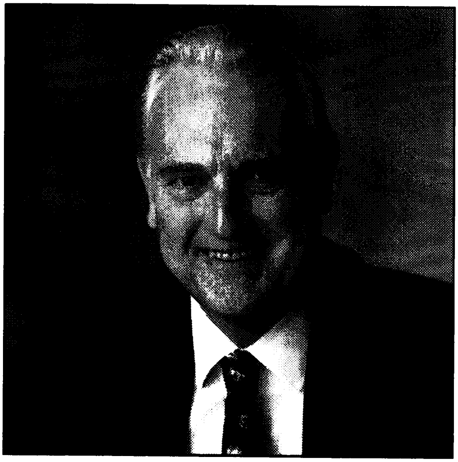
ALAN G. CHYNOWETH



Alan Chynoweth. (Reprinted with permission of American Institute of Physics)

Introduction: To a Shining Laboratory on a Hill

The path that led me to Bell Labs, and the career that I had there, had many unexpected twists and turns. I picture it as a sort of Brownian motion reflecting many unanticipated coincidences or highly fortuitous breaks. But as I look back on it, I am aware of the debt I owe to so many people who gave me those breaks or influenced me at key moments and over various segments of that path.

I suppose the path really started in my boyhood and, no doubt, the genes passed on by my parents had something to do with it. My father, who had been educated as a mechanical engineer, was responsible for the maintenance of a large hospital on an estate of some six hundred acres. The hospital was relatively self-supporting with its own power station, waterworks, and farm. Our home was associated with the farm on this estate, set in the extraordinarily beautiful countryside of southern England and just an easy cycle ride, three or four miles, to the creeks that opened into the English Channel. Tellingly, engineering — especially that associated with mining — is the major theme running through my paternal ancestry, which traces back through many generations involved with copper and tin mining in Celtic Cornwall. "Chynoweth" is a Cornish name, meaning "New House." In "Visitations by the College of Heralds," aimed at verifying the legitimacy of our coats of arms, a family tree shows the origin of the name in the 1400's, in the reign of King Edward IV, with the new house being built in the parish of St. Erth, not far from Penzance. This location is noted for mining activity stretching back to prehistoric times.

Where my mother's side traces back to is much less certain, but her maiden name, Fairhurst, appears to suggest Scandinavian roots. From genealogical researches it is believed that the name is descended originally from Boernicia, a kingdom populated by Scots and Angles in the north of England, along the Scottish border, an area quite likely subjected to raids and settlement by those great seafarers and explorers, the Vikings.

Could it be that those twin threads – the Celts involvement with mining, metals, and engineering, and the Vikings seafaring adventurousness – find echoes in my lifelong enthusiasms for applied science on the one hand and globetrotting on the other?

Even if this is a bit of a reach, my boyhood years provided plenty of stimuli for these interests. I always jumped at the chance to visit "father's" engine room, to see those mighty diesel engines (of early design) driving the electric generators, the water pumps gushing to fill the large tanks in the tower above, the huge coal-burning Lancashire boilers tended by sweating stokers, in a seemingly dreadful inferno, to provide heat and hot water to the hospital. And, in a quiet side room, I was intrigued to watch the stepping switches of an automatic 100-line telephone exchange operate as calls were being placed. (This was in the mid-thirties; when we first arrived in New Jersey in 1953 I was amazed to find that, almost in the shadow of Bell Labs, there was no dial on our apartment's telephone; to place a call we had to lift the receiver and wait for an operator to come on the line!)

Then there was the lust for travel. I especially loved trains, watching them as well as taking them to (seemingly) far off places. And ships. We lived nearby to Portsmouth and Southampton and it was always a thrill to see the warships and liners. How they stirred my imagination, trying to visualize the faraway exotic places that they went to over the sea.

Even as a very young boy, I remember being fascinated by the occasional dramatized accounts of scientific discoveries and about the scientists who made them, which were broadcast by the BBC in "Children's Hour," a program that aired over the radio (in those pre-television days) while the family was gathered around the table at tea-time. The achievements of scientists such as Isaac Newton, Huygens, Davy, Galileo, and Faraday are some that I can recall hearing about.

Perhaps the greatest stimulation, however, was World War II. Starting in September 1939, and lasting for six years, one was exposed to a steady stream of news about developments in military technology, on the one hand, and about military campaigns in many different parts of the world on the other. Where we lived, on the south coast of England, we had a ringside seat for some of the war action. In 1940 we watched aerial dogfights above us, taking a particular interest in the almost daily activities of "our" local airfield, adjacent to our farm, on which were stationed Spitfire and Hurricane fighter squadrons. Then later, as the preparations for D-Day built up, we found ourselves living in the midst of a vast armed camp; we could see military vehicles, tanks, guns and troops, everywhere we cycled, camped in the fields or hidden in woods. And on the creeks nearby, a huge number of landing craft were assembled together with some weird looking contraptions whose intended use we were puzzled by; they turned out to be pieces for the artificial Mulberry Harbors to be established on the invasion shores. It was all heady stuff for a young teenager. When the atom bombs ended the war, I remarked to some farmers with whom I was working during my summer vacation that "next they will be putting atom bombs on rockets." It seemed obvious to me, after our experiences with German "doodlebugs" and rockets, but they were horrified at the suggestion and regarded me as a menace! And I was just as surprised by their reaction.

