Microelectronics: Its Unusual Origin and Personality

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***Abstract*—In the 1950s, Bell Telephone Laboratories (BTL), Murray Hill, NJ, was the dominant player in microelectronics and lent its personality to the fledgling industry. Among the *Transistor Three*, Bardeen was a theorist of unusual depth, Brittain was the creative experimentalist, and Shockley was the versatile scientist, engineer, and inventor. In addition to his well known device and process inventions, he contributed ion implantation and photoresist processing, two of his important innovations that are sometimes overlooked. The bipolar junction transistor (BJT) was his first and very important device invention. While his effort in the business world was notably unsuccessful, it nonetheless unintentionally launched the Silicon Valley phenomenon. At BTL in the 1950s, and subsequently through the industry, heavy reliance on the work of science-educated engineers became the norm. In the late 1950s, Bell failed to embrace the integrated circuit (IC) and persisted in its error for nearly a decade, probably a consequence of "NH" factors. As a result, it forfeited unchallenged world leadership in microelectronics. Texas Instruments and Fairchild Semiconductor launched the IC revolution, with J. Kilby and R. Noyce playing the key respective roles. We now glimpse a different kind of IC that will be fabricated in a fully automatic process.**

***Index Terms*—Bipolar transistors, current limiters, electronics industry, epitaxial growth, FET logic devices, heterojunctions, history, integrated circuits (ICs), ion implantation, JFETs, photoresist, VHF radio communications.**

I. Introduction

WHAT is it that is so unusual about microelectronics? The answer is that it is one of those rare technologies in which science preceded the art and engineering. The science in this case was contributed by notables such as Wilson, Schottky, Pauli, Fermi, Dirac, and Davydov to name but a few. For most other technologies, by contrast, the sequence is the other way around. In the iron and steel industry or the glass industry, for example, centuries or even millennia of empirical craftsmanship and art preceded even the most rudimentary science. Microelectronics also differs from these technologies in that the materials problems they pose are exceedingly complex. Microelectronics, by contrast, focused first on monocrystalline germanium and then silicon, each in its turn declared to be the best-understood material on Earth. (This is a subjective account of the microelectronics revolution; the author was privileged to observe firsthand a number of the major events that determined its path [1].)

The only other industry that is remotely like microelectronics in having its science come first is the nuclear industry, but there the resemblance ends. After spending all of my graduate-study years half a century ago in nuclear physics, I made the transition to microelectronics immediately thereafter. And a few years later, I realized what great good fortune that had been, even though it required long periods of going back to undergraduate subject matter in order to fill yawning background voids.

Now what can we say about the personality of microelectronics? We can agree, I would guess, that companies have personalities, largely based upon the personalities of the leader or leaders, and to a lesser degree, on those of people in the balance of the organization. In the first decade or so of the microelectronics that began in 1947, far and away the dominant player was Bell Laboratories, which gave its personality to the fledgling industry. Hence, I shall cite some events and anecdotes that are personality-revealing, especially concerning Bell people.

II. Early Research

The famous Transistor Three were all physicists by education. Walter Brattain arrived at Bell Labs in 1929, after graduate work at the University of Minnesota, Minneapolis, and William Shockley arrived in 1936 from the Massachusetts Institute of Technology, Cambridge. John Bardeen arrived shortly after World War II. After graduate work at Princeton University, Princeton, NJ, and Harvard University, Cambridge, MA, he had joined the physics faculty at the University of Minnesota in the years before the war. All three of these people had wartime research experience unrelated to solid-state electronics, and there are reasons to believe that this quasi-engineering experience heightened their transistor-era effectiveness. Near the time of his arrival in 1936, William Shockley was given a provocative challenge by Mervin J. Kelly, then Research Director for the Laboratories. Shockley was enjoined to devise a solid-state switch to replace the electromechanical relay. Kelly could well have added, "… and to replace the vacuum-tube amplifier," but the oneness of switch and amplifier was less evident in 1936 than it is today. What motive lay behind this summons to innovate? Just one: *reliability improvement*. In spite of decades of refinement, the telephone relay remained an unreliable device for reasons inherent in its structure and operation. The same was true of the vacuum tube.

Shockley and his colleagues set to work on their task, putting most attention on copper oxide as a semiconducting material. But they experienced one negative result after another, experiences that Shockley was later to describe as "creative failures." Then World War II intervened. During the final months of the war, a remarkable internal document was issued at Bell Laboratories, entitled *Authorization for Work*. It called for the fundamental investigation of a half-dozen classes of materials relevant to solid-state electronics, notably including semiconductors. Soon the Transistor Three were assembled and hard at work.

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A solid-state physics group was formed under Shockley and Stanley O. Morgan, who set about acquainting themselves with wartime progress made at a number of institutions. Particularly significant was work carried out on germanium purification at Purdue University, West Lafayette, IN, under Professor Karl Lark-Horovitz, a program that had an appreciable impact on the future technology. Shockley and Morgan wisely decided on the strength of their investigations to focus on elemental materials.

In the postwar years, attention was concentrated on a primitive MOSFET-like structure, a concept that in a qualitative way went all the way back to the 1920s. But they found that control by their field plate over the conductivity of the thin underlying semiconductor layer was typically only a few percent of that predicted by "the best theory," as Shockley put it. Then, John Bardeen's theory of *surface states*, published in a lucid 1947 paper, got to the heart of the matter. He reasoned that because a surface constitutes a gross defect in the perfection of a single crystal, many electronic states must reside near the surface. Assuming that electrons are able to flow in and out of these states freely as surface potential is manipulated by means of the field plate, the observations can be explained. Under these conditions, the densely packed layer of surface states acted much like an electrostatic shield, protecting the interior of the crystal from the desired conductivity modulation. And because the surface states "trap" electrons spatially, those electrons are unable to contribute to conductivity.

III. The First Transistors

This insight by Bardeen led to new emphasis on devising empirical methods for stabilizing surface conditions, an elusive goal. In a serendipitous moment when the workers had two metal elements in contact with the n-type germanium crystal, they thought they had observed power gain! Refining the structure to two intentional contacts, small in area and closely spaced, they demonstrated the first transistor, a color photo of which has been widely published. Then amplifier and oscillator circuits using this strange new device confirmed the presence of gain.

The terminal designations Emitter, Collector, and Base were contributed by John Bardeen. His choices are significant because he appreciated well before the transistor discovery that a positively biased metal contact to n-type germanium was able to inject, or "emit," minority holes into the crystal, although this view was resisted by others, especially Shockley. The Collector wire played a role analogous to that of the collector region in the more familiar BJT, or bipolar junction transistor. The germanium crystal was termed "Base" because that was its mechanical role in the structure, a role that was completely lost in the transition to the BJT, though the name was preserved for the BJT region that plays an electrically analogous part.

