estimated that earthquakes of magnitudes 3.65±0.5 or 5.0 could be tolerated in the vicinity of Fenton Hill. Recalling that fluid injections at the Rocky Mountain Arsenal had resulted in earthquakes larger than these, the panel felt that we should examine this possibility more closely. In particular, they recommended that we

a. prepare a map showing all building structures within 1 mile of our test well and all sensitive structures and other features susceptible to damage (including possible landslides) within 5 miles; and that we carefully examine the nearby village of Jemez Springs.
b. develop a detailed comparison between our site and that of the Rocky Mountain Arsenal injection well, and between structures near our site and those near the Arsenal well that were damaged by the earthquakes produced there.

6. The panel recommended that additional economic calculations pertaining to HDR systems be made, allowing for probable underestimates of the costs of such systems—since unanticipated problems always occur with a new system.

As usual the panel recommended a number of things that we were already doing as rapidly and accurately as we could (and had described to them at this meeting); a lot that could be interesting and useful in the long range, but that an unofficial unfunded program had neither the manpower nor the money to undertake; and an emphasis on relatively fundamental research that we knew DAT would not support. The tone and some of the contents of the panel's comments and recommendations were such that, in turn, some of the HDR staff commented on them to Dick Taschek. Dick apparently relayed these comments to Orson Anderson in a letter dated December 22, 1972, and Bob Potter visited Orson at UCLA during the first week of January 1973, with additional comments of his own. Orson responded in a letter to Dick dated January 15, 1973, emphasizing that a detailed report on heat flow in our geothermal area would be more than a research report; it would represent good programmatic planning toward a decision to drill a deeper well at Fenton Hill. He presented his view that "the best chance for survival of the LASL GTE program is to project the image of a moving group producing scientific results" and that "One good way to have a high visibility is to have abstracts, talks, and papers in progress."

Of course Orson and most of the rest of the panel were right so far as the scientific community was concerned, and as time went on we did publish good scientific results from both our DAT-funded field studies and background research initiated with DPR funding. However, our main program was engineering development, and the people we wanted most to impress were those in the energy industry. This difference in viewpoint between us and the panel never changed.

16.7. Another early paper

It was not the sort of paper that Orson had in mind, but we had produced another paper, titled "A New Method of Extracting Energy from 'Dry' Geothermal Reservoirs" by D. W. Brown, M. C. Smith, and R. M. Potter. It appeared originally as a LASL preprint (LA-DC-72-1157, dated September 20, 1972), which we circulated quite widely. Don later presented it both at the Southwestern Petroleum Short Course at Texas Tech University in April 1973 and at the Geological Society of America's Annual Meeting in Dallas.

This paper described both our current concept of a commercial HDR system developed at a depth of about 15,000 ft in rock at 300°C and our proposed feasibility experiment on the Jemez Plateau at a depth of about 7500 ft in rock at about 285°C. In this prototype system, we intended to create a single vertical hydraulic fracture with a radius of 1500 ft—small enough so that cooling of the fracture surfaces would significantly reduce the temperature of the produced fluid in less than a year unless (as we predicted) thermal-stress cracking enlarged the fractured region considerably.

16.8. Some Contacts at a United Nations Seminar

On January 8–10, 1973, "A Seminar on Development and Use of Geothermal Energy" was held at United Nations Headquarters in New York City. It was organized and chaired by Joseph Barnea, director of the UN Resources and Transport Division, and attended by about 150 people—including Jim Bresee, Bill Ogle, and me. It was concerned primarily with the UN's own program to discover and evaluate geothermal energy sources in underdeveloped countries but did include some discussion of other programs—in Iceland, Mexico, and at The Geysers in California. There was very little discussion of newer ideas such as development of geopressured or HDR geothermal resources, although I had good informal discussions of our HDR Program with several foreign attendees, outside of the meeting room. Therefore, while the seminar was of general interest, it was important to me primarily as an introduction to the international geothermal community and because of some additional U.S. contacts that I made there.

In particular, I met Charles L. ("Chuck") Baldwin, a consultant to the Senate Committee on Governmental Organization of the California Legislature. He expressed an interest in our HDR Program and later that month wrote to me asking for more information about it—stating his opinion that there were many places in California where man-made geothermal systems would be useful. Of course I agreed, and on February 1, 1973, I sent him some literature and an update on our program. That was the first of a long series of useful contacts with agencies of the State of California.

There was also another contact with a Union Oil Co. representative—Kenneth Stracke, then manager of operations for Union at The Geysers in California. Jim Bresee, Bill Ogle, and I had lunch and about an hour of technical discussion with him. He was interested in the possibility of using hydraulic fracturing to stimulate steam production from currently unproductive wells at The Geysers. When Jim suggested that DAT might be interested in helping to fund such an experiment on a cooperative basis, Stracke quickly informed him that Union would not be interested in such an arrangement; if they undertook such an experiment, it would be done entirely by Union and with its own funding. (It was several years later that Union's view of such an arrangement changed, and a cooperative well-stimulation experiment was indeed conducted at The Geysers.)

16.9. Foreign involvement in the HDR Program

In the years following those covered by this report, worldwide interest in HDR energy systems grew rapidly and led in several countries to development there of R&D programs that were parallel or complementary to or cooperative with our own. In addition to the brief contacts made with foreign geothermal experts at the United Nations meeting, several others were made in 1973 that helped to arouse or increase that interest.

16.9.1. Our first Japanese visitor. On June 20, 1973, Jim Bresee called me from Washington and asked me to arrange an unclassified visit to our HDR Program by a Mr. S. Shikinami, a Japanese national, who would arrive in Los Alamos the next afternoon. In those days an unclassified visit by a foreign national could be arranged with just a couple of phone calls, which I made.

Mr. Shikinami arrived in Los Alamos on schedule on June 21, on the 4:45 Ross Airline flight from Albuquerque. I met him at the Los Alamos airstrip and found that his English was adequate and that he had spent enough time in the United States so that he had no real difficulty with travel arrangements and accommodations. We talked briefly and I took him to the Los Alamos Inn (where we had reserved a room for him), got him settled in, and arranged to meet him there the next morning.

It turned out that Mr. Shikinami was a consulting engineer and managing director of Nippon Industrial Cleaning Services Company of Tokyo, Japan. Somewhat unexpectedly to me, in view of its name, his company held the license from Halliburton Services for hydraulic fracturing and other well services in Japan and wanted to be licensed for use there of our HDR heat-extraction system. He had corresponded about this with Roland A. Anderson, assistant general counsel for patents of the AEC, and had been in Washington to discuss the possibility at AEC Headquarters. Jim Bresee felt that there were important possibilities in this contact because of State Department interests, balance-of-payment implications, and other considerations.

On June 21, I picked Mr. Shikinami up at the Inn, and Don Brown, Francis West, Bert Dennis, and I spent the morning with him. In English he read better than he listened, but when he understood the words he certainly got the message. He urged us to urge the AEC to get Japanese patent coverage on the HDR system immediately.

In the afternoon, Francis and Bert took him to the GT-1 site in Barley Canyon and then back to the Inn, from which a Zia Co. taxi took him to Santa Fe, where he had reservations. We were well impressed by Mr. Shikinami, who was evidently a very competent engineer and organizer.

This was the first of many contacts with Japanese engineers and scientists, a number of whom later came to work closely with us at LASL and eventually returned home to establish an excellent HDR program in their own country.

16.9.2. The NATO-CCMS Pilot Study. At the spring 1973 plenary session of the Committee on Challenges of Modern Society of the North Atlantic Treaty Alliance (NATO-CCMS), the U.S. representative—Russel Train, chairman of the federal council on Environmental Quality (CEQ)—proposed an international study of solar and geothermal energy systems. This was approved by the plenary committee and, as one step in initiating it, it was agreed that a "Meeting of Experts" should be arranged to consider the organization and activities appropriate to a pilot study of geothermal energy.

Organization of that meeting was delegated to Lawrence Livermore Laboratory (LLL). My first word of it come in a telephone call on August 27 from Andrew ("Andy") Lundberg at LLL. He told me that the Meeting of Experts would be held at LLL, in Livermore, California, during the first week in October, that I would be invited to attend, and that the organizers needed ideas concerning projects that might be considered. I gave him a few suggestions.

On August 30, Jack H. Howard called me, also from LLL. He proposed a meeting at LLL to coordinate our ideas of the things that the AEC might propose to CCMS. I agreed to attend. The meeting was held on September 6. In addition to Andy and Jack and me, Dick Lyon came from ORNL and Ken Mirk from LBL. Jack Kahn and Fred Fulton from LLL also attended parts of the meeting. We could not, of course, speak for the AEC, but at that time we did represent its major geothermal-energy projects except for the Plowshare study and could appropriately make recommendations to DAT about its possible participation in the NATO-CCMS study.

Among other things, we agreed that—as sponsor of the Marysville project—the NSF should certainly also be involved. The LLL people subsequently passed the word to NSF Headquarters in Washington and, in a telephone conversation with me on September 27, Glen Graves (then on leave from LASL to work with NSF) inquired about the Meeting of Experts. I told him what I knew, and that afternoon he had a meeting with Frank Hodsel of CEQ, who was already directly involved in other NATO-CCMS projects. Glen told him about both the meeting and our HDR Program and apparently did a thorough job on both. The next day, Hodsel called Jack Howard and asked him to be sure that Jim Bresee—who by then was scheduled to represent AEC Headquarters at the Meeting of Experts—discussed the HDR Program at that meeting. Jack called and asked me to approach Jim about it, so I called Jim. He said that of course he intended to do so.

The Meeting of Experts was held at Livermore on September 30 through October 3, 1973. At it, Jim described the DAT program and plans, including a short description of the LASL program. This aroused sufficient interest among both American and foreign attendees that Jim called on me to discuss it more fully and answer their questions about it. This was one of our first presentations concerning the HDR concept and program to the international geothermal community, and it had an unexpected result. The report of the meeting proposed to NATO- CCMS not only that a geothermal pilot study be initiated, but also that "Exploration of Hot Dry Rocks" be one of five subject areas This was approved later that month by the NATO-CCMS Plenary Committee meeting in Brussels. The pilot study was officially established and included a project called "Exploitation of Hot Dry Rocks." That project was assigned to LASL, where a "Hot Dry Rock Pilot Study" was established with me as lead participant. It continued through 1977 and resulted in many foreign visitors to our program, direct involvement of a number of them in it at Los Alamos, and subsequent establishment of parallel and complementary HDR programs in several other countries.

16.10. More sales pitches

During the winter and spring of 1972-73, there were further opportunities to spread the HDR gospel. A representative sample of the wide variety of audiences that we addressed is the following list of groups and individuals to whom I presented the message during this period.

- Fellows in Public Science Policy and Administration of the University of New Mexico (faculty members and graduate students) at Los Alamos, December 1, 1972.
- Chamisa Elementary School, 5th and 6th grade science classes, White Rock, NM, December 12, 1972.
- Senator Joseph M. Montoya (D-NM) and accompanying press and television crews, at Los Alamos, December 21, 1972.
- USGS representatives from Albuquerque, NM, at Los Alamos, December 27, 1972.
- Dr. Harold Chesnut, General Electric Co., President of the Institute of Electrical and Electronic Engineers, at Los Alamos, January 16, 1973.
- CBS television crew, at Los Alamos, February 7,1973.
- John J. Flaherty, AEC Headquarters, at Los Alamos, February 16, 1973.
- Regents of the University of California, at Los Alamos, April 12, 1973.
- State Convention of the League of Women Voters, at Los Alamos, April 18, 1973.

Of course, other members of the HDR group also told the story to other audiences, and Harold Agnew continued to be our star salesman. Bill Ogle, by then retired from the Laboratory and operating his own consulting firm in Anchorage, Alaska, also joined in. At the first Alaskan meeting of the Western Interstate Nuclear Board in late fall 1972, he made a pitch for geothermal-energy development in Alaska—including HDR. There were undoubtedly other proponents of whom I have no record, and we were getting the message out.

One person who heard the message and took it seriously was our Senator Montoya. As reported in the *Los Alamos Monitor* on January 31, 1973, the Senator had recently called on the Congressional Joint Committee on Atomic Energy to give more attention to the development of geothermal energy. He informed them that, without specific authorization, LASL had been carrying on research on geothermal energy for some time, and said that "This has been an orphan program. It's time that the research gets proper funding and authorization from this committee." Further, he noted that, to that time, LASL had paid for the program (our HDR Program) from its own discretionary funds, and commented that "If the program is important enough to be carried on at all, it is important enough to be funded and authorized by this committee." There was loud, if belated, applause from the LASL participants in the program. That was the kind of sales pitch that we needed most.

17. PREPARATIONS FOR EXPERIMENTS IN WELL GT-1

As has been discussed, our primary purpose in drilling well GT-1 was to explore the geology, hydrology, geochemistry, and heat flow in the geothermal area on the Jemez Plateau just west of the Valles Caldera and to investigate the drillability, hydraulic-fracturing behavior, mineralogy, structure, and permeability of the Precambrian basement rock penetrated by the lower part of the hole. The well was completed on June 30, 1972, at a final depth of 2575 ft, penetrating 470 ft into the basement rock. It was cased at 5-in. diameter to a depth of 2400 ft, leaving 165 ft of 4 1/4-in. diameter open hole at the bottom of the well for hydraulic-fracturing and other experiments.

Among other things, some of the experts whom we had consulted had predicted that we couldn't even drill into the hard basement rock—which, in completing GT-1, we had shown to be incorrect. Others were sure that we could not fracture it hydraulically, and still others were confident that the basement rock would be so highly jointed and fractured that it would not contain pressurized water. We thought otherwise, and we intended to investigate these and other reservoir characteristics experimentally in well GT-1. However, before these experiments could be carried out, it was necessary to procure a workover rig and other necessary equipment and to mobilize it all at the Barley Canyon site.

17.1. Preliminary plans for experiments in GT-1

In memoranda to Dick Taschek on November 17, 1972, and to Dick Baker on November 22, 1972, I outlined our preliminary plans for experiments to be conducted after a workover rig had been erected over well GT-1. As described in my memo to Dick Taschek, these were as follows:

1. Workover drilling rig moved in about November 28.
2. Circulate water through tubing extending nearly to bottom of hole, to flush out solids. 1 day.
3. Fill hole completely and run constant-head test, while also logging temperature repeatedly to observe approach to temperature equilibrium and, if possible, to identify zones of water influx and loss. 1 day. (If large inflows or outflows are observed, the packer-hydrology studies outlined below may be elaborated.)
4. Reduce water level in hole (probably by pressurizing the annulus around the tubing with air) to below what is believed to be the water level in the sediments around the hole—say to 500 feet below ground level. Then monitor water level as a function of time, to detect inflow of water (if any) resulting from negative head in hole. Repeat at depth intervals of perhaps 100 feet until pore pressure and interconnected pore volume in the uncased section are determined at least semiquantitatively. Estimated 2 days.
5. Withdraw tubing and install packer near top of uncased section of hole, in 4 1/4-in. diameter bore section. Pressurize uncased section of hole below packer in increments of perhaps 100 psi until significant rate of water loss is observed or pressure approaches 1000 psi. If significant water loss is observed, determine rate of loss as function of pressure and proceed with straddle-packer tests. If water loss is not significant, omit packer-hydrology experiments outlined in next section. 1 day.
6. If significant water loss is observed in the above experiment from the entire uncased hole section, an attempt will be made to identify the specific regions in which the loss is occurring by repeating the incremental pressurization studies within individual 10- or 20-ft long sections of the hole isolated successively by a pair of straddle-packers. This will complete the second phase of the hydrology studies. 2 days.
7. Upon completion of these hydrology experiments, the existing fracture pattern in the wall of the bore will be mapped as completely as possible by making a series of impression-packer tests along the full depth of the uncased section of hole. 2 days.

8. A series of small hydraulic fractures will then be made in the uncased section of hole, between packers spaced about 5 feet apart, using both downhole and surface measurements of pressure and of acoustic signals. If possible, breakdown, crack-extension, and shut-in pressures will all be measured for each fracture and it is hoped that the results will represent unusually good determinations of the average *in situ* properties of the basement rock and of the local tectonic stress field. It is intended that the fracturing will be done alternately with University of Minnesota and with LASL equipment, beginning at the bottom of the hole and working upward. To avoid forming overlapping crack systems, an interval of about 10 to 20 feet will be left between test sections. If possible, 2 pairs of tests will be made in the amphibolite section, 1 pair in the true granite, and 5 to 7 pairs in the granitic augen gneiss. 10 days.
9. Impression packers will then be used to remap the fractured regions in the hole in order to characterize and correlate the fracture systems produced hydraulically. 1 day.
10. Packers will be set in the 4 1/4-in.-dia. uncased hole section below the lowest hydraulically fractured region and above the highest one. Then one relatively large hydraulic fracture will be produced, perhaps 50 feet in radius, with final injection of a proppant labelled with ^{95}Zr and perhaps of chemically tagged water to permit monitoring of water-return to the hole. Whether this fracture is produced incrementally or in one step will depend largely on the quality of data collected when the small fractures were made. 1 day.
11. The fractured region will again be mapped with impression packers. 1 day.
12. The hole will be logged with oriented-gamma, televiewer, etc., to learn as much as possible about the final fracture system. 1 day.
13. Carefully controlled pressurization and depressurization experiments will be run to investigate water loss from the system and the possibility of crack extension at low pressure. 1 day.
14. A seismometer will be emplaced semipermanently at or near the bottom of the hole for acoustic monitoring of subsequent experiments in other holes, and fiberglass tubing will be installed extending from the surface to near the bottom of the hole for subsequent energy extraction experiments. 1 day.
15. The drilling rig will be removed from the site.
16. Hydrology and energy-extraction experiments may be continued for several weeks, and the hole will be kept instrumented for some months to collect microseismic background and monitor other experiments.

"The workover drilling rig will be required at the site for a total of approximately 25 to 30 days. We are now arranging for the necessary auxiliary equipment, instrumentation, trailers, etc., to carry out this series of experiments."

This memo concluded with the following cautionary paragraph. "Very little field experimentation of this type has ever been done anywhere, and what has been done has in general been fairly unsophisticated. There is, therefore, a minimum of background to guide us in our experiments, and much of our work will be in the development of instrumentation, techniques, and procedures. We expect, however, in this experimental series, to solve a very interesting hydrology and heat-flow problem; to produce some of the first data ever collected on the mechanical behavior of hot, *in situ,* crystalline rock, to make the first measurements of tectonic stresses ever attempted in this interesting area; and to develop a new type of close-coupled microseismic instrumentation with which to observe fracturing events and, eventually, to map the fractures produced. This is, in fact, a unique series of field-research experiments."

My memo to Dick Baker a few days later repeated the above preliminary plan and listed some additional things that we knew should be done, including

- thermal-conductivity measurements;
- mathematical analysis to elucidate the heat-flow situation;
- Investigation of the mechanics of hydraulic

fracturing and crack extension;

- laboratory investigation of the mechanical properties of core samples; and
- development and experimental use of both surface and downhole pressure transducers and microseismometer arrays.

Most of these things, I hoped, would be covered by DPR funding. However, while many of the experiments and investigations listed were completed successfully, for various reasons some were not—as will be discussed in the next chapter.

17.2. Funding and cost estimates

At that time (November 1972) the LASL financial plan for FY73 (the current fiscal year) included $50,000 provided by DPR to support geophysical research related to our geothermal-energy program—heat flow, rock mechanics, and seismology investigations. (Separate funding of $45,000 had also been committed by DPR for the geochemistry studies.) We expected additional FY73 research funds from DPR and major funding eventually from DAT. In the meantime, we had two separate budgets handled primarily through the CMB Division Office—generally by Dick Baker or James R. ("Jim") Lilienthal. One budget was designated E-532, covering work funded by DPR; the other, C-137, covered that supported by various funding sources within the Laboratory. However, assignment of costs to those two accounts was pretty arbitrary, depending largely on which one had some money in it at the time. This arrangement persisted until we finally received the promised DAT allocation, which was quite a while later.

In my memorandum to Dick Taschek, I included the cost estimates listed in Fig. 12. The estimated total cost of $48,850 essentially exhausted the $50,000 of DPR funding in the E532 budget, just for the materials and services required for the experiments proposed for GT-1. It left LASL manpower costs, accessory instrumentation, and a host of other things to be supported by other, unspecified funding—the C-137 budget.

17.3. Preliminary experiments in GT-1

While we were completing plans for the series of experiments in GT-1 and arranging for the equipment and instrumentation needed for it, we did what we could to prepare for and acquire background information concerning those experiments. This included the following activities:

1. Repeated temperature logs. The first temperature log in well GT-1 was run for us by the Birdwell Division of Seismograph Service Corporation on July 1, 1972, about 18 hours after the hole was completed at its final depth of 2575 ft. The second was run by Marshall Reiter of NMIMT on July 15. The third was run on July 30 by LASL personnel using USGS equipment loaned to us by Art Lachenbruch. Using its own newly developed equipment, the LASL crew ran additional temperature logs on September 15, October 26, November 6, and November 28, 1972, and on January 25,1973.

In the early temperature logs in GT-1, an observed warming of the upper part of the uncased section of the hole seemed to indicate a slow upward flow of water—at about 1 gpm—from near the bottom of the hole to near the top of the open-hole section. This possibility was investigated by means of a downhole turbine meter, and it appeared that a barely detectable upward flow might be occurring. In an attempt to measure this flow, on October 18, 1972, a USGS crew from Albuquerque, under Frank Trainer, released some brine at a depth of 2540 ft in GT-1, intending to monitor the rate of its upward movement by logging through the lower part of the hole repeatedly with an electrical-resistivity probe. They later repeated the brine injection at depths of 2435 and 2470 ft and, logging repeatedly through the three brine clouds on October 18, 19, and 27, found no evidence that they had moved. Their conclusion was that there was no evidence of water flow into GT-1.

An unexpected result of the brine injections was that the brine apparently caused flocculation and settling of particles of clay previously suspended in the water already in the well. This produced a

Item		Cost
1. Site Preparation and Restoration		$2,500*
2. Expendable Instrumentation		
a. Two 3-dimensional seismometers, with cabling, total	$4,800	
b. Pressure transducers and thermometers, total	~$ 500	$5,300
3. Services of electronic technician (E-Division), 2 weeks		$ 900*
4. Drilling Rig: Thirty 12-hour days at $30/hr plus mobilization and demobilization (12 hours at $40/hr); includes all costs of water hauling		$14,880
5. Packer Rentals		
a. Straddle packers and Husky packer, 12 days	$2,800	
b. Nine impression packers, 6 days	$7,430	
c. Fabrication of access mandrel	~$ 600	$10,830
6. Hydraulic Fracturing Equipment and Services		
a. Pressurization pump	$ 900	
b. Pump truck (1000 HP), blender, tank and manifold	$2,600	
c. Chemical additives and materials	$ 360	
d. Proppant (Forty 100-lb sacks with 95 Zr tracer)	$ 400	$4,260
7. Commercial Hole Logging		$3,600
8. Trailer Repair, Running Gear, Moving, and Upkeep		$1,500*
9. Road Maintenance: 40 hours at $16/hr		$ 640*
	Total	$44,410
10. Miscellaneous and Contingencies at 10%		$4,440
	Total	$48,850

*Not a firm estimate

Fig. 12. Cost estimates for experiments in GT-1, as of November 17, 1972.

gelatinous layer about 38 feet thick on top of a 17-ft layer of material (probably fine drill cuttings) that had already settled there. Thus the bottom 55 ft of the well was not accessible to temperature sensors during later logging runs—until the well was cleaned out to bottom in later experiments. This and evidence of relatively stable convection cells in the well led to considerable uncertainty in plots of temperature vs depth for well GT-1 at that time and, therefore, in calculations of the rate of terrestrial heat flow in the lower part of the hole.

2. Water-level measurements. Beginning in mid-October 1972 and continuing for several weeks, daily measurements were made of the water level in GT-1. It declined at a nearly constant rate of 1.2 inches per day. Knowing the surface area and depth of the uncased section and (approximately) temperature as a function of depth, this permitted a reasonably accurate calculation of the permeability of the exposed basement rock—which was determined to be about 5.4×10^{-8} darcy. This extremely low permeability (which was confirmed in later experiments) was very encouraging with regard to the probability that a fractured reservoir in the rock would successfully contain pressurized water. It also suggested that, in fact, the permeability was too low to have permitted a significant inflow of water at the bottom of the hole, as had been suggested by the warming noted in our early temperature logs, and no evidence of such an inflow was found in any later experiment.

   During this period, thermal-conductivity measurements on GT-1 core samples were made for us by both NMIMT and the USGS, which are discussed later, and a LASL capability for such measurements on rock samples was being developed.
3. Comparisons of barometric pressure with fluid level in the well. Repeated measurements of the water level in GT-1 and the barometric pressure at the same time showed no correlation between the two. This "low barometric efficiency" suggested that any flow paths in the rock around the uncased section of the hole consisted of fine, poorly interconnected porosity with no large open fractures.
4. Geochemistry. Under John Balagna of CNC Division, a high-temperature, high-pressure, laboratory scale circulation system was developed for chemical and kinetic studies of rock-water interactions. Its purpose was to study the hydrothermal solutions produced when hot water circulates through granite or other crystalline rocks under the conditions of temperature and pressure that exist in a geothermal reservoir, and the mineral-dissolution and alteration reactions that occur in the rock. The first materials to be tested were core samples recovered from the Precambrian section of well GT-1. Basic thermodynamic studies of such rock-water systems were also initiated.
5. Seismology. Under the direction of Ken Olsen of J Division, seismological studies were concerned primarily with development of a seismic net to collect background information on natural earthquakes in the geothermal area around the HDR experimental site and with design and construction of microseismic instruments to investigate the acoustic signals generated when deeply buried rock is fractured by fluid pressure or by thermal stress.

   A sensitive, portable triaxial seismometer and tape recorder were used at several locations in LASL technical areas to record the ground vibrations induced by detonation of small explosive charges in other Laboratory experiments. The system appeared suitable for monitoring the seismic signals generated by hydraulic-fracturing experiments, and an array of four such seismometers was planned for emplacement at the surface within about 1000 ft of the GT-1 wellhead. A small downhole triaxial seismometer was also developed for emplacement at the bottom of a packer in GT-1 to obtain near-source first motions and signal spectra during those experiments.
6. Core studies. The bottom 165 ft of well GT-1 were drilled at 4 1/4-in. diameter with continuous coring, and core recovery was essentially

complete. The recovered core was reassembled at the Laboratory, mapped, and sectioned systematically for petrographic studies and mechanical properties measurement and for selecting the depths at which we would attempt to make hydraulic fractures that would be representative of the various lithologies and structures exposed in the open-hole section of the well. The reassembled core showed a gradation from gneissic granite at the top of the uncased section, through typical granite and gneissic granodiorite, and into a biotite amphibolite that represented the bottom 61 feet of hole.

Bob Potter did a painstaking job of mapping all visible healed, filled, and open fractures on the reassembled core. Healed or sealed fractures, most of which were substantially vertical, were visible in much of the core. In general the fractures were tightly filled with calcite in the granitic regions and with chlorite in the amphibolite section. Two sets of what appeared to be inclined, unsealed fractures were identified within about 8 feet of the bottom of the hole. Otherwise, the basement rock appeared sound, dense, strong, and probably capable of containing a pressurized fluid.

7. Fracture dynamics. In a single uniaxial compression test on a granitic core sample taken at 2450 ft, J. C. Roegiers at the University of Minnesota measured an ultimate compressive strength of 27,500 psi and an initial Poisson's ratio of 0.23. Both values are typical of "good" granite.

   Both Roegiers and B. C. Haimson at the University of Wisconsin made laboratory-scale hydraulic-fracturing tests on core samples. On granitic samples from about 2450 ft, Roegiers measured a breakdown pressure of 3500 psi when no confining pressure was used and 3350 psi when an axial (vertical) confining pressure of 2000 psi was applied. Both fractures were vertical in spite of the fact that the specimen used in the second test contained a "recrystallized" (healed) fracture inclined at about 60° to the vertical—which did not influence the path followed by the hydraulic fracture.

   In an earlier test (described below) Haimson applied 3000 psi of vertical and 2000 psi of horizontal pressure to a granite specimen from a depth of 2457 ft and observed a breakdown pressure of 5000 psi. The fracture path was vertical and was unaffected by directionality and defects in the rock structure. Haimson felt that the breakdown pressure was normal for a strong crystalline rock under stress conditions intended to be representative of those existing at a depth of about 2500 ft.

   Development of a LASL capability for more extensive laboratory-scale measurements of this general type was initiated, together with design and construction of equipment and instruments for field studies of breakdown and crack-extension pressures during hydraulic-fracturing experiments in GT-1.

8. Environmental. Sampling and analysis of natural waters from lakes, streams, springs, wells, and drill holes in the area around our geothermal sites continued, again to establish background conditions against which any effects of our experimental operations could be evaluated.
9. Site preparation. By early February 1973, the GT-1 site had been prepared for mobilization of a workover drilling rig, a water-storage tank had been erected on the site, and our trailers and most of the equipment and supplies needed for our experiments had been assembled there.

17.4. Contacts with the University of Wisconsin

In 1972, Dr. Bezalel C. Haimson was associate professor of rock mechanics in the Department of Metallurgical and Mineral Engineering of the University of Wisconsin, at Madison, Wisconsin. He was well known for his work on hydraulic fracturing and had worked with the USGS on their historic series of earthquake studies and fluid-injection experiments at Rangely, Colorado. Upon learning of our HDR Program in the spring of 1972 (probably from our mutual friends at the USGS) he telephoned me to learn more about it and express his interest in it. Since he was travelling to Denver later in the summer, we invited him to detour through Los Alamos (at our expense) and spend a day with us. In the meantime, of course at Haimson's suggestion, I was invited to give a lecture on geothermal energy and our HDR Program at the biweekly Energy Group Seminar of the University of Wisconsin College of Engineering. I did so on April 3. My subject was "The Status and Future of Geothermal Energy." It was well received and followed by interesting discussions. I then had the opportunity to visit Haimson's laboratory and to discuss with him both our program and his own. I was very well impressed by everything I saw and heard.

Haimson did visit us in Los Alamos on July 24, and it was a very profitable day for us. We discussed our program with him, and he told us about his own work. He generously offered to do laboratory-scale hydraulic-fracturing and tensile tests on cores from GT-1, which by then were available, and at no cost to us. He also made himself available, on either a contract or a consulting basis, to work with us on further laboratory experiments on the mechanics of hydraulic fracturing and on our field experiments.

Following his visit to Los Alamos in July, we sent Haimson three samples of cores from the Precambrian section of well GT-1. From these he prepared three specimens, each 2 1/8-in. in diameter and 4 1/4-in. long, with an axial hole 0.3-in. in diameter and 3.0-in. long for injection of the fracturing fluid. Before testing he applied both vertical and horizontal loads intended to simulate stress conditions at a depth of about 2500 ft. On November 13, 1972, he sent us the following results of his hydraulic-fracturing tests:

- Specimen BH-1, gneissic granite from a depth of 2440 ft. Tested under vertical ("overburden") pressure of 2000 psi and horizontal pressure of 1000 psi. Breakdown occurred at a fluid pressure of 3800 psi. The initial fracture was vertical but it then turned and grew horizontally—which Haimson attributed to misalignment of the specimen's ends. (On the next two specimens this was corrected, and in them the fractures produced were and remained vertical.) Specimen BH-1 was coarse-grained with a preferred direction of grain orientation which, however, appeared not to affect fracture orientation.
- Specimen BH-2, granite from a depth of 2457 ft. Tested under vertical pressure of 3000 psi and horizontal pressure of 2000 psi. Breakdown occurred at a fluid pressure of 5000 psi and the fracture produced was perfectly vertical. Again the granite was coarse-grained with a preferred grain orientation that appeared not to affect fracture orientation. The breakdown pressure indicated a hydraulic-fracturing tensile strength of about 1500 psi. (It is the result of this test, in granite that we believed was typical of that which we would encounter at Fenton Hill, which is quoted in the previous section of this report.)
- Specimen BH-3, fine-grained amphibolite from a depth of 2544 ft. Tested under vertical pressure of 2000 psi and horizontal pressure of 1000 psi. Breakdown occurred at a fluid pressure of 6000 psi and the fracture produced was perfectly vertical. The hydraulic-fracturing pressure indicated a tensile strength of about 4000 psi. (We attached less significance to this result since we did not consider amphibolite to be a typical basement rock in our area. In fact, none was encountered in much deeper holes drilled later at Fenton Hill.)

Haimson considered that the fluid pressures required to produce these hydraulic fractures were normal for crystalline rocks.

Information from these tests was useful to us both as an indication of the pumping pressures and fracturing behavior we might expect in our subsequent experiments in GT-2 and as a basis for comparison of the results of well-executed laboratory tests with those to be observed later in the field.

We subsequently arranged for Professor Haimson to spend two days with us (February 26 and 27, 1973) as a visiting scientist. He addressed a seminar of people interested in rock mechanics on the subject of his laboratory and field studies of hydraulic fracturing, and spent the rest of his time with us discussing and advising us concerning our own planned experiments. Again, it was a very useful visit.

17.5. Contacts with the University of Minnesota

John Rowley was our in-house expert on theoretical and engineering mechanics. He recognized our need for a strong capability in rock mechanics and was probably the person who arranged our initial contact with Charles Fairhurst, head of the Department of Civil and Mineral Engineering at the University of Minnesota (U of M). In addition to an excellent research program on explosive fracturing, Fairhurst and his students had an active research program on hydraulic fracturing—which of course was of immediate interest to us.

