The roots of solid-state research at Bell Labs

The impact of science on industry—and of industry on science—
is nowhere better illustrated than by the origins of the solid-state group
at Bell Laboratories, which gave the world the transistor.

Lillian Hartmann Hoddeson

Solid-state physics has experienced a dramatic growth in the last four decades; whereas in the 1920's the term "solid-state physics" was not yet in use, this is now the single most populated sub-field of physics. Much of this growth has taken place in industry, so that today a small number of industrial laboratories are producing a substantial fraction of contributions in the field.

In this article I explore the roots and beginnings of basic solid-state research in one industrial setting, Bell Laboratories, where crucial advances were made, such as those leading to modern semiconductor electronics. By focussing on these developments we may hope to gain insight into the mechanisms of the contemporary impact of basic physics on industry, as well as into the complementary role that industrial policies have in turn played in shaping specific areas of modern research.

The roots of Bell's solid-state program developed gradually, in a series of stages generated by internal technological needs of the expanding telephone industry. The stages show a striking reciprocal interplay between science and technology in the context of corporate expansion. Let us examine four stages:

1875–1906 A newly invented device establishes an industry.

1907–24 Technological needs called for by the growth of the industry lead to in-house research.

1925–35 Interactions with scientific research outside the Laboratories help focus some of its basic technical studies on even more fundamental scientific issues.

1936–45 The intensified focus on scientific underpinnings of technological problems leads to proliferating scientific and technological developments, among them the formation of the famous solid-state group that in 1947 would demonstrate the first transistor.

Let us recapitulate these stages in more detail, starting at the time of the invention of the telephone.

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Establishing a telephone industry

In 1876 Alexander Graham Bell received a patent for his method of transmitting sounds by electrical undulation, and in 1877 he patented his "magneto-telephone," a device that could actually transmit speech. The telephone industry began several months later when the first telephones were leased to subscribers.

The manufacture, installation and maintenance of telephones in the growing business raised new technological problems.[1] However, the earliest of these did not require scientific training or fundamental research and, understandably enough, the infant company supported neither scientific education nor research. Not even Bell's "mechanical assistant," Thomas Watson, had any formal scientific training. When the technical staff expanded during the next few years, Watson was joined by inventors, not scientists.

To be sure, many telephone problems of the 1880's and 1890's—attenuation and distortion of telephone signals, crosstalk, switching, interference from other electrical devices such as street lighting or electric railways—were caused by electromagnetic phenomena that were just receiving scientific explanation. James Clerk Maxwell's *Treatise on Electricity and Magnetism* had only recently been published (in 1873), and it had limited experimental support. (Heinrich Hertz's experimental confirmation of electromagnetic waves came in 1888.)

The first decisive step towards in-house research occurred in 1885 when Hammond V. Hayes, the first PhD in the Bell System (and holder of the second physics doctorate awarded by Harvard) became chief of the technical staff. But the engineers on Hayes's small staff in the 1880's were not trained in mathematics and could not readily apply electromagnetic theory to the engineering problems they encountered. Approaching immediate practical problems by the cut-and-try approach seemed more promising than taking staff time out to comprehend and develop the scientific underpinnings.

Yet even before the turn of the century, Hayes had hired a handful of university-trained scientists to work on technical problems. In 1890 he recruited John Stone Stone, trained at Johns Hopkins University in advanced mathematical theory, to work on sound transmission; in 1896, George Campbell, an MIT-trained physicist with five years of postdoctoral study, to clarify the role of the inductive impedance in telephone communications, and in 1899, Edwin Colpitts from Harvard, to study alternating electrical currents and inductive interference due to electric trolley cars and power-transmission systems.

Frank Baldwin Jewett (later to become the first president of Bell Labs) was hired in 1904 to work under Campbell as a transmission engineer. Jewett, who was the first member of the technical staff to have some close experience with the atomic physics then being developed, was teaching physics and electrical engineering at MIT at the time he was hired. While in the doctoral program at the University of Chicago, Jewett had been a research assistant to A. A. Michelson and a close friend of Robert Millikan. The latter, then a young physics instructor, exposed Jewett to the new discoveries being made in electron physics. Jewett's association with Millikan would soon contribute crucially to the beginnings of basic research within the Bell System.