In the earliest point-contact transistors, the characteristics radiated in roughly linear fashion from the origin of the output plane, the plane of collector current versus collector–base voltage. The gain properties of these devices were described as an output-voltage increment in response to an input-current increment. In other words, in the language of the network theorist, the point-contact device exhibited "transresistance," or was a *transresistor*. J. R. Pierce, the advanced-tube guru and science fiction writer, proposed the contraction "transistor" for the infant amplifier, a name that has obviously stuck. But early in the point-contact transistor era there evolved from the work of A. E. Anderson an obscure and artistic process termed "forming." Although it was done in various ways, the most common of these involved "dumping" a charged capacitor into the collector wire, causing unknown changes in the contiguous germanium. A result of this step was a tendency for the collector–base current–voltage (I–V) characteristics of the primitive transistor to change, now resembling those of a BJT with a terribly "leaky" collector junction. Thus the device had been changed from an approximate "transresistor" to an approximate current amplifier, but the term *transistor* remained.

The common-base characteristics of the point-contact transistor differed in one profound way, however, from those of the BJT that followed: they displayed current gain. The values typically fell in the range from two to three, although in benchtop experiments with micromanipulators, where we pushed point spacing to small values, we observed current gains some ten times higher. Although this gain property remains a mystery today, the device no longer arouses even academic interest. It was so inferior to its successor, the BJT, in so many ways that it slipped rapidly into obsolescence. There was precedent. Previous electronic devices that were never understood, the "coherer" and the "electrolytic detector," were replaced near the turn of the twentieth century by the Fleming valve. Nonetheless, the point-contact transistor was used in convincing demonstrations of the arrival of the solid-state electronics era. Among these were telephone switching networks, radio transmitters and receivers, and even an airborne computer.

Perhaps the most crucial role of the point-contact transistor, however, lay in its effect on William Shockley. He, after all, had received Kelly's injunction, and he had been supervisor of the discoverers, but he was not a part of the discovery itself, or even present at the event. He later admitted to the towering chagrin he felt at being so near and yet so far, in spite of the success that crowned the efforts of his team. This experience and the resulting emotion wound Shockley's spring so tightly that in the following ten years, he was author of the most remarkable outpouring of inventions, insights, analyses, and publications that technology has ever seen. It is only a slight exaggeration to say that he was responsible for half the worthwhile ideas in solid-state electronics. Others in the field often had the experience of generating a "really great" new idea one day, only to learn later that Shockley had put forward the same idea years earlier. (I have had that experience no fewer than four times).

Just as noteworthy as the number of Shockley's seminal ideas in the new electronics is the fact that these contributions ranged through the realms of invention, engineering, and science, and he was a star in all three. He is usually remembered for his prodigious inventiveness, and his analyses of important problems employing basic science, but he was also an engineer of formidable talent. One of the most incisive relevant comments I have heard is that the essence of engineering is knowing what variables one can afford to ignore [2]. Shockley was a master of the simplifying assumption that got him to an analytical result. Subsequent generations of graduate students have written theses that removed his assumptions one by one, in the process creating ponderous treatments of a problem he had treated economically. I usually found Shockley's terse compositional style tough going. But he had unusual skill in the frequent oral communications we were privileged to hear, delivering talks that were marvels of clarity. (This observation leads to some interesting philosophy, which we shall defer, on important differences between oral and written communications).

The first postdiscovery challenge that Shockley addressed (in early 1948) was to understand the inner workings of the point-contact transistor, especially its common-base current gain. He postulated the presence of two or more curved junctions (PN or Schottky) under the collector point, a kind of speculation stemming partly from the "forming" process, because some of the copper and phosphorus in the collector wire could have entered the germanium as doping impurities.

With such structural images in mind, Shockley took a route that illustrates his innate engineering talent. His approach was to create one-dimensional (1-D) models of the speculative three-dimensional (3-D) structures, permitting him in turn to write the equations needed for analytical understanding. At the time, he was one of the few people in the world equipped to carry out such an exercise. One of his speculative structures had four regions, and was his inspiration for the four-layer or "Shockley" diode, itself precursor to the thyristor family of devices.

Just a few weeks after the point-contact discovery and while immersed in this analysis, he realized that an idealized structure involving only *two* plane junctions was itself possessed of gain, a configuration we now call the BJT. He dutifully recorded his invention, but kept it to himself for the time being, preferring to pursue it further on his own. At one of their regular research meetings, then, just a few weeks after Shockley's BJT insight had struck, John Shive reported an experiment he had devised. He had placed emitter and collector points on opposite sides of a thin germanium crystal, and had observed essentially conventional point-contact-transistor characteristics. Seeing this configuration as a point-contact version of his BJT, Shockley sprang to his feet and gave a detailed discussion of what was taking place, revealing his BJT invention in the process.

Shockley later wrote that "Shive forced my hand," and thereafter placed great emphasis on Shive's experiment. It not only stimulated disclosure to his colleagues of his BJT invention, but also demonstrated once and for all that holes were being injected into n-type germanium by the emitter wire. I became well acquainted personally with John Shive in the 1950s, but such was his modesty that it was not until many years later that I learned how crucial had been his role in the story of the BJT. In fact, there were many modest people at the Labs, but in defense of the "personality" thesis being offered here, we can perhaps agree that brash enjoys a visibility advantage over modest, at least usually and at least in the short term. A patent on the BJT invention was filed by Shockley in June of 1948, even before public announcement of the point-contact transistor. His patent presents several BJT configurations, and gives the now-familiar band diagrams of the BJT. Astonishingly, the patent also offers in an almost casual way a band diagram for the wide-gap emitter BJT, pursuit of which has led in part to a recent Nobel prize in physics for H. Kroemer of the University of California, Santa Barbara.

IV. Later Developments

The unusual Shockley personality had a major effect in shaping that of Bell Labs, and by extension, of the microelectronics industry, a personality that emerges clearly from a Shockley biographical memoir published recently by John Moll [3]. Polishing his image, Shockley tooled about New Jersey in a British two-seater, with appropriate headgear. And he dabbled in magic. For example, at least once he pulled a bouquet of flowers from his sleeve in public. This was early in 1953, on the occasion of an American Physical Society meeting at Cambridge. To attend it, I drove to Boston from Murray Hill with colleagues John Moll and Geoffrey Garrett. At the banquet, Shockley was seated at the elevated head table, alongside such notables as Professor Van Vleck, Enrico Fermi, and Oliver Buckley, long-time president of Bell Labs before Mervin Kelly.

Shockley was present at the head table because the Buckley Prize was about to be awarded to him for his already numerous contributions. He made his acceptance remarks, commendably brief but not noteworthy for modesty, and then produced the bouquet, perhaps as a summary of the praise that had just been bestowed upon him and his work. His famous tablemates were at first startled, but then displayed appropriate amusement.

Shockley coined aphorisms. Reasonably well known are the three postulates that he modestly enunciated in the early 1950s. These were: 1) Shockley is a dope; 2) nature is unkind (I am bowdlerizing here); and 3) there are no mechanical engineers! The last was a comment on Bell Labs hiring practices. They hired large numbers of young people educated in every branch of basic science. But they failed by a significant margin to provide enough support people, technicians and mechanical engineers among them. For people who left Bell for jobs closer to the marketplace, the ready availability of support personnel was one of the most pleasant surprises.