Professor Fairhurst visited LASL in October 1972, accompanied by John-Claude Roegiers—a graduate student working on a Ph.D. dissertation on hydraulic fracturing. I was not involved in that visit but later received a copy of a letter from Fairhurst to Dick Taschek (dated October 27, 1972) discussing it. In that letter, Fairhurst cited an "agreement to work cooperatively [with LASL] on a number of outstanding problems" of hydraulic fracturing. The cooperation suggested by Fairhurst included the following:

1. Fairhurst had already submitted a proposal to NSF for support by the RANN Program of research on hydraulic fracturing at the University of Minnesota. In his letter to Taschek he stated that the proposal "will be modified substantially in view of the joint program with your laboratory on fracturing of the two geothermal holes." He asked Taschek to write to Dr. Ralph Long at NSF in support of the modified proposal.
2. In the course of the Rover Program, LASL had acquired a 5000-ton hydraulic press, which was viewed with great interest by rock-mechanics people for possible use in mechanical testing of very large blocks of rock. Fairhurst was enthusiastic about using the press "to observe both fracture initiation and fracture propagation phenomena under controlled conditions" and "to establish the validity of the whole basis for determining the influence of the applied stress field on [hydraulic] fracture behavior." He noted that "To do this, however, we will probably need to develop some method for applying biaxial compression to the large specimens."
3. Fairhurst invited members of our HDR staff to visit the University of Minnesota in order to acquaint themselves with its staff, facilities, and programs, and also invited someone from LASL to present a seminar there on our geothermal program sometime during the University's winter quarter.
4. He reported that currently they had no financial support for their hydraulic-fracturing research; they needed about $19,489 to support Roegiers while he completed his Ph.D. thesis and also another student who was studying seismic location of hydraulic fractures for a Master's thesis; and that they would appreciate suggestions for obtaining the necessary funding.

Lee Aamodt was involved in the discussions with Fairhurst and Roegiers, which of course must have covered many things not mentioned in Fairhurst's letter to Dick Taschek. In a memo to Dick dated November 16, 1972, Lee expressed enthusiasm about both the meeting with them and the prospect of cooperation with Fairhurst and his students. In particular, Lee noted "The fact that we can use Jean-Claude's hydraulic fracturing apparatus," and stated his own feeling that "If money can be found to put the large press in operating condition, it will be well-spent."

Jean-Claude's fracturing apparatus, which he called a "deep stress probe," was a specially designed inflatable straddle packer (a type of temporary seal described in the next chapter) which he had used successfully to create hydraulic fractures in relatively shallow drilled holes. He also had an "impression-packer" which he had used downhole to locate hydraulic fractures and examine their geometry. (An impression-packer is a cylinder several feet long with a diameter a little less than that of the wellbore, closed at its lower end, and lowered into the well on tubing through which water can be pumped into the packer from the earth's surface. The outer surface of the packer is rubber, specially formulated to take and retain an impression of any surface against which it is forced. In use the packer is lowered into the well and then inflated by pumping water into it, which presses its surface against the borehole wall. It is then deflated by relieving the internal pressure, lifted out of the well, and examined for the surface relief which is a negative image of the wall of the hole. At the time, it was one of a very few types of equipment available for such an examination.)

In a memo to Dick Taschek dated November 17, 1972, I commented on Fairhurst's letter and expressed some concerns about the cooperative agreement with the University of Minnesota. In particular, I noted that my understanding was that our current commitment to the University was only to (1) support Roegiers as a visiting staff member at LASL for about two weeks while we were doing hydraulic fracturing experiments in GT-1; (2) pay shipping costs on the U of M downhole equipment to be used in some of those experiments; and (3) permit Roegiers to use data from those experiments in his thesis.

In the memo I also noted that—since our plans for a second exploratory hole were not yet firm—our "joint program" should not include hydraulic fracturing in two geothermal hole as Fairhurst had suggested, but only in the one hole that we actually had. I was also concerned about tying ourselves too closely to the U of M since we already had an informal commitment to work with Bezalel C. Haimson at the University of Wisconsin, who was doing downhole and laboratory stress measurements using hydraulic fracturing, and we were also investigating the possibility of a cooperative agreement with C. Barry Raleigh of the USGS, who was studying the initiation of earthquakes by fluid injections. I suggested that the appropriate arrangement with Fairhurst was simply to have him available as a consultant and to do what we could to support his proposals to NSF or other funding sources. With regard to the 5000-ton press, I noted that its oil seals needed replacement (which would be a major undertaking); that a great deal of both engineering design and procurement would be required to modify it so that it could apply biaxial compression; that, at the time, we could not support even the design studies, let alone the necessary procurements and skilled labor to do the job; and that, therefore, a program involving that press could not be considered seriously at any time soon.

Nevertheless, in his reply to Fairhurst (dated November 28, 1972) Tascheck (1) proposed that Jean-Claude finish his dissertation at LASL as a long-term visiting staff member; (2) also proposed a visiting staff member arrangement with Fairhurst himself; (3) expressed "complete agreement with respect to the 5000-ton press;" and (4) promised to support Fairhurst's proposal to NSF with a letter to Ralph Long—which he did.

Both Roegiers and Fairhurst were subsequently given visiting staff member status and an agreement was concluded for us to use the U of M downhole packer assemblies—on a no-charge basis, with provisions that we should pay shipping costs, the cost of restoring them to a usable condition when we were done with them, and would pay U of M $22,000 if we damaged them beyond repair.

<u>17.6. Some special procurements</u>

Most of the materials, equipment, and services needed for our experiments in GT-1 were procured from commercial sources through regular LASL purchase orders. However, in addition to the U of M impression-packer and straddle-packer assemblies, other arrangements were made for a variety of other items. Among them were the following.

So far as possible, of course, we used existing LASL supplies, equipment, instruments, and services. For example, with a drilling contractor and various service companies on-site and with a number of around-the-clock experiments to be run, it was necessary for us to have a LASL manager at the Barley Canyon site at all times. To provide office space and overnight housing at the site for him and other LASL staff who might need it and to house our own instrumentation and portable equipment, we borrowed several trailers from various laboratory sites in Los Alamos and moved them temporarily to Barley Canyon. Several pieces of furniture were also obtained from Laboratory stock, including "2 beds with mattress." LASL staff staying there overnight generally provided their own sleeping bags.

In addition to begging, borrowing, or stealing many things from around our own Laboratory, we occasionally profited from the fact that—just for shipping costs—we could get useful items that had been declared surplus at other AEC installations. Much of what we obtained in this way came from the nuclear test site (NTS) at Mercury, Nevada. Francis West, Bert Dennis, and Jim Hill all visited NTS at various times to inspect the available surplus equipment, and during this period we acquired from there a skid-mounted hoist to handle downhole instruments in and out of GT-1; a trailer-mounted diesel-powered 40 kW generator to operate the hoist; two trailer-mounted 15kW diesel-powered generators for other site support; some heavy-duty switchgear; and a small water-to-air heat-exchanger to cool the geothermal fluid when and if we attempted a heat-extraction experiment.

In a few cases we were able to borrow needed equipment from generous industrial concerns on short-term no-cost loans. In this way we obtained some drill pipe and casing protectors from Byron Jackson, Inc., and a magnetic tape recorder from Cleveland Enterprises.

Necessarily, we were stretching our limited budget as far as we could.

17.7. The workover rig

In general, deep holes drilled in the earth must be lined with steel casing for much of their depth to shut off lost-circulation zones during drilling, prevent caving of the borehole walls, and avoid both groundwater flow into the hole and loss of fluid from it. The size of the drilling rig needed to complete such a hole is determined primarily by the hoisting capacity needed to handle the casing into the hole. For deep, large-diameter holes, the weight of the casing may be many tons, and correspondingly large rigs therefore are required. However, for workover operations on existing wells—such as cleaning them out or repairing or stimulating them—handling casing in and out of the well is not usually required and smaller "medium-duty" drilling rigs can be used. In this capacity they are called workover rigs. They represent a smaller capital investment by the drilling contractor, operate with a smaller crew, require less auxiliary equipment, and use less fuel and other materials and supplies, so that their hourly rates are considerably lower than those of larger rigs. Typically, on a workover operation they are operated only one 12-hour shift per day, during daylight hours, and only six days per week—taking Sundays off. Since our planned experiments did not require heavy hoisting and our HDR staff was small, the capabilities and schedule of a workover rig were appropriate for our work in GT-1. In particular, the one-shift, six-day-week rig schedule left us with needed time between downhole operations to analyze results and prepare for the next experiment.

In early October 1972, we submitted a requisition to our Supply and Property Department requesting that they investigate procurement of the services of a suitable workover rig for a period of approximately one month, at an estimated cost of $23,620. This was intended to cover not only the cost of the rig but also most of the necessary materials and supplies for our experiments, plus third-party services (paid for by the contractor, then recharged to us) including such things as site preparation, snow removal, packers, and any necessary special piping and plumbing. The cost estimate was increased on November 10 to $27,500 to provide additional funds for site preparation and a 10% contingency factor.

On October 13, Jim Sahling issued a request for quotations to six drilling firms thought to be suitably equipped for the proposed work. By the due date of October 27, he had received only one positive response—from Stewart Brothers Drilling Co. (who had performed well in drilling our 600-ft heat-flow holes on the Jemez Plateau). Three other drilling contractors did not respond, and two submitted no-bids due to the unavailability of their equipment. Stewart Brothers quoted a cost of $400 for mobilization and demobilization of their equipment and an hourly rate of $40. To insure that these rates were competitive, Jim telephoned the other contractors previously contacted and, from two of them, obtained the cost figures that they would have quoted if they had had rigs available. One was significantly higher than the Stewart Brothers bid. The other was slightly lower, but did not include some of the auxiliary equipment needed for the job (a mud pump, a compressor, and welding equipment) and would not have had a suitable rig available for 120 days. Jim concluded that the Stewart Brothers' bid indeed was competitive, they had a suitable rig available immediately, our previous experience with them had been good, and we all agreed that they were the best choice for the workover-rig contract.

In the meantime, Don Brown had discussed the packers that we would need for our hydraulic-fracturing experiments with their supplier, Brown Oil Tools, Inc. He learned from them that the 2 7/8-in. drill pipe that we had intended to use to inflate the packers was not suitable for that use; its threads were not designed to seal against the high pressures that would be required and would leak excessively. Accordingly, it was decided that we should ourselves purchase 2 7/8-in. steel tubing that would seal against such pressures. Since Stewart Brothers then would not have to provide the 2 7/8-in. drill pipe specified in our request for quotations, they agreed to reduce their hourly rate to $39. However, since it now appeared that work at the GT-1 site would be undertaken during a period when severe winter weather might be expected, Stewart Brothers requested a clause in the contract guaranteeing an eight-hour workday minimum (for days when inclement weather made work impossible or a full twelve-hour workday impractical) and continuous snow removal on the access road to the site and at the site itself. These changes were agreed to, and on this basis Jim estimated the rig cost as follows:

12 hrs/day × 26 days work × $39/hr	=	$12,168
Mobilization and demobilization	=	$ 400
Total rig cost		$12,568

To this was added the expected cost of third-party services, estimated at $14,932, to be ordered by Stewart Brothers who would be reimbursed by us at invoice cost. These services included preparation and maintenance of access to the site and of the site itself, snow removal, packers, other special equipment, plumbing, materials, rental of a mud tank, shipping costs, the services of specialized service-company personnel brought to the site to help with certain experimental operations, and a contingency fund of $2496 to cover necessary but unforeseen expenditures. This brought the commitment to Stewart Brothers to a total "Not to exceed $27,500.00." However, in a later modification to the contract, LASL agreed also to reimburse Stewart Brothers "Not to exceed $1,048.95" to cover the 4% New Mexico Gross Receipts Tax that would be paid by them on certain third-party items purchased for this operation. The grand total committed to the Stewart Brothers contract therefore was $28,548.95.

The contract as described above was approved by the LASL Director's Office, then by the AEC's Los Alamos Area Office, and was issued to and accepted by Stewart Brothers.

17.8. Other major procurements

In addition to the purchase order for the workover rig and the third-party services that it included, separate LASL purchase orders were issued for the items listed in Table 17-1, which were not covered under the Stewart Brothers contract.

There were, of course, a few other, smaller direct LASL purchases of which I have no record, but aside from LASL staff salaries, most other costs were to be covered under the Stewart Brothers contract as third-party services.

Table 17-1.

Preliminary site preparation and subsequent site restoration	$2575.00
2620 ft drill tubing, 2 7/8-in.-diameter, 7200 psi	3116.49
2 geophone systems	4570.00
4 high-pressure transducers	1810.00
Services and materials for hydraulic fracturing (Halliburton Services)	1849.08
Logging and well-surveying services (Birdwell)	3495.53
Total	$17,416.10

18. EXPERIMENTS IN GT-1

It was, of course, impossible for us to begin our experiments in GT-1 at the end of November 1972 as we had hoped. There were purchase orders to issue; service contracts to arrange; and equipment, instruments, and surface facilities to procure and assemble at the site. However, by early February 1973 we were ready to begin our major experimental series there. In conducting it, we were handicapped by subfreezing temperatures, several heavy snowfalls, and the fact that—in undertaking novel and difficult experiments—not everything worked the first time we tried it or as well as we or our suppliers and contractors had expected. We kept the workover rig on site for 53 days instead of the 25 to 30 that we had predicted and completed the series in approximately two months instead of one. However, we accomplished most of the things that we had set out to do, and our results were sufficiently encouraging to justify support for later and more elaborate experiments at Fenton Hill and elsewhere.

18.1. Mobilization

On February 13, 1973, the Stewart Brothers workover rig and crew arrived at the GT-1 site in Barley Canyon. On the same day, CJC, Inc., delivered to the site the 2 7/8-in. steel tubing that we had ordered for our downhole operations. The derrick was erected over GT-1 and rigged up, and a flow nipple was welded to the top of the 5-in. well casing. We were ready to begin downhole operations.

18.2. Cleaning the well

On February 14, the 2 7/8-in. tubing string was run in the hole nearly to bottom, and the well was flushed—first with 2500 gallons of water hauled from Fenton Lake, then with 1800 gallons of clean water brought from Los Alamos by the Zia Co. This removed the thick layer of mud and fine drill cuttings that had accumulated in the bottom of the hole. On February 21, the open-hole Precambrian section was cleaned by wirebrushing and flushing to remove silt and clay clinging to the wall of the hole. This was done so that a clean rock surface would be presented both for inspection of the borehole wall and for seating the packers.

18.3. Logging

At various times, logs of several types were run in GT-1 by the Birdwell Division of Seismograph Service Corporation, Eastman Oil Well Survey Co., and the USGS.

1. Hole depth. Birdwell measured the total depth of GT-1 as 2576 ft; the USGS measured it as 2575 ft. This represents excellent agreement with previous measurements made when the hole was completed.
2. Hole inclination. In conjunction with determinations of the orientations of impression packers, Eastman made seven measurements of hole inclination in the open-hole section. Results varied from 4.5° to 5.8° from the vertical in directions from S72°W to S80°W, with no systematic variation with depth. This is a relatively small departure from verticality for a deep hole drilled with no attempt at drill guidance.
3. Hole condition. After a series of small hydraulic fractures had been made in GT-1 and before a larger one was attempted, Birdwell ran caliper and cement-bond logs in the hole. They determined that the cement job on the 5-in. casing was very poor. Only two zones where cement was bonded to the casing, each about 10-ft long, were found, both near the surface of the basement rock at a depth of about 2100 ft; from there down to the bottom of the casing at 2400 ft, the cement was almost completely unbonded. Also, the two bottom joints of casing were separated at 2335-ft and 2365-ft depth, with the loose joints shifted slightly to one side. (They were probably unscrewed either when cement was drilled out of the pipe shortly after the cementing operation, or during the later coring operation.) This displacement probably accounted for occasional difficulties experienced in lowering packers into the uncased hole

section and some damage to the packers observed after they were brought back to the surface. Otherwise, these deficiencies had no significant effect on the experiments run in GT-1.

4. Bottom-hole temperature. On February 16, Birdwell measured the temperature at the bottom of GT-1 at 210°F (98.9°C). This was only two days after the well had been flushed out with cool water, so that the agreement with the equilibrium bottom-hole temperature of 100.4°C previously measured was surprisingly good.
5. Televiewer results. The borehole televiewer is a downhole instrument that scans the inner surface of a wellbore acoustically and, from the varying time required for the reflected acoustic signals to return to the instrument (a very precise measure of the distance to the reflecting surface), produces a map of the surface relief on the borehole wall. Using two such instruments developed by Mobil Oil Co., Birdwell made four attempts to examine the rock structure in the uncased section of GT-1 before it was disturbed by hydraulic-fracturing operations. In attempts with both instruments on February 16, neither instrument functioned properly. On February 21, after the two televiewers had been reconditioned, the first one still failed to work. However, with the second instrument, two successful scans were made of the uncased section of GT-1. The images produced were difficult to interpret but appeared to indicate the presence of open fractures near the bottom of the hole. This was confirmed by laboratory examinations of the reassembled core, in which there were what appeared to be two sets of inclined, unsealed fractures within about eight feet of the bottom of the hole. In view of the very low permeability of the basement-rock section, it was evident that these did not represent flow paths for water loss, and they did not affect the paths of the hydraulic fractures subsequently made.

Birdwell ran another successful televiewer log of the uncased well section on April 7, following completion of the final hydraulic-fracturing operation in GT-1, which is discussed below.

18.4. Hydrology

Between February 16 and 20, after the hole had been cleaned and the temperature of the water in the well had reached near-equilibrium with the rock around it, the water level was reduced successively to 310 ft, 500 ft, and 994 ft below the ground surface and then was monitored for periods of from 12 hours to about 3 days. From these measurements and those made before the workover rig arrived, it was confirmed that the permeability of the Precambrian rock exposed in the uncased bottom section of GT-1 was of the order of 10^{-8} darcy, indicating that—in spite of the suspected open fractures—the entire rock section was very tight. It was also determined that the pore pressure in this section was subhydrostatic, corresponding to a water level 480 ft below the earth's surface.

These results were again confirmed in both pressurized and unpressurized hydrology tests run in GT-1 after completion of the series of experiments described here.

18.5. Water quality

On February 15, Birdwell obtained a water sample from the well at a depth of 2518 feet. On February 20, the USGS obtained additional water samples at depths of 2400, 2518, and 2567 ft. As might have been expected after only a few days of storage in contact with a very low-permeability crystalline rock, the chemistry of all of these samples was essentially that of the water that had been used to flush out the well. The results were informative only in indicating that there had been no significant flow into the well either of groundwater or of pore fluid stored in the basement rock.

18.6. Packers

A packer is a temporary seal emplaced in a well for any of a variety of purposes and usually removed from the well after its particular purpose has been served. So that it can be inserted into the borehole and then be expanded to seal against its wall, the outer surface of a packer (the sleeve) is made of rubber or some other elastomer. If it is to be used inside casing, it is called a casing packer; if it is to be used in an uncased section of the hole, it is called an open-hole packer; if it is to be used to produce a negative image of the surface relief on the borehole wall, it is an impression packer; and if it is to be left in place permanently, it is a bridge plug. When two separate packers, one some distance above the other, are used to isolate a limited section of the hole, the assembly is called a straddle packer.

Necessarily, so that it can be lowered into the well and then forced against its wall, the packer is a cylinder with an initial outside diameter slightly less than the inside diameter of the well at the position in which it is to be placed. "Inflatable packers" are then expanded to press against the wall by internal pressure developed by pumping water into them through the tubing on which they were lowered into the well. "Compression packers" are expanded mechanically by an internal mechanism that is activated by rotating the tubing through a certain number of turns. When an inflatable packer is to be removed from the well its internal pressure is relieved to deflate and unseat it. In the case of a compression packer, its original diameter is restored by rotating the tubing in the direction opposite to that used to expand it.

Several types of packers were used in the course of these experiments in GT-1. Brown Oil Tools, Inc. (BOT) supplied inflatable packers, compression packers, and impression packers. Lynes, Inc. manufactured the inflatable and impression packers supplied by the University of Minnesota.

The first use of packers in GT-1 was to examine the condition of the borehole wall before it was subjected to the fluid pressures required to produce hydraulic fractures. The first packer emplaced in GT-1 was BOT impression packer No. 1 on February 23. Its active surface was 10 ft long, and it was centered in amphibolite at a depth of about 2563 ft. The packer leaked but, by repeated pumping, it was held at an internal pressure of about 500 psi above hydrostatic for an hour and a half, when the rubber packer sleeve burst. Upon removal from the well, it was found to have a circumferential tear at its upper end. However, it retained a reasonably good impression of the borehole wall, showing what appeared to be an inclined fracture dipping 75 to 80° at a depth of about 2565 ft and a vertical fracture at 2560 ft.

The next day, BOT impression packer No. 2 was emplaced, centered at 2501 ft in a granite section known from core examinations to contain several cemented fractures. It was held at an internal pressure of about 1000 psi above hydrostatic for 75 minutes and then failed suddenly. Later examination at the surface showed that the rubber sleeve had crept off both ends of the mandrel holding it and had failed at the mandrel terminations. However, an excellent impression of the hole wall was retained, showing washed-out cracks and hairline fractures.

The Lynes packers were delivered to the Laboratory on March 2, checked out by Roegiers over a period of several days, and then taken to the GT-1 site. Lynes impression packer No. LI was run on March 12, centered at 2534 ft in an amphibolite section which, from inspection of the core, had no apparent fractures. Its internal pressure was increased to 1000 psi above hydrostatic and maintained for about one-half hour, with no apparent leakage. The unit was designed in such a way that, to deflate it, it was necessary to increase the internal pressure to the point where shear pins in the assembly failed—which was expected to occur when the pumping pressure reached between 1400 and 1650 psi. However, it was necessary to increase the pressure to 2420 psi before failure occurred. Upon removal from the well, the packer surface showed only an array of inclined rows of bubble-like markings. No fractures or other features of the borehole wall were apparent. Roegiers attributed this behavior to aging of the rubber packer sleeve, which was about nine months old. However, another explanation offered was that the epoxy cement used to bond the replaceable outer wrapping to the packer had outgassed at the elevated downhole temperature.

Additional impression-packer runs are described below in connection with hydraulic-fracturing operations. There were also two additional runs of BOT impression packers, not described, in which the packers did not inflate properly—so that no impressions were produced. Those failures apparently resulted from internal leaks in the packer structure.

18.7. Hydraulic fracturing

Seven small and one relatively large hydraulic fractures were produced in GT-1 during this series of experiments.

1. The first attempt at hydraulic fracturing in GT-1 began on February 28, 1973, using a BOT straddle-packer assembly in which the upper and lower inflatable packers were separated by 8.50 ft. The downhole assembly was checked out at the surface, and a LASL pressure transducer was attached to it. The transducer penetrated the pressurizing tubing just above the upper packer and was to be connected to the surface by an instrument cable clamped to the outside of the tubing string as it was lowered into the well. Since it was snowing hard and the water lines were freezing, site operations were shut down for the day at this point.

   The next day, March 1, the downhole assembly was run into the hole to a depth such that the straddled interval was centered at 2499 ft in granite. The instrument cable was attached to the pressure tubing with steel clamps and fed into the well as the tubing was lowered, and the assembly was left in the hole overnight. The first fracturing attempt was made on March 2.

   In preparation for the fracturing attempt, the packer assembly was moved up the well about one foot to straddle an interval centered at 2498 ft, and pumping began to inflate the packers. A small, air-driven, positive-displacement pump was used to inject the water, delivering slightly less than 1 gpm (and this or similar pumps were used to produce all of the seven small hydraulic fractures described below). Unfortunately, in this case the assembly leaked badly and the highest pumping pressure reached was only 700 psi. Brought back to the surface, it was found that the upper packer leaked at an O-ring seal in its lower end and that the inner rubber bladders on both packers had been cut where they crossed sharp edges in bridging a gap in a metal transition piece between the steel braid that supported the rubber outer sleeve and the threaded end piece of the packer. Also there were several places on the instrument cable where the steel clamps used to hold it in place had cut the jacket of the cable and exposed the wire conductors.

   It had been intended that the primary pressure measurement would be that made by the downhole pressure transducer. However, it was backed up by two pressure gauges plumbed into the pressurizing line at the surface. One of these was a Heise pressure gauge with a dial readout visible to the experiment manager at the wellhead. The other was a pressure transducer wired to one channel of an eight-channel strip-chart recorder in an instrument trailer a few yards away. Because of the small clearance between the pressure tubing and the well casing and the fact that the well was not perfectly straight or vertical, it was now evident that it would be very difficult to prevent damage to an instrument cable strapped to the outside of the pressure tubing as it was lowered into the well. Further, except for the large fracturing operation that would terminate this series of experiments, very low pumping rates were to be used to pressurize the system. Therefore, it was assumed that pressures measured at the surface (adjusted for the hydrostatic head of water in the well) would not be significantly different from those existing downhole. Accordingly, and to avoid delays in continuing the series, there were no further attempts to use a downhole pressure transducer. Except when a special downhole pressure recorder (described below) was used, all subsequent pressure measurements were made at the surface, and are reported as surface pressures, pumping pressures, or pressures above hydrostatic.

On March 6, BOT personnel who had been called to the site replaced the O-ring in the upper packer and repaired both packers with cut sections of soup cans soldered in place over the gaps where the rubber bladders had expanded against sharp edges. The packer assembly was run back in the hole with the straddled zone centered at 2497 ft. The packers were pumped up to a surface pressure of 2135 psi, to hold them in place while the straddled zone was pressurized. However, when the pressure in the tubing was released, the check valves in the packers—intended to close and maintain their internal pressure—did not close. The packers therefore vented into the tubing, collapsed, and came unseated. Following the advice of the BOT engineer, the needle valve previously used to vent the tubing was replaced by a 2-in. valve in order to release the pressure more rapidly.

On the following day, March 7, the straddle packers were repressurized to 2250 psi above hydrostatic and when the tubing was vented rapidly through the new 2-in. valve, the check valves operated properly and the packers remained seated. A sliding valve in the straddle assembly, which had remained closed to this point in order to avoid pressurizing the straddled interval, was then opened by dropping a sinker bar down the tubing and pumping into the straddled interval began. When the pumping pressure (read on the Heise gauge) had increased to 1550 psi, it peaked and then began to diminish, indicating that a fracture had opened and was accepting fluid. Since this was not the abrupt pressure drop expected when a hydraulic fracture is initiated in competent rock, it was suspected that a sealed fracture in the granite might have been reopened. Core examinations and the impression packer run on February 24 had shown that there were several cemented fractures in this section of the hole. To investigate this, BOT impression packer No. 4 was run in the hole on March 9 and centered at a depth of 2501 ft. Although it leaked a little, it was kept inflated at surface pressures of 1500 to 1800 psi over a period of about three hours. When returned to the surface, it retained a good impression of the borehole wall—showing a vertical fracture oriented N47W (approximately northwest-southeast). This apparently originated at a preexisting, nearly vertical cemented fracture observed near the center of the pressurized section on the impression packer run on February 24.

After the fracture had been produced, an unsuccessful attempt was made to repressurize the straddled zone. A LASL quick-shutoff valve had been installed in the pressure tubing just above the upper packer. It was designed to remain open during pumping into the straddled interval and then to close quickly in response to the sudden pressure drop expected to occur when the hydraulic fracture opened—in order to minimize growth of the fracture as a result of an influx of additional pressurized water from the tubing above it. Then, when the pressure in the tubing had been reduced (by venting at the surface) to equal the pressure in the straddled interval, the valve was expected to reopen. This would permit controlled pumping into the fracture to determine the pressures required to reopen it (a measure of S_3, the least principal earth stress) and to extend it. This time the valve did not reopen, and it was later determined that it did not function properly because of an accumulation of pipe dope in it. The pressure behavior of the system during this successful fracturing experiment indicated that the valve was not needed, and it, too, was omitted from the downhole assemblies used in later experiments.

The strip-chart record of surface pressure as measured by the transducer in the pressurizing line produced a continuous and more accurate record than did visual observation of the dial gage at the wellhead. Bob Potter later examined the pressure trace on this record to determine the point at which its slope changed from positive to negative, indicating formation breakdown. By drawing straight lines along the slopes of the trace before and after the maximum was reached, he determined that the lines intersected at a pressure of 1320 psi—which is believed to be the pumping pressure at which hydraulic fracturing actually occurred.

An array of four sensitive seismometers had been installed at the surface by J Division, at distances of about 1000 ft from the GT-1 wellhead. They apparently functioned properly during this experiment, but none of them detected any seismic signals from the fracturing event.

2. A blizzard on March 13 left about 13 inches of new snow at the GT-1 site, but with improved weather on March 14, the second hydraulic-fracturing operation was conducted. For this experiment, the Lynes inflatable straddle-packer provided by the University of Minnesota was used. It was lowered to straddle a 10-ft interval centered at 2534 ft, in an amphibolite section believed from core examination to be free of ancient fractures.

   This packer assembly was designed so that when the two inflatable packers had been pressurized to about 1000 psi some shear pins would fail, closing shutoff valves on the packers and almost simultaneously opening a port in the straddled interval to admit the pressurizing fluid. However, although the pumping pressure was slowly increased to a maximum of about 2170 psi the shear pins did not fail. The system was vented and the assembly was tripped to the surface; the shear pins were replaced; and the assembly was run back in the hole to the same depth. The packers were pressurized, but again, the shear pins did not fail at the design value of 1000 psi. However, when the pumping pressure reached about 2250 psi they did fail; the pressuring fluid in the tubing was dumped abruptly into the straddled interval; and surface pressure dropped suddenly to about 1650 psi.

   With additional slow pumping, the pressure increased to a maximum of about 1750 psi and then began to decrease slowly, probably representing extension of a fracture formed at some higher pressure. The shift ended and the system was shut in overnight. In spite of the heating employed in an attempt to prevent it, all surface waterlines froze during the night, and both the Heise pressure gage and the surface pressure transducer were ruined. The Heise gage was replaced the next morning, the system was repressurized, and the pressure decay was monitored on the Heise gage. System pressure dropped slowly to about 1050 psi, where it remained essentially constant for a considerable period, representing what is called a shut-in pressure. At pressures higher than this, the pressure decrease results primarily from rapid extension of the fracture, increasing its volume. The nearly constant shut-in pressure occurs when spontaneous fracture growth slows and finally ceases, and therefore represents the pressure level above which the fracture, will continue to grow. In what follows, it will sometimes be identified as "the minimum fracture-extension pressure," or simply "the fracture-extension pressure," although—as will be explained—that may not be precisely correct.

   Lynes impression packer No. L2 had been included in the downhole assembly just above the upper straddle packer. The two straddle packers were deflated to unseat them, and the downhole assembly was lowered to position the center of the impression packer at 2534 ft—the center of the straddled interval. It was pressurized at 1000 psi for 13 minutes; then, in 200 psi steps, up to 2000 psi over the next 13 minutes; and finally up to 2500 psi where, after 10 more minutes, the shear-pin failed and its internal pressure was released. The downhole assembly was then brought to the surface and the impression packer was examined. As was the case with Lynes impression packer No. L1, it showed an array of bubble-like markings not related to the structure of the borehole wall. However, there did appear to be a trace of a vertical fracture oriented approximately southwest, near the center of the packer but visible only on one side of it.

   Because of the high degree of overpressure and the large and very rapid pressure drop that occurred when the shear pins finally failed, the surface instrument did not respond quickly enough to indicate the actual fracturing pressure. It was certainly less than 2250 psi and probably higher than 1750 psi. The minimum fracture-extension pressure (shut-in pressure) of 1050 psi is believed to be approximately correct. Again, the surface-seismometer array detected no seismic signals from the fracturing event.

3. On March 15, a reworked BOT impression packer (BOT No. 5) was positioned in the hole centered at 2464 ft in granite that, from core examinations, was believed to be free of old fractures. It was pressurized to 1500 psi but leaked badly. By repeated pumping it was kept at pressures above 900 psi for one hour, until the shift ended. The packer was left in place overnight and tripped out of the hole on the morning of March 16. The leakage was explained by a blowout at its upper end. However, it retained what appeared to be a good impression of the borehole wall, which, as expected, was nearly featureless.

   Later that day (March 16) a BOT inflatable straddle-packer assembly was pressure tested at the surface and run in the hole. This time an Amarada pressure bomb had been installed in the tubing between the packers. (The Amarada "bomb" is a self-contained, sealed unit that records pressure as a function of time over a 24-hour period.) The assembly was centered in granite at 2464-ft depth and left in the hole overnight. The next morning an attempt was made to pressurize the packers, but they leaked badly and the run was aborted.

   On March 19 another fracturing attempt was made, this time using a BOT mechanical straddle-packer assembly. In this unit the lower packer is set by rotating the tubing string through a one-quarter turn, which rotates a cam in the packer and expands the rubber sleeve mechanically. The upper packer is set by downward pressure applied by the tubing string, with a hydraulic hold-down positioned in the casing above it to help hold it in position. Both packers were set successfully, straddling a 7.5-ft interval centered at a depth of 2464 ft. The system was pressurized to 1700 psi with no indication at the surface of either leakage or fracture, when pump problems and the end of the shift terminated the operation for the day. When pumping was resumed the next day (March 20), a maximum pressure of 3600 psi was reached with no indication of formation breakdown. It was concluded that a LASL valve in the tubing above the packer assembly was stuck in the closed position and that only the tubing string was being pressurized. Attempts to unseat the valve were unsuccessful. By this time the water lines on the pressurizing pump were freezing, so the run was aborted and the downhole assembly was tripped out of the hole.

   When the pressure-versus-time trace from the Amarada bomb was inspected on the morning of March 21, it was learned that hydraulic fracturing had in fact occurred on March 19 during the first pressurization of this section of the hole—to 1700 psi. The breakdown pressure was 1380 psi, and the shut-in (crack-extension) pressure was 1175 psi.

   Later that day the troublesome downhole valve was refurbished, new shear pins were installed in the upper packer, and the downhole assembly was run back in the hole, with the straddled interval again centered at 2464 ft. The packers were set with no problems, and with slow pumping into the straddled interval there was no leakage. The system was then repressurized repeatedly to above 1175 psi and shut in to allow the pressure to decay. Potter's subsequent analysis of the Amarada pressure trace indicated that—as had been predicted—the crack-extension pressure decreased monotonically as repeated pumping to above its initial value caused the fracture to grow. From flattening of the pressure-versus-time curve during repumping, he also found that the closed fracture reopened at a pressure of approximately 900 psi—a measure of S_3, the least principal earth stress at a depth of 2464 ft.