Thus by 1907 several trained scientists were working in the company, but primarily as engineers, not as part of an organized basic-research program.

Spanning the continent

In 1907 Theodore Vail, who had left the company in a dispute twenty years earlier, was rehired as President. Two decisions Vail made then had major impact on the movement towards establishment of basic scientific research.

First, he brought together all technical workers into a single department. The new engineering department—which ultimately evolved into the Bell Telephone Laboratories—was established at 463 West Street in New York City as a division of Bell's manufacturing arm, Western Electric. Hayes retired and Vail appointed John J. Carty to head the new department. Carty, who at first sight seems a throwback to an earlier era—he had joined the company in 1879 as a boy operator, and had no formal scientific training—actually proved to be closer to the new style. He was a research enthusiast who had by then made an impressive series of technical contributions to the art of telephony, including application of the two-wire metallic circuit, the first multiple switchboard, the bridging bell and the repeating-coil phantom circuit.

Vail's second decision was to build a transcontinental telephone line from New York to San Francisco, in time for the 1914 Panama–Pacific Exposition. It was soon recognized, however, that no such line could be achieved unless a "repeater"—a device that could amplify telephone signals attenuated by distances—could be developed. But to design a usable amplifier for coast-to-coast service would require a detailed understanding of the new electron physics, a subject beyond the working knowledge of anyone then in the company.

HAMMOND V. HAYES, 1907

Attenuation had become a progressively more obtrusive problem as the company's lines lengthened—from approximately two miles between Boston and Cambridge in 1876 to 900 miles between New York and Chicago in 1892, and then to 2100 miles between New York and Denver in 1911. As early as 1899, Campbell had developed a "loading coil," which cut energy losses dramatically by increasing the inductive impedance of the lines; the New York to Denver line could not have been built without it. (Michael Pupin, at Columbia University, also invented the loading coil at this time and won the patent fight against Campbell. The company, however, then bought Pupin's patent, and Campbell went on further to develop the loading coil for telephone application.) But to go farther than Denver it would be necessary to add an amplifier to the system.

A mechanical amplifier designed by Herbert Shreeve, based on a vibrating diaphragm, had been tested as early as 1904. The amplified signal was similar to the original one but it was typically quite badly distorted; Shreeve's repeater tended to favor some frequencies and discriminate against others. When used on lines with loading coils, the signal was all but destroyed.

Something less sluggish than a vibrating diaphragm was needed, such as electrified gas particles, or free electrons. The development of this idea required knowledge of the most recent electron physics. Therefore in 1910 Jewett discussed the problem with his graduate-school friend Millikan, who later recalled[2] that Jewett asked him to recommend "one or two, or even three, of the best young men who are taking their doctorates with you and are intimately familiar with your field. Let us take them into our laboratory in New York and assign to them the sole task of developing the telephone repeater." Millikan recommended his best graduate student, Harold Arnold, who in January 1911 joined Western Electric's engineering department.

The first research branch

Three months later the Bell System established its first research branch as a division of this department. Headed by Colpitts, the new group had as its specific directive to produce "the highest grade research laboratory work." Jewett was given responsibility for directing research on the most immediate problem, the repeater.

A pattern was developing that would deepen throughout the following five decades: The Bell System would support increasing programs of basic research within an expanding engineering effort. In-house research directly pertinent to communications needs would circumvent the necessity of buying patents from other institutions or individuals or, as in the case of the radio research, would protect existing Bell patents.

The trend towards more fundamental studies was reinforced by what was apparently a new, if unwritten, policy: The directors of research were chosen from among the scientists who were trained in the Bell System's own laboratories. Such men understood that creative scientists need freedom to speculate and explore intellectually and to communicate with researchers working on similar problems—even if these were employed outside the company. In short, the scientists required latitude comparable to that available in academic laboratories. Ac-

THEODORE N. VAIL, 1915

GEN. JOHN J. CARTY, WORLD WAR I

tive competition in the larger scientific community would also be recognized in time as the most effective means for Bell to achieve the awareness of scientific frontiers it deemed necessary to maintain its market advantage.