There were probably several reasons for this Bell practice. First, they hoped to net another Shockley along the way. Second, the wartime discovery that scientists are capable of doing engineering when properly motivated (by patriotism in the wartime case) led them to hire young people enticed by an opportunity to walk in the footsteps of the Transistor Three. And third, the advent of the transistor changed the core subject matter of electronics so profoundly that even people in young middle age had difficulty making the transition. Hence, it made sense to hire younger workers who had been already introduced to the new disciplines. These people assumed roles as *science-educated engineers* at Bell Labs, constructing an edifice of art and engineering on top of the existing scientific foundation, and their combination of youth and zeal had a further shaping effect on the personality of microelectronics.

My own thought in the early 1950s was that the Bell Labs tactic in addressing a technical challenge was analogous to the Chinese "human sea" military tactic in the contemporaneous Korean War. "If we put enough Ph.D.s on this problem, something's bound to give." And it usually did. A contemporary of mine once observed that to make progress at Bell Labs, one had to be a "3C" person, standing for Capable, Contentious, and Condescending (I felt that I met the first criterion, but could never quite get the hang of Contentious and Condescending).

Capable surely rubbed off on the industry, and so did Contentious, in the positive sense of striving and competing. But happily, Condescending did not. Condescension caused Bell Labs to pay a heavy price, as we shall see.

Two anecdotes further illustrate Shockley's dominance in microelectronics during his postwar decade at Bell Labs. Although the science was well in place, the infant transistor technology was "technique poor," and science-educated engineers searched frantically for new and better fabrication methods. Junction-forming innovations dominated the early years, but other kinds of fabricational advances proved just as important. Here again, Shockley was a role model. In 1954, he made two major contributions to device fabrication. Ironically, he is often not credited with either. I was on the fringes of both events and have anecdotes to relate.

The first, photoresist processing, was a Shockley contribution, and it became what has been accurately described as the key to modern microelectronics [4]. He hired a technician chosen for his manual dexterity: his skill in handling a "one-bristle brush." Jules Andrus was his name, a commercial artist by profession, and he lived a few hundred yards up the hill from us. For several years, he and I were in a three-man carpool. Then one day in 1954, Jules commented to us, "I won't be with you fellows in the carpool tomorrow. The boss has some crazy idea about using photoresist on germanium, so he's sending me to [Eastman Kodak at] Rochester to pick up a bottle of the stuff and some instructions on how to use it." Interestingly, at that time, photoresist was a well-established product for use on a macro scale: the cutting out of aluminum panels for aircraft manufacture through an etching process. Making the macro–micro transition certainly required imagination. Shockley left Bell Labs shortly after this all-important experiment was run, and the *Andrus patent* issued about a decade later, in 1964. My record does not indicate whether Shockley simply "gave" the invention to Andrus, or whether its handling by the Bell patent authorities was a result of Shockley's departure.

The second story is about another world-class contribution by Shockley, ion implantation, sometimes overlooked perhaps because of a surprisingly long interval between its conception and serious application. The reason that the delay is surprising is that the 1950s were so technique-starved, and also because it occurred in spite of vigorous advocacy in that interval by the world's most prominent specialist, Shockley himself, who repeatedly seized a bully pulpit to urge some serious attention to it. In the mid-1950s, I encountered my thesis adviser from Case Institute of Technology (now Case Western Reserve University), Cleveland, OH, nuclear physicist E. F. Shrader, at an American Physical Society meeting in New York City. He inquired about solid-phase diffusion, which had just come prominently on the scene for BJT fabrication. I told him what I could about the principles and applications of the diffusion process, causing him to ask, "Why don't you shoot 'em in?" And then he reminded me of a course wherein he had stressed that a heavy projectile such as an atom (unlike an electron) has a well-defined and energy-dependent range in a solid. An ion beam of controlled energy and intensity would get the job done, he noted.

Subsequently, I discussed the concept with various colleagues and grew progressively more excited about prospects for the idea. Much of the appeal for me was in its interdisciplinary nature, since I was still adrift in large parts of solid-state electronics as a result of my shift of specialty. I began digging in every literature area that made sense in relation to ion implantation, found nothing, and then through a chance lead, secured a copy of "Shockley 84," the eighty-fourth invention disclosure that Shockley had filed at Bell Labs. In spite of the fact that it was written in 1954, it reads like a document written yesterday, and not surprisingly focuses heavily on the BJT.

I had reported the ion-implantation idea promptly to James M. ("Jim") Early, my supervisor for a couple of years in the mid-1950s, but he displayed little interest. The interdisciplinary feature of the projected technology likely was as unappealing to him as it was appealing to me. When I later reported my discovery of Shockley's pending patent to Jim, we both dropped any thought of pressing the concept. We were confident that its future was assured, since there existed no more potent and eloquent advocate (something that every idea needs) in microelectronics.

V. Microelectronics Moves West

Shockley was not satisfied with stunning achievements in science, engineering, and invention, but yearned to excel also in another realm, business entrepreneurship, an activity that we might describe as the fourth technical dimension. Accordingly, he left Bell Labs in the mid-1950s and set out to make his fortune. With backing from Arnold Beckman, in Stanford's industrial park he built a laboratory that became, as we know, the foundation stone of Silicon Valley. But in spite of the profound consequences of his move, his own venture didn't work out.

In the course of setting up his new enterprise in Stanford University's industrial park, Stanford, CA, Shockley succeeded in recruiting a young and unusually talented cadre of science-educated engineers. The new team continued the well established Shockley practice of doing outstanding work in science, engineering, and invention. But the leader's unfortunate shortcomings in business matters struck early, on first-product choice. He set out to commercialize his favorite invention, the four-layer diode.

After a few years, some of his young crew grew increasingly restive, feeling that his earlier invention, the BJT, was a vastly more promising candidate for commercialization. Circuit designers, they argued, preferred both switching and amplifying devices that had control electrodes, and found two-terminal devices to be inherently inferior in most applications. Being unable to persuade the boss to change direction, eight of them obtained independent backing from Sherman Fairchild, the light-airplane and aerial-photography pioneer, and launched Fairchild Semiconductor. On product choice they were right, and Fairchild Semiconductor prospered. The "traitorous eight," as Shockley dubbed his young defectors, hitched their wagon to the BJT and the *planar process*. Jean Hoerni, its inventor, supplied a small but crucial added step to "masked diffusion" beyond those that had been demonstrated at Bell Labs.

Shockley left soon afterward, taking a chair at Stanford before the end of the 1950s decade. During the 1960s Shockley Laboratories passed through a series of hands. The last company owner was ITT, and as their employee, it was my sad duty to close down the Shockley operation in 1968. Four-layer diode development had continued until the plant was closed. Shockley simply did not have the temperament or instincts of the successful businessman. One of his original lieutenants, and then a member of the Traitorous Eight, Jay Last, told me that if Shockley Labs needed a new micromanipulator, for example, he would find his boss spending hours, or even days, with the machinist, developing the world's greatest micromanipulator, instead of simply pulling a catalog off the shelf. Our conversation took place at the Wescon Show in 1960, where we had happened to meet. On that occasion Jay greeted me with a grin and the observation, "I've just made Shockley a grandfather." His was one of the early moves in the creation of Silicon Valley, and his reference was to the fact that he had just left Fairchild to found Amelco Semiconductor. They went to work on the junction field-effect transistor, or JFET, yet another Shockley invention.