   By 3:30 p.m. it was snowing hard, the surface water lines were freezing, and the straddle packers would not release. Work at the site was terminated, and the packers were left overnight stuck in the hole.

4. With a great deal of difficulty, the BOT compression-packer assembly was pulled out of the

hole on March 22. It was discovered that the upper packer contained a split-nut locking device to prevent its release after it had been set, so we were very fortunate to have retrieved the downhole assembly. The split-nut locking assembly was removed and the straddle packer was tripped back into the hole. The packers were set five times at various depths in the vicinity of 2475 ft, but in each case they leaked at pumping pressures of a few hundred psi. The shift ended with the packer assembly left in the hole.

The packers were tripped out of the hole the next morning, March 23. The rubber sleeve on the lower packer appeared still to be in excellent condition, but that on the upper packer was seriously abraded and had a piece about the size of a quarter torn out of the top. The conclusion reached by both BOT and LASL personnel was that the tubing weight alone was not sufficient to hold the upper packer in place when the straddled zone was pressurized, so that this packer was being forcibly moved upward. The leakage then resulted from abrasion against the wall of the hole. The tear in the top was believed to have occurred when the expanded packer was pulled up through the casing shoe and probably did not contribute to the leakage.

The proposed solution was to improve the upper-packer seal by increasing the downward force on it above that produced just by the weight of the tubing string. This was done by putting a pull-down on the Kelly at the top of the tubing string, and was successful in subsequent runs with the compression-set BOT packers.

The packers were redressed and pressure-tested, and the reset Amarada pressure bomb was inserted. The assembly was run in the hole, with some difficulty getting past an apparent obstruction in the open-hole section at a depth of about 2500 ft. The packers were set with the straddled interval centered at 2545 ft—in amphibolite known to contain several cemented fractures. The rig-operated pull-down was installed, and the straddled interval was pressurized. The packers sealed well. Pressure rose steadily to a maximum of 1323 psi (as indicated by the Amarada pressure trace) and then, while pumping continued, began to decrease. On March 23, we had produced the fourth small hydraulic fracture in well GT-1. However, again, the surface seismometers did not detect any acoustic signals from the event.

The straddled zone was shut in and, when pressure had decayed to about 1190 psi, the upper packer began to bypass fluid. Increasing the pull-down force on it did not stop the leak, and the leak continued when the packers were reset about 70 ft higher. The run was terminated with no measurements of shut-in (fracture-extension) or fracture-opening (earth-stress) pressures.

5. On March 24 the packer assembly was brought out of the hole and a new BOT compression-set straddle packer was assembled, with a "cup" packer added above the upper straddle packer. The assembly was run in the hole and the packers were set with the straddled interval centered at 2424 ft in an augen-gneiss section known from core examinations to contain at least four cemented fractures. However, when pumping into the straddled interval began there was serious leakage around the upper packer. This was repeated when the assembly was moved 2 ft down the hole. However, when it was moved 2 ft farther down the hole (centered at 2428 ft), both packers sealed and the fifth hydraulic fracture was observed to occur at a pumping pressure of 1200 psi (measured at the surface). Later, Bob Potter's analysis of the pressure record showed that the breakdown pressure was actually 1170 psi above hydrostatic. No related acoustic signals were detected by the surface array of seismometers.

   After the hydraulic fractures had been produced, the system pressure was reduced to 400 psi by venting at the surface, and the system was shut in. The pressure was then observed to increase slowly to a maximum of about 680 psi as a result of spontaneous return to the well of a significant volume of pressurized water that had permeated the rock around the straddled section.

   The system was then put through four

cycles of repressurizing, shutting in, and observing the pressure decay. Three of these were done with the packers at their original location and the fourth after they were moved 2 ft down the hole. Later examination of the Amarada pressure record showed shut-in pressures of 1000, 1025, and 1015 psi at the higher location and 1015 psi at the lower one. Three measurements of the fracture-opening pressure gave values of 911, 931, and 927 psi. It was concluded that the best value of the minimum crack-extension pressure was 1015 psi and that of S_3 was 925 psi.

The system was vented and the downhole assembly tripped out of the hole.

6. On Sunday, March 25 (the rig crew's day off) Bert Dennis, Everett D. Holmes, Jr. (another LASL volunteer), and the BOT field engineer spent the day at the GT-1 site rigging up the BOT compression-packer assembly with a J Division three-component geophone (a downhole seismometer). The geophone package was threaded into a specially designed subassembly inserted into the bottom of the lower packer—to be positively coupled to the rock formation around the hole and close to the location of the anticipated fracturing event. An instrument cable was threaded through the packer mandrels and out of the pressure tubing through a special pack-off above the upper packer, so that it could be clamped to the outside of the pressure tubing as the tubing was run in the hole and then extended to the strip-chart recorder in the instrument trailer.

On March 26, the assembly was run in the hole and the packers were set with the straddled interval centered at 2454 ft in granite believed, from core examinations, to be free of fractures. However, because of electronic problems with the downhole geophone system, the fracturing attempt was postponed until the next day.

Although there was still excessive noise on the geophone channels, an attempt was made on March 27 to pressurize the straddled interval. However, the system leaked and continued to do so at an increasing rate when the packers were reset four times at positions both above and below the initial one. The run was aborted and the assembly brought out of the hole. It was found that downhole temperatures had softened the plastic sheath around the instrument cable in the pack-off above the upper packer and, when the straddled interval was pressurized, the plastic had extruded outward into the annulus around the pressure tubing. This permitted bypass flow up the annulus at a rate that increased with repeated pressurization of the straddled interval. An Amarada pressure bomb attached to the downhole geophone (below the lower packer) showed that there had also been bypass flow around the lower packer.

The packers were redressed and the downhole seismometer and associated fittings were removed. Because of time and cost limitations, there were no further attempts to use a downhole seismometer during this series of experiments. Necessarily, thereafter we relied only on the surface array of seismometers to detect any acoustic signals originating in the hydraulic-fracturing events.

Still on March 27, the assembly was run back in the hole, again centered in gneiss at 2454 ft. The packers were set and the straddled interval was pressurized. Although there was a little leakage around the upper packer, pumping pressure increased continuously to a maximum of 1702 psi (subsequently read from the Amarada record) and then, while pumping continued at the same rate, it began to fall off. The sixth small hydraulic fracture in GT-1 had been produced.

Pressure in the system was bled off, and the packer assembly was left in place in the hole for further experiments the next day.

On the morning of March 28 there were 8 inches of new snow at the GT-1 site, which led to some start-up problems. These having been overcome, the straddled interval fractured the day before was repressurized at a constant pumping rate, to a maximum of 1490 psi. It was then shut in and the pressure decay was observed. Subsequent examination of the Amarada pressure record for the repressurization cycle showed that the minimum crack-extension pressure was 1300 psi and that S_3 was 965 psi. No significant acoustic signals from the fracturing event were detected by the surface seismometers.

7. Continuing the experiments on March 28, the BOT mechanical-packer assembly was moved 10 ft up the hole so that the straddled interval was centered at 2444 ft in granite believed, from previous core examinations, to be free of ancient fractures. The packers were reset, and pumping into the straddled interval began. At a constant pumping rate, pressure measured at the surface increased to 1545 psi and then began to drop off. There had been some leak-off around the upper packer, but an Amarada pressure recorder stationed below the lower packer showed that that packer had not leaked. The seventh small hydraulic fracture in GT-1 had been produced.

   The system was shut in to observe pressure decay and then repumped to a maximum pressure of 1409 psi. It was then shut in again until pressure had decayed to 1235 psi, after which the system was vented and the experiment terminated.

   Later examination of the Amarada pressure record showed that formation breakdown had actually occurred at 1515 psi above hydrostatic; the minimum crack-extension pressure was 1250 psi; and S_3 was 920 psi. Again, no significant acoustic signals from formation breakdown were detected by the surface seismometers.

8. We had now produced seven small hydraulic fractures at various depths and in several rock types in the open-hole section of GT-1, using very low pumping rates to pressurize the straddled intervals. Our next and final fracturing operation in this series of experiments was to be an attempt to produce a relatively large hydraulic fracture using commercial pumping equipment and a much higher rate of pressurization. To accomplish this, Halliburton Services brought its equipment and field crew to the GT-1 site on March 28. When the straddle assembly used to make the seventh small fracture had been tripped out of the hole, preparations began for the large fracturing operations.

   It was intended in this experiment to pressurize a straddled interval about 117 ft long that included previously fractured gneiss, granite, and amphibolite. The bottom "isolation" packer (a compression-set "bridge plug") was set at a depth of 2544 ft—in competent rock just above what had appeared to be natural open fractures in the lower part of the amphibolite section. This packer was detached from the rest of the assembly by rotating a subassembly just above it and was left permanently in place. The inflatable upper packer was then moved up the hole to a depth of 2427 feet, just above the uppermost of the seven small hydraulic fractures. However, when pumping began there was no pressure increase, and it was concluded that the packer was malfunctioning. Accordingly, the upper packer assembly was pulled out of the hole. Examination of it the next morning showed that the seat in a ball-type check valve had been left out so that it did not seal against flow through the packer—which then bypassed the uninflated annulus around the pressure tubing.

   On March 29, the inflatable packer was replaced with a cam-set compression packer. This was followed in the downhole assembly by a hydraulic hold-down about 62 ft (two joints of tubing) above it, and the packer was installed upside down from the way that we had previously used it—so that it would be held in place by tension instead of compression. The assembly was run into the hole, but in spite of repeated attempts it could not be forced past a very solid obstruction at the top of the last joint of casing—at a depth of about 2365 ft. It was brought back out of the hole. (At this time it was snowing very hard.)

   The packer was redressed and reinstalled in the compression-held orientation normally used. Again, however, it could not be forced past that obstruction. By then severe blizzard conditions had developed at the site, and it was shut down for a long weekend with the packer assembly left in the hole.

   On April 3, the upper packer was moved up the hole about 25 ft, to near the top of the second joint of casing. An attempt to set it there was also unsuccessful. To rotate the internal cam that expanded the packer sleeve, it was necessary with this type of packer that steel jaws on the packer make sufficiently firm contact with the wall of the hole so that rotation of the pressure tubing would turn only the cam, and not the entire packer. The packer was designed for, and worked effectively in, the 4 1/4-in. inside diameter open hole, but the 4 1/2-in. inside-diameter of the casing was apparently too large for the jaws to contact it—so that the packer could not be set in the casing by the usual procedure. Instead, therefore, it was lowered against the obstruction in the casing where, by applying the full weight of the tubing string, it was held firmly enough so that it was set successfully.

To check the cement seal behind the 5-in. casing, water was pumped down the annulus between it and the 7 5/8-in. casing. Flow was observed out of the top of the 5-in. casing, indicating poor cement around it.

After a 2-hour delay to dig the water truck out of deep snow at the west end of the rig pad, about 800 gallons of water were circulated down the tubing string to clean out the hole. Then the packer was tripped out.

For a further check on the cement job, an inflatable packer was positioned in the top of the 5-in. casing, with the intention of pressurizing the entire hole below it—down to the bridge plug. Again, the check valve on the packer would not close and the packer could not be set. It was pulled out of the hole.

To investigate the obstruction in the casing at about 2365 ft, a flat-bottomed, 4 1/8-in.-diameter lead impression-block was run in the hole. Miraculously, it moved down the hole and past the obstruction with no indication of interference.

For information on the cement around the casing, the condition of the casing itself and the nature of that obstruction, Birdwell was called in to run cement-bond and caliper logs in the well. Late on April 3, they went in the hole with the caliper tool. It too passed the obstruction and encountered the top of the bridge plug at 2543 ft—almost exactly where it was supposed to be. The Birdwell crew worked through the night and, as was described in Section 18.3 of this report, found that the cement job around the 5-in. casing was very poor, and that the two bottom joints of casing were separated at depths of 2335 and 2365 ft. The troublesome obstruction apparently was the top of the bottom joint, which, being poorly cemented in place, had shifted slightly to one side. It is not clear why it did so or why it subsequently shifted back, but in any case it gave us no further trouble.

On April 4, the repaired inflatable packer was run in the hole and set at a depth of 2427 ft, in unfractured gneiss. However, when the straddled interval was pressurized at a low pumping rate, it did not appear to be holding adequately. The large Halliburton pump was plumbed in and the packer was reset at a much higher pressure (4500 psi compared to the previous 3400 psi). However, when the Halliburton pump was used to pressurize the straddled interval—at a pumping rate of the order of 180 gpm—the upper packer began to move up the hole as the pressure reached about 1800 psi. This caused the pressure tubing to rise, coming up about 6 inches out of the hole, and pumping was quickly stopped. When it was resumed in a second attempt to pressurize the straddled interval, the injection pressure reached about 2200 psi and again the tubing began to come out of the hole. Pumping was discontinued, the packer came unseated, and the downhole assembly was tripped out of the hole.

On April 5, another BOT inflatable packer was run in the hole and positioned at a depth of 2423 ft. A hydraulic hold-down on the tubing string was positioned in the casing above the two separated joints and, at the surface, the tubing was chained down to the casing head. On the third attempt, pumping slowly with LASL equipment, the packer was set and sealed satisfactorily at a pressure of 2200 psi. It was decided not to pressure-test the straddled interval this time but instead to pressurize it rapidly with the large Halliburton pump—in the hope that a sudden large pressure difference across the packer would cause the rubber sleeve to "bunch up" and prevent the packer from unseating as it had when the interval was pressurized slowly. (This procedure proved to be successful.)

The Halliburton pump was plumbed in and approximately 100 gallons of water were pumped in at a rate of 180 to 200 gpm, with a surface pressure of 2200 to 2600 psi. This was followed immediately by 300 gallons of sand slurry containing one pound of 40-to-60 mesh sand per gallon plus a gelling agent to keep the sand in suspension until it had entered the hydraulic fracture created by the water pumped down ahead of it (approximately 880 gallons, including the water initially in the surface plumbing, the tubing string, and the straddled section of open hole). Finally, about 780 gallons of water were pumped down to displace the sand slurry into the fracture. Pumping stopped and the well was shut in. Halliburton disconnected its equipment, loaded it, and left the site. A relatively large hydraulic fracture had been produced and propped open with sand. Its approximate volume was 1180 gallons, or 158 cubic feet, resulting (with the assumption that the fracture was circular) in a calculated fracture radius of 140 ft.

About two hours after the fracturing operation had been completed, the packer began to leak. That evening it came unseated and slipped several feet down the hole. Fortunately, it was restrained from going clear to the bottom by the chain attached to the pressure tubing at the surface. With some difficulty, it was retrieved the next morning.

On April 6, a BOT impression packer was run in the hole and centered at a depth of 2479 ft—which should have been about the midpoint of the large hydraulic fracture. However, the packer leaked badly at about 100 psi and could not be inflated. It was tripped out, and no further attempts were made to use impression packers.

Birdwell was on site, and attempted a borehole-televiewer scan of the fractured region. The first instrument tried did not function property. However, Birdwell had a spare televiewer along, worked with it through the night, and finally got a good scan at about 5 a.m. on April 7. It showed an apparently continuous, substantially vertical fracture, oriented northwest-southeast, and extending the entire 117-ft length of the straddled interval.

Because of the high pumping rate used, our surface instrumentation gave us no record of the pressure at which formation breakdown occurred—and it is, of course, quite possible that the large fracture actually was formed by extension and merging of the seven small fractures previously made in the straddled interval.

Birdwell having completed logging, the well casing was pressure-checked by setting a BOT compression-set casing packer at a depth of about 1500 ft and pressurizing the casing above it to a maximum of 815 psi. There was no indication of a casing problem to that depth, and the packer was unseated and brought out of the hole.

A Halliburton mechanical packer that could be set in either compression or tension was then run into the hole to a depth of 2425 ft in competent gneiss just above the top of the large hydraulic fracture. It was set there in tension, but leaked when the open hole below it was pressurized to 500 psi. It was moved 5 ft up the hole, set at a depth of 2420 ft, and pressurized to 540 psi with no indication of leakage It was left in place there to be used in future testing of the fractured interval.

Probably discouraged by the fact that its surface array of seismometers had detected no acoustic signals from the seven small hydraulic-fracturing events described above; by delays in getting this experiment under way; and having a need for the seismometers somewhere else; J Division personnel removed the entire surface array from the site on April 4—about 2 hours before the first large-scale fracturing attempt. Since we had on hand a spare triaxial geophone (identical with that previously used downhole), it had been frozen into the ground about 85 ft south of the GT-1 wellhead. Climatic conditions were such that this did not take long. (Bob Potter referred to this as "Ice Station 1.") When the J Division array was removed from the scene, this surface geophone was wired to a recorder in the instrument trailer.

During the large (Halliburton) fracturing operation, surface pressure and output of the three components of the frozen-in geophone were recorded on magnetic tape. Subsequent examination of the tape showed that strong acoustic signals—with both compression (P-wave) and shear (S-wave) components clearly resolved—had been recorded from all three axes of the geophone during pressurization of the straddled interval on both April 4 and April 5. Expanded sections of these signals are reproduced in Figs. 13, 14, and 16. They indicated that hydraulic fracturing in fact had occurred during the aborted experiment on April 4, apparently followed by additional fracturing on April 5. As is described below, the pressure record for the April 4 experiment showed a shut-in pressure for the vertical fracture of about 1090 psi, which was within the range of the minimum fracture-extension pressures measured in the earlier small fracturing operations.

Analysis of the pressure trace recorded on April 4 also indicated that pressurizing the straddled interval to above 2200 psi had overpressured it sufficiently to create a horizontal fracture as well as the vertical one. As is shown in Fig. 15, as the pressure in the system slowly decayed, the pressure-vs-time curve developed two distinct plateaus. The first plateau represents the shut-in pressure of the horizontal fracture at a downhole pressure of approximately 2580 psi. (Subtracting the hydrostatic head of about 900 psi, this represents a minimum fracture extension pressure for the horizontal fracture of approximately 1680 psi—measured at the surface.) The second plateau, at a downhole pressure of approximately 1990 psi, represents the shut-in pressure of the vertical fracture. Again subtracting the natural hydrostatic head of about 900 psi, this indicates a minimum fracture-extension pressure (measured at the surface) of about 1090 psi.

Further, examination of the record of surface pressure (the uppermost trace in Fig. 16) also indicated that, when the Halliburton pump was turned off, the momentum of the water already moving down the tubing created a rarefaction wave that resulted in a series of pressure oscillations. Presumably these oscillations excited the fracture cavity since, 6.2 seconds after pumping stopped, a relatively large acoustic signal was recorded (the two lower traces in Fig. 16). This was accompanied by a change in the amplitude and frequency of the pressure oscillations in the pipe and probably represents fracture extension. A series of small acoustic signals was also observed following the large event, suggesting a series of small additional fracturing events—much like the aftershocks that often follow a natural earthquake.

This was the end of our first major set of HDR field experiments. The exploratory hole in which they were conducted, well GT-1 in Barley Canyon, was left with a mechanically set packer in the uncased section of the hole at a depth of 2425 ft in competent gneiss, just above the top of the large hydraulic fracture. The orientation of the packer was such that it would be tightened rather than loosened by an upward force, such as would be developed if the section of hole below it were pressurized. To maintain its seal, tension was applied to the tubing string by the workover rig and, to maintain it after the rig was gone, Francis West installed a LASL fabricated "strongback"—a rigid steel frame resting on the cement around the casing, spanning the wellhead, and firmly clamped to the pressure tubing.

With this accomplished, the Stewart Brothers workover rig was dismissed. On April 11, its crew dismantled the rig, collected and loaded all of their equipment, struggled through the snow back to the state highway, and headed for home.

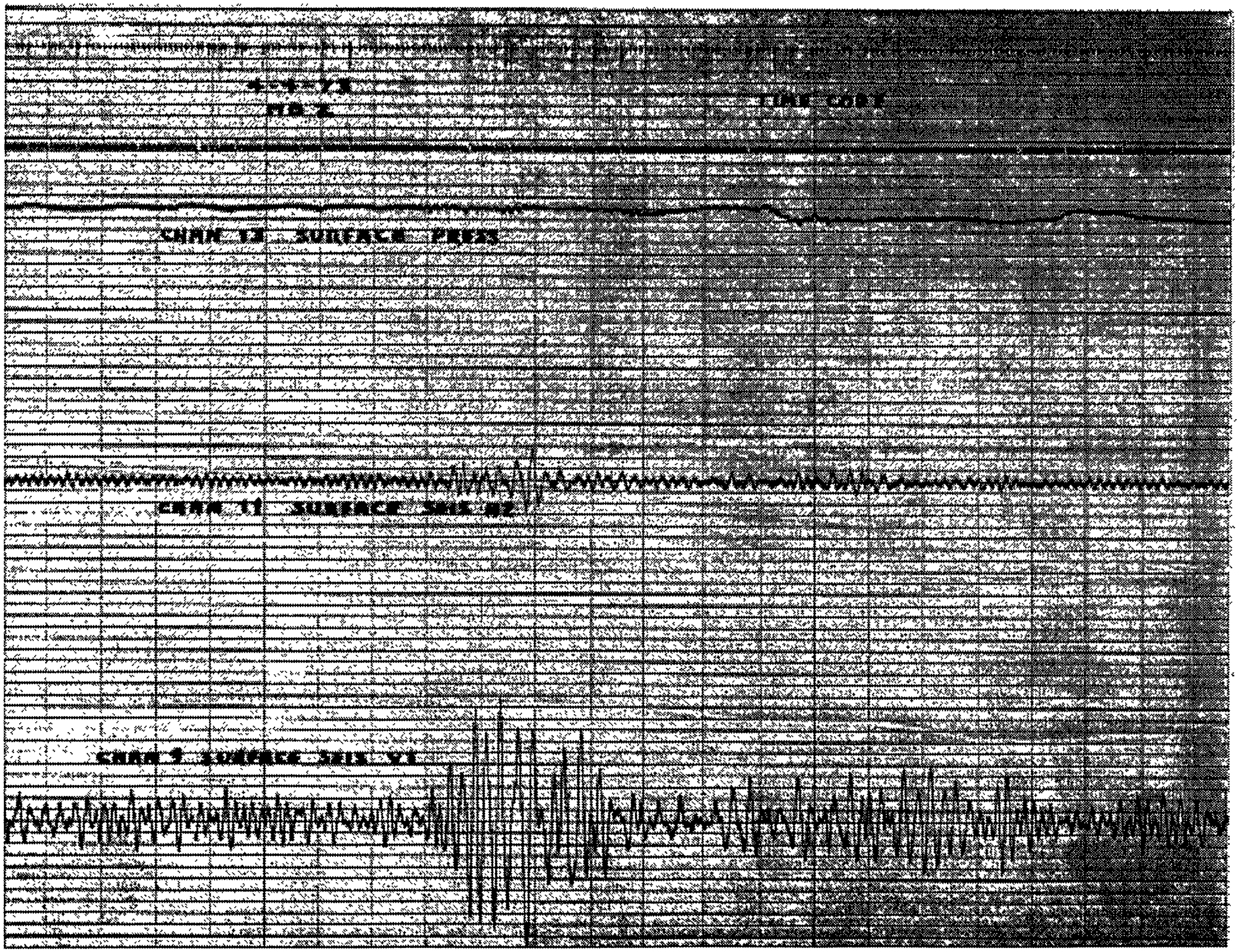

Fig. 13. Record of surface pressure and the output of a vertical (V1) and one of two horizontal components (H2) of the surface seismometer during the large fracturing experiment on April 4, 1973 (Dennis and Potter, 1974).

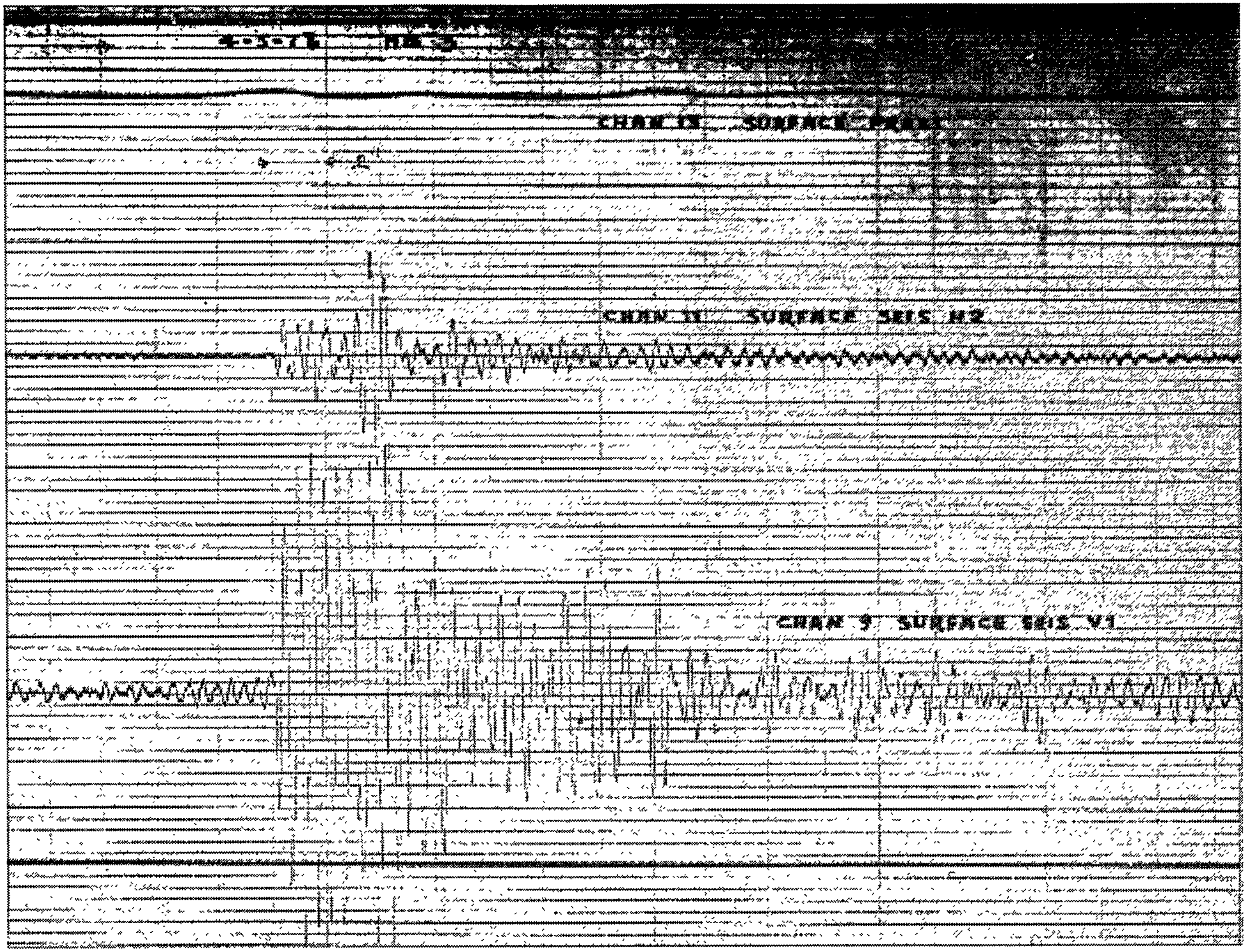

Fig. 14. Record of surface pressure and the output of a vertical (V1) and one of two horizontal components (H2) of the surface seismometer during the large fracturing experiment on April 5, 1973 (Dennis and Potter, 1974).

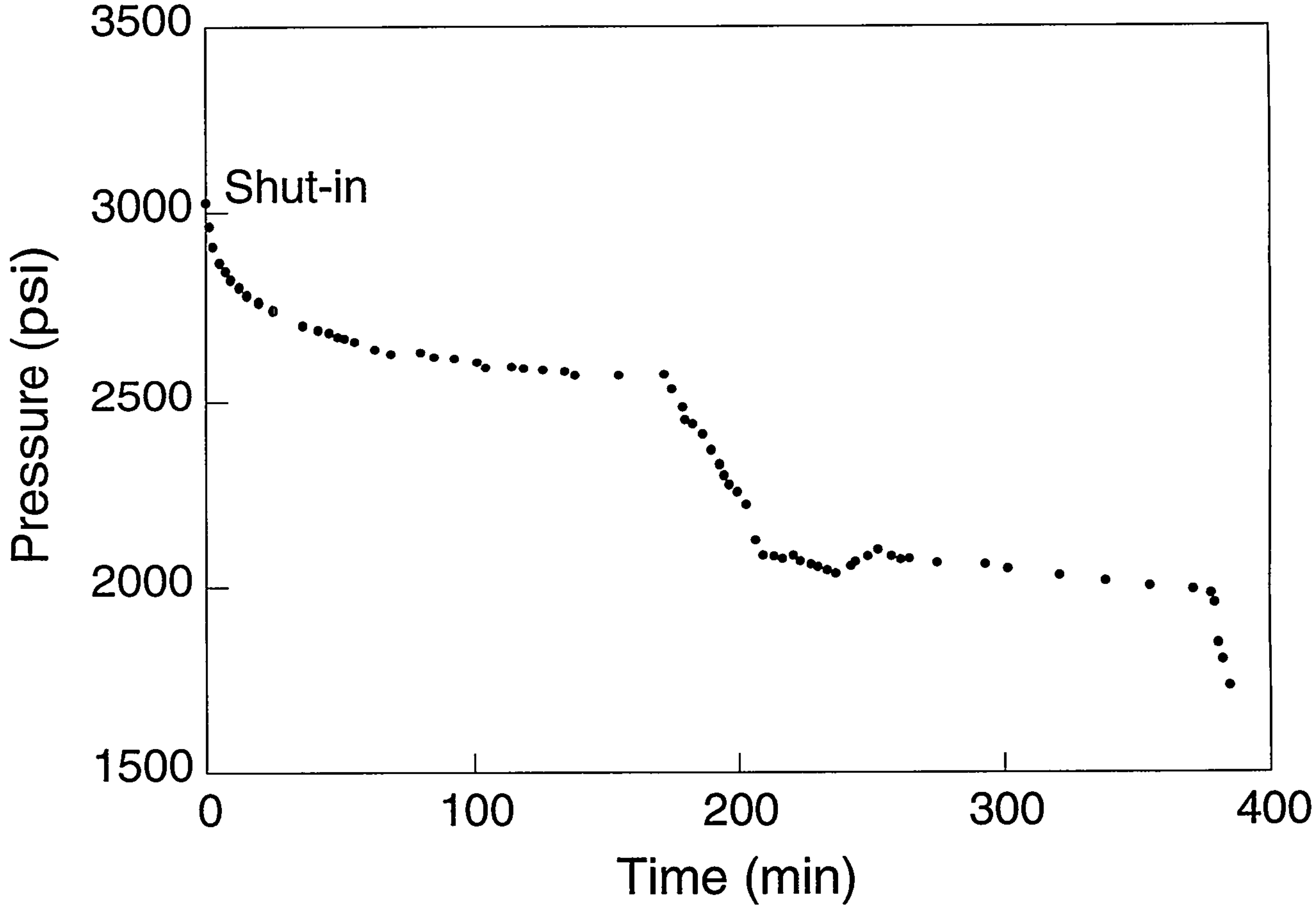

Fig. 15. Downhole pressure vs time after shut-in of large hydraulic fracture on April 4, 1973 (Aamodt, 1974).

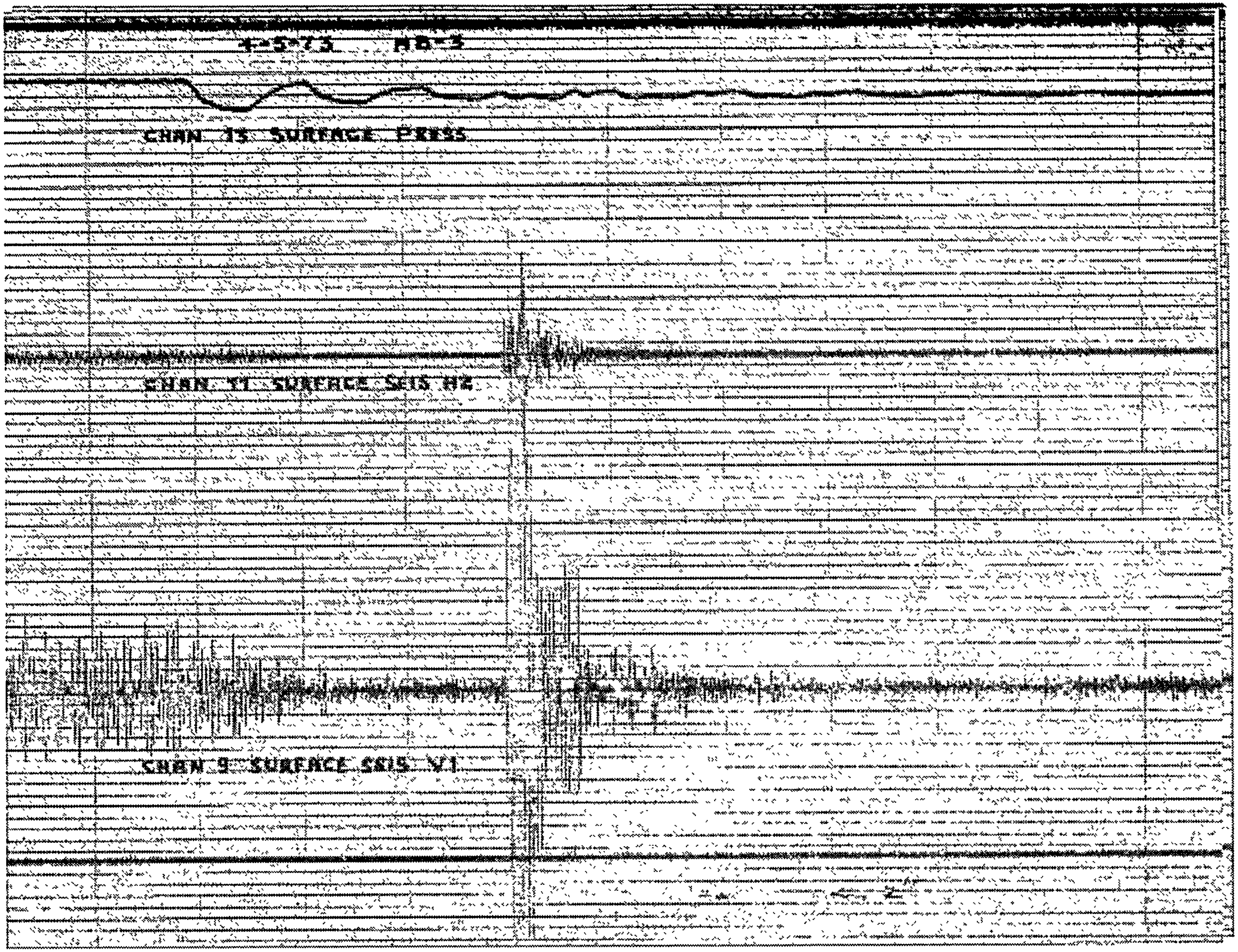

Fig. 16. Record of surface pressure and the output of a vertical (V1) and one of two horizontal components (H2) of the surface seismometer during the large fracturing experiment on April 6, 1973 (Dennis and Potter, 1974).