The solution to the amplifier problem began with the triode offered in 1912 to American Telephone and Telegraph by Lee De Forest. The triode could amplify weak signals; however, due to the relatively large telephone currents required, the gas inside the tube would ionize. As John Mills recalled, "[the tube] would fill with blue haze, seem to choke, and then transmit no further speech until the incoming current had been greatly reduced."[3] That problem was eventually solved by Arnold's development of a high-vacuum version of De Forest's triode.

The first transcontinental line opened in time for the Exposition; in January 1915 Alexander Graham Bell in New York reissued his famous command to his former assistant in San Francisco: "Mr Watson—come here—I want you." To this Watson replied, "It would take a week to get there."

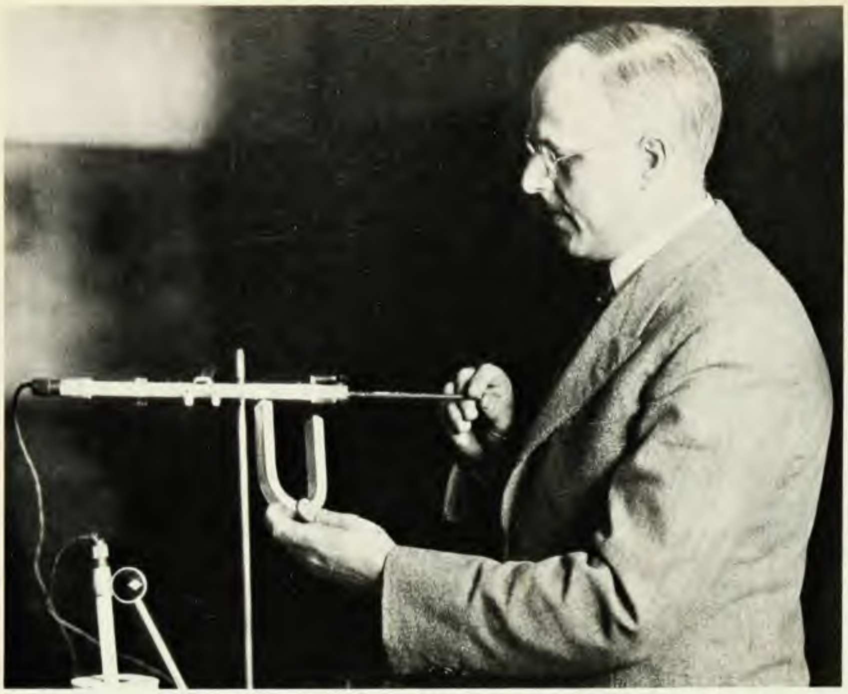
Basic research takes root

In the third stage (1925–35) basic research took firm root in company policy. In 1925 a new corporation headed by Jewett—the Bell Telephone Laboratories—took over Western Electric's engineering department. But the organizational changes of 1925 did not alter the new research policy.

Basic research continued to expand and diversify in the Bell System. The trend may be illustrated by the work of the vacuum-tube department, an organization that originally had evolved out of the earlier research on repeaters. By 1930 this department was staffed by almost 200 scientists and co-workers organized in subgroups focussing on specialized aspects of vacuum-tube phenomena. These included thermionic emission and the interaction of electrons with solids. Examples of fundamental research that grew out of such investigations during the later 1920's are the well known studies on thermionic noise by J. B. Johnson and Harry Nyquist, Harold Black's important study of negative feedback, and the famous experiments by Clinton Davisson and Lester Germer that provided experimental verification of the wave behavior of electrons. (It is of interest that Davisson and Germer were not initially aware of the relation their experiments had to quantum mechanics. Instead these experiments were in part an outgrowth of Arnold's desire to understand fully the issues raised in his patent fight with Irving Langmuir over the development of the high-vacuum tube.[4,5]) Among other examples of fundamental research in this period was that carried out by Richard Bozorth on magnetic materials.