Shockley's poor fit in the world of business was presaged a few years earlier. Remember that in the early 1950s, WW II was a recent memory, and the Cold War had just begun. Recent news included reports on how the ruthless Stalin had slaughtered millions of his own citizens, and banished millions more to virtual slavery in Siberia. Shockley's rueful name for Allentown, PA, was "Siberia," a jocular epithet that revealed his contempt for manufacturing engineering. A Western Electric plant was located there, along with a branch laboratory charged with transistor development for manufacture, an arrangement like that for various other products at other Western Electric plants. A person who hopes to succeed in a manufacturing business, however, must hold manufacturing skills in high esteem, and not in contempt.

The list of "four-dimensional" technologists in the sense just indicated is fairly short. Surely Shockley's protégé Robert Noyce qualifies, as coinventor of the integrated circuit (IC), and with equally well known achievements in the other three areas of technical endeavor. Edwin Land of Polaroid fame is certainly another. And returning to electronics, Edwin Armstrong, who invented FM and other trenchant electronic concepts, was another.

Walter Brattain had a long and fruitful career at Murray Hill, NJ, staying on until his retirement. He then entered into a gratifying relationship with Whitman College, his alma mater back in Walla Walla, WA. John Bardeen's Bell Labs career, by contrast, was short and fruitful. He left in 1951, eagerly seeking a return to the theoretical pursuits that so engaged him. His first impulse was to return to Minnesota, where he had enjoyed his physics-faculty colleagues and duties more than a decade earlier. But it was not to be. It is often the case with physics departments around the nation and the world, as many readers know, that an ultimate pejorative term is "applied." It was the sense at Minnesota that Bardeen's work at Bell had been a bit too "applied" for their taste, so they declined to offer him tenure. As a result, he went to the University of Illinois , Urbana, and became the only person in history to receive two Nobel prizes in physics! (In making personal assessments, one should indeed be cautious.)

The University of Illinois rejoiced in the presence of John Bardeen. He was warm in personal encounter, but reticent, even halting, in public speech, quite in contrast to his erstwhile colleague, the fluent Shockley. The April 1992 issue of *Physics Today* was a memorial to John Bardeen that stressed his many contributions, but also related some revealing anecdotes. A story that epitomizes his legendary modesty came from one of his passions, golf; late in his life a golfing buddy of very long standing asked the double-Nobel laureate casually, "John, what kind of work do you do?" The special issue honoring Bardeen was assembled by Nick Holonyak, one of Bardeen's earliest Ph.D.s from Illinois and also one of my 1950s associates at the Labs.

With my first Ph.D. advisee, B. L. ("Bernie") Grung, I published the book in 1983 [1], containing my subjective history of microelectronics as Chapter One. I wrote to the three transistor inventors, offering to send a copy of our book to each of them, mainly for a critique of the history chapter. John Bardeen responded promptly and cordially, which was most gratifying. Our tenures did not overlap at Bell, but I had met him a short time before on a speaking visit he made to Minnesota. Almost as promptly an answer came from Mrs. Brattain, with the sad news that her husband had recently been admitted to a nursing home, an event made necessary by the incidence of Alzheimer's disease. I speedily and gratefully sent off copies of our book to both of them. Shockley did not bother to respond at all.

The 1950s started with a bang at Murray Hill. In 1950, the Teal–Little process was announced, which permitted the growth of a relatively large germanium crystal "from the melt." A crucial variation of the process was the successive addition of impurities to the melt during growth to create a pair of closely spaced junctions in the finished crystal. Using this method, reduction to practice of the BJT was achieved and reported in a pair of papers in 1951 by Shockley, Morgan Sparks, Gordon Teal, and Ernest Buehler.

There are several names that, like Shockley's, occur repeatedly in the early transistor story, and Teal's is one of them. In the following year, 1952, Teal left Bell Labs for family-health reasons and joined an obscure geophysical company named Texas Instruments, where he was invited to set up a research department. Not surprisingly, he went to work on germanium grown-junction transistors and had a mature product line in place by 1954. These were then applied in the world's first commercial transistorized pocket radio, the Regency, an arrangement worked out by Patrick Haggerty, president of TI at that time. Also in 1954, Teal attended a meeting where a series of speakers commented on the near-term hopelessness of achieving transistors made of silicon. When his turn to speak arrived, he tossed a handful of the first practical silicon transistors on the table, and then told how they had been made, which of course was by junction growth from the melt.

VI. Bell Labs Reprise

The development effort that led to the Teal–Little process, and thence to the first practical germanium and silicon junction transistors, did not just happen. It was commissioned, in fact, by an aggressive and farsighted Bell Labs manager named J. A. ("Jack") Morton, a dapper man, bow-tied and crew-cut. Jack was Head of the Device Development Department when he hired me in 1952, and a Vice President at the time of his death in the 1970s. Morton reasoned correctly that superb control over the basic material in the new solid-state age was *sine qua non*. In this, he opposed the advice of no less an authority than Shockley himself, who believed that adequate single crystals could be selectively extracted from polycrystalline ingots. But in the face of subsequent events, in particular the successful grown-junction transistor, Shockley, not noted for graciousness, graciously acknowledged his debt to Jack Morton and his own error.

Morton was one of a small and elite group of people who made major contributions in the vacuum tube era, and then went on to do the same in the transistor era. His major earlier contribution was the development of a device with more closely spaced electrodes than those of any previous vacuum tube, this for the sake of bandwidth and gain. This *Morton triode* was one of the keys to success for the postwar transcontinental microwave radio-relay communications network put in place by the Bell System. When the transistor emerged from the Research Department, a natural move was to hand it off to Morton and his Device Development Department, seeking to capitalize on the pattern of progress and success he had established. Thus it was that in early transistor days, tube and transistor development were somewhat intermixed. I enjoyed interacting with members of the old Morton crew, who were still winding grids and pressing the vacuum-tube art, to tell them about my own experience in the previous decade with a primitive version of their continent-spanning relay system.

During WWII, when the front was moving faster than a pole line could be laid, it was the custom to extend wire circuits by means of radio. For this purpose, the Signal Corps had a four-channel FM radio-relay system that operated in the VHF band, which followed by just a few years Armstrong's first convincing demonstrations of the FM concept. Another radio officer and I (one of many similar teams in Europe) shared responsibility for a circuit between the headquarters of Generals Patton and Bradley. My new friends in the Morton crew listened to my stories indulgently enough, but were unimpressed by the VHF band, which they liked to describe as "dc."

In consecutive papers in a 1956 issue of BSTJ, the Bell System Technical Journal, Charles A. Lee described a germanium "diffused-base transistor" (as it was known for many years thereafter), while Morris Tanenbaum and Donald E. Thomas reported on silicon devices exploiting diffused junctions. In the 1950s I became closely acquainted with Don Thomas, who was an unusually talented circuit designer in the Research Department, and who also had a talent for showmanship. It got him on the Today Show to discuss transistors in the Dave Garroway era, and on an unrelated earlier occasion, provided a coup for Jack Morton.