18.8. Results and conclusions

These experiments had broken new ground, scientifically and literally, and on the whole we were extremely pleased with their results.

1. The principal results from our hydraulic-fracturing experiments in well GT-1 can be summarized as follows.

   At depths around 2500 ft and temperatures near 100°C, these typical crystalline basement rocks—granite, gneiss, and amphibolite—can be fractured hydraulically by fluid pressures in the range 1170 to 1702 psi above hydrostatic. These pressures are very much lower than those required in the laboratory to hydraulically fracture at room temperature small samples cut from cores recovered from similar depths during the drilling of GT-1. (See results of laboratory hydraulic-fracturing experiments at the University of Wisconsin, page 139, and at the University of Minnesota, page 138.) This difference is probably just a size-effect. A small sample containing a serious defect would probably not even survive machining into a specimen to be tested hydraulically in the laboratory, and one that did survive machining would be far less likely to contain a major defect than would the much larger sample represented by the straddled interval pressurized in a downhole hydraulic-fracturing experiment. In these experiments, the highest fracturing pressures were recorded for tests in sections of granite that, from core examinations, appeared to be free of pre-existing fractures. However, the comparison with laboratory results suggests that, in fact, the hydraulic fractures produced downhole may have been initiated at structural defects that were not detected in macroscopic core examinations.
2. At this location and depth, the least compressive earth stress is in the range 900 to 965 psi and is horizontal in a northeast-southwest direction. As a result, opening against the least stress, the hydraulic fractures formed at these pressures are substantially vertical with an approximately northwest-southeast orientation. (This is consistent with the observed orientations of normal faults found north and west of the Jemez Plateau.)

   In these experiments, the least principal stress was determined by locating the first break (a sharp reduction in slope) in the curve of pressure vs time for repressurization at a constant pumping rate of a section of hole already containing a hydraulic fracture. At that point, the fluid pressure is just sufficient to reopen the fracture so that it accepts fluid at a significant rate. It therefore measures the earth

Table 18-1.

Date	Fracture No.	Mean Depth, ft.	Pumping Pressure, psi: Break-down	Pumping Pressure, psi: Fracture Extension	Least Principal Stress	Rock Type
3/24/73	5	2428	1170	1015	925	Gneiss, at least 4 cemented fractures
3/28/73	7	2444	1515	1250	920	Granite, free of fractures
3/27/73	6	2454	1702	1300	965	Granite, free of fractures
3/19/73	3	2464	1380	1175	900	Granite, free of fractures
3/07/73	1	2497	1320	——	——	Gneiss, several cemented fractures
3/14/73	2	2534	——	1050	——	Amphibolite, free of fractures
3/23/73	4	2545	1323	——	——	Amphibolite, several cemented fracs.
4/04/73	8	2484	——	1090	——	Entire 117 ft of open-hole section

stress that tends to hold the fracture closed.

This was a new method of determining the minimum principal stress. It is believed to be much more accurate than the method generally used in the past, which was to determine the shut-in pressure. Here we have assumed that the shut-in pressure actually represents a close approximation to the minimum pressure required to extend an existing fracture. In these experiments, it was of the order of 100 to 350 psi greater than the minimum earth stress measured by repressurizing an existing fracture.

3. In these experiments it was found that, once opened, the vertical fractures could be extended by pumping pressures initially in the range 1000 to 1300 psi. However, in agreement with hydraulic-fracturing theory, it was found that the pressure required for further extension of an existing fracture decreased as the size of the fracture increased.
4. As is commonly true, at this location the greatest earth stress is the vertical compression produced by the weight of the overburden, and (in the absence of a significant horizontal increment of tectonic stress) it is of the order of three times the minimum stress, which is horizontal. By pumping rapidly, it is possible to overpressure the well to the point at which horizontal as well as vertical fractures are created. However, after pumping is discontinued, the overburden pressure causes the horizontal fracture to close, forcing the highly pressurized fluid out of that fracture and back into the well. As a horizontal fracture closes, a vertical one continues to grow—until pressure in the well drops below its minimum crack-extension pressure.
5. In spite of the presence of sealed ancient fractures (and some that appeared to be open) the permeability of this section of basement rock at near-hydrostatic pressures is very low—of the order of 10^{-8} darcy. The basement rock at this location, depth, and temperature evidently is capable of containing a pressurized heat-extraction system with acceptably low rates of fluid loss to the unfractured rock around it. Further, when system pressure is reduced, most of the pressurized fluid contained in inflated fractures returns to the well. This offers the possibility not only of a thermal-energy production system that is conservative of water but also of man-made underground storage systems for liquids, gases, and energy.
6. Unfortunately, our only seismic records of hydraulic-fracturing events were made during our only large hydraulic-fracturing operation, by a single geophone intended for downhole service but actually installed in frozen ground at a surface location quite close to the GT-1 wellhead. It produced good signals from its vertical and both horizontal components, with well-resolved P-wave and S-wave arrivals. By itself, this was not sufficient to locate the sources of the signals—the locations in three dimensions of the fracturing events that produced them. It did, however, indicate that a well-designed and appropriately located seismometer array should be capable of collecting the information needed to map hydraulic fractures as they were created.

   Failure of the J Division surface array to detect acoustic signals from the seven previous small hydraulic-fracturing experiments indicated that those signals were not intense enough to reach the surface (at distances of about one-half mile, much of which was through porous or poorly consolidated sediments and volcanics) with enough energy left to induce an identifiable response from the seismometers. This suggested that, at least for small events and those produced at still greater depths, accurate seismic mapping of hydraulic fractures would require an array of very sensitive downhole instruments emplaced either quite close to the pressurized region or in competent rock that was a good transmitter of acoustic energy, thus avoiding long transmission paths through poor acoustic conductors.
7. In the absence of a capability for accurate fracture mapping, it was not determined whether—away from the borehole—the fractures produced were in fact "penny shaped," as

was generally assumed, or instead were influenced in their growth by changing stress conditions or the presence of variously oriented, cemented, ancient fractures. Also, it was not determined whether, or to what degree, fracture geometry and the pressures required to create the fractures were affected by the very low pumping rates used in the seven small fracturing experiments.

8. Aside from the severe winter weather, the major problems and delays encountered in conducting this series of experiments resulted from the inadequacies of all of the types of open-hole packers that then were available to us. Major improvements obviously were needed in inflatable, compression, and impression packers for them to be reliable in this type of service.

In spite of its problems, deficiencies, and uncertainties, this was an extremely important series of field experiments. In particular, it demonstrated that, in hot basement rock believed to be typical of that in many other parts of the world, it is possible to create and extend hydraulic fractures at moderate pumping pressures and that, in spite of the presence of variously oriented ancient fractures, permeability of the basement rock was low enough so that it could contain pressurized water with very low leakoff rates. These results added needed credibility to the concept of developing HDR energy systems based on creating hydraulic fractures and circulating a pressurized heat-extraction fluid through them. They were sufficiently encouraging so that we were subsequently able to gain financial support from the AEC to create and operate a prototype recirculating, pressurized-water, heat-extraction loop to investigate and demonstrate the technical feasibility of such a system. To anticipate, we later did that successfully at Fenton Hill.

18.9. Costs

In the continued absence of funding from DAT, we officially had available just $50,000 of DPR money to pay for this series of experiments. As described in Section 17.2 of this report, as of November 17, 1972, our estimate of its total cost was $48,850, not including salaries and overhead for our HDR staff (who would be supported by internal Laboratory funds).

Our original purchase order to Stewart Brothers was for an amount "Not to exceed $27,500," which was broken down into a rig cost of $12,568 plus $14,932 for third-party services to be paid by them and recharged to us. To cover the New Mexico gross receipts tax paid by Stewart Brothers on some items purchased by them from third parties, we later added to this an amount "Not to exceed $1,048.95," bringing our maximum commitment to them to $28,548.95. In spite of our problems and delays and the resulting fact that the rig and crew were on site much longer than we had predicted, our total final payment to Stewart Brothers was only $28,326.60.

There were a number of other major procurements (listed in Section 17.8 of this report) that we paid for directly. Those that I can account for added up to $17,416.10, bringing our total cost to $45,742.70. This was $3107.30 less than our November 17 estimate and ostensibly left $4257.30 in our DPR account. Undoubtedly, there were other minor expenses (such as the cost of items drawn from LASL stock) of which I have no record, but these probably amounted to less than the underrun reported above.

Considering the novelty of our experiments, the deficiencies in many of the commercial capabilities required to perform them, and our own inexperience in planning and undertaking them, this was a surprising and gratifying financial outcome. We weren't always that lucky.

18.10. Subsequent uses of well GT-1

With the departure of Stewart Brothers from the Barley Canyon site, we capped and locked the GT-1 wellhead (so that nobody would drop rocks into it), brought out a few tools and instruments, and closed down the site until such time as most of the snow had melted and the roads had dried out.

We had hoped to develop and operate a one-hole heat-extraction system in GT-1 after our hydraulic-fracturing experiments in it had been completed, and the possibility of doing so was mentioned in some of our early proposals and sales literature. Bob Rex, in particular, was convinced that a demonstration of energy production from it was essential to gain the support that we would need to continue and expand the HDR Program. He made an excellent case for undertaking a huff-puff experiment in GT-1 for that purpose. Lee Aamodt was equally enthusiastic about such a heat-extraction demonstration and, in an internal memo dated October 10, 1972, he recalled that "As Edward Teller suggested, good news from this hole would silence those few, but influential critics of the program." However, Lee favored a one-hole continuous-circulation loop and was particularly interested in possible extension of the hydraulic fracture by thermal-stress cracking as the rock around it was cooled. In a memo dated February 26, 1973, he outlined the general nature of such a heat-extraction experiment, the information that could be gained from it, and the equipment and instrumentation that would be required to conduct it.

There was a great deal of discussion of these possibilities within our HDR group before and during our experiments in GT-1. I agreed with Lee in favoring a continuous-circulation experiment if we could manage it. However, most of the rest of the group—notably Bob Potter and Don Brown—preferred a huff-puff experiment, in part because the physical arrangements for it would also permit us (with just our own equipment) to do pressurization, fracture-extension, and shut-in tests involving the full length of the large hydraulic fracture. The argument was settled primarily by the Birdwell discovery of the poor condition of the 5-in. casing and the cement around it. There was also some uncertainty about the quality of the hydraulic connections along the height of a large fracture made, at least in part, by the merging of seven separate small fractures. These considerations made it appear unlikely that we could develop a successful continuous-circulation system in GT-1, at least without reworking it extensively. Therefore, as has been described, at the end of the series of hydraulic-fracturing experiments, we set a compression packer in the open hole just above the top of the large fracture—the ideal position for a huff-puff experiment. Unfortunately, we never got to undertake that experiment. One reason was lack of money for the surface facilities that it would require. Another was that our small staff was very busy digesting and reporting the results of the experiments that we had already done, while also planning and preparing for the drilling of our second, deeper, exploratory hole—well GT-2, at Fenton Hill. However, we did not forget GT-1.

On May 9, 1973, Francis West and Bob Potter were again able to visit the GT-1 site. The Forest Service roads leading to it were still closed, so they hiked in—up Barley Canyon from Fenton Lake. They found that the water level in GT-1 had dropped about 36 ft since we left the site, which was compatible with the permeability of the basement rock and the surface area calculated for our large hydraulic fracture. However the drop was the same in the tubing and in the annulus between it and the casing, suggesting that the packer leaked. The trailers and equipment at the site were in order, and the road through Barley Canyon was still very soft in spots—although it was improving.

After the roads became passable later in May, Francis resumed water-level measurements in GT- 1 and continued them at regular intervals throughout the summer and fall. The rate of drop in water level was almost constant at 0.10 ft per day, and again indicated a permeability of 7.7×10^{-8} darcy—perhaps a little higher than that of the basement-rock section before we began our hydraulic-fracturing experiments.

In earlier experiments involving increasing the water level in the hole to above the natural groundwater level, an injection test between straddle packers, and repressurizing a hydraulic fracture, permeabilities of the basement rock were measured at pressures of 190 to 2570 psi. The range of measured permeabilities was 5.4×10^{-8} to 6.0×10^{-3} darcy. This is consistent with the rate of increase in permeability with fluid pressure for fractured porous media as reported in the literature, and even the highest of these values is in the range ordinarily considered to represent "impervious" or "essentially impermeable" rock.

Principally for lack of manpower, money, and time, but also because the packer left in it appeared to leak, we attempted no more experiments in GT-1. However, that well later served an important purpose during our experiments at Fenton Hill. The pressure tubing and packer were removed and a downhole geophone was emplaced near the bottom, firmly coupled to the crystalline basement rock. It became part of a "Precambrian seismic net" that made it possible to map quite accurately the hydraulic fractures created at greater depth at our second experimental site about a mile and a half away.

19. Q-22, THE GEOTHERMAL ENERGY GROUP

In the early 1970s, there was an acute energy-supply problem in the United States, and a number of R&D programs had been developed at LASL intended to contribute to its solution. In addition to the HDR and Subterrene activities, these included the following active programs:

- Cryogenics: particularly applied superconductivity projects, including superconducting magnetic energy-storage and superconducting power-mission lines
- Controlled Thermonuclear Research (CTR): development of a reactor capable of producing energy from nuclear-fusion reactions, to support low-cost generation of electricity

Other LASL "projects" still in the preliminary investigation or proposal state were the following:

- Coal gasification and utilization,
- Synthetic fuels, initially emphasizing the use of hydrogen as a fuel, and
- Technology assessment, including technology transfer.

By January 1973, LASL management had decided that these and other energy-related activities should be consolidated in a new technical division to be called Q Division after the very large "Q" unit of energy, 10^{18} Btu. Edward F. ("Ed") Hammel, then group leader of the LASL Cryogenics Group, was put in charge of a new Energy Office to formulate plans for the new division. On January 15, 1973, Ed circulated a draft memo that outlined the rationale for formation of Q Division and a proposed general program and organizational structure for it. Among other groups, it listed a Subterrene Group led by John Rowley and a Geothermal Energy Group led by Morton Smith.

On January 31, 1973, a LASL news release announced the Laboratory's decision to form a new Energy Division, to be identified as Q Division and to be activated on March 1. The announcement included the statement that the new division would include the LASL geothermal energy program.

19.1. Q Division organization

As had been announced, Q Division officially came into existence on March 1, 1973. The first public notice of that fact appeared in the March 2 LASL *Bulletin*, together with a listing of its component groups and major administrative personnel.

Robert B. ("Bob" or "Duff") Duffield had been appointed leader of the new division. He was a Ph.D. chemist who had been a staff member at LASL from 1943 to 1946, during the Manhattan Project; he had served as a consultant to the Laboratory from 1948 to 1957; and he had maintained close contact with it until he returned in 1973 as a division leader. In the meantime, he had taught at the University of Illinois at Urbana from 1946 to 1956; was an assistant director of the John Jay Hopkins Laboratory of General Atomic from 1956 to 1967; and was director of Argonne National Laboratory from 1967 to 1973. His professional specialty had become fission reactions and reactors, but he soon became interested and deeply involved in all of the LASL energy programs—including our HDR Program.

There were two new associate division leaders: Ed Hammel for Applied Technology (including geothermal energy) and Fred L. Ribe for Controlled Thermonuclear Research (CTR). The division office staff included Maurice W. ("Maury") Katz as assistant division leader, Roger W. Westcott as an administrative aide, and Rod Spence as a program advisor. Eleven Q division groups were listed, as follows:

- Group Q-DOT was the Office of Analysis and Planning;
- Groups Q-1 through Q-6 were CTR groups;
- Group Q-22 was Geothermal Energy (our HDR Program;
- Group Q-23 was the Subterrene Group;
- Group Q-25 was Advanced Heat Transfer Technology (primarily heat pipes); and
- Group Q-26 was Cryogenics.

(The gaps in the group numbers were reserved for additional energy and energy-related groups that might be formed in the future.)

As initially constituted, Group Q-22 consisted of the following six people:

- Morton C. Smith, group leader,
- Donald W. Brown, assistant group leader,
- R. Lee Aamodt,
- Bert R. Dennis,
- Robert M. Potter, and
- Francis G. West.

Transfer of all six of us from our original groups into Q-22 was effective on March 1, 1973. On a temporary basis, Georgia P. Courtney was to continue to serve as our part-time secretary, and it was understood—under what was called the "Form B" procedure—that our work would continue to be supported by a number of full-time and part-time personnel in other LASL groups who had special expertise in such areas as hydrology, seismology, geochemistry, and various engineering disciplines. Most of these people were already working with us in those areas, supported at least in part by our DPR funding. It was also understood that Q-22 would become responsible for their full support when our DAT funding finally arrived.

19.2. Finding a home

With this commitment of the Laboratory to formation of an official Geothermal Energy Group, it became necessary to find a home for the new Group Q-22 other than the limited office space generously provided for some of us by Dick Baker and Bob Gibney in wing one of the CMR building. We needed office space for the entire group in one location, with room for expansion of group personnel and space for laboratories, shops, and storage of materials and large equipment. That amount of concentrated, unoccupied space was clearly not available in or around the CMR building or anywhere else in the Laboratory's main technical area. Necessarily, we began to search for it among the Laboratory's dozens of outlying sites scattered across the Pajarito Plateau and in the canyons that dissect it.

An obvious possibility was TA-46, the technical area that had been occupied principally by N Division's Rover Program. However, John Rowley—who had been an N Division group leader—was already based at TA-46 and had first priority on the available space. He had most of what was available staked out for his Subterrene Group, but he did offer me two staff-member offices and a secretary's office in Building 1 for the Geothermal Group. Since our group was to begin with six staff members and we expected it to expand to about twenty-four within a year or two, that obviously was not room enough for us. We looked elsewhere.

It happened that a weapons-development group, WX-4, was being phased out at that time, its members being transferred to other WX Division installations elsewhere. With the exception of a tritium facility in Building HP-86, WX-4 had occupied all of TA-33 (also called Hot Point or HP Site). This was an area of four square miles about eight miles southeast of the Laboratory's main technical area (but about half-again that far by road). It was mesa and canyon country, adjacent to Bandelier National Monument to the south and west, to White Rock Canyon of the Rio Grande on the east, and to State Highway 4 and Ancho Canyon on the north. It was largely covered by pinon pines, junipers, native grasses, rocks, and cactus. In addition to a central complex containing an office building, shops, and a warehouse, there were many small outlying buildings, trailers, firing sites, and control bunkers scattered about the mesa, and a considerable number of small Indian ruins. Besides a few humans and an occasional cow that wandered in from Ancho Canyon, the inhabitants included deer, coyotes, rattlesnakes, horned toads, elk down from the Jemez Mountains for the winter, a couple of wild burros, an occasional bobcat or mountain lion, and many birds and small varmints. It was an interesting area, and a great place for hikes and hunting for artifacts. However, it was also a long way from the main technical area, which was an advantage in some ways but a serious disadvantage in others—such as access to the main technical library, LASL stock warehouses, and the help we needed from such organizations as the Supply and Property Department, the main LASL shops, and the Travel Office. It was also abundantly booby-trapped with leftovers from the many and varied weapons-development activities that had been carried on there, about which most of our group knew very little. Finally, it was a fenced security area, with a guard at the gate (one of whom subsequently informed me that, with regard to the cows, he had no authority to stop any wild range cattle that chose to enter). We hoped and expected that that (including stopping the cows) would be changed so that people without security clearances could be admitted freely. Our HDR Program was, of course, entirely unclassified, and we expected as time went on to have a lot of uncleared visitors.

Most of the people whom we talked to who had worked there thought that TA-33 was great. Unfortunately, that opinion was not universal. Lee Aamodt, in particular, felt that it was far too remote from the main technical area, where so much of the interesting and important action of the Laboratory occurred. In his opinion, the only appropriate future use of TA-33 would be to turn the barbed wire on the security fences inward and populate the site with LASL employees who had actually retired but hadn't yet told the Laboratory about it. (I think that even Lee eventually changed his mind about all that.)

The possibility of finding a home for Group Q-22 was put on hold until Q Division and its Geothermal Energy Group were officially formed on March 1, 1973. On March 2, Bob Duffield held the first Q Division group leaders' meeting. Among other things, we considered the problem of finding space for Q-22 and other Q Division groups still to be formed. Rod Spence had already discussed this with Raemer E. ("Schreib") Schreiber, then LASL's deputy director, and reported that Schreib favored moving the Geothermal Energy Group to TA-33—largely so that the site would continue to be occupied by LASL personnel. Rod and Bob Duffield and I agreed, and I was instructed to make the necessary arrangements (which sounded pretty simple).

Accordingly, in a memo dated March 6, 1973, to Marvin D. ("Marv") Linke, the LASL space coordinator, I made the case for assigning space in the central building complex of TA-33 to Group Q-22. I outlined our immediate needs for office space, laboratories, a staff shop, and warehouse space, and suggested that specific room assignments be made later when we had become more familiar with the buildings there. I suggested that, if this was approved, we would begin to move in in about three weeks. I also expressed the hope that by then the arrangements could be made to convert the central building complex into a nonsecurity area.

In a memo to me dated March 7, Schreib said that he had discussed the matter with Harold Agnew and they agreed that Q-22 should occupy TA-33, and that I should work out the details with Marv Linke. He noted that this would be expensive for laboratory support services for such a small population and expressed the hope that other LASL activities would be relocated there as soon as possible. By copy of that memo, he asked Robert ("Bob") Pogna, who was in charge of LASL security, to initiate arrangements to remove TA-33 from the security-area classification.

That was not as simple as it sounds. For example, in accordance with the "need-to-know" criterion concerning security matters, the standard security clearances and technical area personal identification badges that we all had did not automatically permit access to all LASL security areas. In areas in which particularly sensitive or potentially hazardous work was being done, entry was controlled by adding a number specific to that area to the badges of people who worked there or had reason to visit there frequently. For TA-33, where such work had been done for many years, the entrance credential was the numeral 7 on the badge, which was inspected carefully by the Protective Force guard stationed at the gate to the site. Since none of us in Q-22 had previously had a legitimate reason to visit TA-33, that number was not on our badges. We had to make special arrangements for the guard at the gate to admit us.

My own first visit to TA-33 was on the afternoon of March 20, in the company of Dwight S. Clayton, assistant head of the LASL Supply and Property Department. We were signed in and conducted on a tour of the site by John E. Dougherty, the WX-4 group leader, who was still in residence. The place was full of surprises. Outstanding among them was an extensive array of artillery pieces on the East Point—a promontory overlooking some rapids on the Rio Grande. The guns were emplaced to fire into a 10 x 10 x 140-ft channel filled with sawdust, backed up by an earth berm intended to keep anything not retained by the sawdust from tumbling into the Rio Grande. The area had been used to study setback effects in projectiles fired from the guns, the largest of which was an 8-in. gun whose inside diameter was large enough to stuff in a football.

There were many other unexpected things about the site, but the central complex in which we were to be based appeared to have the major facilities that we would need, and a lot more. I, at least, looked forward to moving there. However, there were additional arrangements to be made—particularly with regard to restrictions on access to the site and the disposal of WX-4 property still there (including the guns).

Dwight Clayton was faced with the formidable task of transferring or disposing of the equipment, supplies, and materials at TA-33, except for the tritium facility in Building HP-86, which was to be retained by Group WX-5 and to remain a security area. It took him a long time in the case of such things as the artillery, gun mounts, and extra gun tubes out on the East Point. However, he took care of us promptly and compassionately. Of course WX Division had first priority in claiming and removing anything that was still useful to them and by this time they were busy doing so.

It had previously been decided that whatever was left behind by WX Division would be made available to any other LASL group that could use it, with any conflicting requests to be resolved by the Director's Office. However, on March 21 Dwight wrote a memo to Bob Van Gemert suggesting that, instead, the new occupants of the site (Group Q-22) be given first priority to acquire the useful things that were left behind. Among other things, Dwight cited the fact that we were still "operating on credit" and were "having a difficult time acquiring equipment"—which we certainly were. This priority was approved by Van Gemert and by Schreiber, and allowed us to keep a great deal of office, laboratory, shop and electronic equipment, and other supplies. On March 30, Bert Dennis agreed to serve temporarily as the Q-22 property representative, to work with Dwight on the transfer of excess TA-33 property to Q-22. Bert did a fine job of finding, identifying, and saving for us a great deal of useful equipment and supplies.

At the same time, Bob Pogna—working with Jason Arter, chief of the LAAO Security Branch—had the almost equally difficult job of revising security arrangements at TA-33 to accommodate the unclassified operations of Q-22 and to permit access to the site by both cleared and uncleared visitors. This of course was complicated by the fact that classified operations would continue in Building HP-86, at the south end of the central building complex. Fortunately, that building was surrounded by its own security fence so that, if necessary, access to it could be controlled separately by a guard at the entrance to that limited area.

Pogna outlined several possible arrangements for controlling access to the rest of TA-33 in order both to safeguard LASL property and to protect visitors from any hazards that they might encounter there (not counting rattlesnakes and mountain lions). All of these were unavoidably complicated, and none could be implemented quickly. Accordingly, as a temporary expedient, I requested (in a memo to Bob Pogna dated April 3, 1973) that whatever arrangements were necessary to permit immediate and continuing access to TA-33 be made just for the individuals composing Group Q-22. In addition to its charter members—Aamodt, Brown, Dennis, Potter, Smith, and West—this list now included Glenna Newman.

On March 12, Ed Hammel had approved my request to hire a full-time secretary for Group Q-22. I arranged for that job opening to be advertised in the March 16 LASL *Bulletin*, and interviewed several candidates for it. On March 30, I offered the job to Glenna, who at that time was employed in Group CMB-6. She accepted, and I started the paper work necessary to transfer her to our group. Her transfer was not effective until April 9, but in the meantime we wanted her help in inspecting the facilities and planning our actual move. That was arranged. Glenna was energetic and effective in helping with the move and was not squeamish about stepping on an occasional toe (including mine) to get things done right—occasionally by her own definition of what was right. She did get things done and was a great help to us all.

On April 4, L. F. O'Connor, chief of the Protective Force Section of the LAAO Security Branch, sent a "Supplement Security Order" to Guard Station 470 at the gate to TA-33, instructing the guards there to permit entry to the seven individuals listed above on presentation of their Tech Area badges. We now had free access to the site, and we took advantage of it to inspect the site more closely and decide on space assignments.

In a memo to Bob Pogna dated April 20, 1973, R. W. Drake—the WX Division leader, pointed out that there was no longer any purpose in requiring the numeral 7 on the Tech Area badge as an entrance credential to TA-33. He proposed that the requirement be eliminated immediately and that the regular Tech Area badge be the only required credential. This was approved by Pogna and Jason Arter and became effective on May 1. Thereafter, anyone with a Tech Area badge could pass the TA-33 guard station, and it became possible for us to bring in uncleared visitors under escort. However, it was still a very long time before it became a nonsecurity area. There were still a lot of scattered miscellaneous equipment to be taken care of and a great many potential hazards that might be encountered by an inquisitive, casual visitor.

Francis West became the Q-22 representative on the Q Division Safety Committee and, with people from the Laboratory's Health Division and Supply and Property Department, he spent a great deal of time during the summer and fall of 1973 evaluating and eliminating a great many of those hazards.

19.3. We move to TA-33

The actual move to TA-33 began on April 12, 1973, first of all involving mostly cleaning up the office spaces that we intended to occupy and searching the site for furnishings for them. WX-4 had not left much office furniture behind, but Dick Baker and Bob Gibney generously let us take along that which some of us had been using in the CMR building. On April 17, Lee Aamodt, Bert Dennis, and Glenna Newman moved in, although for a while they were pretty much camping among cardboard boxes. Bob Potter and Francis West were in Washington at an AGU meeting, and Don Brown was still very busy up in Barley Canyon, so they moved in a few days later. I was tied up for a while completing my obligations to CMB-13, which included disposing of all classified materials in my possession, helping to arrange for continuation of the projects in which I had been involved there, and transferring my materials-research equipment to those who would be using it after I left. On April 27, I too moved to TA-33.

We were now an organized, official Geothermal Energy Group, assembled in a generally suitable facility, and with a very promising hot dry rock energy program well under way. Of course, except for the research funding from DPR, we were still living largely on credit, and by now our credit rating was well into the negative figures.

19.4. The financial roller-coaster

At a meeting in Los Alamos on May 1, 1972, attended by both John Flaherty and Jerry Johnson, Jack Vanderryn told me in a private discussion that he would try to arrange for DAT funding of $3 million for our major HDR field experiment in FY73 (the next fiscal year, beginning July 1, 1972) and $3.5 million in FY74. Since Jack was Flaherty's technical assistant, this gave us hope for prompt and adequate funding from DAT. However, in a draft memo dated June 23 on DAT's proposed "Geothermal Energy Development Program," Tony Ewing reduced this to $1.0 million for HDR in FY73 and $3.0 million in FY74. Even that sounded good to us at the time.

In a white paper, "Energy Production from Geothermal Sources" presented to an OST Panel in Washington during the last week of June 1972, the AEC showed $2.5 million for the first year of our

HDR Program and $3.5 million for the second year. The actual fiscal years were not stated, but Lee Aamodt—back from Washington in early July—indicated that year one of the AEC geothermal program apparently was in fact to be FY74. However, in his discussions at AEC headquarters, he had learned that AEC would actually get only $1.5 million for geothermal programs in FY73 but still planned to give us $1.0 million for our HDR Program. Apparently FY73 was year zero of the AEC program, and we were to get a head start.

This was confirmed approximately by Tony Ewing at the first Ogle Committee Meeting in Oakland, California, on July 18, 1972. He announced that AEC would spend $1.5 to $2.0 million on geothermal energy studies in FY73 and that LASL would get $1.0 to $1.5 million of it for our HDR Program.

There was no further news of that funding until Harold Agnew received a letter from Jerry Johnson dated October 13, 1972, in which Jerry stated that "It is our intention to provide funds for your proposed geothermal energy development effort as soon as it can be arranged." Of course by then we had for some time been expecting that to "be arranged" early enough in FY73 so that we could spend the money intelligently during the rest of that fiscal year, and had submitted budgets and program outlines and descriptions based on that assumption. Unfortunately, DAT was almost as new in the funding game as was our HDR Program and, like HDR, did not yet appear in the AEC budgets. The intention was that existing AEC funds would be reprogrammed to support our work, but that involved a variety of bureaucratic hassles that neither we nor Jerry Johnson had the background to predict.

In the meantime, there were many discussions of schedules, budgets, and manpower, both within LASL and with AEC Headquarters personnel. For example, at the request of Al Blair (then an assistant to Dick Taschek) on December 12, 1972, I prepared and reported to him a proposed financial plan for the geophysical research sponsored by DPR (LASL budget E532) during the rest of FY73. It showed that the current LASL financial plan included $50,000 of DPR money (already committed) and that we expected to receive, from DPR an additional $50,000 in operational funding plus $20,000 for capital equipments—a total of $120,000 from DPR for FY73. My report also included the DPR funding anticipated for FY74, which was $402,000 plus $80,000 for capital equipment. (DPR funding for geochemistry was now being handled independently by CNC Division, whose funding request for FY74 was $160,000.) Of course the work supported by DPR was to be integrated with that to be undertaken with DAT funding when it arrived.

However, as I explained in a memo to Bob Van Gemert dated December 20, 1972, "Our plans for GT-2 and for the main GTE Project are completely dependent on what support DAT gives us, and when. We still have no word on that, and so we have no firm plans or commitments beyond our series of experiments in the existing hole, GT-1. If we receive sufficient funding from DAT and it comes soon enough, we still hope in FY73 to drill GT-2, experiment with it, and then drill the first hole for the 'main' experiment. But time is getting very short and, like you, we are much concerned about lead times."

We were not aware of it at the time, but—as reported in the *Los Alamos Monitor* on January 31, 1973—AEC chairman James R. Schlesinger had told Senator Joseph M. Montoya that $500,000 for development of geothermal energy by the AEC had been authorized during both FY72 and FY73, but that that money had never been released by the Office of Management and Budget (OMB). Evidently the AEC and Congress indeed were trying to arrange for financial support for programs such as ours but had not so far convinced the federal administration that it was a good idea.

At the end of January 1973, we prepared midyear summaries of ongoing HDR projects for Ed Hammel in the newly formed LASL Energy Office. He then edited and assembled them and submitted them to the appropriate people in DPR and DAT at AEC Headquarters. His report included requests for FY74 funding of $482,000 for DPR-supported geophysics research, $160,000 for DPR-supported geochemistry research, and $3.7 million for DAT support of the Dry Geothermal Source

Demonstration—the current name of our HDR Program. In the meantime, on February 1, 1973, Ed received word from Brian Belanger at AEC Headquarters that DAT's FY74 budget included no money for geothermal energy programs. Apparently this also resulted from an action by OMB and was part of an administration effort to limit federal spending.