In the following decade interactions increased between researchers at the Laboratories and those in universities both here and abroad. The new quantum physics entered Bell Laboratories re-

JOSEPH A. BECKER AND J. A. COLBECK, 1927



HAROLD D. ARNOLD, 1931

search and contributed towards still more intensive focus on fundamental questions. The quantum theory of solids, developed between 1926 and 1933 by Wolfgang Pauli, Werner Heisenberg, Arnold Sommerfeld, Felix Bloch and others would create a context for Bell's innovations of the subsequent decades in solid-state physics.

The quantum theory of solids was soon recognized as relevant to technical studies at Bell such as thermionic emission, photoelectricity and conduction. Walter Brattain and Joseph Becker, for example, drew upon the classic work of Arnold Sommerfeld and Lothar Nordheim in 1928 on the electron theory of metals to compute thermionic emission formulas.[6]

New ideas

The quantum theory entered through a number of avenues, some of them uncommon for an industrial laboratory of that period. One of these was Bell's lively colloquium series, organized in 1919 "to review scientific progress by means of contributed papers and general discussions of current scientific literature." In the early years, most of the talks were given by Bell Labs scientists; during the 1920's, however, researchers from all over the world spoke there on recent advances in physics and chemistry. Prominent European visitors during the period included Sommerfeld, from Munich, who spoke in 1923 on "Atomic Structure" and in 1929 on "The Photoelectric Effect in a Single Atom and in a Metal;" Ernest Rutherford, from the Cavendish Laboratory, who in 1924 spoke on "Recent Researches Concerning Atomic Nuclei;" Erwin Schrödinger, from Zurich and Berlin, who in 1927 spoke on "The Undulatory Theory of the Electron;" Eugene Wigner, of Berlin and Princeton, who in 1932 discussed "Applications of Quantum Mechanics to Chemistry," and Paul Ewald, from Stuttgart, who in 1936 spoke on "Crystal Growth and Crystal Perfection."

Distinguished American scientists from other institutions who delivered colloquia at Bell in the same period included Robert Millikan (Cal Tech) in 1925, Robert Mulliken (New York University) in 1927, Edward Condon (Princeton) in 1928, Harold Urey (Columbia University) in 1932, I. I. Rabi (Columbia) in 1933 and John Van Vleck (Harvard) in 1936. In December 1933, there was a symposium on the recently discovered "Positive Electron." Speakers included Bell's Karl K. Darrow, who gave an historical review, and Gregory Breit, then at NYU, who presented P. A. M. Dirac's theory of holes; Rabi led the discussion.

Much of the impetus behind Bell's colloquium came from Darrow, who had been on Bell's staff since 1917. Particularly during the summer months, Darrow would visit major European and American research centers and attend physical-society meetings. Scientists often would accept Darrow's invitation to visit Bell Labs and give colloquia there. During the period, Darrow also helped transmit new ideas in physics by writing a semipopular series, "Some Advances in Contemporary Physics," for the *Bell System Technical Journal.* The topics included "Waves and Quanta" (1925); "The Atom-Model" (1925); "Statistical Theories of Matter, Radiation and Electricity" (1929), and "The Nucleus" (1933). The series was widely read and often evoked strong response; Brattain, for example, claims his awareness of Bell Labs was stimulated by Darrow's articles written, as Brattain put it, "in his gorgeous language."[7]

Individual study and self-education provided another path of entry for new ideas at Bell, aided ironically by the Depression, which caused a reduction in 1932 of the work week for Bell's staff from 5½ to 4 days. (In 1934 the staff went back to a 4½-day-week and in 1936 to five days.) In a number of cases the extra time was devoted to individual study of quantum physics or to course work at Columbia and elsewhere. Some study efforts were disseminated more widely in the Laboratories; Brattain, on his return to Bell after attending Sommerfeld's lectures on the electron theory of metals at the 1931 Michigan Summer Symposium, gave a series of informal lectures on that theory.