Using a single point-contact transistor, Don assembled an FM transmitter in a small plastic box. Adding to it a miniature microphone, Don created the world's first cordless lapel microphone. In those days Morton gave frequent talks on transistor development. And on the next occasion he wore Don's cordless microphone while pretending to use the microphone mounted on the lectern, although it was actually switched off. At the conclusion of the talk, then, he stepped down from the stage and proceeded toward the rear of the hall, explaining the ruse he had perpetrated on his audience. When he stopped talking, there was a brief stunned silence, and then thunderous applause.

At the very end of the 1950s, an additional crystal-growth and junction-forming process was successfully applied, namely silicon epitaxial growth from the vapor. A patent on this process had been filed in 1951 by Howard Christensen and Gordon Teal. After a delay of years, the versatile epitaxial process became highly significant. Howard was a close acquaintance who labored alongside many of us in the Transistor Development Department, rather than in the more rarified atmosphere of the Research Department. During the two-year period in the mid-1950s, when both of us were reporting to Jim Early, Howard was given the tough assignment of finding a better way to attach leads to the emerging diffused-base BJT's. Jim had previously given me the assignment, but an illness took me out of work for a period of weeks, and so he wisely reassigned the project. Howard, as was his style, reached out for collaboration with people who resided across departmental lines. And in a remarkably short period his team produced the concept and reality of thermocompression bonding.

It is likely that a number of readers are acquainted with my colleagues of that era in addition to those already mentioned, in name, if not in person. They ranged from casual to very close acquaintances. Fascinating stories go with each, and with many others, of course. I justify this name dropping through the belief that you will appreciate the personality influences of these people, if you do indeed know some of them. They were people such as George Bemski, Bill Boyle, Carl Frosch, Conyers Herring, Joe Kleimack, Ian Mackintosh, Sol Miller, Gerald Pearson, Art Schawlow, and Art Uhlir, to name a few of them alphabetically.

As a step on his way to a vice presidency, Jack Morton received a significant augmentation of his device-development domain, a large department devoted to the refinement of passive components that the Bell System used in huge number and variety. In a farsighted initiative, Jack suggested that semiconductor technology might have a contribution to make to passive-device technology, in a sense presaging monolithic integration. In 1957 Morton asked me to join a small group of younger semiconductor engineers he had accordingly hired for that department.

The Passive Components Department was headed by Henry A. Stone, Jr., who was both a highly creative engineer and an unusually skilled manager. He caused the people in our new little group to "play above their heads" for extended periods. He involved himself in our research, seizing the opportunity to master the new world of silicon devices, and repeatedly provided a key idea, or took the lead in attempts to carry through mathematical analysis of new structures, usually with success. I stayed in touch with Henry Stone in later years, and we established that his Stone ancestors and my Warner ancestors had interacted in Hartford, in the 1630s, 1640s, and 1650s.

The immediate group of engineers addressing Morton's charter included C. J. ("Larry") Spector, physicist, W. J. ("Bill") Grubbs, EE, and Howard Lawrence. Our foursome is depicted in Fig. 1. Howard is a German-born chemist of strong personality, who inspired us to letter a neat sign in the German style for posting on the door of our four-man office,

Fig. 1. Made from two Polaroid snapshots taken minutes apart, this composite photo shows Die Aufgufen Gruppe with Ed O'Connell. Standing, left to right: Howard Lawrence, Larry Spector, Ed O'Connell, and Bill Grubbs. Seated: Ray Warner.

a sign reading Die Augufen Gruppe. We had good reason to appreciate Stone's indulgence with us, because the "Gruppe" was known to repair occasionally to Keller's Grove to observe time-honored traditions, such as ordering a pitcher of German beer, and tossing off some German beer-drinking songs. The fifth person in Fig. 1 is one of our technicians, Edward F. ("Ed") O'Connell, with whom I had the unusually good fortune of working for two years. His effectiveness in the lab and his talent for succinct expression made him a colleague who converted work from a job into an experience, with a couple of examples offered below.

My new colleagues and I surveyed the various opportunities for implementing Morton's idea. One of the most appealing challenges we unearthed was in a program that had been proposed by John Moll and that was being immediately supervised by Jim Ebers. Jim was developing a small network known in telephone jargon as a "PBX," or private branch exchange, a self-standing network such as a department store might employ. Realizing that Shockley's diode had some potential as a switching device (a so-called "crosspoint") in such a system, Moll made the radical suggestion that it be done in silicon. The crosspoint developers had the additional thought that a nonlinear load device with a current-regulating property would be well-adapted to the four-layer diode, literally a mate, and would improve the efficiency of the projected system.

One way of achieving the constant-current characteristic he sought seemed to us to lie with the JFET, in a two-terminal embodiment. The JFET was a considerable novelty at the time, and as noted above, another of Shockley's contributions. In fact, he had correctly analyzed the JFET before reduction to practice, just as he had done previously for the BJT. A few JFET prototypes had been made using germanium and an awkward alloying method, confirming theory but being an unattractive fabrication option. As a practical matter, there were no devices to study, so we set out to make some.

We chose to use silicon and the diffusion method (then both new and popular) for our purposes. We were able to make some devices in fairly short order, but one of our several problems was the classic instability of their silicon surfaces. Faced with this problem, I appealed to M. M. Atalla, who with his coworkers was setting out to stabilize silicon surfaces by means of thermal oxidation. Atalla was Egyptian, but he preferred to be addressed by his American nickname "John" rather than by his given name, Muhammad, often explaining that the two names were in a sense equivalent. I had met him when interviewing in 1952 at the West Street Lab in New York, where he led a serious effort to improve relay reliability by applying the most basic science to contacts and arcs. But in the mid-1950s he was moved to Murray Hill to take on the silicon-surface challenge. It was clear by then that microelectronics would indeed push the telephone relay into obsolescence.

One of Atalla's lieutenants was Edward J. Scheibner, who gave me an added entrée to the group through shared experience. Ed had also been a radio officer in Europe and we were on the same troopship for a wartime cruise from Marseilles to Manila. Atalla and Scheibner agreed to treat some of our devices in an effort to stabilize their properties, but when the samples were returned, we observed inversion layers on their surfaces that served admirably as field-effect channels! This caused me to propose in a technical memo a structure that today would be described as a "planar FET with an inversion-layer channel," depicted in Fig. 2. Unfortunately, I didn't propose applying a metal field plate to the oxide above the channel, being satisfied to use the substrate as a gate electrode. But it did occur to Atalla and another of his lieutenants, Dawon Kahng, and some months later they added a field plate to my planar FET to produce the MOSFET! … (metal-oxide-silicon field-effect transistor).