In the FY74 budget that the President had submitted to Congress, he requested $772 million for energy research (most of which was intended to support development of a fast breeder nuclear reactor). Congress voted this full amount and added $60 million for research on coal, geothermal, and solar energy. Apparently it was some part of this $60 million that the AEC had counted on to support its geothermal program in FY74. However, again as an economy measure, OMB also impounded the $60 million, and the prospect of HDR funding from DAT during FY73 apparently vanished along with the funds to continue it in FY74.

Nevertheless, I took it as a hopeful sign when Jim Bresee called me from AEC Headquarters on February 5, 1973, and asked me to be ready on February 8 (at the third Ogle Committee meeting in Las Vegas, Nevada) to discuss with him what HDR work we would undertake between April 1 and September 30, if AEC could find the funds to support an active geothermal energy field program during that period. I prepared for such a discussion by putting together a proposed project schedule and detailed manpower and cost breakdowns for it.

- The project schedule showed completion of our experiments in GT-1 by late July, including a three-month, one-hole, heat-extraction experiment. This was to be followed by drilling of a second exploratory well (GT-2), about 4500 ft deep at Fenton Hill and a series of experiments in it similar to those run in GT-1—although with no attempt at heat extraction.
- The separately funded DPR-supported geochemistry and geophysics projects were expected to continue and to cover the costs of completing our experiments in GT-1. The cost breakdown that I gave Jim therefore was limited to site preparation at Fenton Hill; drilling, completion, and logging of GT-2; and the experiments to be done in it. Including a 10% contingency factor, I estimated $175,000 for materials and services, $198,000 for subcontracts (drilling and workover rigs), and $40,000 for capital equipments, a total of $413,000. To this I added $165,000 for LASL salaries plus $84,000 for LASL indirect expenses ("overhead"), bringing the grand total to $662,000 for the six-month period—hopefully to be funded by DAT.
- My manpower breakdown showed full-time support of the six charter members of the HDR group, a group secretary, an electromechanical technician (still to be hired), and part-time to full-time support of personnel in several LASL divisions who were working with us in such areas as drilling, engineering design, and seismology. This added up to 7.5 man-years of effort by LASL personnel during the six-month period.

Jim and I discussed all this privately in Las Vegas on February 9. He was even newer to the geothermal-energy business than I was, and neither of us realized quite how optimistic my schedules were. Subsequently he asked me to write a few paragraphs describing our program plan and to dictate them over the telephone to his secretary at AEC Headquarters. I did so on February 12, elaborating in a page and a half on the schedule that Jim and I had discussed in Las Vegas.

John Flaherty was scheduled to visit Los Alamos again on February 16. In preparation for his visit, I revised some of the dates on the project schedule that I had given to Jim Bresee and, on February 13, discussed this revision with Bob Duffield and Rod Spence. They felt that the schedule for work at Fenton Hill was still too optimistic (which it was), so I revised it further.

At our meeting with Flaherty on February 16, I briefed him on what we had done and were doing in GT-1, and our plans and hopes for experiments at Fenton Hill. In a series of transparencies I showed him the schedules that I had prepared for the six-month period April through September 1973. To explain how this led into a long-range program, I also showed him the extended time schedule for subsequent creation and experimental operation of a two-hole heat extraction loop at Fenton Hill (as I had revised the schedule after discussions with Duffield and Spence). Flaherty continued to be interested in and supportive of our HDR Program and inquired into some of its details. In particular, he asked what we were doing about environmental concerns, which—especially with regard to hydrology, water quality, and seismology in and around our experimental areas—was quite a lot.

The six-month period shown in my detailed schedules covered the fourth quarter of FY73 (April through June) and the first quarter of FY74 (July through September). In response to one of Flaherty's questions, I estimated that we would need about $3 million to fund our proposed work during the last three quarters of FY74, ending June 30, 1974. (This was consistent with the estimates presented in our January 1973 midyear review.) Flaherty had no quarrel with the estimates and schedules that I had presented, and he was optimistic that DAT funding would materialize to support our long-range program. However, discussions with him and others were enlightening with regard to DAT's problems in quickly finding the money to support our work. The federal budget for FY74 did, in fact, include $25 million for geothermal R&D, but it was in the Department of Interior's budget, intended principally for the USGS. In the absence of direct funding or authorization to transfer funding from some of its other programs, the AEC had expected that part of Interior's $25 million would be transferred to the DAT geothermal-energy programs, but so far Interior had not officially committed itself to that transfer. Until it did so, OMB remained unconvinced that our HDR activity represented a continuing program and refused to permit AEC to reprogram any of its existing funds in order to support our work during the six-month period considered above. Further, since they had already been active in geothermal studies for several years, the USGS was not enthusiastic about funding the AEC to initiate programs in that field. Understandably, they felt that it was an infringement on work that they were already doing and a threat to its continuation. There was, then, reason for concern about the transfer of those funds. Accordingly, we and DAT were left in limbo with regard to financial support for our HDR Program.

In response to the midyear review of our work, John M. Teem—then director of DPR—wrote to Harold C. Donnelly (manager of AEC's Albuquerque Operations Office) on February 20, 1973, confirming that FY73 operational funding, of $100,000 and capital-equipment funds of $20,000 had been recommended to AEC's controller to support our HDR-related geophysics activities. We appeared to be on reasonably firm ground with regard to DPR support for completion of our experiments in GT- 1, although financial support for anything beyond that obviously was still very uncertain.

On April 9, 1973, Lee Aamodt again visited AEC Headquarters in Washington, primarily to discuss a proposal for a georesources study that would extend some of our ideas to energy-related activities other than extraction of thermal energy. On his return to LASL the following day, he reported (among other things) that Jerry Johnson had responded negatively to a suggestion that we should seek geothermal-energy funding from NSF. Jerry felt that DAT could and would support our work and would be able to do so in FY74 to the extent of about $3 million. That again sounded hopeful for our long-range HDR Program, although it left us dependent solely on DPR and internal Laboratory funding for the rest of FY73.

On April 25, three of us—Lee Aamodt, Bob Potter, and I—represented HDR in a briefing for Walter Skallerup, a legal counsel to the Congressional Joint Committee on Atomic Energy and a field representative of Senator Henry Jackson. He was helping to establish priorities in case money became available for energy development in general. Evidently the AEC position with regard to funding for expansion into that area still was not firm, in spite of the fact that we were almost through the first month of the half-year period for which we had hoped that DAT could support our work.

However, on April 30, Ed Hammel reported optimism with regard to geothermal-energy funding by both Jerry Johnson at AEC Headquarters and Paul Craig at NSF.

On May 2, Jim Bresee called me again from AEC Headquarters. He said that it looked very hopeful that AEC would indeed get $4.7 million from the Interior Department kitty to fund geothermal-energy projects and that our HDR Program would be given top priority in spending it. He needed information on our work and plans to give to Raymond ("Ray") Zahradnick, the geothermal coordinator at NSF. I suggested several of the documents that I had already given Jim, but evidently they weren't really necessary. Jim had apparently at least discussed our HDR Program in considerable detail with Zahradnick, who visited us the next day (May 3) and was already quite well informed about it. Bob Potter, Don Brown, Ken Olsen and I briefed him concerning it at considerable length, and I took him on a tour of the caldera area and Fenton Hill. Zahradnick seemed really interested in our program, impressed by our program, and optimistic about our funding. Apparently as an outgrowth of this visit, a news item in the *Albuquerque Journal* on May 17 reported an NSF announcement of possible funding for our program which sounded hopeful if somewhat confusing. We had submitted no proposal to NSF, Zahradnick had not requested one, and Jerry Johnson didn't want us to submit one.

In the meantime, FY73 was almost over, with no DAT funding for our HDR Program actually in sight. Our May 12, 1972, proposal—which requested FY73 funding of $3.838 million—obviously would not be acted upon; with formation of Q Division and our Geothermal Group (Q-22) our LASL organization had changed significantly; with our experiments in GT-1 completed, we had a much better idea of what would be involved in developing a two-hole heat-extraction loop; and we had picked a site for that prototype system and prepared preliminary plans for its development. It was time to write a new proposal, which we did.

The new proposal was dated May 10, 1973, and submitted to Jerry Johnson at DAT by Dick Taschek on May 22. This time Bob Duffield was listed as the person in charge, with Mort Smith still shown as principal investigator. It requested a total of $4.420 million for FY74, during which it was planned to prepare the site at Fenton Hill for experiments there; drill and complete exploratory hole GT-2 to a depth of about 4500 ft; conduct a series of experiments in it similar to those done in GT-1; and, if the results indicated that the site was suitable for it, drill and complete two energy extraction holes (EE-1 and EE-2) about 200 ft apart to depths of the order of 7500 and 5500 ft and connect them at depth by hydraulic fracturing. Then, in FY75, we would complete the surface facilities required for a recirculating pressurized-water heat-extraction loop and begin operating it. The total funding request for FY75 was $1.748 million, which would include the cost of exploring for a second experimental site in geology different from that at Fenton Hill. Circulation through the Fenton Hill loop would be continued through FY76 and a detailed evaluation made of the selected second site—at a total cost in that fiscal year of $2.554 million.

Another ray of hope appeared when Representative Manuel Lujan addressed an IEEE meeting in Los Alamos on May 19. Among other things, he expressed a strong interest in geothermal energy and stated his belief that AEC would soon get funds from the Department of Interior to support geothermal programs. Unfortunately, this did not mean that Interior was actually committed to such a transfer. (It still wasn't.)

In fact, at the fourth Ogle Committee meeting in Oakland on May 24, Jim Bresee presented a very discouraging report on funding prospects for DAT's proposed geothermal-energy projects. The $25 million that the Department of Interior had received was, in fact, for energy R&D in general and not just for geothermal energy. Interior had established a new Energy Office to decide how it should be spent, and apparently only $4.1 million of it would be available for geothermal-energy projects. The USGS needed that much for its own projects. The AEC still hoped to spend $4 million on geothermal projects in FY74, but Jim said that there was no reason to hope for it before October 1973 (which was the beginning of the second quarter of FY74).

However, at this meeting, Jim asked me (by a week from the next Monday) to update our 5-year plan for HDR development, this to go to Ray Zahradnick—who, it turned out, was also on an energy-advisory committee to the federal administration. Of course I did so, although I did not understand the politics of all this. (I still don't.)

An Associated Press release on June 6, 1973, quoted Representative Manuel Lujan as saying that the Joint Committee on Atomic Energy had authorized several energy related projects, and that the authorizations included $4.7 million for geothermal, Subterrene, and geoscience research.

Apparently as a result of the Joint Committee's action, FY74 funding of $4.7 million was included in a U.S. House of Representatives supplementary appropriations bill—specifically to fund AEC geothermal-energy projects. However, in late June it was deleted from the bill by the House Appropriations Committee. Fortunately for us, in a brave if flagrant violation of a long-standing tradition, Representative Lujan offered an amendment to the appropriations bill when it reached the House floor on June 28, restoring that $4.7 million. It was the only amendment offered, and it passed. Thereafter, the AEC was sufficiently confident of this funding so that they considered it part of their budget, and our HDR Program was still scheduled to receive. $3.0 million of it. However, this was FY74 money, to be distributed some time later—after the President had signed the bill, OMB had released the money, and the AEC had put it into the LASL financial plan. Before all that could happen, FY73 had ended on June 30, and our only official funding was that provided by DPR. However, our local credit rating had now improved considerably and, within limits, we were able to borrow LASL internal funding to supplement that from DPR. We were able to continue our work, although at a much slower pace than we had planned.

In an energy message to Congress on April 18, 1973, President Nixon had discussed the serious energy-supply problem then facing the United States and outlined some initial steps to overcome it. These included several pieces of proposed legislation, none of which were directly concerned with geothermal energy. However, he elaborated on this in an energy statement released on June 29, in which, among other things, he

- stated that he was initiating a 5-year, $10-billion program of R&D in the energy field, this to begin in FY75;
- directed the chairman of the Atomic Energy Commission, then Dixie Lee Ray, to undertake an immediate review of federal and private energy R&D efforts, to recommend an integrated energy R&D program for the nation, and to report to him by December 1 both the recommended 5-year program and her recommendations for energy R&D programs that should be included in the President's FY75 budget; and
- announced that an additional $100 million in FY74 funds would be provided to initiate these programs, accelerating certain existing projects and undertaking new ones in a variety of critical R&D areas (including "geothermal steam"). He directed Chairman Ray, in consultation with the Department of Interior and other agencies, to recommend to him by September 1 specific projects to which this $100 million should be allocated.

The AEC was confident that at least this initial $100 million would materialize, that the AEC would handle it, and that DAT would have the major responsibility for spending it. To begin implementing all this, Chairman Ray directed DAT to organize a task force to prepare initial plans for federally supported energy projects for FY74, for FY75, and for the 5-year period FY75 through FY79. The task force was organized, with me as the geothermal member. Its principal product was "The Ray Report," whose preparation is described in Appendix B of this history. In it, the proposed FY74 funding for HDR remained at $3.0 million. While the Ray Report had little immediate impact, it did appear to represent long-term support for federally funded energy R&D programs such as ours.

Also, on June 29, 1973, Chairman Ray distributed an announcement of the President's new energy initiatives to all AEC offices and laboratories, including LASL. In response, Harold Agnew wrote to John Flaherty on July 10, listing areas of energy R&D in which LASL was prepared to play a lead role. These included geothermal energy (specifically, HDR), georesources development (discussed below, in section 20.12), the Subterrene, and applied geosciences (including geophysics and geochemistry). In an attachment to this letter, the present and proposed LASL programs in these areas were spelled out in some detail.

On August 17, 1973, President Nixon signed the Congressional supplementary appropriations bill that contained FY74 funding of $4.7 million for AEC geothermal projects—presumably including $3.0 million for HDR. However, while the money was there, it still required release by OMB to appear in the official AEC budget and distribution by the AEC to appear in the LASL financial plan. Nobody knew when those things would happen.

On September 27, I received a telephone call from Glen Graves, formerly an assistant to Dick Taschek at LASL, but at that time working at NSF in Washington. Among other things, he expressed optimism about release of our HDR funding, although he did not think that it was imminent. Later the same day, Louis B. ("Lou") Werner called me from DAT at AEC Headquarters and told me that the feeling at Headquarters was that our funds would be released very soon—perhaps within a week. However, in another phone call the next day, Jim Bresee said that he had no news concerning our funding.

Later on September 28, Douglas ("Doug") Balcomb, then a LASL liaison with Washington, called to say that he had just had a long talk with Congressman Manuel Lujan. Lujan had talked to John C. Sawhill at the Federal Energy Office, who said that there was no problem with release of the $4.7 million to AEC, and that OMB would probably release it the following Friday. Lujan had also talked to Hollingsworth, general manager of AEC, who said that the AEC would distribute the $4.7 million as soon as they received it, and that $3.0 million of it would come to LASL for our HDR Program.

On October 11, 1973, President Nixon announced that he was going to commit $115 million "in additional funds" to energy research during the current fiscal year (FY74)—including $7 million for geothermal R&D.

On October 12, Rod Spence called to report news from Washington that OMB had indeed released that $115 million for federal energy programs. Ostensibly this represented the $115 million that the President had just promised for FY74. Actually, it was only $55 million of new money, plus $60 million that OMB had impounded from the earlier supplementary appropriation by Congress. However, it did sound as though our FY74 funding was finally in sight, although by then we were already well into the second quarter of FY74—and we had been fooled before.

At this particular time, funding for the entire Laboratory was very tight, and our HDR Program was still operating largely on promises and credit. We were trying to order the long-lead-time items that would be needed for our planned experiments at Fenton Hill, but by then our credit rating had nearly vanished. Nevertheless, I tried to put through a purchase order for $32,000 worth of logging cable, which was to be manufactured specially to meet our rather unusual specifications. That was all right with our Supply and Property Department, but not with Dick Taschek. He called me up on October 31, explained the funding difficulties faced by the Laboratory, listened to my explanations of the critical need for that cable and the lead-time involved in getting it made, spared me no sympathy, and instructed me not to order it or anything else until our funding was actually released so that we could pay for it. It was probably a necessary decision on his part, although of course it was an unwelcome one to us.

In fact, OMB had not released the AEC geothermal-energy funds. On November 2, 1973, Jim Bresee called me again from Washington and explained that part of the argument delaying that release was that too much of the money was going to our HDR Program instead of to R&D on hydrothermal systems (natural steam and hot water). In fact, our proposed $3.0 million represented more than half of the total AEC budget for geothermal-energy R&D. (That fact had already been brought to my attention pretty emphatically by several people who had proposed other types of geothermal studies, and who appeared in the FY74 AEC budget inadequately or not at all. Evidently they or their representatives had also talked to OMB!) Jim wanted arguments that our HDR work would also contribute to the development of hydrothermal systems. I gave him some and later, after consulting with Don Brown and Lee Aamodt, dictated some more over the phone to Jim's secretary. These, by the way, were legitimate arguments. We had already learned a great deal that was new about subterranean geology, hydrology, and stress conditions that would be useful in the development of wet geothermal systems, and we were certain to learn more. We were improving existing techniques and equipment for geothermal exploration, drilling, hydraulic fracturing, and acoustic mapping of fractures and developing strong programs in geochemistry, reservoir modeling, and power-conversion cycles. We also foresaw the potential usefulness of hydraulic fracturing for stimulating fluid production from hot water and steam wells, and of acoustic methods of mapping natural as well as man-made fractures.

Jim McKeown from DAT visited us on November 6 with more disturbing news. When released, the AEC geothermal funds would contain no money for capital equipment. Apparently that was the result simply of an oversight by the people who had prepared the appropriation bill, but it was certain to create some major difficulties for us.

A delegation from the House Committee on Science and Astronautics visited LASL on November 19. It included Representatives George E. Brown, Jr., Barry M. Goldwater, Jr., Mike McCormack, and James W. Symington, and Scientific Advisors Joe Del Riego and Tom Ratchford. I briefed them on the HDR concept and our program to develop it, and they expressed great enthusiasm for our ideas and program. However, they also expressed great pessimism that OMB would release our funding "before the big picture has been resolved," which they predicted would not occur sooner than a year from then. This was bad news indeed, from Congressional insiders who were directly involved in supporting and funding energy programs. We were already deeply in debt to other LASL organizations and couldn't possibly survive for another year without that DAT funding—as Dick Taschek pointed out to me quite emphatically after our meeting with the committee members. The meeting and my subsequent discussion with Dick had been in TA-3, the main technical area, and I drove slowly back to TA-33 sadly wondering how to tell the Q-22 troops that we were about to cancel all orders, declare bankruptcy, close up shop, and start looking for other employment.

However, marvellous to relate, when I reached my office at TA-33, there was a phone call waiting for me from Bob Duffield. Jim McKeown had called him from AEC Headquarters and reported that OMB would release the AEC geothermal money (still $4.7 million including $3.0 million for us) not later than the next day. Several other people had also called Bob later with the same news, and on an evening radio news broadcast, Senator Montoya also announced release of the funds. By then it sounded very official but I had already been taken in so often by what appeared to be official announcements that I was not completely convinced even by this one. However, it was true and was confirmed the next day by phone calls to me from Harold Agnew and Jack Vanderryn and by reports in several newspapers. Final confirmation came in a letter dated November 23, 1973, from Jerry Johnson to Harold Agnew stating that "The purpose of this letter is to inform you that $3 million will be forthcoming in your next financial plan to be used for geothermal resource development (dry hot rock)."

On November 26, Lou Werner called me from AEC Headquarters to say that a letter was in the mail putting the HDR money in the LASL financial plan. (In fact it was, and it did not get lost in the mail.) Lou gave me verbal permission to start spending the money immediately, and we did. As Jim McKeown had warned me, there was no money in the budget for capital equipment. However, we were experienced in begging, borrowing, renting, and improvising.

We paid our debts to other LASL organizations from whom we had borrowed (particularly to W Division), firmed up our plans for the rest of FY74 and beyond, began hiring the people we would need to carry them out, and placed the orders for that instrument cable and a lot of other things. Five months of FY74 had already passed, and our program was about a year behind where we had hoped it would be. However, that was not because we had been idle. We had already accomplished a great deal, not the least of which was simply raising the money to proceed with a program that we were sure was important to the energy future of the United States and the rest of the world.

20. OTHER ACTIVITIES

The principal objective of the LASL Hot Dry Rock Geothermal Energy Program has always been to develop and demonstrate practical engineering systems capable of economically extracting usable thermal energy from naturally heated crustal rock that, initially, was not economically productive of natural steam or hot water. Necessarily, this has required improvements in methods of drilling down to, and into, the hot rock and of creating the connected permeability and heat transfer surfaces that would make possible efficient extraction of heat from the rock by a fluid circulated through it. To plan intelligently and to understand the results of experiments intended to develop and demonstrate such energy systems has also required fundamental and applied research in a variety of earth-science areas, many of which are basic to natural hydrothermal systems as well as to man-made HDR systems. Further, to attract and maintain financial support for these engineering and research activities has required a great deal of time and effort devoted to advertising and public-relations activities within and outside the scientific community; to preparing and revising funding proposals and budgets; and to reporting the results of our work to our funding sources, at technical meetings, in scientific journals and the popular press, and to everyone who was interested—or at least willing to listen. This was a large order for our small geothermal energy group, but we worked hard at filling it.

Our major field activity during the period covered by this report was preparing for, drilling, and completing exploration well GT-1 and conducting in it the experiments described above. However, a large number of other related activities were also carried on, some of which have been mentioned in earlier sections of this report while others have not.

20.1. Funding and manpower

From its informal beginnings in the spring of 1970 to early July of 1972, the HDR Program was supported entirely by operational funds transferred from other LASL programs. At least semiofficially, this has been reported as about $5000 in FY70, about $130,000 in FY71, and $278,000 in FY72, all of which has been attributed to Supplementary Research funds provided to LASL by the AEC's Division of Military Application (DMA). This of course does not take into account a large contribution of volunteer effort outside of Laboratory working hours.

Our first line-item funding came from the AEC's Division of Physical Research (DPR) in FY73 and included $93,000 for geophysics research and $45,000 for geochemistry research, a total of $138,000. This was supplemented by $26,000 of DMA Supplementary Research funds, so that our total funding for FY73 was $164,000.

For FY74, the LASL financial plan included DPR funding of $45,000 for geochemistry research (which went directly to CNC Division) and $190,400 of operational funds plus $25,000 of capital-equipment funding for geophysical research (to be handled by Group Q-22): a total of $260,400 of DPR funds. For the first five months of FY74 (until the DAT money was released near the end of November 1973), the HDR Program survived on this plus DMA funds borrowed from funded weapons programs. The borrowing was done with the understanding that it would be repaid after the DAT funds arrived by temporarily supporting the equivalent of four W Division staff members. (Using the Form B procedure, we did indeed repay it in this way later in FY74.)

The DPR funding was intended to support research related to but not directly part of the HDR Program, which was engineering development and, at least in principle, was supported by DMA funds. In fact, during this period the research and engineering activities were so intimately intertwined that assignment of costs to the separate DPR (E532) and DMA (C137) budgets was largely a matter of bookkeeping convenience. However, I believe that both organizations got their money's worth.

As was described in section 19.1, the original LASL Geothermal Energy Group, Q-22, consisted of Lee Aamodt, Don Brown, Bert Dennis, Bob Potter, Mort Smith, and Francis West, soon joined by Glenna Newman. Not part of the group, but working closely with us in DPR supported activities, were Bill Purtymun in H Division, John Balagna in

CNC Division, and Ken Olsen in J Division. Darrell Sims' time was divided between the Geothermal and Subterrene Programs. By the summer of 1973, Bob Hendron and Dick Foster from ENG Division were working with us full-time, as was Jim Hill from J Division. Jean-Claude Roegiers, supported by DPR funding, was a temporary member of Q-22, and at the end of the period covered by this report he was joined briefly by Osamu Kudo as a short-term visiting staff member. As part-time visiting staff members, we also had the services of Burt Slemmons and Priscilla Dudley. Roland Pettit and Dan Miles from the Zia Company also donated a great deal of time helping us as very useful volunteers. This sounds like quite a large group of people, but many of them were part-timers and, taking this into account, our geothermal staff was actually very small for all of the things that we were trying to do.

20.2. Geology

Using both low-sun-angle aerial photography and surface geology, Burt Slemmons made a detailed study of faults in and around the Jemez Plateau, west and southwest of the Valles Caldera. He found that the area containing our experimental sites (Barley Canyon and Fenton Hill) was within a large fault block with a surface area of the order of 25 square miles, within which no faults were found. Their absence indicated that the seismic risk associated with hydraulic fracturing and operation of a pressurized-water heat-extraction system within that area was essentially zero.

Fault structures and exposures of the Precambrian basement rock west and north of this block were investigated by Francis West and Bob Potter during the summer and fall of 1973. They found an interesting group of faults whose trends correlated well with the northwest-southeast orientation of hydraulic fractures produced in GT-1, suggesting that fracture orientation was controlled by a tectonic stress field much like that which existed when those faults were formed.

As is described below, the subsurface geology of the Jemez Plateau was investigated quite extensively in our heat-flow holes and exploratory well GT-1.

20.3. Seismology

The earthquake history of the region was also assembled and examined by Burt Slemmons, who found no record of any earthquake centered in the Jemez Plateau. This confirmed the low seismic risk of experiments there.

However, to improve coverage of our field sites, conventional seismometers were installed at Fenton Hill and on St. Peter's Dome, about 19 miles southeast of there. This was actually an extension of a regional array developed by Ken Olsen and his J-9 Group, designed to monitor natural earthquakes throughout northern New Mexico—primarily to evaluate the earthquake hazard that existed in the Laboratory's technical areas. The extended coverage was intended to increase seismic knowledge of an area that in the past had not been well covered, which was important to our environmental studies. It also was there to inform us promptly if we did in fact trigger any detectable earthquakes, so that we could shut things down in a hurry if we did, and to document the fact that we were not responsible for any natural earthquakes that might occur in that part of New Mexico. Over a period of six months, to the end of the time covered by this report, the extended array detected no natural seismic events near our experimental sites that were above the detection threshold of the seismometers. In fact, the Jemez Plateau was found to be abnormally quiet seismically compared to most of the rest of New Mexico.

As has been described, very sensitive surface seismometers were developed and checked out by J-9 and installed near the GT-1 wellhead in Barley Canyon. They detected no acoustic signals from our small hydraulic-fracturing experiments, but unfortunately were removed from the site before the large fractures were made.

A three-component downhole microseismometer was also developed and tested successfully at shallow depths in GT-1. Problems with the instrument cable connecting it to the surface prevented it from being used during the hydraulic-fracturing experiments. However, a similar instrument used at the surface did detect acoustic signals from the large fracturing events, with sufficient amplitudes and resolutions to indicate that detailed analysis and mapping of underground hydraulic fractures should be possible. Accordingly, working with J-9 personnel and the LASL electronics group, Bert Dennis continued development of improved downhole microseismic instruments for more sophisticated experiments and for mapping hydraulic fractures at still greater depths and higher temperatures and pressures.

20.4. Hydrology

In cooperation with Frank Trainer of the USGS, Francis West and Bill Purtymun conducted a continuing program of sampling and analysis of waters from springs, wells, streams, and lakes on and around the Jemez Plateau. This was both a fundamental hydrologic study of the area and development of baseline data that would permit prompt detection and correction of any hydrologic effect of our experiments there. It included our intermediate-depth heat-flow holes as well as GT-1. Although Francis found that its quality decreased slightly with depth, water sampled throughout the entire area was potable (Purtymun et al., 1974). Even samples taken at the bottom of GT-1 were very soft, low in chloride content, and suitable for a domestic water supply.

Downhole water-level measurements in GT-1 made at fluid pressures both above and below the natural hydrostatic pressure established that there was no significant waterflow into or out of the uncased section of basement rock even after extensive hydraulic fracturing and that the permeability of the basement rock at such pressures was extremely low—of the order of 10^{-7} to 10^{-8} darcy. It increased normally with increasing pressure, but measurements made during the hydraulic-fracturing experiments indicated that, at pressures up to at least 2000 psi above hydrostatic, the rate of water loss from the fractured reservoir was still very low—not greater than 30 gallons per day.

20.5. Core examinations

Samples of Precambrian basement rocks from GT-1 cores and from surface outcrops around the Jemez Plateau and as far away as the Sandia Mountains east of Albuquerque were examined macroscopically by Francis West, Bob Potter, Orson Anderson, Burt Slemmons, John Handin, and others, and petrographically by Priscilla Dudley. Their observations indicated that the crystalline-basement rocks penetrated by GT-1 were typical of the Precambrian rock of the region and that still deeper holes drilled in this area should encounter principally granitic rocks similar in chemical and mineralogical composition, structure, and properties to the Precambrian section penetrated by GT-1. This in turn suggested that, in the absence of local faults, pressurized-water heat-extraction systems could be developed and contained successfully elsewhere in the region and at still greater depths where rock temperatures would be higher.

20.6. Temperature gradients and heat flow

Heat-flow studies were made both in the temperature-gradient holes drilled in the Jemez Plateau and, over a period of about one year, in well GT-1. They indicated that the entire region is indeed one of high-temperature gradients and heat flow, both increasing as the rim of the caldera is approached. Bob Potter concluded that heat flow on the plateau at a distance of about two kilometers from the Valles Caldera ring fault was equal to or greater than 5 heat-flow units (5×10^{-6} cal/cm • sec, more than three times the world average terrestrial heat-flow). Within a mile or two of the caldera rim temperatures above 200°C should be reached at depths less than 10,000 feet.

Bob spent much of the summer and fall of 1973 investigating heat flow on the Jemez Plateau. Among other things, he determined (from thermal-conductivity measurements on cores from GT-1 and our intermediate-depth heat-flow holes) that one particular shale-sandstone stratum underlying the plateau at a depth of a few hundred feet was particularly consistent in its thermal conductivity. Using this as a "flux plate" in conjunction with carefully determined temperature gradients in that formation he derived consistent and reliable heat-flow values over the area of our investigations. Presence of this flux plate also presented the opportunity for a detailed but fairly inexpensive future study of heat flow around the Valles Caldera utilizing relatively shallow heat-flow holes.

Repeated temperature logs in GT-1 (Fig. 17) showed continuous changes in downhole temperatures over a period of about three months after drilling, suggesting that the common practice of logging temperature just once soon after a hole is drilled can be very misleading with regard to actual rock temperatures at depth. The logs also revealed anomalous changes in apparent heat flow in vertically adjacent formations that differed in their thermal conductivity and periodic local variations in temperature that appeared to result from formation of a series of convective cells in the water-filled well. These observations cast serious doubt on the validity of individual downhole temperature measurements made in large-diameter water-filled holes where geothermal gradients are high, even after thermal equilibrium has apparently been established. They were also the principal bases of the concerns of Orson Anderson and the Geosciences Advisory Panel regarding our heat-flow studies. At the end of the period covered here, analytical and experimental investigations of these problems were still in progress.

While the differences finally were reasonably well resolved, the initial thermal-conductivity measurements on GT-1 core samples made by two outside laboratories disagreed seriously. Since quick, reliable, conductivity measurements would be essential to an extensive program of heat-flow studies planned for the future, at the end of this period an existing LASL capability for conductivity measurements was being expanded to handle rock samples.

20.7. Rock mechanics

Using LASL pumping equipment and very low rates of pressurization, seven small hydraulic fractures were made in the hot granitic rock and amphibolite in the uncased section of well GT-1. Fracturing pressures measured at the surface were between about 1200 and about 1700 psi and correlated well with the rock structures in the intervals tested (as determined from the corresponding core samples). Pressures required to extend the fractures were determined and, as predicted, they decreased as the size of the fractures increased. By permitting the cracks to collapse and then determining the pressure required to reopen them, unusually good measurements were made of the least principal *in situ* earth stress. The fractures produced were substantially vertical and had a consistent northwest-southeast orientation, controlled by the regional tectonic stress field.

Two larger hydraulic fractures were then made in the uncased, previously fractured basement-rock section of GT-1 by an oil-field service company that specializes in hydraulic fracturing. The pumping rates used were low by commercial standards but were very much higher than those previously used in producing the small fractures. One result of the higher injection rates was that the apparent fracturing pressures were of the order of 1000 psi higher than those determined at the low pumping rates, in spite of the fact that small hydraulic fractures were already present in the section pressurized. Apparently this was because, initially, the ability of the fracture openings to accept water was overwhelmed by the relatively high pumping rate, and the wellbore was severely overpressured. Since hydraulic-fracturing experiments and earth-stress measurements made by others had normally been done using commercial pumping equipment and fluid-injection rates, these observations cast serious doubt on the validity of most of the experimental results on these subjects that had so far appeared in the literature.

Another result of this overpressurizing was that it produced a horizontal as well as vertical fracture. The collapse of each could be observed in the pressure vs time record. The unexpected formation of a horizontal fracture made possible a direct measurement of overburden pressure at the depth of the horizontal fracture that had previously been considered impossible at such depths.