At high school, my favorite subjects, naturally, were science and mathematics on the one hand, and geography, learning about the world, on the other. But it was physics, seen as a key to understanding many technologies, that captured most of my attention. So, having passed the necessary examinations, in 1945 I went up to London University King's College to "read" physics. The department was just recovering from wartime depredations and there was only an acting head. About two months into the first term, we saw him approached by two men whom we took to be visitors. That evening, on the BBC news, we learned that he had been arrested as an "atomic spy" (the first). Subsequently, the College hired a new professor as head of department, John Turton Randall; it would turn out that he would have quite an influence on my subsequent career.

I enjoyed all the undergraduate courses but Randall's lectures intrigued me the most. He was a very low-key lecturer, not at all the sort one would expect to inspire students. His lectures were on "Modern Properties of Matter" yet, for some reason, I was "turned on" by such topics as crystal structures, band structures, and the mechanisms of various types of luminescence. Perhaps it was because before the war Randall, along with his colleague Maurice Wilkins, had worked with the General Electric Company (G.E.C.) research laboratories in Wembley on luminescence in various phosphors, work that led to a line of luminescent lamps manufactured by G.E.C. It was an example of the development of scientific understanding leading to practical applications, a theme that I found very satisfying.

The physics department at King's was named after its founder, Wheatstone, and his original "Bridge" was on display in a cabinet. Wheatstone himself was a quite prolific inventor and among his other inventions on display were early electric telegraph devices. I also heard about a more modern possibility for telecommunications. A fellow student in the physics department was Arthur C. Clarke, who later became famous as a science fiction writer. But I recall hearing him give a talk at one of our afterhours departmental Maxwell Society meetings (James Clark Maxwell did much of his seminal work while head of the physics department at King's). His topic was based on an article he had just published in a radio journal, *Wireless World*, in which he proposed that rockets could one day place telecommunications relay satellites in stationary earth orbit. I can still visualize him drawing the illustrative diagrams on the board.

For living quarters while at college I was extremely lucky to be accepted for one of the very few places in a university hostel, Connaught Hall. (Most students had to live in private lodgings.) It was an eclectic lot of students who stayed there, about sixty in all. They covered a wide range of disciplines, there were both undergraduates and graduate students, and, between them, I remember counting seventeen different countries represented. At one time we even had a monk from Tibet! Interactions and conversations among the residents could only stimulate my curiosity about other fields of study and also whet my appetite to see other countries in future. Looking back on it, my four years in the Hall were an extraordinarily enriching experience.

I had originally expected to go in for teaching when I got my degree but meanwhile, chatting one time with one of the graduate students in Connaught Hall who was doing research in some area of physics, my eyes were opened to a possible career path that I had not been aware of before. It seemed like it would be fun. Fortune smiled. On completing the undergraduate degree, I was invited by Professor Randall to embark upon research. There were various fields of study available in the department: nuclear physics, theoretical physics, radio and atmospheric physics, and a new-fangled area that Randall was initiating with his former colleague, Maurice Wilkins, called bio-physics. I chose nuclear physics. When Randall asked me why I did not choose his area, I replied that, with the enormous national interest in atomic energy, I felt that it was more likely to offer job prospects afterwards. In retrospect, it was an unwise decision since the resources available for doing leading edge nuclear physics research were not really adequate. I embarked on assembling instrumentation for studying nuclear radiation spectroscopy but, by a stroke of sheer luck, Randall received and brought to my attention a thesis publication he had just received from a scientist in Holland: it was on the use of electrically insulating crystals such as some alkali halides and even diamonds as induced conductivity nuclear radiation detectors. Consequently, my research veered into a study of these potential devices.

Our relatively small nuclear physics group was housed in a somewhat isolated area, mainly in subterranean cellars. There was a variety of work, including quite spectacular cloud chamber studies of nuclear reactions and photographic plate studies of cosmic ray showers. One very talented student, who became a close friend, was Raja Ramanna from India. On completion of his doctoral work he returned to India where he achieved considerable fame and recognition, coming to be regarded as the "father" of India's atom bomb and eventually serving as Minister of Defense.

One of the laboratory assistants who served the group was a young woman who had arrived in England just before World War II as a refugee from Austria. Her surname was Kompfner. One day she brought in a visitor, her brother, Rudolph, who, I believe, was working at the Clarendon Physics Laboratory in Oxford University. Supported in this work by the Admiralty, he was credited with inventing the traveling wave tube microwave amplifier. He showed a delightful, enthusiastic interest in all the work going on in the group. I could not have guessed that our paths would converge again one day.

As a result of Randall's intervention, my work veered away from nuclear physics and more towards the field that later became known as solid state physics. I set out to study crystals as an alternative to Geiger counters for monitoring radiation. Fortunately, the College's professor of geology, William T. Gordon, was a recognized authority on diamonds; he spent many a summer vacation grubbing around in African diamond fields and had amassed an extremely impressive collection of natural diamonds in his filing cabinet.

He happily allowed me to rummage through and select half-a-dozen for my studies. They were beautiful, large, clear specimens shaped as slices through an octahedron, some more than a centimeter across and three or four millimeters thick, just perfect for mounting electrodes. Gordon was perfectly willing to lend them to me, but he made a careful note of their catalogue numbers and of my borrowing them!