Although our aim was supplying a field-effect diode, we appreciated the triode properties of our devices using the substrate as the gate, and we explored them at some length. In one instance Bill Grubbs and I used our N-channel devices in combination with P-channel JFETs to assemble complementary FET gates and latches, in the process noting their advantageous properties; this was in 1958, a few years before CMOS (Complementary MOS technology) went to the top of the charts. The P-channel devices we used have a story of their own. Shockley's internal-channel JFET ingeniously bypassed the semiconductor-surface problems that had plagued so many for so long, and we recognized that such a device would be beneficial to us in this project. It occurred to us as we searched for devices in the near term that the "Tetrode BJT" was topologically equivalent to the JFET, and hence might serve as a *grown-junction* JFET, though not ordinarily so represented. This BJT, with a pair of base leads on opposite sides of the device, was developed by R. L. Wallace to confine transistor action electronically to a tiny region, in order

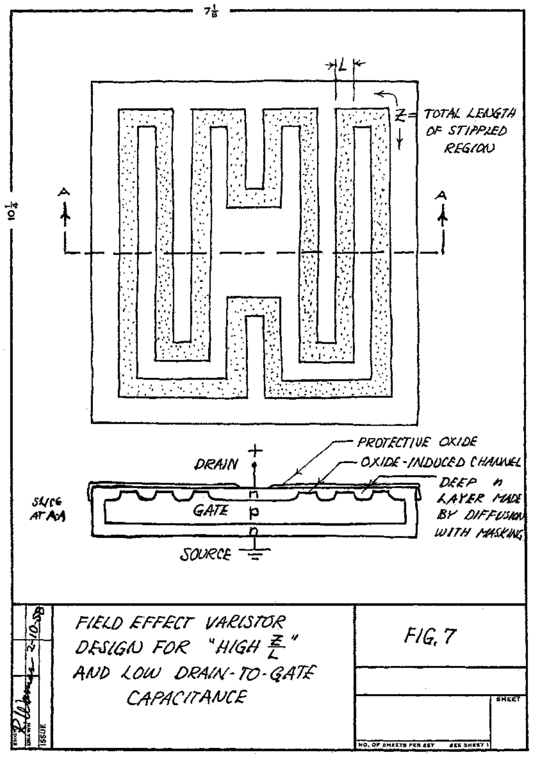

Fig. 2. This diagram is taken from the Bell Laboratories Technical Memorandum doubly numbered MM-58-2542-8 and MM-58-2525-4, dated February 17, 1958. Used by permission. It was reproduced as [1, p. 50, Figs. 1–10].

to enhance gain-bandwidth properties, and it worked famously, delivering a gain-band product of 0.8 GHz, astonishing for the time.

I appealed to the amiable and cooperative Emil Dickten, Wallace's technician, for some devices to study. He reached into his bottom drawer and pulled out a handful with the words, "Here, you're welcome to these—they're nonlinear as hell." The base-to-base nonlinearity he alluded to was precisely that of the JFET! In these particular devices, the Gummel number was lower by a factor of five or ten than the optimal BJT value, just right for a JFET. This experience caused me later to describe a four-terminal *prototransistor* structure [6]. Combining two directly opposite electrodes (bases) yields a triode BJT, while combining the other two, (gates) yields a triode JFET.

One morning while driving to work I had a bright idea that sent me racing the rest of the way to the lab. There I grabbed a handful of clip leads, some of our constant-current diodes, and some so-called "Zeners," or constant-voltage diodes. Hooking together a two-terminal circuit that you can pretty easily visualize, I connected it to the curve tracer, and sure enough, it was a "passive" circuit with a staircase current–voltage characteristic. Excitedly I called Ed O'Connell over to have a look at this marvel. He absorbed the breakthrough in just a moment or two, thought about it for a few seconds more, and then said, "Do you know what you have here, Ray … you have a medicine for which there's no known disease." The forty ensuing years have proven him exactly right!

There are many more "Ed" stories. I maintained contact with him and we had some additional delightful exchanges. On one occasion decades later I observed to him with a sigh that, while I had not been the inventor of the MOSFET, at least I had been the inventor of the "OSFET." His rejoinder was instantaneous: Being the inventor of the "OSFET" is like being the inventor of "6-UP."

VII. The Integrated Circuit

In the late 1950s there were rumors that some U.S. firms were considering a possible "circuit in silicon," to use the phrase that sometimes surfaced at Bell Labs. After all, one piece of silicon could accommodate a number of transistors formed simultaneously; also, silicon had resistance, junctions had capacitance, and parallel-plate capacitors were possible too with a silicon–dioxide dielectric, but just how to make something useful out of all this was far from clear. Though some laboratories took this possibility very seriously, at Bell, one lone technician was assigned to the subject. After the rumors evolved into product announcements and public discussion commenced, the level of effort was not changed so far as I know, having left before then.

What did occur, though, was an upswelling of high-level efforts to ridicule the idea of what became the integrated circuit, or "IC." It is regrettable that Jack Morton, who had led the way with so many right technical decisions, often against stiff opposition, took the lead in this last, and disastrously wrong, major technical decision of his career. Adding to the irony is the fact that he had demonstrated his belief that transistor technology had something to say to time-honored passive components. His primary put-down of integration was the so-called "tyranny of numbers" argument. Realists of the era knew that fabricating transistors at a yield of 50% constituted a "stretch goal." Hence, if one were foolish enough to put ten transistors on one piece of silicon, to say nothing about all the other silicon devices that were being vaguely considered for circuits in silicon, he would have a yield of 0.5 raised to the tenth power, about a tenth of one percent, and hardly an attractive commercial proposition.

The argument was fallacious. In truth, even in those primitive times, the inherent transistor yield on a semiconductor slice that had been processed without error was closer to 99% than to 50%. Also the argument overlooked the huge yield cost of separating the transistors, mounting them, attaching leads to them, and encapsulating them. His lieutenants, however, picked up the negative theme and elaborated it. One of their favorite refrains concerning the proposed new look in electronics was, "The parasitics will kill it." They claimed that they could do better with discrete devices in any application. They did not foresee the dimensional shrinkage that was a major factor in diminishing parasitic capacitances, inductances, and resistances. And the third argument they devised, and offered in a somewhat disdainful way, was that integration could not possibly deliver the precision needed for "telecommunications." We can probably attribute this costly lapse by Bell Labs to the NIH factor—not invented here, or "not in-house." The personality factor of Condescension mentioned above seems consistent with this explanation. Bell stayed out of the IC arena for nearly a decade, and with 20-20 hindsight we can note that unchallenged world leadership in solid-state electronics was forfeited as a result.

In the summer of 1959, I went on an interviewing swing through the West, and it was an eye-opening experience. At Fairchild Semiconductor, I had the heady experience of being

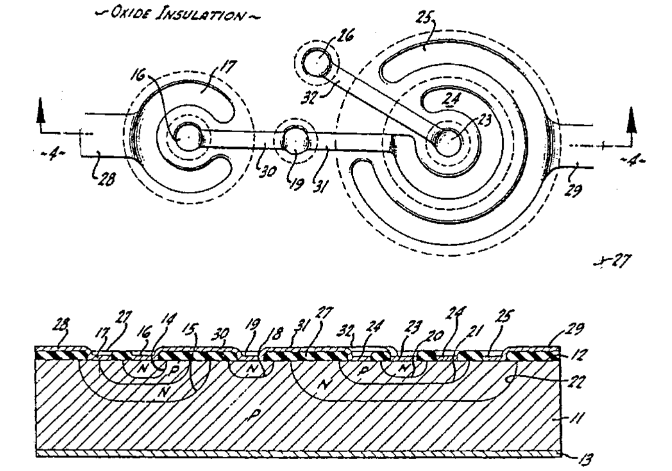

Fig. 3. (Top) Plan and (bottom) elevation of integrated-circuit structure described by R. N. Noyce in U.S. Patent 2 981 877, filed on July 30, 1959. His concept adapted the then-new "planar" process to the junction isolation of transistors and other components.

taken to lunch by the seven of the Traitorous Eight who happened to be in town on that day. They were still in their first crowded temporary quarters, but the sense of excitement was palpable. There was of course not a hint given of the fact that this was within days of the time that Robert N. Noyce had filed a patent on his version of the planar-BJT monolithic-IC structural concept, one that had a very long life (Fig. 3).