At the end of the period covered by this report, a laboratory building at TA-33 had been dedicated

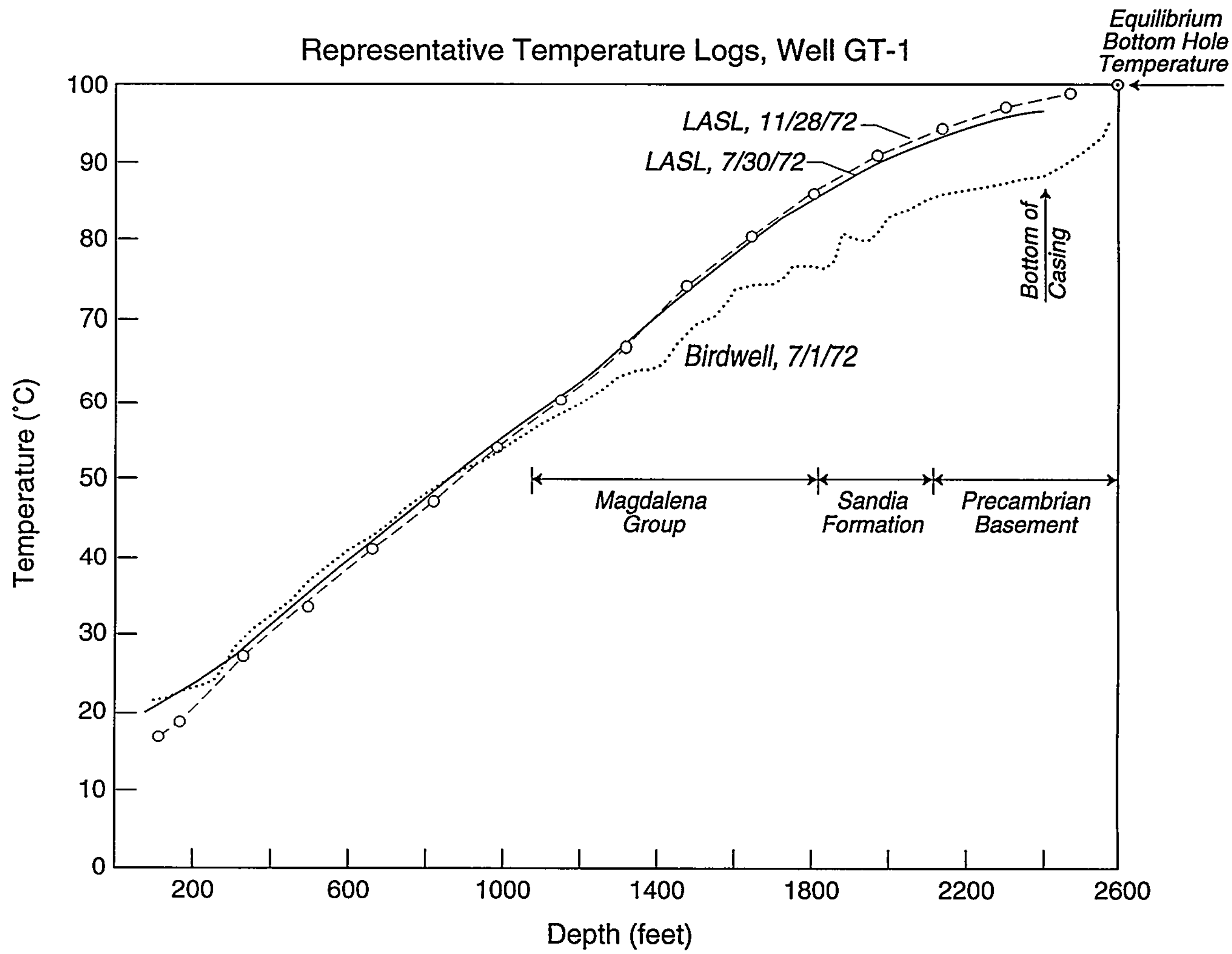

Fig. 17. Representative temperature logs, well GT-1.

to use as a rock-mechanics laboratory and was being equipped for both preparation and testing of rock samples. Hydraulic presses with 10,000-lb and 50,000-lb capacities had been installed, arrangements had been made to add a 1000-ton press; and plans were developed to modify and use a 5000-ton press in another laboratory area. Some mechanical testing had been done on the first testing machine installed there, and an existing finite-element computer code had been modified for use in modeling the stress-strain behavior of rocks under the temperature and pressure conditions that exist in geothermal-energy systems.

However, the initial laboratory activity was principally a preliminary study of the effect of stress state on the permeability to water of relatively dense rocks such as granite. Confirming the hydrology studies in GT-1, it was found that a very large increase in permeability occurred as compressive stress in the rock was reduced—as occurred in the field when fluid pressure in the GT-1 wellbore was increased and overcame a progressively larger fraction of the *in situ* compressive earth stress in the surrounding rock.

20.8. Thermal-stress cracking

For a very preliminary investigation of thermal-stress cracking, a concrete disc six feet in diameter and one foot thick was poured in the shade of the main TA-33 office building on June 3, 1973. Thermocouples were embedded in it at various depths and radial locations to observe its cooling history, and microphones were later positioned on its upper surface to detect any audible sounds produced by fracturing events.

On June 19, Jim Hill, Roland Pettit, Dan Miles, and Bob Potter cooled the central pad of the concrete disc's upper surface by maintaining a puddle of liquid nitrogen on it. A large number of fine, closely spaced, randomly oriented cracks soon appeared on the surface. Most of these then closed as, with continued cooling, a coarser pattern formed by extension of a small number of the fine cracks. Finally, one very large radial crack appeared, probably relieving most of the cooling stress, but in several areas away from the large fracture a crack pattern remained that approached a close-packed-hexagonal geometry with a characteristic size of about five inches across the flats of the hexagons. All of the cracks closed quite tightly when the concrete was permitted to warm back up to atmospheric temperature. However, before the cracks closed completely, they were labelled with dyes of various colors so that their relative ages could be distinguished. The pattern that remained on the cooled slab is shown in Fig. 18.

The microphones on top of the slab did pick up acoustic signals while it was cooling. Initially the signals were sharp and intense, as would be expected from a long series of individual fracturing events. Later they were both less sharp and less intense, perhaps resulting from crack extension rather than crack formation.

Roland and Dan later measured the mechanical properties of the concrete on separately cast samples from the same batch, 28 days after they were cast. They determined that the concrete weighed 137.2 pounds per cubic foot, had a compressive strength of 6755 psi and an average tensile splitting strength of about 1250 psi. Its Young's modulus was 3.913×10^6 psi and its Poisson's ratio was 0.22. It was also determined that the thermal diffusivity of the concrete was only about 40% of that of granite and that its compressive strength was only about 25% of that of a granite core sample from GT-1 tested by Jean-Claude Roegiers. The concrete was not a good stand-in for granite. Accordingly, we hoped to repeat the experiment on a large slab or natural outcrop of real granite, but we never had the opportunity or leisure to do that. However, this exercise was encouraging with regard to the probabilities that thermal-stress fracturing would occur in an HDR heat-extraction loop, and that we might be able to observe and map it with sensitive downhole geophones.

20.9. Geochemistry

Water samples collected from wells, springs, streams, lakes, and—at various depths and time intervals—from exploratory drill holes were

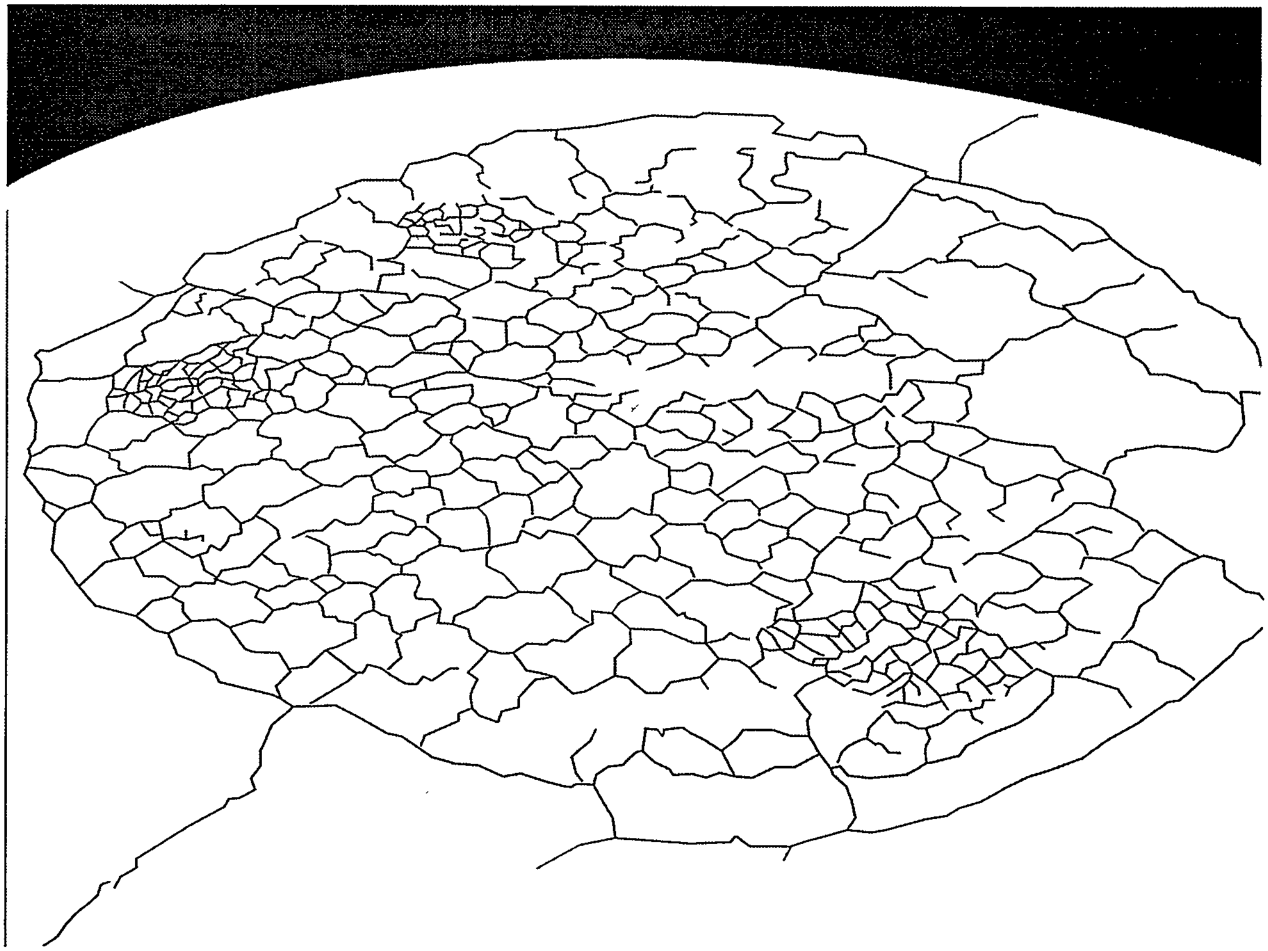

Fig. 18. Thermal-stress cracks produced by cooling the upper surface of a concrete slab.

analyzed chemically and isotopically to

- assist in hydrology studies;
- supplement existing knowledge of geothermal waters;
- identify water sources;
- estimate temperature at depth;
- estimate ages both of the water and of the rock through which it had circulated; and
- permit comparisons of natural waters with those produced in laboratory investigations.

Our initial proposal to DPR for research on "Chemical Equilibrium and Materials Transport in Systems Involving Pressurized Superheated Water and Silicate Minerals" was amended on June 27, 1972, then resubmitted on July 10, 1972, to show George Cowan (the CNC division leader) as the person in charge, with John Balagna and Jack Barnes as principal investigators. Thereafter, although John kept in close touch with us concerning this work and its results, that aspect of the DPR-supported work was handled primarily by CNC Division, who proposed extensions of it in revised proposals dated August 1, 1972, and March 23, 1973. In addition to static ("bomb") tests of mineral solubilities and dissolution rates, a laboratory-scale circulation loop was constructed to investigate experimentally the chemical and physical interactions between various rock samples and circulating water under conditions of temperature and pressure representative of those existing in geothermal reservoirs. Tested and untested samples, fluids, precipitates, and mineral-alteration products were investigated by such methods as chemical, activation, atomic-absorption, and Auger-electron analysis, X-ray diffraction, scanning electron microscopy, and petrography. Initial results from circulation of hot, pressurized water over GT-1 core samples suggested that the circulating water in the proposed HDR heat extraction loop would remain low enough in dissolved minerals to represent high-quality drinking water and to develop only minimal plugging and scaling problems in that loop.

However, because silica was the compound expected to dissolve in the greatest amount in high-temperature geothermal systems and to be most troublesome in the associated surface plant, a fundamental study of silica chemistry was initiated. Preliminary results suggested that, to an important degree, it might be possible to control both the dissolution and the reprecipitation of silica by relatively straightforward adjustments of the chemistry of the recirculated heat-extraction fluid—particularly of its tendency to oxidize or reduce the minerals present in the rock and in the solution itself. (The possibility of this type of chemical control over the geothermal fluid is one of the great advantages of a man-made HDR system over natural hydrothermal systems.)

Because of the large number of mineral species present in rocks such as granites, a great variety of chemical interactions is possible among the circulating solution and those minerals including dissolution, precipitation, mineral alterations, and reactions occurring both *in situ* and in solution. To investigate these, a proposal was submitted to DPR on March 23, 1973, for "The application of physico-chemical methods to the characterization of molecular species in hydrothermal systems and theoretical models to describe experimental behavior of the system." It listed George Cowan as the person in charge with John Balagna, C. E. ("Charley") Holley, Claude C. Herrick, and R. C. ("Roy") Feber as principal investigators. This proposal was expanded and, on November 9, 1973, resubmitted under a new title, "Experimental and Theoretical Investigations of Solution Chemistry Problems Associated with Geothermal Energy." Tested and untested samples from the circulation loop—rocks, minerals, mineral-alteration products, precipitates, and fluids—were investigated by such methods as chemical, activation, and atomic-absorption analysis, X-ray diffraction, Auger electron spectroscopy, and low-energy electron diffraction. Computerized chemical-thermodynamic studies were undertaken to investigate these reactions, largely using logic developed elsewhere but not previously run in full anywhere else (principally because other organizations lacked computer facilities of the type available at LASL). The predictive capability of this approach was established by excellent agreement between its predictions and experimental data from the laboratory system described above.

Special equipment was also being developed by Bert Dennis for downhole sampling of liquids and gases and for continuous monitoring of circulating fluid in the proposed HDR heat-extraction loop.

20.10. Fenton Hill

As has been described in Section 15 of this report, long before we had even begun our fracturing experiments in GT-1 we had selected Fenton Hill as the best available site for a deeper exploratory hole (well GT-2) and—if that lived up to our expectations—for subsequent development of a two-hole HDR heat-extraction system.

Both for our proposal and budgeting exercises and as a first step in site planning, we asked the LASL Engineering Department to prepare a conceptual design and a cost estimate for the surface plant required for the two-hole system. On April 12, 1972, Phil Reinig—then head of the LASL Engineering Division—agreed to put two men on it, with Bob Brasier as their contact with us. The men turned out to be Bob Hendron and Dick Foster, both design engineers, plus Ken Bowman, a draftsman. Although they soon began to work full-time for Q-22, they all remained in Group ENG-7, and for a time continued to work in the ENG-7 offices in the main technical area. This, however, required many telephone calls and trips to and from TA-33. With the approval of the ENG and Q Division Offices, in May 1973 I arranged for them to move into an office in the Q-22 headquarters building at TA-33. In addition to design work on the Fenton Hill surface plant, Bob, in particular, soon became directly involved in many other of our Q-22 activities.

In the meantime, in a report dated May 9, 1972, they showed a preliminary site plan that covered a fenced area of 2.07 acres at Fenton Hill. They estimated costs for road and site improvement, utilities, living and office trailers, an experimental building and a storage building, process systems (including pumps, piping, valves, heat-exchangers, etc.) and flow- and seismic-monitoring systems. The total came to approximately $1.7 million, including design and inspection costs (but not including well costs).

This site location, like that of GT-1 and our heat-flow holes, was in the Jemez District of the Santa Fe National Forest. We had discussed its possible use informally with local and regional Forest Service officials in Jemez Springs and Santa Fe, and they all reacted favorably to our proposed use of the site—as a demonstration of acceptable multiple use of the National Forest. With their permission, we arranged for the LASL Engineering Department to survey the site and its immediate surroundings topographically during the summer of 1972. The resulting map was used for more detailed site planning, as part of a formal request for permission to occupy the site during the period of our experiments, and as baseline information for an environmental impact statement concerning those experiments. This map, with the approximate location of the proposed GT-2 site indicated on it, was also attached to a memo dated November 22, 1972, from Don Brown to H. Jack Blackwell, then area manager of the AEC's Los Alamos Area Office. The memo described our need for a second exploratory well and our general reasons for choosing this Fenton Hill location as the preferred site for it. It concluded by asking for Jack's help in requesting a special-use permit from the Forest Service for our temporary occupancy of the site.

A detailed site plan for GT-2 drilling and downhole experiments was completed by Bob Hendron on December 20, 1972. It was laid out on the topographic map and now showed a site area of 4.9 acres, including a stump-disposal area in a small drainage swale and a mud pond needed for the drilling operation (later to be used for water storage).

With completion of our downhole experiments in GT-1 in April 1973, we were able to give more attention to detailed planning for GT-2. On May 4, 1973, I completed an informal paper on "Site Selection for a Geothermal Exploration Hole." It included discussions of the HDR concept, our need for a deep exploration well, our search for a suitable site for such a well and our reasons for selecting

Fenton Hill as the best available choice. On May 22, Don Brown sent a copy of that paper to Jack Blackwell at LAAO, accompanied by another memo that summarized our programmatic need for well GT-2 and again solicited Jack's help in securing Forest Service approval of our temporary occupancy of the site to drill the well and conduct experiments in it.

Governmental protocol required that such an arrangement be formalized between the two federal agencies involved, so this request was appropriate (and I believe that Jack welcomed it as documentation of our need for such an arrangement). Don sent copies of his memo and my paper to Dick Taschek, Bob Van Gemert, Bob Duffield, Ed Hammel, Rod Spence, and I believe that they all approved their content and purpose. However, between November 22, 1972, and May 22, 1973, our status had changed significantly; we were now a formal Laboratory group instead of an informal bunch of volunteers. We were so used to working informally and independently and had previously worked directly with LAAO on such matters for so long that we had forgotten that there was also a Laboratory protocol to be observed. I was promptly reminded of that fact by a very terse memo from our division leader pointing out that LASL was operated by the University of California and that such communications with an outside organization—in this case LAAO, which we had always considered part of the team—were to be submitted to and distributed by the division office. In general, thereafter, they were, which was a significant change in our operating mode. (We were sometimes dismayed by the editing that then occurred in the division office, although in many cases it was clearly appropriate.)

In any case, as a result of this memo and the informal discussions that preceded it, a meeting was held on June 8, 1973, of representatives of the Forest Service, LAAO, and LASL to consider use of the Fenton Hill site. (Our HDR Program was represented by Don Brown, Bob Hendron, Ed Hammel, Harry Jordan, and—from LAAO—Bud Wingfield.) At that meeting the Forest Service agreed to issue a Memorandum of Understanding permitting our use of the site to drill and experiment in well GT-2 and, if our results from those experiments justified it, then to develop there a two-hole HDR energy-extraction demonstration system. That memorandum was issued soon thereafter and was duly signed by senior AEC and Forest Service officials.

One significant provision in the Memo of Understanding was that the storage pond was to be constructed in accordance with Forest Service standards and, when we vacated the site, it was to be left as a water supply for livestock and native animals and for fighting forest fires. That made it a permanent improvement, which qualified as "construction" under the Congressional Davis-Bacon Act and caused the entire site-preparation contract to be considered "construction." In this case that was not important, since the contractor eventually selected to prepare the site was qualified to work under the conditions imposed by the Davis-Bacon Act.

At the same meeting with Forest Service and LAAO personnel it was agreed that an environmental-impact statement would be required for our Fenton Hill activities. I had expected that and had been working on one. On June 11, 1973, in draft form, I circulated it within LASL and to ALOO (the Albuquerque Operations Office) and LAAO, for comment and criticism. I got plenty of both. I had attempted to cover the drilling of GT-2, the experiments to be run in it, and the subsequent construction and operation of a two-hole heat-extraction system at the site. That was a very large order, and it become apparent that it would take a long time to complete a satisfactory document and get it reviewed and approved by the various agencies that would be involved. Therefore, Bob Hendron prepared a simpler "Environmental Assessment" that covered just the drilling of and experiments in GT-2. This required approval only by the AEC, which—in spite of local predictions that such an assessment would not be considered sufficient—we hoped might be approved quite promptly. Bob completed the assessment on July 7 and submitted it (through the proper channels) to the AEC, accompanied by a Forest Service statement that discussed land use and supported our proposed operations at Fenton Hill. By August 27 the assessment had been reviewed and approved by both LAAO and ALOO and sent on to AEC Headquarters. On September 25 it was returned to ALOO with requests for some minor changes. On September 29 it come back to LAAO, and thence to Bob. He revised and resubmitted it on October 11, and it received AEC approval on November 14. That was just in time, since it was less than a week later that we received word that the funds for drilling GT-2 were being released.

In the meantime, I had prepared a second draft of the environmental-impact statement, responding to comments and criticisms from LAAO, ALOO, and within LASL, which I circulated on August 10. This was followed by a first revision of the second draft, on October 5, 1973, prepared by Bob Hendron, which responded to further AEC comments. However, by then the AEC had determined that a full environmental-impact statement would in fact not be required unless and until it was decided to build a power plant at Fenton Hill. Since that was far beyond our immediate horizon, we shelved the impact statement until such time as it would be needed.

Many other preparations were made for the Fenton Hill operations during the summer and fall of 1973. Among them were the following:

- Darrell Sims and Don Brown had been working on plans for drilling and completing well GT-2. In July they negotiated with Smith Tool Co. for the purchase of special coring bits to be used in sampling the formations penetrated by the well. Twenty of those bits were ordered in August. These were a modification of a bit that Darrell had helped to design for the JOIDES deep-sea drilling program. In discussions with manufacturers of diamond coring bits, it had been determined that those were too expensive and too slow for continuous coring of the Precambrian section of well GT-2. For the MOHOLE project, Smith Tool Co. had manufactured 9 7/8-in. carbide-insert roller-cutter bits with 2 1/2-in. central openings to produce a core. Darrell persuaded them to make some 9 5/8-in. bits of that type for us. In October, a single 10 1/8-in. JOIDES-type bit (which Smith Tool Co. by then manufactured routinely) was ordered for coring in the sedimentary sections of GT-2. It had chisel teeth instead of the rounded roller-cutters used on hard-rock bits.

  On September 4, 1973, Don issued a purchase order for 5000 ft of 10 3/4-in. casing for GT-2.
- In August, LASL Group ENG-2 prepared a cost estimate of $21,500 for preparation of the GT-2 site. This included clearing and grubbing the burned-over site area; disposing of stumps and downed trees in the designated disposal area; constructing a mud pond for use during drilling, later to be used for water storage during experiments in GT-2; levelling the drilling area; and covering, then seeding, the stump-disposal area, using soil from levelling and pond construction. In October, our plans for the site having matured further, Don Brown prepared formal time and cost estimates for the site preparation. In November he completed the plans and specifications for it, to be used as part of a bidding package, and on November 29 Bob Hendron arranged for them to be issued for bidding.
- In November, as part of our environmental surveillance, an extensive series of pictures of the site was taken to document its condition before we occupied it and began the site preparation. They showed mostly snow, stumps, and downed, partially burned trees.
- Detailed planning of the experiments to be run in GT-2, together with identification of the equipment and instruments that would be needed, began in August. All of our HDR group was, and continued to be, involved, and on September 14 Don Brown circulated an informal paper called, "The Objectives of Drilling and Casing Programs for GT-2." It outlined our reasons for drilling and experimenting in well GT-2 as well as the drilling, casing, and logging programs, and the problems that might be encountered. Jim Hill, who was now working full-time with us, agreed to

coordinate and manage the experimental program. In November, Don Brown summarized all this in a detailed report on our plans for experiments in GT-2 during the rest of FY74.

- In early September, Darrell and Don completed the first draft of the final drilling plan for GT-2. There was general agreement that the plan should be reviewed by drilling experts outside of LASL, so on September 10 Don wrote a memo to Bob Duffield explaining that need and justifying selection of Fenix and Scisson (F&S) to review the plan. This was approved, and on September 24 and 25, at LASL, three F&S drilling engineers (J. A. Walker, J. A. Cross, and J. R. McLaughlin) did review it with Don and Darrell. They suggested some useful minor changes, which were agreed to by all parties, and in a letter to Bob Duffield on October 24, R. L. Littlejohn of F&S reported the changes and, with those changes included, expressed general approval of the plan.

  On October 30, Don released a time and cost estimate for drilling, casing, and logging GT-2, assuming a 9 5/8-in. diameter hole 4500-ft deep. The totals were 57 days of drilling-rig time and (with no contingency factor) $426,000. On November 21 he delivered the drilling package to our Supply and Property Department. It was issued later that month as an RFQ (Request for Quotations) on an inquiry-only basis.
- During October, Bob Hendron and Dick Foster prepared a preliminary design for the surface plant of the two-hole heat-extraction system.
- Jim Hill prepared a piping schematic for that surface plant and investigated the costs and availability of the necessary hardware. On an inquiry-only basis, Bob wrote an RFQ for the circulating pump that would be needed. In November, Jim worked with Bob and Dick in preparing working drawings for the surface plumbing of that system.
- In July, Bert Dennis prepared a summary of our instrumentation and associated equipment needs for the GT-2 project together with statements concerning our current situation with regard to procurement of each item. One urgent need was for a logging van, and in September he furnished our Supply and Property Department with specifications and a formal justification for its purchase. He also prepared specifications for a downhole instrumentation and control package, and arranged for the LASL electronics group to begin design work on it. With Francis West and Jim Hill, Bert undertook a broad survey of existing commercial equipment that might satisfy our special needs. (This involved a number of visits to various manufacturers of such equipment—many of them in Houston and Dallas.) In October, he prepared specifications for much of the suitable electronic equipment that was available commercially, which were sent out on an inquiry-only basis, and he spent much of November reviewing quotations from possible suppliers. In the meantime, Francis was investigating the possible usefulness of tiltmeters in our experiments, as well as the availability and suitability of commercial geophones and seismometers.
- In November, Darrell Sims completed the design and bidding specifications for the core-barrel assemblies that would be needed for core-drilling in GT-2. Don Brown prepared the purchase request for the big-bore drilling-collar assembly needed for continuous coring.
- Don also spent much of October and November locating and evaluating potential suppliers of drilling rigs, drill pipe, drilling mud, mud engineers, compressors, diesel fuel, logging services, water-hauling, and other anticipated needs.
- In late November, Bob Hendron arranged for a purchase request to be issued to the Jemez Mountains Electrical Cooperative (based in Espanola) to install a substation at the GT-2 site to supply electrical power to the Fenton Hill operations. He also made arrangements for telephone service to the site.

When, at the end of November 1973, DAT funding finally was released for the GT-2 project, we were well-prepared to spend it—and (largely on credit) we had already made a good start in doing so. By checking locally and visiting the Nevada Test Site, Bert Dennis, Francis West, and Jim Hill had also acquired some very useful surplus equipment at no cost to us except for a few shipping charges. This included a trailer for housing personnel at Fenton Hill and a small air-cooled heat exchanger for possible use in a heat-extraction experiment in well GT-2.

20.11. Group Q-21, Geosciences

From June 1972 through November 1973, our only line-item funding was from the Division of Physical Research of the AEC, and was for geophysical and geochemical research supportive but independent of our HDR Program. The HDR Program was to be engineering development of a new kind of geothermal system, which we expected to be funded by the AEC's Division of Applied Technology. As was described in Section 16.6 of this report, at the fourth meeting of our Geosciences Advisory Panel at the end of November 1972, I had emphasized this distinction and the separation of the research and engineering development functions of the overall program. I noted then that, at least in principle, our Geothermal Energy Group would be involved only in engineering development, and we expected that the relatively fundamental background research would be carried on by others.

Since the GAP felt that a stronger research emphasis was needed than we could provide, they favored this separation of functions. So did the LASL administration, the LASL Energy Office, and—after its formation on March 1, 1973—the Q Division administration. However, aside from the geochemical research (which already had largely been taken over by CNC Division) Q-22 was still responsible for the DPR-supported activities; there simply was no one else to assume that responsibility. Among other things, therefore, during March I prepared an up-to-date summary of those activities for Harold Agnew to deliver to Dixie Lee Ray at AEC Headquarters; discussed the DPR funding with the Q Division administrators; and, with Maury Katz, worked out the FY74 budgets for the various DPR-supported research activities.

At a meeting on March 29, 1973, with Ed Hammel, Rod Spence, and Orson Anderson, I was informed that a new Geosciences Group, Q-21, had been established, with Orson as its acting group leader. It was to take over the DPR-supported research and broaden it into a strong LASL earth-sciences program—with particular emphasis on rock mechanics. Its headquarters were to be at TA-33 (which I was to arrange). However, between that meeting and the end of November, Orson concentrated primarily on a search for a permanent group leader for Q-21. He did visit us on May 2 to insist that we promptly publish some good scientific papers and that Bob Potter should hasten to complete his study of heat flow on the Jemez Plateau. He appeared again on July 19 and 20, when he and Rod Spence and I discussed Q-21 budgets and manpower requirements, and Orson and I threshed out Q-21 office and laboratory space assignments at TA-33. In Orson's almost continuous absence from Los Alamos, the day-by-day Q-21 affairs remained our responsibility.

For example, with formation of Q Division and Group Q-22, it became necessary to again revise our proposals to DPR. We prepared a new version of our "Heat-Flow Study of a Potential Geothermal Energy Source," dated May 14, 1973. In this one, Bob Duffield was listed as the person in charge with four principal investigators: Don Brown, in charge of drilling and field measurements; W. L. ("Bill") Sibbitt, from the Subterrene Group, in charge of thermal-conductivity measurements; Bob Potter, in charge of local and nationwide heat-flow analyses and interpretations; and Orson Anderson, whose duties were not specified. Funding requested for FY73 (ending June 30) was reduced to $30,000 and for FY74 to $75,000—the latter primarily for thermal-conductivity measurements, data analysis, and modeling the thermal regime of the Jemez Plateau. Extension of the program into FY75 was proposed, with funding for that year at $119,000 to pay for drilling and logging additional heat-flow holes on the Plateau, more sophisticated computer modeling, and extension of heat-flow studies to other promising HDR areas elsewhere in the U.S. This proposal was revised again on August 31, 1973, and submitted to DPR as "Heat-Flow Studies of Potential Geothermal Energy Sources." The principal changes were an expansion of the proposed studies in geothermal areas other than the Jemez Plateau and an increase in the funding requested for FY74 to $200,000.

A revision of the proposal called "Fracture Dynamics of Hydraulic Fracturing and Thermal Stress Cracking in Crystalline Rocks" was also prepared, dated May 16, 1973. This listed Bob Duffield as the person in charge. The principal investigators were John Rowley, Bob Potter, and an unnamed rock-mechanics expert to be recruited. Funding requested for FY73 was $65,000 and for FY74 $117,000. Proposed laboratory and field experiments included static and dynamic testing of rock samples under multiaxial loading, computer modeling, thermal-stress-cracking experiments, and permeability studies. This proposal was again revised on August 31, 1973, the principal change being an increase in the funding request for FY74 to $150,000 to cover additional laboratory and field experiments.

There was also a revision of "Seismic Studies Related to Artificial Geothermal Energy Sources," dated May 14, 1973. This listed C. I. Browne as the person in charge with Ken Olsen, Orson Anderson, and Bob Potter as principal investigators. It proposed funding of $25,000 for FY73 and $91,000 for FY74, primarily for field studies of regional seismicity and the microseismicity produced by hydraulic-fracturing experiments. This proposal was revised again on August 31, 1973, the principal changes being development and application of instrumentation and techniques for investigation of acoustic signals from fracturing experiments under controlled conditions in the laboratory and an increase in FY74 funding to $150,000.

Jean-Claude Roegiers presented a special and continuing problem. When he first visited Los Alamos in October 1972 (in the company of Charles Fairhurst), Jean-Claude was a graduate student at the University of Minnesota working on a Ph.D. dissertation on hydraulic fracturing. Dick Taschek was interested in arranging for him to come to LASL as a visiting staff member, to work with us during our experiments in GT-1, and later to return to Los Alamos to finish his dissertation—which was concerned primarily with the use of hydraulic fracturing for downhole stress measurements. In a letter dated November 28, 1972, Dick contacted Charles Fairhurst and proposed such an arrangement. Fairhurst approved and Jean-Claude had already expressed his interest. Barbara Crabtree, a LASL personnel representative, contacted an assistant vice president of the University of Minnesota to confirm that the university approved of the proposed arrangement. The university did. Therefore, I was directed to arrange for a quick visit to Los Alamos by Jean-Claude to formalize the arrangement, which I did.

Jean-Claude visited Los Alamos from December 12 to 15, 1972. It turned out that, because he was not a U.S. citizen, AEC approval was required for a long-term appointment as a visiting staff member. However, an agreement was made with Dick Taschek that, when AEC approval had been granted, Jean-Claude would indeed be employed at Los Alamos as a visiting staff member until he had obtained his Ph.D. (an estimated period of three months) and that then there would be the possibility of extending his employment for two years. In the meantime, until January 31, 1973, he would be under contract to LASL as a consultant, and that arrangement required that his visits to LASL be limited to a maximum of three days each.

During the December visit, Jean-Claude spent much of his time with Lee Aamodt discussing rock-mechanics experiments that might be run on the 5000-ton press and the modifications to the press that would be needed to run them. There were also discussions with Lee and others of mechanical tests on GT-1 core samples that might be run at the University of Minnesota (at an estimated cost to us of $25,000). Core samples from GT-1 were later sent to Jean-Claude at the university, who ran compression and hydraulic-fracturing tests on them—with the results reported in item 7 of Section 17.3 of this report.

As was described in an earlier section, with AEC approval Jean-Claude did return to LASL as a visiting staff member in February 1973, and in March participated in some of the downhole experiments in GT-1 using the University of Minnesota packer assemblies. He then returned briefly to the university before coming back to Los Alamos for an extended stay. Since he was a Belgian national and had no security clearance, it was necessary to find office space for him in a nonsecurity area. Therefore he was assigned temporarily to the Q Division Cryogenics Group, Q-26, whose office and laboratory building was in such an area. Presumably he completed his doctoral dissertation there. Technical supervision of his activities was assigned (by Ed Hammel) to Lee Aamodt.

Jean-Claude's expertise was in rock mechanics, which was intended to be a particularly strong area of research in the new Geosciences Group. Therefore, when formation of Q-21 was announced in late March 1973, it was decided (presumably by Orson) that Jean-Claude should become its first staff member. Since Q-21 was to be based at TA-33 along with Q-22, I was directed to arrange office and laboratory space for the new group. With approval of the Division Office and the LASL space coordinator, I reserved an office for Jean-Claude in our headquarters building and space in an adjacent building (HP 113) for a rock-mechanics laboratory. Through the good offices of Bob Pogna, it became possible on July 20 for uncleared Laboratory employees and visitors to enter TA-33 without an escort, and on that day Jean-Claude moved in with us. Since the Q-21 organization was still unofficial, he was temporarily assigned to Q-22. However, he remained a visiting staff member, supported by our DPR funding, for the rest of the period covered by this report. His first major task at TA-33 was to begin organizing the new rock-mechanics laboratory.