It was the frequent custom for members of the department to congregate in the afternoons in the departmental "tea-room". There we had the opportunity to discuss our work informally with any interested colleagues and faculty. There were also journal racks. One day, scanning through the *Physical Review*, there was a short article on bombardment induced conductivity in diamonds authored by someone at a place called Bell Telephone Laboratories. (I remember thinking it was a strange topic for a telephone company!) Maurice Wilkins, who later shared the Nobel Prize with Crick and Watson for unraveling the double helix structure of DNA, was particularly helpful to me thanks to his previous experience with luminescence phenomena and encouraged me to submit a short article on the results I had been getting, which I did.

I also remember the stir among those in the tearoom when an article appeared in the *Physical Review*, also from Bell Telephone Laboratories, announcing the observation of a voltage or signal amplification effect in a semiconductor crystal portending a solid state device, for which the name "transistor" was proposed. The significance of this news – of a potential miniature, low-power replacement for triode vacuum tubes – was not lost on my more senior colleagues.

As my thesis work seemed to be proceeding quite satisfactorily towards its conclusion, I started to think about the next step in my career. I particularly felt it would be a good time to try to see some of the world before settling down in Britain. I scoured the advertisement pages in the scientific journals with a view to seeing if there was anything attractive on offer in other countries, particularly English-speaking ones. I was attracted by an announcement by the National Research Council (NRC) of Canada of the availability of post-doctoral research fellowships in various fields of research. I applied, stating my preference, in order, for three of the several fields offered: i) Nuclear Physics (at Chalk River), ii) Solid State Physics, and iii) Physical Chemistry, these latter two stationed at the headquarters laboratories in Ottawa. To my surprise, I received an offer – in Physical Chemistry! Of course, with the youthful confidence that a physicist ought to be able to turn his hand to almost any branch of science, I accepted. When word got out around the department, I remember one of my biophysics colleagues, the ebullient Raymond Gosling, greeting me with the remark, "I hear MacKenzie-King has sent for you!" (M-K was Canada's famous wartime and immediate post-war prime minister). Looking back on my thesis work, it had the result of starting a new field of research for the physics department: studying the electronic and optical properties of diamond, both natural and synthetic, with particular emphasis on the roles of impurities and imperfections in what is, after all, the most extreme member of the semiconductor series, germanium-silicon-carbon. This program has now been in existence for sixty years.

So, with my new bride, Betty, who I had known since high school, we sailed for Canada in October, 1950. When I arrived at the NRC, I and other newly-arriving post-docs were greeted by the genial head of the Chemistry Division, E. W. R. Steacie. He expressed the hope that we would make good use of the fine facilities at NRC to further our scientific careers but he also urged us to find time to get to know Canada, to "try to acquire a car and see something of the country." (In retrospect, perhaps Lady Luck was shining on me yet again. The joke among the post-docs was that those arriving in the Physics Division, being greeted by G. Herzberg, were told that "first, they should get a pass so that they could work in the labs on weekends!"). We took Steacie's advice. With Betty helping by earning some money by becoming a secretary, we were able to buy a car and with it we explored a lot of territory in Ontario, Quebec, and across the border, in New England. Towards the end of my two-year appointment, we even took off for three weeks and drove out to the West Coast and back via the northern States and the Rockies.

The physical chemistry group was headed by W. G. Schneider. I was thankful that the work was more physics than chemistry. My main line of work was a study, by ultrasonic interferometer methods, of critical point phase transitions in gases and liquids. Schneider also was curious about how organic compounds in the eye's retina converted incoming light into electrical signals. He suggested studying photo-effects in such molecular materials as beta-carotene but I felt it would be better to start by picking something physically simpler to work on, namely, readily-available single crystals of anthracene. I was able to obtain results on photoconductivity in these crystals which suggested that they behaved, conductivity-wise, in some ways similar to inorganic insulating crystals, such as the diamonds I had done my thesis work on.

One day, Schneider drew my attention to a meeting of the American Physical Society that was to be held at the G. E. Laboratories in Schenectady, New York and suggested that I might submit an abstract on the diamond crystal work I did in London. Consequently, I gave my short talk at the meeting and afterwards a gentleman came up and introduced himself to me. (Lady Luck again!) He was J. B. Johnson (of Johnson-Nyquist Noise fame) from Bell Telephone Laboratories. He told me of some of the related work on diamond conductivity that was going on in the physics research group at Murray Hill and invited me to visit them. A few weeks later, I traveled by overnight train from Ottawa to New York and made my way to the old West Street Laboratories. From there, a ride on the company car service took me to Murray Hill where I was met by K. G. McKay, my host for the visit.

What an eye-opener! For the rest of that day and much of the next I enjoyed a series of visits to members of the physics research group that was headed at the time by A. H. White. I was fascinated by the variety and quality of the work that was described to me. Besides McKay's studies of bombardment-induced conductivity in diamond, there were investigations under way of surface physics, thermionic cathodes, breakdown in gases, theoretical physics, electrical breakdown in semiconductors, and secondary electron emission, to name just a few. I was struck by the whole ambiance of the laboratories, the sophistication of the equipment, and perhaps most importantly, the enthusiasm of the members of staff and the courtesy that they and the managers showed me. It really did seem to be "a shining laboratory on a hill." On taking my leave at the end of the visit, I enquired of McKay what possibility there might be of my joining the Laboratories at the end of my two-year post-doctoral appointment in Ottawa since I desired to round out my North American research experience with two or three years in the United States before returning to

Britain. In reply, it was suggested that I make another visit so as to explore the possibilities more broadly.