The dynamic Les Hogan, the quintessential General Manager of Motorola's Semiconductor Products Division, and one of his rising stars, Don Dickson, hired me a bit later, and put me to work on several device-development tasks, reporting to Don and heading a capable group of engineers and technicians. Within months of my arrival, Don commanded me to give a course to my people, probably assuming that anyone from Bell Labs must "know all that stuff." Well, it was a period of forced learning for me, and couldn't have been a more beneficial experience. Don's domain then included the highly profitable "Zener" diodes, and the even more profitable zero-temperature-coefficient "Zeners," which he had invented. Another huge program, well under way, was the automotive rectifier, a product with a fascinating story.

I occasionally worked on the fringes of those big programs, but mainly on some startup devices. One was a silicon JFET, which had caught Hogan's interest on interview day. The Teal–Christensen epitaxial-growth process found its way into a few laboratories in Europe and the U.S., and an old college acquaintance, Ted Benedict, came visiting from one of them to sell the concept of selling custom-grown epitaxial layers to customer specifications. It sounded to me like an attractive option for creating a JFET channel, difficult to do by way of diffusion technology, so I ordered some material and discussed the idea with several colleagues. One was Wilf Corrigan, who went on later to become President of Fairchild, and after that, founder of LSI Logic in Silicon Valley. In Wilf's view, a materials challenge such as epitaxial growth should be an in-house effort, for a long series of reasons. He set up a small reactor, and in fairly short order was supplying us with slices from which we were able to make first-rate JFETs with internal epitaxial channels. Discrete JFETs never found more than a niche market, and a tiny one at that, but nonetheless virtually all were made with epitaxial channels for about ten years, until the practical advent of ion implantation. Though we were pleased by the success of this joint effort, a development from a different direction came to have much greater importance.

In June of 1960, the annual Device Research Conference was held at my alma mater, Carnegie Tech, and included a pair of provocative device innovations. Curiously, both were relevant to the JFET project. One was the announcement by Atalla and Kahng of the MOSFET concept, the FET variation that, in contrast to the JFET, has almost literally taken over the world. A second announcement, also by Bell Labs, was on the epitaxial BJT, clearly a major device improvement. Motorola, having a significant headstart on epitaxial growth through JFET work, seized on this opportunity to gain an advantage in the BJT marketplace. Wilf scaled up his capabilities at a furious pace, and Motorola's BJT business prospered. In an instructive aside, we can note that in developing apparatus for handling dozens of slices at a time, he found himself working on the most relevant problems. Bell for years stayed with the single-slice epitaxial reactor, and was working on the wrong problems much of that time.

Mastering the monolithic IC art in the early 1960s was a challenge that led Les Hogan into a typically brash initiative. His thought process went something like this: Even though we haven't yet tamed the IC tiger, if we offer a course to the industry on how to make ICs, everyone will think we have mastered the art, and we may just figure out how to do it in the meantime. It worked! Two or three dozen senior engineers were sequestered in a motel for a couple of weeks to draft the notes for a course to be given a couple of months later, in the summer of 1963. A teaching lab was established for instruction on photoresist processing, diffusion, epitaxial growth, wirebonding, and the like. The two-week course was fully subscribed in spite of its astronomical tuition fee. Each student was given two gigantic loose-leaf binders filled with hastily printed instructional materials.

A few weeks later Les asked me to take on the task of editing the notes that had been used in the course, readying them for publication in book form. Recruiting Jim Fordemwalt as Associate Editor, I wrote some introductory material, and we simultaneously addressed the mountain of lecture notes. It took us approximately a year, since it was done in addition to our regularly assigned duties. One can readily imagine that written matter prepared so hastily had certain deficiencies in unity, coherence, emphasis, and in style uniformity. This experience was yet another period of intense forced learning. Our book, published by McGraw-Hill, stayed in print for about 25 years. This longevity was truly gratifying, suggesting that the book retained value in spite of vast evolutionary changes in the products that it treated.

My last year or two at Motorola gave me weekly opportunities to interact with a fascinating pioneer, Daniel E. Noble. Motorola's founder, Paul Galvin, had hired Noble in the 1930s to put his company in the business of two-way radio, an effort that obviously succeeded. Noble had been a professor at the University of Connecticut, where he had created such a system for the State Police. His wartime achievements at Motorola had included the walkie-talkie and the handy-talkie. Also, he was the prime mover in introducing Motorola to the new world of transistors, a technology he insisted must be entered. And he chose the Phoenix area (a place he had "discovered" as a young man) as a site for the work. In spite of his valid conviction that transistors were important, he kept his feet firmly planted in vacuum-tube technology, and rarely became involved in day-to-day company operations from his vice-presidential chair.

The President of Motorola in the early 1960s was a company veteran who had been an associate of Paul Galvin even back in the 1920s, in the days of the "B-battery eliminator" (a term guaranteed to mystify some and sound nostalgic chords for others). The President pleaded with Les Hogan to give up the costly IC "nonsense" in order to stick to proven money makers such as Zener diodes and germanium power transistors. But Les maintained that ICs were the future, a fact that had to be faced. Then he took the even more costly route of setting up three competing IC departments, while thinly disguising the fact of competition. He expected one of them to "win," and that proved to be the case. It was Wilf Corrigan's department.

A recurring case of itchy feet took me to Dallas and TI in the summer of 1965, and I had the extreme good fortune to go into a slot alongside about ten colleagues reporting to Jack S. Kilby, coinventor with Robert Noyce of the integrated circuit. Kilby's lieutenants were known as "Branch Managers," a bit of title inflation. My group was assigned to develop the technology needed to put TI in the MOS business. My MOS group was already up and running, thanks to the brilliant Bob Biard, inventor of Schottky TTL, and also inventor with Bob Crawford, of the kernel of the programmable logic array, or PLA. Bob is a colorful and self-effacing, but brilliant, product of Texas A&M. My arrival permitted him to drop his dual role, and to concentrate once more on his first love, compound-semiconductor materials and devices.

My group included some extremely capable people such as Bob Crawford, who wrote the first book on the MOSFET, and L. J. Sevin, who later founded MOSTEK. "LJ," as he was universally known, also became the person to bring Shockley's dream of ion implantation to fruition. The break came in the late 1960s when Sprague Electric in North Adams, MA, invested effort on implantation in a research setting. Because Sprague was a backer of the fledgling MOSTEK Corporation, they received a visit from LJ who immediately sensed the unique value of implantation to MOS technology, and applied it right at the beginning of the 1970s, causing the race to be on. Ion implantation quickly achieved the status of a standard and indispensible semiconductor technology, after an astonishing delay of 16 years.