Another University of Minnesota graduate student, Osamu ("Sam") Kudo, was brought to our attention in a letter from Charles Fairhurst in October 1972. Sam was a Japanese engineer, employed by a petroleum company in Japan, but at the time—under Fairhurst's supervision—he was working toward a master's degree in rock mechanics. His thesis study was development of an acoustic method for remotely detecting and determining the location of a fracture inside a rock mass. He expected to receive his degree from the university in July 1973 and then to remain in the United States until December. Jean-Claude reported that Sam was looking for employment for two or three months during that period and that his expertise could certainly be helpful in the LASL rock-mechanics studies. Primarily through the efforts of Lee Aamodt, an arrangement was made for Sam to come to LASL as a short-term visiting staff member. He arrived in early October and spent about two months with us, working closely with Jean-Claude and—like Jean-Claude—supported by our DPR funding.

When Sam arrived, Jean-Claude was making permeability measurements on rock samples, including core samples from GT-1. Sam joined him in this and also in doing stress calculations on inclined boreholes. Then the two of them proposed an experiment, using the general technique reported in Sam's thesis, to determine the dimensions of a crack in a rock mass using acoustic techniques. It involved sending a sonic pulse down one side of a crack, receiving it on the other side, and calculating the crack length from the transit time of the pulse as it travelled around the crack. Shear waves of two polarizations were to be generated as well as a compression wave. A Zia Company sonic generator and transducers were borrowed from Roland Pettit for the experiment and, on November 5, Jim Hill had a concrete block 2 ft by 3 ft by 4 ft cast for the experiment. It had a cast-in slit 1/8-in. wide and 1-ft deep to represent the crack. Crystal holders and transducer-mounting blocks were fabricated, and Bert Dennis installed strain gages on the block and checked them out. It was found that the strain gages were not sensitive enough to detect the sonic pulse, but that an acoustic receiver worked very well in detecting and resolving wave forms transmitted through the block. The subsequent experiments, which ultimately were quite successful, were still in progress at the end of the period covered by this report.

In the meantime, the rest of us in Q-22 were also involved in many other of the Q-21 affairs. We briefed several DPR representatives on the programs that they were funding, including A. R. Van Dyken on April 20, 1973, Enloe Ritter on June 1, and John Teem on November 12. In May we revised the proposals for the DPR research activities. In August, Harold Agnew asked DPR for additional funding for geophysics research, and Bob Potter and Lee Aamodt again revised our proposals—putting them in a new format (the "Pastore form") requested by the AEC and justifying the additional funds requested by Harold. Then, in September in Washington, Ed Hammel and I redid the Pastore forms—in accordance with another AEC request.

In June 1973, I prepared an FY73 budget summary for the Q Division Office, and scheduled the expenditure of the last of that fiscal year's DPR funds. In July I prepared a summary of our FY73 DPR-supported research for Dick Taschek, and in September an updated one for Dick to send to John Teem. Also, on September 20, 1973, George Kolstad requested monthly progress reports on our DPR-supported work, and in October I prepared and sent him the first such report. On October 10, George telephoned me, asking for a breakdown of our proposed DPR-funded activities for the five-year period FY75 through FY79, which I prepared and telephoned back to him the same day. There were, of course, many discussions of all of these things among our geothermal-energy group, with the Q Division Office staff, with Orson Anderson when he was here, and with AEC Headquarters personnel. All of the members of our group were also busy digesting the results of our experimental and field work, writing papers, giving talks, and briefing a steady stream of interested people from within and outside the Laboratory.

All of this was very time consuming, and on a number of occasions I pointed to our Q Division management that—superimposed on preparations for DAT-supported work at Fenton Hill—it was simply more than our small group could handle. For example, in a memo dated August 31, 1973, to Ed Hammel in the Division Office, I began with the following paragraph.

"The problems of Q-21 continue to plague us, and to get less attention than they deserve because Q-22 has more problems of its own than it can handle. When OMB releases the funds appropriated for the Q-22 demonstration project, we will be even busier than we are now and, in whatever condition, Q-21 will then either sink or swim. At least in an acting capacity, somebody is needed now who is responsible in behalf of Q-21 for preparing proposals, writing program plans, signing orders, arranging space, installing equipment, supervising personnel, preparing reports, and protecting the Group from Q-22—which is busy using up all of the money and occupying all of the space."

Of course, this sort of complaint brought me very little sympathy and no immediate remedial action. However, later that fall the Division Office did ask Lee Aamodt to become acting group leader of Q-21, pending selection and arrival on the scene of a permanent group leader for it. His immediate tasks were to design the experiment that Sam Kudo and Jean-Claude Roegiers were to run on that cracked concrete block; to investigate the probable usefulness of the 5000-ton press in rock-mechanics studies; and to set up a group office in preparation for the arrival of a new Q-21 group leader. Busy as he was with other matters, Lee of course did a fine job on all this.

Nevertheless, on November 27, 1973, when I received permission to start spending DAT money, I was busy preparing a memo to John Teem on the allocation of the FY74 funds provided by DPR to support the Q-21 geophysical research.

While much remained to be done, our DPR-supported research did establish a basis for later development of what became a very broad and strong earth-sciences research program at LASL. It should not be forgotten that it was the efforts of the original, small geothermal-energy group that began all that.

20.12. Group Q-24, Georesources Development

Lee Aamodt was a consistent and prolific source of ideas and had both the background and the energy to analyze and document them in useful ways. Among other things, he had been directly involved in the Plowshare Program, an important part of which was the use of nuclear explosives to stimulate production of petroleum and natural gas. This led him to consider the possible uses of hydraulic fracturing in similar and related applications. He outlined some of these ideas in a memorandum to Harold Agnew dated January 11, 1973, and followed it with the draft of a proposal to DAT (dated January 16, 1973) for a "Georesources Development" project.

The subject of Lee's memo to Harold was "Industrial Applications of Large Fractures, with Special Reference to Systems in which Fluids Circulate through the Crack." He suggested the following applications, all of which were and are at least potentially useful: (1) regasification of liquefied natural gas; (2) exploitation of tight gas reservoirs; and (3) other promising applications, including pumped storage of compressed gas to supply peaking power for generating electricity; underground storage of water; systems for recharging aquifers; and improved systems for flooding depleted fields for increased recovery of petroleum.

In his draft proposal to DAT, Lee outlined a five-year program with a total cost of $9.2 million, beginning with a fundamental study of hydraulic fracturing, thermal contraction of rocks, and thermal-stress cracking, and continuing with a series of large-scale field experiments in stimulation of tight gas formations and regasification of liquefied natural gas. Among other things, he suggested additional uses of the general technique, including: recovery of water, brine, and helium; *in situ* leaching of solid hydrocarbons and metallic and nonmetallic minerals; underground reaction vessels and sources of low-grade heat for large-scale physical and chemical processing; production of large, well-shielded, interconnected void volumes for storage of gases and liquids; and development of subterranean waste-disposal systems.

In both the memo to Harold and the draft proposal, Lee emphasized the use of hydraulic fracturing to produce large, interconnected crack systems, and then the circulation of a cool fluid through the cracks to enlarge them by thermal contraction and thermal-stress cracking of the rock. As Lee pointed out in his draft proposal, "Hydraulic fracturing offers an alternative to the use of nuclear explosives in many applications which previously have been considered in Plowshare studies, and appears in some of these to have definite engineering, economic, and environmental advantages."

A slightly revised version of the draft proposal was sent to AEC Headquarters, where it aroused considerable interest—particularly among the Plowshare crowd, who were suffering from increasing public antipathy to any use of nuclear explosives.

Lee then prepared a paper on one of the activities that might be appropriate to such a program: "A Georesource Project: Regasification of Liquefied Natural Gas (LNG)." He sent copies both to AEC Headquarters and to Philip L. Randolph, then manager of the Nuclear Group at El Paso Natural Gas Co. Randolph, together with Lee, had been involved in Plowshare gas-stimulation experiments, and his company burned a great deal of natural gas in regasifying imported LNG.

By telephone, correspondence, and personal visits, Lee maintained discussion of an AEC sponsored Georesources Development Program and the LNG regasification idea. He also prepared and circulated a paper called, "Hydraulic Fracturing as an Alternative to Nuclear Explosions for Stimulating Tight Natural Gas Reservoirs"—another project appropriate to such a program. As a result of all this, Jerry Johnson requested a revised, formal proposal to DAT for the program which he said would probably be conducted under Plowshare auspices. Such a proposal was submitted on March 26, 1973. It emphasized the application of hydraulic fracturing to stimulation of production from tight gas-bearing sands at a location near that at which a nuclear-stimulation experiment had already been conducted, permitting a direct comparison of the effectiveness of the two techniques.

While such a project would have many interests in common with our HDR Program, it was to be done under Plowshare auspices, and so with separate funding and under a different set of program managers at AEC Headquarters. It would also have different interests and goals than the HDR Program and—if undertaken in Q-22—would represent a serious distraction to our limited HDR staff. Within LASL, therefore, I urged that a new and separate LASL group be formed to handle it and other georesources projects as they materialized. I also felt that Lee Aamodt was the obvious person to head up such a group if it were formed, and learned from him that he would be willing to serve in that capacity.

Lee's proposal was welcomed by Plowshare people in Washington and elsewhere. On July 7, 1973, Lee met in Washington with Edward ("Ed") Fleming, then assistant director for Nuclear Engineering in DAT, and Jack Kahn who represented Lawrence Livermore Laboratory's Plowshare interests. A five-year program was planned for the hydraulic-fracturing gas-stimulation project. It was to involve industrial as well as LASL and LLL participation. LASL's part was to be primarily in the field experiments.

On June 28, 1973, we were told by headquarters personnel that $800 thousand of FY74 Plowshare funds were "in the bag" for the gas-stimulation project. A new LASL group—Q-24, Georesources Development—was formed to handle LASL participation in the project, and Lee Aamodt was appointed LASL project manager. As expected, Ed Fleming was to be the program manager at AEC Headquarters, and on July 23 he reported that LASL was "solidly funded" for $300 thousand for its participation. However, until that money actually appeared in the LASL financial plan—which had not occurred at the end of the period covered by this report—Group Q-24 remained an unofficial LASL organization, in the same general situation as Group Q-21.

In the meantime, Lee remained an active member of Group Q-22 and continued to contribute to the planning and progress of the HDR Program. Necessarily, however, he spent some of his time on Q-21 affairs and also a great deal of time on planning and budgeting for the AEC's and LASL'S-Georesources Development Program and on arrangements for the proposed gas-stimulation project. This involved extended visits to Washington, many memos, letters, and budget exercises, and numerous telephone calls and briefings. At the end of the period covered by this report, in late November 1973, Lee was busy putting together another revision of a five-year program plan for LASL Group Q-24.

While it didn't all happen immediately, several major georesources projects did materialize later, in which LASL played a major role. Again, this all started within our original small geothermal-energy group.

20.13. Presentations and publications

During this period, members of our Geothermal Energy Group gave a great many talks and presentations to a wide variety of audiences, some of which have been listed in earlier sections of this report. Two particularly important presentations, to subcommittees of the U.S. Senate and House of Representatives, are described in Appendix B of this report.

There were also a few publications concerning our plans, activities, and results during these first three years of the HDR Program which are listed in a later section of this report.

21. ACCOMPLISHMENTS

While we were not the first to recognize the existence of an essentially inexhaustible reservoir of potentially useful thermal energy at accessible depths in the earth's crust, we were the first to propose and undertake the development of a practical and environmentally benign method of extracting some of that energy from subterranean crustal rocks and bringing it to the earth's surface. The LASL Hot Dry Rock Geothermal Energy Development Program, established to investigate and develop that method, culminated a few years later in the successful construction and operation of HDR energy production systems at Fenton Hill, New Mexico, the first of those in 1977. The basis for those demonstrations was established during the period whose geothermal-energy activities have been described in this report.

During the years 1970 through 1973, the Los Alamos HDR Program produced a long series of ideas, developments, and discoveries, ranging from trivial to some that are truly important to the world's energy future. Among the more important of these were the following.

21.1. Invention

In 1970, a new method of extracting thermal energy from naturally heated rocks in the earth's upper crust was invented by a small group of LASL scientists and engineers. It uses fluid pressure to produce large cracks that connect two wells drilled into hot rock of low initial permeability and free-water content. Pressurized water circulated through this connected underground loop extracts heat from the rock and brings it to the surface. A patent on this concept was applied for in 1972 and granted in 1974 in the names of Robert M. Potter, Eugene S. Robinson, and Morton C. Smith, with the patent assigned to the AEC.

21.2. Organization and financial support

Initially, the investigation and development of this concept was conducted by an informal group of LASL volunteers as a part-time activity, largely during their own free time. The background information that they collected and reported was of sufficient scientific and engineering interest to attract financial support for continuing HDR-related geophysical and geochemical research, first from the Supplementary Research funds provided to LASL by the AEC Division of Military Application and then by the AEC Division of Physical Research. The results of this research indicated that the HDR concept was sufficiently promising to justify support of engineering development of a prototype HDR heat-extraction loop, which was sponsored by the AEC Division of Applied Technology. A formal LASL Geothermal Energy Group, LASL Group Q-22, was formed in 1973 to undertake that development. Its successes have since led to the formation of parallel and complementary research and development HDR groups in several other countries, notably in the United Kingdom, Germany, France, Japan, Sweden, and Russia, and to investigations of the HDR concept and its possible application in a number of other countries.

21.3. Exploration and site characterization

Hot, dry holes have frequently been drilled in unsuccessful explorations for petroleum, natural gas, and natural steam and hot water. For the first time ever, the HDR Program undertook a search for an area in which hot, dry holes could be drilled intentionally—reaching usefully high temperatures at moderate depths in low-permeability rock devoid of significant amounts of free water. Geologic, geophysical, hydrologic, geochemical and heat-flow studies identified a large area in which this subterranean environment appeared to exist, on the western flank of a large volcanic caldera in the Jemez Mountains of northern New Mexico. Its existence was confirmed by drilling an exploratory well (GT-1) in that area, which at a depth of 2105 feet entered hot, crystalline, basement rock that in fact was very low in permeability and free-water content. The existence and feasibility of locating a site for development of an HDR energy system was thus established, together with the fact that such an area could occupy many square miles—providing great flexibility with regard to site location and permitting development of very large energy systems by creating either a number of closely spaced parallel units or a single large unit consisting of a number of interconnected injection and production wells. This investigation demonstrated the advantages of slim-hole drilling for determining the nature and potential usefulness of a geothermal prospect area.

21.4. Drilling

In discussions with a number of experts before the first deep exploratory hole was drilled, we learned that at least some of them felt that it would be impossible to drill in hot, competent basement rock with conventional oil-field drilling equipment. In fact, when GT-1 was drilled, it was found that there were fewer problems and higher average drilling rates in the Precambrian basement than in the overlying sedimentary formations. With the drilling bit rotating on bottom, penetration rates were relatively low in the basement rock, and bit wear was relatively rapid. However, there were no problems from lost circulation, caving of the borehole wall, swelling clays, or stuck bits, all of which are common in drilling many sedimentary formations—and were in drilling through the overlying sedimentary sections of GT-1.

21.5. Formation permeability

It had also been predicted that the presence of joints and pre-existing natural fractures in the basement rock would cause its initial permeability to be so high that it could not successfully contain a pressurized heat-extraction fluid. The basement-rock section of well GT-1 was indeed found to contain many ancient joints and fractures. These, however, were so well sealed by recrystallization and the deposition of secondary minerals that the permeability of the basement rock was so low that, by most definitions, it would be considered impermeable. Its properties were such that (as was later confirmed in pressurized circulation systems) the rate of water loss from an operating pressurized heat-extraction system should be very low. Since GT-1 was within about 2.5 miles of a major fault system produced by collapse of the Valles Caldera, this suggests that the same may be true of basement rock in most locations which are not less than 2 or 3 miles away from any fault systems that have been active in recent geologic time.

21.6. Hydraulic fracturing

There was also widespread uncertainty concerning the possibility of creating hydraulic fractures in competent basement rock. In fact, hydraulic fractures were produced in the Precambrian section of GT-1 at moderate pumping pressures and with no major problems except with the open-hole packers used to isolate the section of borehole to be pressurized. Although most of the basement-rock sections fractured hydraulically contained variously oriented ancient joints and fractures, the hydraulic fractures themselves were substantially vertical and had a consistent northwest-southeast orientation, evidently controlled by the *in situ* tectonic stress field.

21.7. Seismic fracture mapping

No identifiable acoustic (microseismic) signals were recorded from a surface array of very sensitive seismometers during hydraulic-fracturing operations using very low fluid-injection rates. However, a single three-component surface instrument did detect well-resolved microseismic signals from fracturing events that occurred during fluid injection at a much higher pumping rate. This demonstrated that—at least under some conditions—the source locations of acoustic signals generated by relatively small hydraulic-fracturing events in hot basement rock at considerable depth could be determined to produce maps of the fractures themselves. However, it also demonstrated the need for better understanding and much further development of microseismic fracture-mapping techniques.

21.8. Seismic hazard

The "microearthquakes" produced by hydraulic-fracturing events in GT-1 were too small by several orders of magnitude to be felt or even detected by ordinary seismometers at the earth's surface. As has been true of hydraulic-fracturing operations conducted at reasonable distances from active fault systems in many other places, our fracturing operations in GT-1 evidently involved no real risk of triggering a damaging earthquake.

21.9. Earth stresses

A new method of measuring the least principal earth stress at depth was developed, involving measuring the fluid pressure required to reopen an existing hydraulic fracture.

It was also discovered that by pumping rapidly it was possible to overpressure the well to the point at which horizontal as well as vertical fractures could be created. Repressurizing the well to the point at which an existing horizontal fracture reopens gives a direct measure of overburden pressure which—on the Jemez Plateau as in most other places—is the greatest principal earth stress. It was also learned that fluid injection at the very high pumping rates normally used in commercial hydraulic-fracturing operations gave apparent fracturing pressures much higher than the actual pressures required to create hydraulic fractures at very low pumping rates.

21.10. Crack-extension pressure

When a hydraulic fracture has been created and the well has been shut in, pressure in the well declines due to the combination of fracture growth and permeation loss of fluid to the surrounding formation. Finally a "pressure plateau" is reached called "the shut-in pressure," during which the pressure declines very slowly. Then a more rapid rate of pressure decline is again observed, due to permeation loss of fluid from the well and the fracture surfaces.

In the past, it has usually been assumed that the shut-in pressure represents the least principal earth stress. In fact, it is significantly higher than the least principal earth stress and actually represents the fluid pressure required to extend the existing hydraulic fracture.

For the hydraulic fracture to grow, it is necessary for the region behind the crack tip to open more and more widely. This increase in aperture occurs by elastic compression of the rock behind the surfaces of the existing fracture, representing an increment of compressive stress above that of the earth's local field. However, after the well is shut in, the fluid pressure in it declines quite rapidly. When it drops below that required to extend the fracture further—at the "minimum crack-extension pressure"—the crack begins to close, progressively relieving the stress that had kept it open and forcing its contents back into the wellbore. Return of this fluid tends to keep the pressure in the well nearly constant, producing the pressure plateau observed at the shut-in pressure. (However, as a result of the progressive decrease in compressive stress in rock adjacent to the fracture, this "plateau" has a definite negative slope.) When closure of the fracture is nearly complete, the rate of fluid return to the well diminishes to the point at which it no longer compensates for the rate of fluid loss by permeation, and the rate of pressure drop in the system then increases quite sharply.

21.11. Downhole temperatures

Repeated logs in GT-1 showed continuous changes in downhole temperatures for about three months after drilling was completed, showing that the common practice of logging temperatures just once soon after the hole is completed can be misleading with regard to equilibrium rock temperatures at depth. Periodic local variations in downhole temperature apparently resulted from formation of a series of convection cells in the water-filled hole and indicate that individual temperature measurements made in large-diameter water-filled holes where geothermal gradients are high can also be misleading, even after thermal equilibrium in the well has apparently been established.

21.12. Geochemistry

Results from laboratory circulation tests of the interactions of water with GT-1 core samples at elevated temperatures and pressures indicated that water circulated through the proposed HDR circulation loop at Fenton Hill should remain low enough in dissolved solids to present little or no plugging or scaling problems in that loop. This may also be true in other locations when the basement rock is similar to that underlying the Jemez Plateau—which probably is quite common, at least in the western United States.

Computerized chemical-thermodynamic studies of possible reactions in such a system produced predictions that agreed closely with the experimental results from static "bomb" tests and from the laboratory circulation loop.

21.13. Geosciences and Geoengineering

The relatively fundamental and more applied R&D undertaken during the early years of the LASL HDR Program led directly to establishment of individual Geosciences and Georesources Development groups at the Laboratory. These in turn developed into broad and successful programs of fundamental earth-science research and to engineering applications of the concepts and techniques that originated in the HDR Program.

ACKNOWLEDGMENTS

For a variety of reasons, this history is deficient in several important respects. It is particularly and regrettably so in often failing to give credit where credit is due—in not identifying the individuals directly responsible for many of the activities described here and the results listed. In large part that is because—during most of the period covered—ours was an informal and unofficial group of volunteers with no formal requirement and very little time for record keeping. As a result, such documentation of our early activities as still exists is largely in the form of copies of such things as letters, internal memoranda, funding proposals, and notes for talks and from meetings. Most of these simply describe what had happened without saying who did it. Further, after twenty-odd years, the human memories that might supplement the sparse written records have become vague, and in some cases are gone forever. Several of the important contributors to our early efforts have died. Most of the rest have retired and some have left town. The few who remain have largely been either too busy or too modest to claim their fair share of whatever glory there may be in having contributed to the invention and early development of the HDR energy concept. I am, however, grateful for the comments and corrections concerning certain sections of the manuscript of this report supplied to me by

- R. Lee Aamodt,
- Bert R. Dennis,
- David V. Duchane,
- Robert H. Hendron,
- James H. Hill,
- Berthus B. McInteer,
- William D. Purtymun, and
- John C. Rowley.

Going back to its very beginning, credit for the invention and initial analysis of the HDR energy concept must be shared among all members of the original Subterrene Committee:

- Dale E. Armstrong,
- Theodore P. Cotter,
- Berthus B. McInteer,
- Robert L. Mills,
- Robert M. Potter,
- Eugene S. Robinson, Chairman,
- John C. Rowley, and
- Morton C. Smith.

Fortunately for the program and for me, Bob Potter shared my conviction that the HDR energy resource was certain eventually to be important to the world's energy future and was willing to work with me in an effort to initiate a Los Alamos program to investigate, develop, and demonstrate the HDR concept for production of thermal energy from that abundant resource. We were soon joined in the effort by Donald W. Brown and Lee Aamodt, and then by Bill Purtymun, Bert R. Dennis, and Francis G. West.

Thanks particularly to the interest in the program of LASL directors Norris E. Bradbury and Harold M. Agnew, of LASL's assistant director for Research Richard F. Taschek, and of LASL division leaders Richard D. Baker, Roderick W. Spence, William E. Ogle, and Duncan P. MacDougall, our initial background and field studies were supported primarily by discretionary research funds provided to LASL by the Division of Military Application of the AEC. Then, through the influence of John J. Flaherty, the AEC's assistant general manager for Energy and Development Programs, and the invaluable help of George A. Kolstad, assistant director of the AEC's Division of Physical Research (DPR), we received direct funding from DPR for geophysical and geochemical research in support of our HDR Program. This permitted an expansion of our background and field studies, to which John P. Balagna, Kenneth H. Olsen, and Bill Purtymun were major contributors. We were also joined in our efforts by Bob Hendron, Darrell M. Sims, Richard D. Foster, Roland A. Pettit, and Daniel J. Miles.

During this period we received a great deal of information, advice, and direct assistance from members of the U.S. Geological Survey, particularly from Frank W. Trainer and Arthur H. Lachenbruch. Members of the LASL Director's Geosciences Advisory Panel were also very helpful, in particular Orson L. Anderson, Priscilla Dudley, David B. Slemmons, Hatton S. Yoder, and—again—Art Lachenbruch.

The promising results of our preliminary studies and experiments led to formation of an official LASL Geothermal Energy Group, Q-22, formed to continue and expand the investigation and development of HDR energy sources. The original members of Q-22 were

- Lee Aamodt,
- Don Brown, Assistant Group Leader,
- Bert Dennis,
- Bob Potter,
- Mort Smith, Group Leader, and
- Francis West.

We were soon joined by Glenna Newman as group secretary.

All of these members of Group Q-22 were dedicated to successful development of HDR energy systems, and their individual contributions (which too often have not been identified in this report) made possible our early progress toward that goal. Fortunately, we had the active and continued support of the Q Division administration, particularly of Robert B. Duffield, Edward F. Hammel, and Rod Spence. In times of financial distress, which were common, we received invaluable help from Robert J. Van Gemert, James H. Sahling, and Dwight S. Clayton.

Finally, at the end of the period covered by this report, we received major funding from the AEC to undertake the next major step in the HDR Program: development and operation of the world's first HDR energy production system, at Fenton Hill. For this we are indebted particularly to Gerald W. Johnson, Jack Vanderryn, and James C. Bresee of the AEC's Division of Applied Technology.

There were, of course, many others who contributed directly or indirectly and in a wide variety of ways to the establishment and progress of the Los Alamos HDR Program. They are far too numerous to list here, but—like the individuals named above—I acknowledge with gratitude the help that they gave us during these stressful but productive years.

Morton C. Smith

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APPENDIX A

DRILLED HOLES

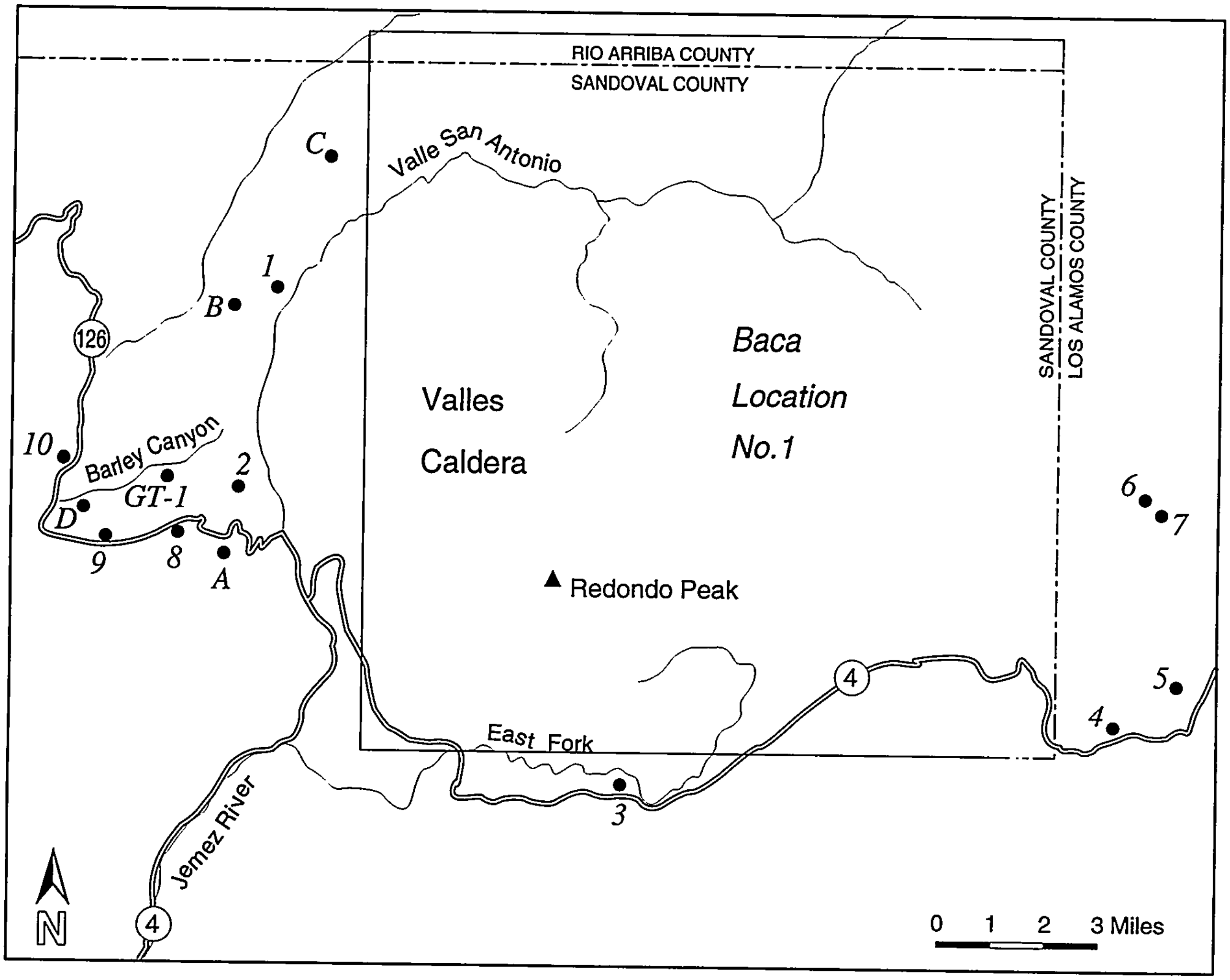

Fig. 19. Approximate hole locations. Numbered circles indicate shallow heat-flow holes; letters represent intermediate-depth heat flow holes. GT-1 is our first deep exploratory hole.

1. SHALLOW HEAT-FLOW HOLES

Site 1

Location:	In Oat Canyon near Forest Route 376, about 0.5 miles NW of San Antonio Hot Spring, about 4.5 miles N and 1 mile E of Fenton Hill and about 1.9 miles NW of ring fault
Drilled:	December 7,1971
Total Depth:	72 ft. bottomed on hard rock, probably latite of the Tschicoma Formation
Casing:	70 ft of 2-in.-diameter plastic tubing, extending 2.0 ft above land surface. Annular space between bore wall and casing filled with cuttings from the hole.
Geologic Section:	0-4 ft: soil, dark brown 4-72 ft: tuff, light gray
Temperature Log:	December 19, 1971, by NMIMT crew. Gradient was 235°C/km in depth interval 10m to 20m.

Site 2

Location:	Beside Forest Route 376, about 1 mile N and 0.5 mile E of Fenton Hill and 1.5 miles west of ring fault
Drilled:	December 10, 1971
Total Depth:	52 ft. bottomed on hard rock, probably latite of the Tschicoma Formation.
Casing:	49 ft of 2-in.-diameter plastic tubing, extending 0.5 ft above land surface. Annular space between bore wall and casing filled with cuttings from the hole.
Geologic Section:	0-34 ft: pumice, dark brown, weathered 34-52 ft: tuff, brown, weathered to clay
Temperature Log:	December 20, 1971, by NMIMT crew. Gradient was 145°C/km in interval 10m to 14m.

Site 3

Location:	Near State Highway 4, about 1 mile W of Las Conchas Campground and about 1 mile S of ring fault
Drilled:	December 13, 1971
Total Depth:	103 ft
Casing:	102 ft of 2-in.-diameter plastic tubing, extending 0.5 ft above land surface. Annular space between bore wall and casing filled with cuttings from the hole.
Geologic Section:	0-67 ft: pumice, weathered, containing much moisture 67-103 ft: tuff, reworked, brown
Temperature Log:	December 20, 1971, by NMIMT crew. Gradient was 232°C/km in pumice in interval 10m to 19m, and 99°C/km in tuff in interval 19m to 30m.

Site 4

Location:	Near State Highway 4, about 1 mile E of Sandoval County line and about 3 miles SE of ring fault
Drilled:	December 15, 1971
Total Depth:	74 ft. bottomed in welded tuff.
Casing:	73 ft of 2-in.-diameter plastic tubing, extending 0.5 ft above land surface. Annular space between bore wall and casing filled with cuttings from hole.
Geologic Section:	0-3 ft: soil, brown 3-72 ft: tuff, gray 72-74ft: tuff, dark gray, dense
Temperature Log:	December 21, 1971, by NMIMT crew. Hole has a reversed temperature gradient and is not suitable for heat-flow measurements.

Site 5

Location:	Near State Highway 4 about 3/4 mile NE of Site 4 and also about 3 miles SE of ring fault
Drilled:	December 17, 1971
Total Depth.	98 ft. bottomed in welded tuff.
Casing:	95 ft of 2-in.-diameter plastic tubing extending 0.5 ft above land surface. Annular space between bore wall and casing filled with sand.
Geologic Section:	0-72 ft: tuff, brown, weathered to clay 72-92 ft: tuff, light gray 92-98 ft: tuff, dark gray, dense
Temperature Log:	December 21, 1971, by NMIMT crew. Gradient was 84°C/km in interval 22m to 27m.

Site 6

Location:	Near Pipeline Road, about 4 miles N of Site 4, 3/4 mile east of Sandoval County line, and about 1 1/2 miles E of ring fault
Drilled:	December 16, 1971
Total Depth:	9 ft. in welded tuff. Abandoned and filled.

Site 7

Location:	About 1/2 mile SE of Site 6
Drilled:	December 16, 1971
Total Depth:	24 ft. in welded tuff. Abandoned and filled.

Site 8

Location: On Fenton Hill, near HDR test site, about 2 miles W of ring fault
Drilled.: February 1, 1972
Total Depth: 103 ft
Casing: 98 ft of 2-in.-diameter plastic pipe
Geologic Section: 0-4 ft: soil
4-19 ft: tuff, dark brown, moderately welded.
19-103 ft: tuff, gray, nonwelded. Dark brown clay 33 to 35 ft. Dacite rock fragments 58 to 78 ft.