A second visit was duly arranged. I arrived at Bell Labs not quite knowing what to expect. I was naive. I should have realized that I would be asked to give an account of my research work, but I came totally unprepared for this initial step in the assessment ritual. It came as a complete surprise to be ushered into a room to find a number of researchers waiting to hear what I had to say! Without slides or props of any kind, all I could do was ad lib at the blackboard. Afterwards came a series of interviews in which I learned more about what was going on while, at the same time, the interviewers would be forming their own impressions of me. It was a procedure that I was to become very familiar with in later years.

The year was 1951. Excitement was in the air at the Labs. The transistor had recently been demonstrated, not only with point contact structures but with grown p-n junctions in germanium and silicon crystals. There was the feeling that a whole new field had been opened up in which exploration of the properties of single crystal materials might yield a cornucopia of new phenomena and potential applications. Clearly, my timing was fortuitous as there must have been a real hiring effort underway to build up the relevant research programs. The interviews revealed many more exciting studies in progress, especially exploring the properties of semiconductors. One of the interviews was with a couple of organic chemists who were particularly interested in my work on photoconductivity in anthracene; in retrospect, I now understand why knowing looks were exchanged between them since one of them was Bill Baker.

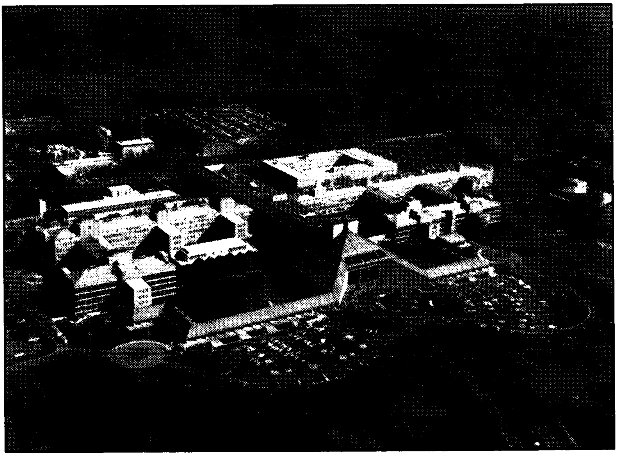
I suppose candidates from abroad were still somewhat of a novelty because, apparently, the Director of Research, Jim Fisk, and the Vice President for Technology, Ralph Bown, had asked to meet me. I was ushered into their presence. After the usual courtesies, Fisk, pulling on his cigar and noting that I was from the physics department of London University King's College, started to enquire as to the professor there: "Was it X?" No, I replied. "Oh! Was it Y?" No again. I thought this was proving to be quite an easy interview as Fisk seemed to be enjoying his guessing game, so I did not interrupt it. Bown looked on with an amused smile. Eventually, Fisk gave up but when I told him it was Professor Randall he exclaimed loudly, "Of course!" Apparently, during World War II, Randall had delivered to Fisk the prototype of the cavity magnetron that he, together with colleagues Boot and Sayers, had invented and which played such an important role in the Allies' conduct of the war. Fisk had been involved in developing the magnetrons and getting them into production at Western Electric. After chatting some more, Bown suggested that while I was visiting I might like to see some of the interesting work going on in the device development area. As tactfully as I could, I declined the suggestion as at that time, as an ignorant foreigner, I had only a rather negative image of what the atmosphere might be like in an American industrial development division. Perhaps Hollywood was to blame but later in my career I was to find out just how exciting the Bell Labs Development Area could be.

Over lunch, the conversation at one point turned to describing some of the benefits provided to employees. I was told by McKay, perhaps a little apologetically, that the vacation allowance was only two weeks but that this could sometimes be stretched a bit. For example, Walter Brattain had done some nice work recently (co-inventing the transistor) and so the company allowed him to go on a two-week lecture tour, visiting universities around the country. Times have changed!

I was certainly enjoying my visit and the interviews until I was brought up short by a session with W. Shockley. He had just completed his book *Electrons and Holes in Semiconductors*, an extraordinary piece of work with chapters that generally alternated between fundamental properties of semiconductors on the one hand and possible electrical circuit applications of transistors on the other. Each chapter ended with a set of problems "for the student." Shockley's interviewing technique was to ask me to work out some of these problems at the board! After this embarrassing ordeal, I felt there was little likelihood of my getting an offer of employment and I returned to Ottawa in despondent mood.

On returning to NRC, I immediately visited D. K. C. McDonald, who had recently been hired into the physics division from Oxford University to build up a low temperature physics group. He indicated he could offer me a junior scientist position but, shortly afterwards and to my enormous surprise, a letter arrived from Bell Labs offering me a position in McKay's new group. So, after an extraordinary, Kafkaesque experience with US government bureaucracy, which needed Bell Labs' lawyers to sort out (a story in itself), I became a member of the technical staff (MTS) in the physics research division at Murray Hill on March 3, 1953.

At the Shining Laboratory on a Hill



The Laboratories at Murray Hill. (Reprinted with permission of Alcatel-Lucent USA Inc.)