The monolithic integrated circuit stands as the most towering innovation since the transistor itself, and for this Bob Noyce and Jack Kilby properly share credit. Noyce offered a structural concept of very long life (Fig. 3). Kilby, on the other hand, filed earlier (Fig. 4), and with the aid of his gifted lieutenants, Jay Lathrop and Bob Cook, simultaneously fabricated prototype monolithic oscillators and latches. (Bob Cook later worked for me in Florida, and Jay Lathrop and I are still in regular contact.) In addition, Jack's IC patents, all filed in 1959, articulated the stakes that are inherent in monolithic integration in a most lucid way. These accomplishments have recently been honored

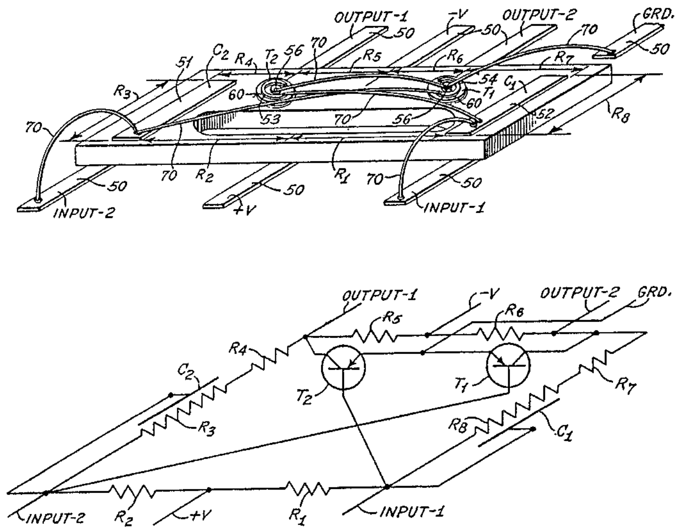

Fig. 4. (Top) Oblique view and (bottom) associated circuit schematic diagram for an integrated circuit presented by Jack S. Kilby in U.S. Patent 3 138 743, filed on February 6, 1959. His invention had precedence in reduction to practice.

by award of the Nobel prize in physics to Jack Kilby. The two years of working under him were perhaps the most educational of my entire life, notwithstanding the forced learning in those near-term earlier years. He was not a compulsive teacher, but rather, one of the most laid-back people on earth. Nonetheless, the knowledge and philosophy were there if you wanted to access them. I learned the Kilby catechism, as it could be labeled, which can be summarized as follows.

The designer of an electronic system should seek to maximize four features of his product: its reliability, economy, performance, and functional density. And the way to do this is to observe four minimization principles, which I have named (with Jack's acquiescence) the Kilby principles. One should minimize the number of parts in the system, the number of different materials in the system, the number of process steps required to fabricate the system, and the differences among these process steps. In other words, with respect to the last principle, one should choose (Kilby's term) compatible processes, those of similar pressure, temperature, and apparatus requirements. Doing as many processeses as possible in the same apparatus is a step in the right direction.

Integrated-circuit history has demonstrated the validity of Kilby's insights beyond any shadow of doubt, with dramatic improvements in all four of the desirable system properties he cited. Forty years of technology refinement have involved new materials (e.g., polysil, silicon nitride, multimetal contact systems) and additional process steps (e.g., going from about five masks to about 25). But the negative effects (in the terms of the Kilby principles) of these additions have been completely overpowered by the benefit of the increased scale of integration (via feature-size shrinkage) stemming from refinement in microelectronics technology. In other words, parts minimization is rightly the number-one principle.

This astonishing record of product improvement is, however, not the only result of technology refinement. The added steps and processes underlie the march to the "two-billion dollar fab," a number that is still rising. Since 1976, some colleagues and I at Minnesota have been developing a concept for a radically different fabrication approach [6]. As the ideas have evolved, we foresee that a silicon single crystal will be grown under *fully automatic* control, in a *single* chamber, with a *single* pumpdown, in a *single* step, that is *continuous*, except for momentary interruptions of growth to perform *fully compatible* procedures on the surface of the growing monolith, procedures that create the 3-D doping pattern. The pressure remains constant at about one mtorr, and the global temperature, in the 400–700 °C range. Because wide-ranging product can be made in this apparatus by reprogramming the controlling computer, these versatile apparatuses will be produced by the tens of thousands. For the first time, the economies of mass production will enter the world of the equipment used for microelectronics fabrication. The new products will have modest capabilities at first. Hence, the refinement process will start all over again but from a new base. We will have reset the Moore's Law clock with conversion to a fully automatic, one-apparatus process.

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Raymond M. Warner, Jr. (SM'59–F'77–LF'92) received the B.Sc. degree from the Carnegie Institute of Technology (now Carnegie Mellon University), Pittsburgh, PA, and the M.S. and Ph.D. degrees from the Case Institute of Technology (now Case Western Reserve University), Cleveland, OH, all in physics.

He was Professor of electrical engineering, University of Minnesota, Minneapolis, from 1970 until his retirement in 1989. He was an Army Signal Corps Radio Officer in the European and Pacific Theaters in WWII, in the first case sharing responsibility for a VHF radio circuit between the headquarters of Generals George Patton and Omar Bradley. Subsequently, he had 20 years of industrial experience, including glass-capacitor development, at Corning Glass Works, Corning, NY, transistor-development work at Bell Labs, Murray Hill, NJ, through most of the 1950s, and thereafter, positions as Director of Engineering with Motorola, Phoenix, AZ, MOS Branch Manager with Texas Instruments, Dallas, TX, and U.S. Technical Director for the semiconductor operations of two places: ITT, West Palm Beach, FL, and Union Carbide, San Diego, CA. He is author of over 70 technical publications and inventor of 29 issued patents. In 1969, he helped to conduct an NSF-sponsored seminar on solid-state electronics at Pilani, India. He was principal editor with J. N. Fordemwalt of the Motorola book *Integrated Circuits: Design Principles and Fabrication* (New York: McGraw-Hill, 1965), principal author with B. L. Grung of *Transistors: Fundamentals for the Integrated-Circuit Engineer* (New York: Wiley, 1983) (reprinted, Melbourne, FL: Krieger), of *Semiconductor-Device Electronics* (Philadelphia, PA: HRW Saunders, 1991), and of *MOSFET Theory and Design* (New York: Oxford Univ. Press, 1999). His work has mainly been on new-device development, the modeling of semiconductor phenomena and devices, on yield-reliability issues, and on the fully automatic fabrication in a single chamber of monocrystalline three-dimensional integrated circuits. The last effort led to the creation of SemiCube, Inc., Eden Prairie, MN, in 1999. This work and consultation he has continued to pursue in retirement. His consultation has served some 16 industrial firms, seven law firms, the National Science Foundation, and the United Nations.

Dr. Warner was a founding member of Minnesotans for an Energy-Efficient Economy (ME-3) and a cofounder of Citizens for Personal Rapid Transit (CPRT).