Site 9

Location: Near State Highway 126, about 1.2 miles W of Fenton Hill HDR test site, and about 3.3 miles W of ring fault.
Drilled: February 2, 1972
Total Depth: 103 ft. bottomed in what appeared to be Abiquiu tuff
Casing: 92 ft of 2-in.-diameter plastic pipe
Geologic Section: 0-5 ft. soil brown
5-103 ft: tuff, very fine grained, nonwelded, badly weathered, light brown. Water at 29 ft.

Site 10

Location: Near State Highway 126, about 1.2 miles north of Fenton Lake and about 5 miles west of ring fault.
Drilled: February 3, 1972
Total Depth: 59 ft. bottomed in dense, fine-grained tuff.
Casing: None
Geologic Section: 0-3 ft: soil, dark brown
3-59 ft: tuff, brown, weathered. Occasional dacite rock fragments. Water at 32 ft.

2. INTERMEDIATE-DEPTH HEAT-FLOW HOLES

Hole A

Location: NE1/4, NW1/4, SW1/4, Sec. 18, T19N, R3E. In Lake Fork Canyon beside a private road, about 0.7 mile SE of Fenton Hill and 1.9 miles SW of the ring fault. Elevation of land surface: 8450 ft.

Drilled: Bentonite mud, rotary, by Stewart Brothers Drilling Co. Drilled at 9 5/8-in. diameter to 100 ft, 6 1/4-in. to 590 ft.

Total Depth: 590 ft. Completed in Abo Formation.

Casing: 7-in. OD to 97 ft, cemented in place
4 1/2-in. OD to 578 ft. (installed by Shamrock Drilling Co.)

Completed: April 10, 1972

Geologic Section: 0-30 ft: Bandelier tuff, light gray, nonwelded to moderately welded, rhyolitic; crystals and crystal fragments of sanidine and quartz, with rock fragments of pumice and latite, in gray ash matrix.

30-155 ft: Abiquiu tuff.

- 30-50 ft: tuff
- 50-100 ft: sandstone, light gray, tuffaceous, friable; mafic minerals and a few quartz crystals; rock fragments of pumice, latite, and rhyolite, in a fine ash matrix, with mafic minerals altered to stain matrix light yellow.
- 100-155 ft: conglomerate, light gray, tuffaceous, friable, with rock fragments of quartz and chalcedony.

155-590 ft: Abo Formation:

- 155-260 ft: silty sandstone, brownish-red.
- 260-320 ft: shale, dark red
- 320-380 ft: silty sandstone, red.
- 380-460 ft: shale, red.
- 460-590 ft: shale, dark red.

Water Levels: 490 ft in 1972 and 485 ft in 1975. Water occurs in the fine sediments of the Abo Formation. The drilling contractor did not believe the well was capable of yielding enough water for a stock well.

Water Quality: Potable, with 498 ppm dissolved solids.

Heat Flow: 5.13×10^{-6} cal/cm^2/sec

Hole B

Location: NE1/4, NW1/4, NE1/4, Sec. 31, T20N, R3E. In Oat Canyon, about 4.5 miles north of Hole A, 3/4 mile west of San Antonio Hot Spring, about 2 miles west of the ring fault. Elevation of land surface: 8625 ft.

Drilled: Bentonite mud, rotary, by Stewart Brothers Drilling Co. Drilled at 9 5/8-in. dia. to 100 ft, 6 1/4-in. to 650 ft.

Total Depth: 650 ft. Completed in Abo Formation.

Casing: 7-in. OD to 97 ft, cemented in place.
4 1/4-in. OD to 566 ft. (Installed by Shamrock Drilling Co. A string of tools was lost in the hole during casing.)

Completed: April 13, 1972.

Geologic Section: 0-380 ft: Bandelier tuff, light gray to gray, nonwelded to moderately welded, rhyolitic, containing crystals and crystal fragments of quartz and sanidine and rock fragments of pumice, latite, and rhyolite, in an ash matrix.
- 0-140 ft: tuff, gray, moderately welded.
- 140-230 ft: tuff, light gray, moderately welded.
- 230-380 ft: tuff, light gray, nonwelded, made up mostly of pumice.

380-440 ft: Tschicoma Formation, latite, dark gray, vugs in places, with some calcite crystals. Some thin lenses of light gray clay.

440-650 ft: Abo Formation, shale and silty sandstone, dark red, with some lenses and clots of white and gray to dark gray, arkosic.
- 440-560 ft: shale, dark red.
- 560-650 ft: silty sandstone, dark red.

Water Levels: 451 ft in 1972 and 453 ft in 1975. Water occurs in the fine sediments of the Abo Formation and the lower part of the Tschicoma Formation. The drilling contractor believes that the well would yield 2 to 5 gpm.

Water Quality: Potable, with 406 ppm dissolved solids.

Heat Flow: 5.50×10^{-6} cal/cm^2/sec.

Hole C

Location: SE1/4, SW1/4, SE1/4, Sec. 9, T20N, R3E. Just south of Road Canyon, about 2.7 miles northeast of hole B, and about 2.3 miles northwest of the ring fault. Elevation of land surface: 8700 ft.

Drilled: Bentonite mud, rotary, by Stewart Brothers Drilling Co. Drilled at 9 5/8-in. diameter to 100 ft, 6 1/4-in. to 750 ft.

Total Depth: 750 ft.

Casing: 7-in. OD to 97 ft, cemented in place.
4 1/2-in. OD to 750 ft. (Installed by Shamrock Drilling Co.)

Completed: April 16, 1972.

Geologic Section: 0-240 ft: Bandelier tuff, light tan and light gray to gray, nonwelded to moderately welded, rhyolitic, containing crystals and crystal fragments of quartz and sanidine and rock fragments of pumice, latite, and rhyolite, in an ash matrix.
0-30 ft: tuff, light gray, nonwelded, pumiceous.
30-80 ft: tuff, gray, moderately welded, dense.
80-190 ft: tuff, light gray, moderately welded, pumiceous.
190-240 ft: tuff, light gray, nonwelded, made up mostly of pumice fragments.
240-580 ft: Abiquiu tuff. Sandstone and conglomerate, light gray to gray, tuffaceous, containing sand grains of quartz, feldspar, and light- to dark-colored subrounded to rounded rock fragments, some of which are granite schist and quartzite, in a fine- to medium-grained matrix.
240-410 ft: sandstone, light grey, tuffaceous, friable.
410-480 ft: conglomerate, gray, made up mostly of pebble-size pumice.
480-580 ft: conglomerate, gray to dark gray, tuffaceous, containing rock fragments of pumice, granite, quartzite, schist, and unidentified dark igneous rock.
580-750 ft: Abo Formation. Shale and fine-grained sandstone, red to dark red, with some lenses of light gray and white, arkosic.
580-620 ft: shale, dark red.
620-680 ft: sandstone, fine-grained, alternating red and gray to white.
680-750 ft: shale, dark red.

Water Levels: 324 ft in 1972 and 316 ft in 1975. Aquifer is in the Abiquiu tuff which is quite permeable. Hole could possibly yield 3 to 5 gpm.

Water Quality: Potable, with 138 ppm dissolved solids.

Heat Flow: 5.88×10^{-6} cal/cm^2/sec.

Hole D

Location: SW1/4, NW1/4, SE1/4, Sec. 10, T19N, R2E. In Barley Canyon, about 1 mile northeast of Fenton Lake, about 2 1/2 miles west-northwest of Fenton Hill and about 4 miles west of the ring fault. Elevation of land surface: 7900 ft.

Drilled: Bentonite mud, rotary, by Stewart Brothers Drilling Co. Drilled at 9 5/8-in. diameter to 100 ft, 6 1/4-in. to 500 ft. Completed in Abo Formation.

Total Depth: 500 ft.

Casing: 7-in. OD to 97 ft, cemented in place.
4 1/2-in. OD to 500 ft. (installed by Shamrock Drilling Co.)

Completed: April 18, 1972.

Geologic Section: 0-120 ft: Bandelier tuff, gray, rhyolitic, moderately welded, containing crystals and crystal fragments of quartz and sanidine, mafic minerals, rock fragments of pumice, rhyolite, and latite, in gray ash matrix.

120-500 ft: Abo Formation. Shale, fine-grained sandstone and clay, ranging from tan to dark red, with stringers and lenses of white and gray.

120-190 ft: clay, dark gray.

190-200 ft: clay, tan, some mafic stains and light pinkish clots.

200-240 ft: shale, light red with gray clay lenses.

240-280 ft: shale, red, with dark gray to white lenses of shale.

280-320 ft: shale, red, with lenses of white to gray shale.

320-390 ft: shale, dark red, with lenses of white to gray shale.

390-430 ft: shale, dark red.

430-460 ft: sandstone, fine-grained, dark red with lenses of dark gray.

460-500 ft: shale, dark red.

Water Levels: 70 ft in 1972 and 61 ft in 1975. Aquifer is in the Abo Formation and the lower part of the Bandelier tuff. The hole would yield 2 to 5 gpm of water.

Water Quality: Potable, with 272 ppm dissolved solids.

Heat Flow: 2.20×10^{-6} cal/cm^2/sec.

3. DEEP EXPLORATION HOLE

GT-1

Location: NW1/4, Sec. 12, T19N, R2E. In Barley Canyon beside Forest Road 378, about 1.5 miles north of Fenton Hill and 2.5 miles west of the ring fault. Elevation of land surface: 8475 ft.

Drilled: Air-mist rotary, by Roberts Drilling Corporation. Spudded May 9, 1972.
Drilled at 13 3/4-in. diameter to 280 ft.
9 7/8-in. to 1600 ft.
6 3/4-in. to 2410 ft.
4-in. to 2430 ft. Completed at this depth June 1, 1972. Reentered later in June 1972 and drilled with water, diamond-core rotary:
4 1/4-in. to 2575 ft. Completed at this depth June 30, 1972.

Total Depth: 2575 ft. Bottomed in Precambrian amphibolite.

Casing: 10 3/4-in. OD to 258 ft.
7 5/8-in. to 1357 ft.
5-in. to 2400 ft.
Open hole, 2400 to 2575 ft.

Completed: June 30, 1972.

Geologic Section: 0-60 ft: Bandelier tuff: gray, moderately welded, rhyolitic; crystals and crystal fragments of quartz and sanidine; rock fragments of pumice, rhyolite and latite, in ash matrix.

60-160 ft: Abiquiu tuff: sandstone, light grey, with pebbly conglomerate containing rock fragments of pumice, latite, and rhyolite and unidentified rock fragments ranging from light gray and green to dark gray and black.

160-1070 ft: Abo formation: Shale and fine-grained sandstone, some clay lenses; predominantly red to dark red with some lenses of white to gray; arkosic, with a few beds of limestone.
- 160-290 ft: shale, dark red.
- 290-350 ft: sandstone, fine-grained, dark red.
- 350-680 ft,: sandstone, fine-grained, alternating with shale, dark red.
- 680-1030 ft: shale and sandstone, fine-grained, predominantly red, with lenses of white to gray shale and sandstone, a few thin beds of limestone.
- 1030-1070 ft: clay, dark red, with minor lenses of shale and sandstone.

1070-1815 ft: Magdalena Group. Upper member is Madera Limestone: limestone alternating with gray and red shales and sandstone. Arkosic.
- 1070-1250 ft: limestone, gray, alternating with sandstone, fine-grained, red.
- 1250-1330 ft: shale, red, with some thin lenses of limestone, gray.
- 1330-1440 ft: limestone, gray, with some lenses of sandstone, fine-grained, red, and shale, light red.
- 1440-1530 ft: shale, dark red, with lenses of limestone, dark gray.
- 1530-1670 ft: limestone, gray, with thin lenses of light red and gray sandstone, fine-grained.
- 1670-1815 ft: lower limestone member: limestone, dark gray, and thin lenses of white to gray shale and fine-grained sandstone.

1815-2105: Sandia Formation:
- 1815-2050 ft: upper clastic member: limestone, gray, with lenses of gray shale and fine-grained sandstone, ranging from light gray to light green.
- 2050-2105 ft: lower limestone member: limestone, dark gray, siliceous, dense.

2105-2575 ft: Precambrian rocks:
- 2105-2430 ft: augen gneiss, brownish gray, with inclusions of pink plagioclase.
- 2430-2480 ft: granite, reddish brown, medium-grained.
- 2480-2520 ft: gneiss, reddish brown, medium-grained, foliated.
- 2520-2575 ft: amphibolite, dark gray, fine-grained.

Water Levels: Approximately 320 ft in 1972 and 480 ft in 1973.

Water Quality: Potable, with 560 ppm dissolved solids.

Temperature Gradients: Overall, surface to bottom, average = 112°C/km
In Precambrian basement rock = 45°C/km

Heat Flow: 5.00×10^{-6} cal/cm^2/sec.

APPENDIX B

I. THE RAY REPORT

II. TESTIMONY BEFORE A U.S. SENATE SUBCOMMITTEE

III. TESTIMONY BEFORE A U.S. HOUSE OF REPRESENTATIVES SUBCOMMITTEE

1. The Ray Report

As was mentioned in section 18.4 of this history, in his energy message to Congress on June 29, 1973, President Nixon announced that $100 million in FY74 funds would be provided for energy R&D to accelerate certain existing projects and undertake new ones in a variety of critical areas (including "geothermal steam"). He directed AEC Chairman Ray to recommend to him by September 1 specific projects to which this $100 million should be allocated. He also announced that he was initiating a 5-year $10-billion program for R&D in the energy field, this to begin in FY75, and directed Dr. Ray to undertake an immediate review of federal and private energy R&D efforts; to recommend an integrated R&D program for the nation; and to report to him by December 1 both her recommendations for energy R&D programs that should be included in the President's FY75 budget and her recommended 5-year federal R&D energy program.

Anticipating at least the $100 million, Jerry Johnson organized an informal "ad hoc committee for allocation of 1974, supplementary funds." It was to be composed of representatives from the National Laboratories and AEC headquarters. On June 6, 1973, Jim Bresee called from AEC Headquarters to inform me that I would probably be asked to serve as the geothermal member of that committee, which would meet at AEC headquarters (then in Germantown, Maryland) from July 9 through July 18. I told him that I already had one other firm commitment during that period—to give a talk at the Aerospace Corporation in El Segundo, California. He felt that we could work around that and, in a letter dated June 26 to Harold Agnew, Jerry Johnson asked for my help during that period, at AEC headquarters. On July 2, Harold wrote back with his permission for me to do so, and on the same day Jim Bresee's secretary called me to confirm the request and the dates. So I travelled to Germantown as requested, on July 8.

At the first meeting of the committee on the morning of July 9, Jerry Johnson asked us—by noon on July 12—to prepare plans to spend the $100 million of supplementary funds proposed for energy R&D in FY74. To avoid creating unnecessary excitement, we were instructed not to call up any potential contractors or any other agencies outside the AEC with regard to their programs or plans. We were on our own, with very little time, so where there was a gap in our information we simply made something up to fill it. The end result was correspondingly spotty.

I was responsible for the geothermal energy projections and was too busy with them to learn much about what happened in other energy areas. With regard to LASL, I felt that we had already asked for about as much money as we could spend intelligently on HDR in FY74, so I left that at $3.0 million. However, I did propose additional DAT funding to support the more programmatic aspects of our geophysics and geochemistry projects: $0.3 million for "Geophysical Research" and $0.2 million for "Geochemistry of Hot Rock Systems." I also proposed additional funding for geothermal studies at other national laboratories (BNW, LBL, LLL, and ORNL) and for three commercial organizations (Aerojet Nuclear Co., Sperry Rand, and Magma Power, whose geothermal interests had been discussed at various meetings of the Ogle Committee). The total FY74 funding recommended for all of these programs was $11.2 million, compared to the $4.7 million that had been budgeted for them by AEC to that time.

As agreed with Jim Bresee, I took July 11 off from committee work at headquarters, and it was a very long day. It included driving a rental car from Germantown to Dulles Airport; taking a very early fligh to Los Angeles, driving another rental car down to El Segundo; giving a colloquium at the Aerospace Corporation titled "The Outlook for Geothermal Energy" and discussing that and some carbon and graphite studies with Aerospace personnel; driving back to Los Angeles; flying back to Washington; and driving back out to Germantown.

While I was fairly weary on the morning of the 12th, I was sufficiently alert to discover that, in my absence, Bill Ogle and Don Stewart had come by and made some significant changes in my geothermal-energy projections. In particular, they had reprogrammed the preliminary FY74 geothermal budget, very much to the benefit of BNW and largely at the expense of LBL. I promptly restored it to its original condition. Jim Bresee later approved my having done so, although of course Bill and Don emphatically did not. (It took me quite a while to restore good working relations with the two of them.)

On July 13, Chairman Ray discussed our FY74 plan with OMB, who were reported to have received it favorably.

In the meantime, an Energy Research and Development Task Force had also been formed by the AEC to define specific R&D projects and funding to meet the goals of the 5-year $10 billion program proposed by the president. In addition to high-level people from the AEC and the national laboratories, it included representatives of the Office of Coal Research (OCR) and the Electric Power Research Institute (EPRI). LASL's representatives on the task force were Harold Agnew, Dick Taschek, and Ed Hammel. The first meeting of the task force was at AEC Headquarters on July 12 and 13, 1973. Harold, Dick, and Ed were all there on the first day of that meeting, but only Ed and I on the second day.

On July 12, presentations to the task force were made by Dixie Lee Ray, Dick Balzhiser (then at EPRI) and George Hill of OCR. (I missed these because our ad hoc committee was still finishing our initial report on spending the $100 million in FY74.) On July 13, John Cowles of DAT outlined our proposals for that $100 million. Among other things they requested $12.4 million for "*In Situ* Processing" and $44.6 million for "Energy Technology." The latter included $3 million for our HDR Program and $4 million each for basic research and environmental studies.

Jerry Johnson then outlined the organizational structure, working arrangements, and personnel of the task force required to define the specific R&D projects and funding needed to meet the goals set in the President's energy message. The task force was not to concern itself with the $100-million FY74 program outlined by our committee and discussed by John Cowles, which was to be accepted as the basis for FY75 planning. The task force was, however, to define the R&D program and budget for FY75. Jerry appointed George Hill, the director of OCR, to be Chairman of a Coal and Shale Subcommittee; Ed Fleming from the AEC Plowshare Program, to chair an *In Situ* Technology Subcommittee; and Jim Bresee from DAT to be Chairman of an Advanced Technology Subcommittee. (Ed Hammel was to serve on the Advanced Technology Subcommittee and Lee Aamodt on the *In Situ* Technology Subcommittee.) Jerry set July 31 as the date for the subcommittee chairmen to review their work plans with him and October 15 for the finished draft of a report for Chairman Ray to submit to the president on December 1.

With adjournment of the Energy R&D Task Force meeting, our ad hoc committee went back to work. Our next job was to produce a draft of proposed projects and budgets for the task force subcommittees to work on. Jerry instructed us, by noon on July 16, to prepare nonnuclear energy program plans for FY75 assuming (Plan A) that the $100-million add-on for FY74 did not materialize, and (Plan B) that it did. We proceeded to do so, as frantically and imaginatively as before. On July 17, Jerry discussed our FY75 plan with OMB, and again it reportedly was well received.

Jerry had also asked us, by the afternoon of July 18, to prepare 5-year program plans (FY75-FY79) as a first step in planning the president's proposed 5-year $10-billion energy program. Again these were to be based on both the Plan A assumption of no big funding add-on in FY74 and the Plan B assumption of a $100-million add-on in FY74. This was to be broken down so far as possible by task and contractor and also by the operational equipment, and construction funds required. This of course was a particularly imaginative exercise, but we did our best, gave Jerry the product, and headed for home.

Upon my return to Los Alamos, and reflecting my optimism about greatly increased funding for energy-related LASL projects, our HDR group began writing new proposals for DAT support of the more programmatic aspects of our geophysics programs and for increased funding for the relatively fundamental work supported by DPR. I also called Lamar Johnson in Group H-8, suggesting an update and expansion of an H-8 proposal to DBER (the AEC division that supported environmental studies) for an environmental study of Fenton Hill. He and Harry Jordan then prepared and submitted such a proposal, "Ecological Investigation of the Development of 'Dry' Geothermal Energy Sources at Los Alamos." Lee Aamodt was also working on his georesources development proposals.

In the preliminary plan for geothermal R&D in FY74 that I had prepared at AEC Headquarters, I had included funding of $0.2 million for a relatively programmatic study called "Geochemistry of Hot Rock Systems," to be funded by DAT and to supplement the more fundamental work supported by DPR. On July 26 I called John Balagna and suggested that he write a proposal for that $0.2 million, with him as principle investigator and me as the responsible person. John checked the idea out with his group leader, James E. ("Jim") Sattizahn, who approved it, and with his division leader, George Cowan, who said that was fine, except that it should be kept entirely in CNC division. John told me that George intended to talk to Bob Duffield, our Q division leader, about it on the following Monday. I intended to talk to Duffield first, but I didn't get the chance.

On July 27, John came to my office to discuss the proposal with me. While he was there, Harold Agnew called me. He had run into George Cowan the day before, and George had become "very emotional" about me "managing" a program in his CNC Division. Harold said that he had soothed George a little, and asked me to soothe him some more. I talked it over with John, and then called George. I believe that I did soothe him some, but we did not really reach a meeting of the minds. Then I called Bob Duffield, who had already heard from George. Bob thought it would be alright to let CNC handle the program, but with a "steering committee"— chaired by me—to insure that programmatic needs were met. He asked me to write a memo to Cowan on the subject, which I did. I distributed it on July 31, with copies to Agnew, Taschek, Duffield, Hammel, and Don Brown. In it I explained that our present arrangement with the DPR-supported geochemistry research (for which Cowan was now the person in charge, although I had written the original proposals for it) was working fine, primarily because Balagna did a fine job and he and I worked well together. With regard to the proposed new, more programmatic, geochemistry program, I simply wanted Q-22 to be guaranteed a fairly strong input. With regard to a steering committee, I remarked that "I wince at the thought of another committee, and think that it might infringe more on CNC prerogatives than I would by myself." However, I agreed to accept whatever George and Bob Duffield agreed upon with regard to the proposal and subsequent management of the program.

Harold Agnew was remarkable in that he actually read and promptly responded to such trivia as my memo to George. On August 1, he called me to say that he had read my memo, thought it was fine, and that "It should get Cowan down off his high horse." George Cowan also responded, in a memo dated August 2. He said that he did not understand the implications of the organizational arrangement that I seemed to favor, and that he favored a working-group arrangement (which, in my turn, I didn't understand) with me as chairman.

In any case. John Balagna came by on August 3 with a well-prepared Form 189a proposal for "Geochemical Diagnostic Support of Dry Geothermal Source." It showed John as principal investigator with Jim Sattizahn as the person in charge. That was fine with me, and the proposal was later submitted to DAT.

As a member of the *In Situ* Technology Subcommittee, Lee Aamodt had spent July 26 and 27 at AEC headquarters working on the 1975-1979 program plans. With Don Stewart of BNW and Ken Mirk of LBL, he concentrated primarily on the geothermal-energy budgets. No major changes were made in what our ad hoc committee had proposed. (Lee was called back to headquarters for another subcommittee meeting on August 14 and 15, but at that one there was no further consideration of geothermal projects.)

In the meantime, on July 31, 1973, Chairman

Ray circulated a memorandum to federal agencies with energy-related interests that requested proposals for significant R&D efforts that they could accomplish in FY74 if a portion of the anticipated $100 million were allocated to them. The proposals were to be submitted by August 15 in a new format that came to be called, "the Pastore form"—after Richard Pastore, Dr. Ray's staff director for this exercise. A message from Jerry Johnson requesting Pastore-form proposals for FY74 work was received at LASL at 11:30 a.m. on August 6, and Francis West, Bob Potter, and I promptly began putting our DAT proposals in that form. It wasn't easy. The form included some essay questions, which were no problem, plus a three-page matrix, most of which asked for information that had no relevance to what we were proposing to do. Our initial effort therefore left many blanks in the matrix and a lot of spaces marked "0" or "NA."

On August 9, 1973, Harold Agnew Telexed to John Teem a list of proposed LASL projects that were being prepared for submission to DPR for FY74 funding. Those related to geothermal energy were the following:

- From John Balagna: "The Application of Sophisticated Physico-Chemical Methods to the Characterization of Molecular Species and Concentrations in Hydrothermal Systems at High Temperature and Pressure; The Construction of Theoretical Models to Describe Experimental Behavior of the System."
- From Ken Olsen, Orson Anderson, and Bob Potter: Increased funding for "Seismic Studies Related to Artificial Geothermal Energy Sources."
- From Don Brown, Bill Sibbitt, Bob Potter, and Orson Anderson: Increased funding for "Heat-Flow Study of a Potential Geothermal Energy Source."
- From John Rowley and Bob Potter: Increased funding for "Fracture Dynamics of Hydraulic Fracturing and Thermal Stress Cracking in Crystalline Rocks."

John Balagna subsequently prepared another proposal to DPR for a study of "Chemical Equilibrium and Materials Transport in Systems Involving Pressurized Superheated Water and Silicate Minerals." At the same time, we were preparing two proposals to go to DAT:

- From Mort Smith: "Dry Geothermal Source Demonstration." (This was an update of our proposal for the Fenton Hill experiments.)
- From Francis West and Bert Dennis: "Geophysical Research and Development." (This was concerned primarily with the development, testing, and field application of improved instrumentation and techniques for downhole investigation and evaluation in the high-temperature, high-pressure environment of a geothermal well.)

On August 10, Jerry Johnson forwarded another memo from Chairman Ray requesting Pastore forms for energy R&D projects to be considered for inclusion in the president's FY75 budget—the first increment of the $10-billion, 5-year effort. This memo was circulated not only to federal departments and agencies, but also to representatives of many scientific and technical societies, independent research organizations, and industrial associations. It asked both for proposals for appropriate projects to be funded in FY75 (these due by September 10) and (by September 1) for names and qualifications of individuals qualified to serve on program-review panels who could be available to do so for three or four weeks during September and October.

Several of the task force subcommittees met at AEC Headquarters on August 14 and 15. Ed Hammel attended as a member of the Advanced Technology Subcommittee, Lee Aamodt as a member of the *In Situ* Technology Subcommittee, and I was invited to assist the Advanced Technology Subcommittee as a member of what amounted to a general Energy Subcommittee. In addition to headquarters personnel, there were eight of us on the sub-subcommittee from the various national laboratories: two each from Brookhaven and Oak Ridge, and one apiece from Livermore, Los Alamos, Savannah River, and Battelle Northwest. However, only Gary Higgins, from Livermore, and I had any real background in geothermal energy and Gary was involved principally with a fossil fuels sub-subcommittee. Nevertheless, among us we put together a report on geothermal-energy proposals for FY74 funding that amounted to a collection of Pastore forms titled "Extraction, Conversion, and Utilization of Geothermal Energy." This was assembled and edited at AEC headquarters and on September 17, mailed back to each of us for corrections and comments. The principal change with regard to geothermal energy was that the FY74 add-on for it was increased from $73.5 million to $77.5 million, with no change in the proposals for LASL projects.

In the meantime, in response to Dr. Ray's most recent request, on August 24 Rod Spence asked me to get all of our AEC proposals for FY75 funding into the Pastore format by September 1, and Bob Potter started doing so. However, when I called Dick Taschek's office the next Monday, August 27, at 10:30 a.m. to find out what was going on, his secretary informed me that all of those proposals had to be in her office within the next 10 minutes for Bill Kirk to hand carry them to Washington. That, of course, was impossible. However, we photocopied what we had—much of which was still hand written and incomplete—and I delivered the package to Bill at the Los Alamos Airstrip at 11:05 (10 minutes before the departure of his plane). Fortunately, Lee Aamodt was in Washington on August 30 helping to prepare the final report of the *In Situ* Technology Subcommittee. He completed the Pastore forms for our HDR proposals on that day. Ed Hammel and I were both in Washington on September 17 to 19, and together we reworked our DAT proposals. On September 24, Lou Werner called me from headquarters to say that our FY75 Pastore-form proposals had all been approved by the AEC.

At Lou's request, I returned to AEC headquarters on October 23 and 24 to do some writing and editing for the Subpanel on Geothermal Energy of the Energy R&D Task Force. I reviewed a group of subprogram write-ups prepared by subpanel members; rewrote one that Jerry Johnson felt was weak; read the written comments of the consultants whom the panel had called in; added what seemed useful from these to a 15-page "Overview" that Jim Bresee had prepared; and finally condensed the overview to 5 pages. Although they were to be accompanied by about 40 pages of appendices, I was told that it was those 5 pages that the review panel would actually review, that Chairman Ray would take to the White House, and that the president's staff would read. That was the end of my direct involvement in the Ray Report.

As she had been directed to do, Chairman Ray delivered her report to President Nixon on December 1, 1973. It was titled "The Nation's Energy Future" and offered a very ambitious 5-year program of energy R&D during the years FY75 through FY79. For geothermal energy, it recommended a total of $185 million, of which $85 million was for "short-term objectives" and $100 million for "midterm objectives." (Agency projections for FY75-79, prepared before the president delivered his Energy Message, had been only $20 million total.)

Separately listed for geothermal energy were $3.8 million ("actual") for FY73; $11.1 million ("planned") for FY74; and $40.0 million ("recommended") for FY75, of which the $40.0 million was part of the $185 million recommended for geothermal energy during the years FY75-79.

Except for the additional $100 million to be distributed for energy R&D in FY74, all of this had little immediate impact on the nation's energy programs, but it did appear to represent long-term federal support for energy R&D programs such as ours.

2. Testimony before a U.S. Senate Subcommittee

In a telephone call from Washington on May 2, 1973, Jim Bresee warned me that I would be asked to testify before the Water and Power Resources Subcommittee of the Senate Committee on Interior and Insular Affairs, chaired by Senator Frank Church of Idaho. This would be in Washington, probably on June 13. It would be the first presentation to a congressional body on the concept and was obviously important. Unfortunately, I knew nothing about how such committees operated and was a little frightened by the prospect. However, I began to prepare some written testimony for the meeting.

On May 19, Dick Taschek told me informally and with no details that I would be asked to testify before Senator Church's committee in Washington, sometime in June. Three days later, on May 22, I happened to run into Raemer Schreiber, who told me that Harold Agnew had received a letter from Jerry Johnson concerning my proposed testimony, and that Dick Taschek had the letter. So I called Dick for details. It turned out that Jerry had outlined the subject matter that he wanted me to emphasize—which differed considerably from what I had already written—and asked for a copy of my written testimony by May 29. I rewrote the thing and mailed it to Jerry on May 29.

There were, of course, several discussions of my written testimony with LASL administrators, and, by telephone, with Glen Graves at NSF, Jim Bresee at AEC headquarters, and Gary Higgins at Lawrence Livermore Laboratory (who was also going to testify). Then I went to Washington and on June 12 discussed my testimony with Jerry Johnson, Jim Bresee, and Jack Vanderryn.

The subcommittee hearing on "The Production of Power from Geothermal Resources," was held in the Dirksen Office Building on June 13. Bob Duffield was there as an observer. I was introduced by Jerry Johnson and presented my oral testimony—pretty nervously at first. However, I was soon put at ease by Senator Church and the other subcommittee members, who seemed really to be interested in the HDR concept and asked a great many good questions about it. I was very pleased by their reactions and comments, which in general were very favorable and appeared to represent strong support for federal funding of our HDR program.

A transcript of my oral testimony and the discussions of it, and also my more formal written testimony, appear in a volume on "Geothermal Resources" published in 1973 by the U.S. Government Printing Office (Smith, M.C., 1973a).

On August 10, 1973, I attended another meeting of the same subcommittee in Idaho Falls, Idaho, which was concerned largely with the geothermal potential of Idaho. I was not asked to testify but was pleased that the chairman, Senator Church, remembered something of my testimony at the earlier meeting in Washington, and commented briefly on the energy content of hot dry rock and its potential contribution to the energy needs of the western United States.

3. Testimony before a U.S. House of Representatives Subcommittee

On August 1, 1973, Tom Ratchford telephoned from Washington inviting me to testify concerning our HDR Program at a hearing on geothermal energy to be held in Washington on September 18 by the Subcommittee on Energy of the Committee on Science and Astronautics of the U.S. House of Representatives. (Tom was a scientific advisor to that subcommittee.) Having survived testifying before a Senate subcommittee, of course I agreed. Tom said that a formal invitation to testify would come to me through Harold Agnew, and it did—promptly, and directly from Harold.

Of course I immediately began preparing a written version of my testimony and attempted on August 27 to circulate a draft of it within LASL for comment. It got only as far as Dick Taschek, who objected to one paragraph. I rewrote that, circulated it again, got LASL approval of it, sent a copy to AEC headquarters, and on September 10 received the approval of Lou Werner at DAT to use it. The next day I sent 25 copies of it to Tom Ratchford to be previewed by the members of the subcommittee. Then it was back to Washington to testify.

On September 18 at the Rayburn House Office Building, I appeared before the Subcommittee on Energy. It was chaired by Representative Mike McCormack, whom we had previously briefed in

Los Alamos—which helped—and I was much less nervous this time. Again, the subcommittee members seemed really to be interested in HDR and also in the Subterrene—which I had mentioned in my testimony. There were a lot of good questions and favorable comments, and Chairman McCormack offered the support of the subcommittee if additional funding were needed to accelerate development of HDR energy systems. I was asked to supply additional written information for the meeting record, which I mailed in on October 10. It included an article by Don Brown called, "The Potential for Hot-Dry-Rock Geothermal Energy in the Western United States," which he had previously prepared for Representative Manuel Lujan (Brown, D. W., 1973) as well as answers to a long list of questions submitted by the committee members. My testimony, the discussions of it, and this supplementary material, all appear in the published report of the hearings (Smith, M. C., 1973b).

My testimony before the subcommittee was followed by that of Bob Rex, who had kind words to say concerning our HDR Program, and then by that of William R. ("Bill") McSpadden of BNW, who reported on the Marysville, Montana project funded by NSF and expressed the hope that eventually we would be working with them. (I testified again before this subcommittee in Washington on February 11, 1974, when our HDR Program was considerably more advanced.)

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