The day after I started, who should appear at the door of my laboratory to welcome me to Bell Labs but Rudy Kompfner. A small world! Since my meeting him at King's he had been invited by fellow inventor John Pierce to join Bell. Rudy, with that wonderfully friendly manner of his, quickly helped to make me feel at ease in my new surroundings.

As an MTS in physics research during the fifties, there was never a dull moment. Lunch conversations, seminars, and interactions with various colleagues all provided an outpouring of new discoveries, phenomena, ideas, and inventions, on an almost daily basis. Looking back, I often think of the 1950s as the halcyon days of solid-state physics. And Bell Labs was a wonderful place to be doing such research. As was often realized, we had all the benefits of academic freedom, along with good resources, and none of the teaching or administrative loads that our counterparts in academia usually faced. Furthermore, compared to academia at that time, the pay was relatively good. After I had been there for a couple of years, McKay asked me whether I was still intending to return to England, but Bell Labs by then had seduced me! I chose to stay if they would have me.

Perhaps naively, at the working level we were hardly aware of "management," especially at the lofty strata above our immediate supervisor. It was clear to me in later years, however, that at that time it must first have been managers such as Ralph Bown, Jim

Fisk, Bill Baker, the president Mervin Kelly, and others that had the vision and wisdom to ensure the support of this creative environment. Further, I hardly noticed whatever changes took place in the ranks among senior management but at some point in the 1950s, Bill Baker became Vice President, Research. Then one day I realized, much to my amazement, that Bill had become aware of me. It was when I found myself pushing my tray along in the cafeteria right behind Bill. He turned to me and remarked: "Alan, that's very nice work you are doing on the emission of light from switching in ferroelectric crystals!" He got it half right. At the time I was pursuing two separate lines of research: light emission from avalanche breakdown in silicon p-n junctions and the mechanism of ferroelectric domain switching in barium titanate crystals. I murmured appreciatively, feeling it prudent not to correct him. But that aside, Bill was renowned for his prodigious memory, not least of all for remembering the names of countless employees and what they were doing even though he seldom met them.

But for most people, most of the time, Bill seemed a rather remote, even mysterious person with whom those at the lower levels communicated, if needs be, through the interposed layers of management. Likewise, when Bill's views came back, it often seemed that those middle managers relayed the messages in an almost hushed, or reverent, tone as if they had just been in touch with a deity. Many a time those managers also had another task to perform, namely, translating what Bill had said or meant. Bill's complex prose was legendary. When the need arose, he could be very clear and direct but most of the time he seemed to take great delight in being enigmatic, even ambiguous. Perhaps he sometimes did it deliberately in order to stimulate others to think for themselves. If so, he certainly succeeded. Deciphering his messages became a teasing and frequently amusing challenge for many of us.

After a while I had established some quite fruitful lines of research, which brought me into contact with a growing number of colleagues, not only in research but in the device development area as well. These interactions steadily broadened my interests as well as making my work seem all the more relevant to possible applications (that combination of basic understanding and its applications that I had originally recognized as an important "turn-on" for me). The breadth of my interests started outstripping what I could effectively encompass in my own research and so, though I had never contemplated becoming a research manager, perhaps it was inevitable that eventually, and to my surprise, I was invited to become a research department head with responsibility for a quite small but diverse range of research programs.

Many immigrants from time to time experience feelings of homesickness. This was especially true, I can personally attest, for non-working spouses. And the uncomfortable and unaccustomed heat and humidity of typical New Jersey summers in those pre-air conditioner days, compared to more temperate climates, only served to intensify the feeling. Thus, after three years in America, Betty and our two-year old son sailed back to England for a summer vacation. I, of course, with a measly two-week vacation allowance, was hardly in a position to accompany her in those days before air travel became feasible. But after another two years passed, I had carried over some vacation so that I could afford to make the transatlantic trip. My boss at the time, Gerald Pearson, generously suggested I might extend my vacation by visiting some industrial research laboratories in England and on the continent. Of course, I was pleased to follow up the suggestion and so was able to combine my interest in traveling to other countries with some work-related purpose.

In the coming years I was to have many more opportunities for foreign travel. Some of these were to International Semiconductor Conferences in Prague, England, Paris, Moscow, and Japan. But another purpose arose for making regular visits to Europe. Recruiting of new members of technical staff was usually done via knowledgeable scientists and engineers visiting the universities from which they had graduated. But Bell Labs also frequently received enquiries about employment prospects directly by way of letters from abroad, from science and engineering graduates or from their professors writing on their behalf. Naturally, such letters would make it seem that the applicant could "walk on water." And since there seemed to be no substitute for screening the candidates with face-to-face meetings, the notion got raised that I, sometimes together with Klaus Bowers, should make trips to Europe twice a year to follow up on these letters and interview the candidates and relevant professors. These trips regularly took us to such places as Oxford, Cambridge, Imperial College London, and ETH in Zurich. Other universities were visited as the need arose. These recruiting trips lasted through the sixties and resulted in over one hundred graduates being added to Bell Labs' technical staff. It was a tough job but someone had to do it! There were many offers of help from colleagues but we willingly made the sacrifice. This recruiting activity, which was undoubtedly blessed by Bill Baker, certainly brought an enriching diversity of knowledge, backgrounds and perspectives to the research and development organizations. I even heard my own department referred to as "Chynoweth's foreign legion!" However, by the late 1960s, as Europe steadily recovered from the war, more and more opportunities were opening up closer to home while, at the same time, the Viet Nam war was having a negative effect on America's image. Consequently this source of new employees tended to diminish.