By this time other industrial firms were also making strides in the application of research at their institutions. The extent to which leaders of research saw such activity as a common enterprise is illustrated by the joint monthly luncheon meetings of some twenty industrial laboratory leaders including Charles Kettering of General Motors, Kenneth Mees of Eastman Kodak, Willis Whitney of General Electric and Jewett of Bell. They discussed shared problems and issues, such as organization, personnel, patents and the relation of industrial research to economic conditions. Sometimes the group of "directors of industrial research," as they called themselves, would visit each others' laboratories. A tradition of individual visits to other research laboratories also evolved in this period.

Industrial researchers were frequently included in programs of the academic community. For example, during the late 1920's and early 1930's, MIT ran a colloquium series within their electrical-engineering department in which members of various manufacturing, operating and engineering companies, including Bell Laboratories, were invited to lecture on how fundamental science could be applied to engineering problems. In 1928 Bell's Mervin Kelly spoke in this series on "Thermionic Filaments of Vacuum Tubes used in Wire Telephony;" in 1936, Bozorth reported on "Recent Research in Magnetic Alloys." By the mid-1930's the problems, approaches and atmospheres of fundamental research at Bell Labs were remarkably similar to those in university laboratories.

Establishing solid-state research

The fourth stage, the establishment of basic research in solid-state physics culminating in the development of the transistor, began in 1936, when Kelly was appointed director of research. Kelly, like Arnold and Jewett before him, had taken his doctorate in physics at Chicago

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(where he had worked with Millikan on the oil-drop experiment), and had for a period (1928–34) led the vacuum-tube department. Kelly had become very much aware of the potential value of an amplifier without vacuum tubes—which were large, expensive, fragile, slow, relatively noisy, and often unreliable and short-lived. He is said to have manifested an interest in the early 1930's in developing an amplifier based on the properties of solid materials.

Some researchers on Bell's staff were already exploring the amplification properties of semiconductors. For example, Becker and Brattain were studying the properties of copper oxide but they did not fully understand the physical basis for their observations. Raymond Sears, who worked closely with Becker during the 1930's, recalls:[8]

> "Becker all along felt that there was something in a copper-oxide rectifier that ought to have an analogy to the vacuum tube. There was a nonlinearity of the conduction in the forward and in the reverse direction. And so Joe himself would try to imbed a wire mesh in the oxide layer of copper oxide, in order to almost try to make a grid, like in a vacuum tube. I do well remember that. And Brattain and I would tell him, 'Look, that's not the way to go about it. You've got to understand how things work.' "

Brattain describes[9] his original motivation for attending the Michigan Summer Symposium in 1931 as his desire to obtain "a thorough knowledge" of the work function in thermionic emission and the photoelectric effect.

Kelly became convinced that the route to a solid-state amplifier was a deeper understanding of the basic physics of solids. By the mid-1930's he began to indicate a desire to create a new kind of research team to be composed of chemists, physicists and metallurgists who would focus on basic solid-state physics. This interest, which according to Bozorth was expressed even earlier by Oliver Buckley, director of research at Bell from 1933 to 1936, probably motivated Kelly's hiring of theoretical physicist William Shockley in 1936. (In 1952, the year after Buckley retired as president of the Laboratories, Bell Labs and The American Physical Society established the Oliver E. Buckley Prize in solid-state physics, thus commemorating Buckley's long-standing interest in fundamental solid-state physics research.)

Shockley's thesis adviser at MIT, John Slater, was head of one of the two major US training centers of that period for young solid-state physicists. The other was Princeton; three of Eugene Wigner's graduate students there, John Bardeen, Conyers Herring and Frederick Seitz, became part of the first generation of physicists to refer to themselves as "solid-state" physicists. Connections were close during the early 1930's between the physics departments at MIT and Princeton; Bell's three leading solid-state theorists during the mid 1940's—Shockley, Bardeen and Herring—had known one another during their graduate-school days.