After a few years as a physics research department head another invitation came – as a complete surprise – namely, to become a co-director, with Jack Scaff, of the Metallurgical Research Laboratory. What did I know about metallurgy? I couldn't help but think of that patter song out of the Gilbert and Sullivan operetta, *HMS Pinafore*, in which Sir Joseph Porter, KCB, proclaims, "He had never seen a ship and never been to sea, so they made him the ruler of the Queen's navee!" (Was it in any way pre-ordained by those Cornish genes?) I was intrigued and accepted. Some of my physics colleagues raised their eyebrows at my transferring to 'The Forge,' as they rather disparagingly nicknamed it, perhaps reflecting the traditional pecking order one often finds on the university campus.

In this position I was succeeding another physicist, Al Clogston. Clearly, the opportunity was there to continue bringing a physicist's modern perspective to the venerable field of metallurgy – or so I thought. I was soon disabused of that thinking when I was introduced to the wide range of exciting activities going on in this relatively large organization. Besides the expected physical metallurgical work on various metals and alloys, including magnetics and superconductors, there were fundamental studies on crystal growth, especially magnetic garnet crystals, piezoelectrics, and various crystals for possible use in optical communications devices, on thin films, glasses, and ceramics. There were metallurgists in the organization, of course, but members of other disciplines were strongly represented as well, including physicists, inorganic chemists, ceramists, and some mechanical engineers.

It was truly a large, interdisciplinary laboratory. And besides the range of fundamental research, there was nearly as much work going on in development work, in which there was close cooperation not only with development areas within Bell Labs but with Western Electric manufacturing locations as well. It was a truly rich and exciting mix of activities ranging from science to what could only be described as art.

The example of the latter that stands out in my memory involved a researcher who was attempting to find a suitable combination of oxides for making ceramic substrates with various critical properties essential to supporting hybrid integrated circuits; it was pure witchcraft as far as I could tell, relying entirely on an empirical approach and the accumulated experience of the researcher. Physics had nothing to offer to this sort of project.

Despite what seemed to me to be an extraordinarily important range of work going on, I discovered that morale in at least parts of the Metallurgical Laboratory was quite low. There was the feeling that they were regarded less favorably, budget-wise, by upper management and got less publicity than the glamorous activities of the Physics Research Laboratories. I saw no reason for such an inferiority complex. Apparently, at one time they had sought to get the name of the laboratory changed but Bill Baker had turned down their request. I did not understand this and took it upon myself to give it another try. I proposed that the name should be changed to "Materials Research Laboratory," arguing that this term was a more appropriate fit to our activities. Bill immediately approved it! Maybe my timing of the request was fortunate, but it certainly anticipated other events yet to come.

Besides the Metallurgical Research Laboratory there was the separate and large Chemical Research Laboratory, focused mainly on polymers and plastics, very much Bill's original area of expertise and responsibility. Together, these two laboratories made up the Materials Division and perhaps a possible confusion of organizational names explains why Bill was initially against changing the Metallurgical Laboratory's name. There was a friendly rivalry between the two laboratories, particularly regarding whether metals or plastics would be best for some applications in the Bell System, or whether our work on glass fibers for optical communications, for example, was more important than the chemist's work on anti-oxidants for stabilizing cable sheaths. Of course, some of these rivalries would be aired in the annual merit and salary competitions conducted by our mutual boss, Bruce Hannay. On one occasion, my counterpart in chemistry, Bill Slichter, trying to score some points, remarked that they had a few modest salary adjustments to propose. That triggered me to exclaim, recalling Winston Churchill's famous comment on Clement Attlee, "They have much to be modest about!"

In retrospect, I think Bill Baker must have had a soft spot for the work of the Materials Division, not only because it was close to his own technical expertise but because it gave him a steady stream of contributions to the technology of the Bell System, which he could use to advantage in his negotiations and other interactions with our parent organizations, AT&T and Western Electric. (I suspect it also helped him demonstrate repeatedly the value of research to his fellow officers in Bell Labs, some of whom were possibly inclined to claim that their areas were more than capable of managing without "help" from research!) But there was also another motive at work. Bill recognized, and was a profound believer in, the power of mission-oriented interdisciplinary research, which the materials field exemplified so abundantly. At the same time, his connections in Washington, in the National Academies of Science and Engineering, and in various government advisory committees, gave him opportunities to promote this view with evangelistic zeal. In particular, he was a prime mover in urging the formation and support of interdisciplinary materials science centers in universities. This was at a time when the word "interdisciplinary," let alone the idea of mission- or problem-oriented research, seemed often to be anathema to universities organized along traditional disciplinary lines. Their bias was against working on anything that smacked of applied research. After all, this type of work could put a young faculty person at quite a disadvantage if tenure was being sought! Yet I believe that applied research is often more challenging than so-called pure, or basic, research. In the latter, the researcher can generally pick idealized models or set the boundaries to the research in such a way as to provide a reasonable chance of finding an answer in due course. In applied, problem-oriented research, the desired goal is more or less set for the researcher at the outset and the challenge is to find a way of reaching it, usually on a definite time-scale and with only sketchy prior knowledge of the fundamentals or underlying science. In the field of materials research, and even more so later on when I became responsible for major electronic and photonic device development programs, I was to have numerous occasions in which this observation got reinforced.

A major example of this, which I remember well, was when AT&T Long Lines, in association with its counterparts in Britain and France, decided to put a transatlantic optical cable into service some eight years hence even though, at the time, we were not at all sure that we would be able to develop the optical devices (semiconductor lasers) with the excruciatingly high degree of reliability required. In my mind I likened it somewhat to President Kennedy's announcement of going to the moon in ten years' time even when there were so many unknowns about the necessary underlying technologies.

I was to have another and totally unexpected opportunity to help promote the image and value of the interdisciplinary materials research field. From time to time, the National Academy of Science supported studies of the status and potential of the major disciplines of science – particularly physics and chemistry. I do not know what discussions led to a study of the materials field, but I am sure Bill played an important role. As a result, a study of the field of materials science and engineering (MSE) was commissioned under the auspices of the National Research Council, an operative arm of the science and engineering academies. It was called the Committee on the Survey of Materials Science and Engineering, COSMAT for short. The highly esteemed professor of metallurgy at M.I.T., Morris Cohen, was appointed chairman of the survey and Bill was appointed as co-chairman. To assist in the sizeable amount of work entailed, Morris brought in one of his metallurgical colleagues but Bill quickly realized that, in spite of all the good will in the world, the study risked over-emphasizing the metallurgical field to the detriment of the far wider field of materials that Bill had in mind. I was therefore brought in to help with the study. There followed an intensive couple of years of work, organizing panels, soliciting input from leaders in academia, industry, and government, and writing reports. It was an extraordinary exposure to the technological heart of the U.S. economy.

One of the first tasks that faced the committee was to figure out who worked in, or which disciplines comprised, this field of MSE. We solicited views from various research organizations and professional societies by means of surveys. I recall the surprise with which one well-known metallurgist greeted the results of the survey: "Even the solid state physicists see themselves as working in the field of MSE!" And together with ceramists, polymer and plastics chemists, and many branches of the engineering disciplines, it was clear that the field of MSE was, indeed, interdisciplinary. The overall Survey went on to outline what Bill described as a veritable blueprint for America's technology. Morris Cohen, armed with the imprimatur of an Academy study, went on to proselytize the message about the value of the interdisciplinary field of MSE in academia and the professional world in general. The Survey may well have given added impetus to the support of the Materials Science Centers at universities. It also saw the birth of the Materials Research Society, largely through the efforts of Survey member Professor Rustum Roy of Penn State University. This has become a leading professional society with an impressive meetings calendar and steady output of technical papers.

If the materials field was not broad enough for me, there were always the richly diverse activities of the Bell Labs research itself. Many of these we first got to hear about at Bill's monthly three-level staff meetings. Crass administrative matters only rarely intruded on these meetings. They were much more opportunities for the twenty or so directors and executive directors responsible for the various laboratories making up the research area (including John Pierce and Rudy Kompfner) to "show and tell." We made sure we were always in place waiting for Bill to sweep in, utter his enthusiastic greeting – "Good Morning, Gentlemen!" – and start the proceedings with some fresh and often fascinating news or insights from his current experiences in Washington or, sometimes, from developments with our parent company, AT&T. (On one occasion he was giving us some news from the White House when his secretary came in and whispered to him; he left the room quickly saying, "Speak of the devil!") After Bill's opening remarks, the rest of the morning would be taken up going round the table. To me, it was always stimulating to hear about the new discoveries, results, theories, ideas and inventions emanating from the various centers, ranging from the physical sciences to communications technologies, software to economic theory, mathematics to behavioral sciences, chemistry and even patents. It was nearly always leading-edge stuff. And Bill had a remarkable ability for moving the meeting along in good-humored fashion and at the same time, showing knowledgeable appreciation and having something nice to say about each presenter's contribution.

Although we all took the show-and-tells seriously, there were often lighter moments, even when we had to spend a few minutes on administrative matters.

On one occasion, we were all given print-outs of the capital equipment on our books to help us identify items that should be junked. As I looked at my list I blurted out in surprise at finding there was a rolling mill with an acquisition date of 1911! Bill leaned over, remarking in his soothing voice, "Perhaps you should donate it to the Smithsonian!" On another occasion, Bill's administrative assistant, "Pat" Keenan, was reviewing his estimates of how many people we would be able to hire the following year, including how many people would have to be replaced because of transfers, resignations, and retirements. He also said he allowed for one death, at which John Pierce piped up, no doubt reflecting the concern we always had for our personnel, "Has he been told yet?"

Eventually, after twenty-two years in the Research Area, I got another surprise. I was invited to take a position in the Development Area as Executive Director of the Electronic and Photonic Devices Division, responsible for the development of essentially all devices intended for use in the Bell System except silicon integrated devices, those being the responsibility of two other, parallel divisions. This Division was a very good fit for my interests and background, as it was a rich mix of device physics and materials technology embracing a wide range of products either introduced, or about to be introduced, into production lines at more than half-a-dozen Western Electric manufacturing locations. But it meant I was no longer directly in Bill's domain though I remember one senior colleague remarking to me at the time, somewhat darkly I thought, that he could "see some plan afoot." I had no idea what he meant but I deemed it better not to seek clarification. Now I reported to Vice President John Mayo, another person for whom I had great respect and who, in the following several years, gave me great support and opportunities. During this period Bill became President of Bell Labs and I experienced an event which gave me a personal and very touching example of his concern for his people. After about four years in the new position, I had a sudden angina attack which resulted in my having by-pass surgery two weeks later. To my amazement, while I lay in hospital, I received a beautiful letter of encouragement from Bill. He had taken the trouble to find time in his back-breaking schedule to compose quite a long letter, which I do not need to go into, but I couldn't help but read with a chuckle his little dig about how, given time for nature to do its magic, it would be those organic chemical molecules that would get me mended.