CLINTON S. DAVISSON AND MERVIN J. KELLY, 1951

At Bell, Shockley first worked on vacuum-tube phenomena but soon joined a new research group under the direction of Harvey Fletcher, the well known acoustics researcher who was director of physical research at that time. This group was recently described[9] by Joseph Burton, who later became one of its members, as "... a group of fairly new people. Wooldridge, Townes, Shockley and Nix. All had been brought up to some degree in modern solid-state physics." In this group, Foster Nix engaged in a series of studies of phenomena in metals and alloys, and interested Shockley in the order–disorder phenomenon in alloys. Shockley thus moved closer to basic solid-state physics, in which he had been trained at MIT. Dean Wooldridge, who had joined the Labs in 1936, was at this time working on the theory of secondary emission, magnetic sound recording and television; Charles Townes, who joined in 1939, soon became involved in radar bombsight research.

Nix recently described[10] his impressions of that group: "When Kelly created this little group of independent people—there were Shockley and I and Wooldridge—under Fletcher, we were told, 'You do whatever you please; anything you want to do is all right with me'..."

Shockley and Nix were central to the organization in 1936 of an informal study group approved by Kelly that came to function as another important avenue for entry of the quantum theory of solids. The group—including Shockley, Townes, Nix and Wooldridge, as well as Brattain (chiefly working on copper-oxide rectifiers), Alan Holden (whose speciality was crystals), Addison White (working on dielectrics), Bozorth (researching magnetic materials) and Howell Williams (in magnetics under Bozorth)—met weekly for more than four years to discuss the then recent works on quantum solid-state physics, including the books by Nevill Mott and H. Jones, Mott and Ronald Gurney, Richard Tolman and Linus Pauling. James Fisk (initially studying nuclear fission with Shockley) and Burton (working on photoelectron emission) joined the study group in 1939.

The transistor

Meanwhile, an advance took place at Bell's radio lab in Holmdel, which would contribute fundamentally to the invention of the transistor. Several researchers noticed that some samples of the semiconductor silicon were effective detectors of high-frequency microwaves. One of the researchers, Russell Ohl, became interested in obtaining pure silicon samples and involved several of Bell's metallurgists in the problem. During the cooling of hot silicon ingots, Jack Scaff and Henry Theurer produced the first silicon p–n junction; a substantial photovoltaic effect was produced when the silicon was illuminated (this was in 1940).

When Kelly learned of this he recognized that here might be the key to the solid-state amplifier. Brattain recalls,[6]

> "Becker and I were invited to a conference in Kelly's office to discuss the meaning of this phenomenon. We were presumably the physicists who

were supposed to know something about semiconductors . . . [and] we were completely flabbergasted at Ohl's demonstration. The effect was apparently at least two orders of magnitude greater in room light than anything we'd ever seen . . . I even thought my leg maybe was being pulled."

The beginnings of solid-state physics at Bell Labs and the first steps towards the transistor were therefore definitely under way at the advent of World War II. War research at Bell and elsewhere led to new advances, such as resonance techniques, thermal-neutron scattering and improved computing methods, which would in the postwar period contribute in fundamental ways to solid-state physics. And perhaps most important to the advancement of solid-state electronics, a large effort was invested in development on an expanded scale of materials with very small quantities of impurities. Silicon and germanium became prototypes for the study of solid-state physics after the war, in part because the technology for producing them had become well developed.

The war also led to nationwide recognition of both industrial and academic research as a national resource, contributing to Bell's growing support of in-house basic research. President Buckley

OLIVER E. BUCKLEY, 1952

Saving the history of physics

This article, like many works in the history of modern physics, could not have presented as complete a picture without oral history interviews. The historian sat down with scientists and tape-recorded their recollections, making a permanent record of events and ideas that otherwise would have been lost to posterity. These interviews were done with the detailed advice and cooperation of the Center for History of Physics of the American Institute of Physics, which aids such work and also conducts its own interview programs. The interviews used for this article will be stored permanently at the Center's Niels Bohr Library (under restrictions specified by the people interviewed), along with hundreds of other interviews of eminent scientists.

Since memory is imperfect, original written materials are also essential for historical studies. The Center will retain a record of the nature and location of the correspondence and other documents used in writing this article. These, along with many other entries, will be in its *National Catalog of Sources for the History of Physics and Astronomy*. The Center's *National Catalog* and interview collection are only two of a number of programs by which it ensures the preservation of the history of modern physical science. The aim is to make the record available to scholars and educators so that future generations, and our own as well, may know what has been done in our times.

Much work remains to be done—for example, it has been possible to tape-record the recollections of only a small fraction of all living eminent scientists. The Center therefore supplements its AIP funding by soliciting donations. Members of The American Physical Society will have an opportunity this month to make a donation to the Center by adding it to a special line on their bill. Others are encouraged to send their (tax-deductible) donations to the Friends of the Center for History of Physics, AIP, 335 East 45th Street, New York, N.Y. 10017.

—*Spencer R. Weart, AIP*

expressed his attitude in a letter to *The New York Times* of 25 August 1949:

"One sure way to defeat the scientific spirit is to attempt to direct inquiry from above. All successful industrial research directors know this, and have learned from experience that one thing a 'director of research' must never do is to direct research, nor can he permit direction of research by any supervisory board. Successful research goes in the direction in which some inquiring mind finds itself impelled. True, goals are set, goals of understanding in the case of fundamental research .... The director of research does his part by building teams and seeing that they are supplied with facilities and given freedom to pursue their inquiry. He also insures for them contacts essential to their work, but at the same time protects them from interference or diversion arising from demands of immediate operating needs ..."

As to solid-state physics proper, the long discussions between Buckley and Kelly on Bell's basic research during the late 1930's and throughout the war years resulted in formal authorization in January 1945 of the mixed group of researchers that Kelly had envisioned for so long—the group of physicists, chemists, physical chemists and metallurgists, jointly directed to pursue basic research in solid-state physics. The solid-state research group was co-headed by the chemist Stanley Morgan, who had been at Bell since the mid-1920's, and the physicist Shockley. The authorization reflects the vision Kelly had during the 1930's, of a unified approach to all solid-state problems.

Two other basic research groups were also established at the same time; one, headed by James Fisk, to pursue fundamental studies in electron dynamics, and another, headed by Wooldridge, devoted to basic research in physical electronics. Fisk suggested to Kelly that he invite Bardeen, by this time recognized as one of the outstanding solid-state theorists in the country, to join the new solid-state group. Bardeen joined in 1945, and in the following year Herring was hired into the new physical-electronics group. With the addition of Bardeen and Herring, Bell Labs became more than able to hold its own as a leading research institution, in theoretical as well as experimental solid-state physics.

A subgroup of the new solid-state division under Shockley's direction—Bardeen, Brattain, experimental physicist Gerald Pearson, physical chemist Robert Gibney and circuit expert Hilbert Moore—began to focus on the semiconductors silicon and germanium. In December 1947, Bardeen and Brattain demonstrated the first point-contact transistor and in the following year, Shockley developed the first junction transistor. In 1956, Bardeen, Brattain and Shockley received a Nobel Prize for invention of the transistor.

First by necessity, then by design

The cycle was now complete: Bell's program of basic research, which had evolved out of technical concerns of an industry initially generated by one device, had given birth to another device. And soon the cycle would expand dramatically, for the transistor would increase the financial base—and the size—of solid-state physics and begin the age of solid-state electronics.

A highly successful union had been achieved in the Bell System of two traditionally distinct—now proven complementary—approaches to the physical world, the more particular approach of the technical worker and the more abstract approach of the research scientist. It was a union initiated by necessity and only later welded by design.

* * *

This article is excerpted from a more extensive monograph now in preparation. The research, based on documents (including notebooks, letters and technical memoranda) and tape-recorded interviews with Bell Laboratories scientists, was made possible by the cooperation and support of several institutions, the most prominent among which are Bell Laboratories and the Center for History of Physics of the American Institute of Physics.

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