Recalling this anecdote brings me to make a few remarks about how I saw Bill as a person. Bill was a most unusual executive, a complete antithesis of how some may think a person in such a position behaves. In his Bell Labs office he was no clean desk freak; far from it. Visiting him there one time, I thought he was not there until I heard a voice from behind the enormous piles of papers and reports on his desk. And when he went home from work, he always had one or two bulging brief cases. He lived about five miles from the Murray Hill Labs and for many, many years commuted in his ancient and rather shabby car, a Pontiac I think it was. One day upon leaving the building he discovered his car was missing. Unbelievably, it had been stolen! It turned up later in downtown Newark. Bill's car was somewhat symbolic of his modesty. He lived a very quiet and unostentatious private life, which also helped to further surround him with that air of mystery. About his own achievements he was modest to a fault yet he always made sure that others got credit for theirs, sometimes embarrassingly so. But Bill reveled in his professional, and perhaps even his managerial interactions, with other intellects while drawing on his legendary knowledge and memory. He seemed to get particular pleasure out of composing challenging prose, frequently using unorthodox grammatical constructions. And, often, with a twinkle in his eye he would enjoy word-sparring with us. One instance comes to mind: We were seated together on the plane going to Washington. I was going there to manage a COSMAT panel at the Academy with Bill as my co-chair, but first Bill had to be at the White House. He asked me: "Alan, what time would be the best time for me to turn up at the panel meeting?" Now, Bell Labs subordinates were not accustomed to being put into the position of telling Bill what to do! After a brief pause, I replied: "Any time you come, Bill, will be the best time!". He thought for a moment and then said, smilingly, "That was a very elegant answer."

Phew!

Off to Bellcore

In 1982, disaster struck the Bell System with the court-ordered break-up of the company, as a result of the famous anti-trust suit. The major result was for the formation of seven regional Bell Operating Companies to be spun off from the parent AT&T. The newly formed companies decided that they needed a piece of Bell Labs to serve to support them as a consortium. I did not think it would affect me, as I had never had any interactions with operating companies. In fact, one time when I was wearing my hat as secretary to the Electronic Technology Council that John Mayo had set up, made up of executives from AT&T and Western Electric as well as Bell Labs, I suggested that, on hearing of the break-up arrangements, AT&T's technical strategy should be clear: We should push broadband optical technologies for the long distance parts of the network and for dedicated services to major customers, and push cellular radio systems to by-pass the local companies' loop plant! But I was in for another surprise in my career. Besides the more obvious engineering and software support, the companies concluded they needed in their consortium a substantial applied research organization as well, and I was invited to create it and to head it as its Vice President. I was intrigued by the challenge, which I found impossible to resist. Now I had to reverse roles and try to figure out how to compete against the AT&T strategy I had previously thought obvious. There followed an extraordinary few months during which a cross-section of suitable experts from all across Bell Labs had to be identified and given the chance to join in this new adventure. In the end, we fashioned a quite remarkable outfit which, I believe, served our new owners well. (Of course, in this belief I am probably a bit biased just as Bill may have been about his beloved Bell Labs Research Area).

With the formation of Bellcore, as the consortium came to be called, fear of breaking the anti-trust conditions caused, in effect, an Iron Curtain to descend between hitherto close colleagues. We were afraid to have any discussions with each other except in the presence of lawyers. Gradually, we learned what we could do and not do but it was a very distasteful and dissatisfying experience, to say the least. And in particular, with the splitting of Bell Labs, so ended my ties to anything going on there and to anyone working there, including Bill. I was to be at Bellcore, having yet another extraordinary, exciting, and very different type of experience till my retirement ten years later, but that is another story.

Closing Thoughts

Looking back over the years that Bill was responsible for the research area, the word that comes to mind is Camelot. What Bill created and fostered was truly unique: an eclectic range of research work of the highest quality, which opened doors for Bell Labs researchers all around the world, thereby giving us and our colleagues in the development areas early information on discoveries and developments happening elsewhere. Could something akin to a Bell Labs be created again? In other fields, even? I am asked that from time to time but I think the answer is, "not likely," since the US government, with its anti-trust laws, destroyed a unique support arrangement, that of a regulated monopoly that had an on-going mission to advance the nation's telecommunications network. This mission gave the corporate management a direction that helped focus the technical programs. The monopoly status gave management some confidence that they could plan for the long term and therefore undertake bold new engineering programs as well as some related research that might take a decade or even two before reaching telecommunications customers. In particular, the Bell System was able to operate as a vertically integrated enterprise involved in all stages of the food chain from basic research through device and systems development to network deployment and service provision. On the downside, the Bell System may have sometimes been a bit slow to install a particular technology but when it did decide to do so, and had the regulators' approval to do so, it had the muscle to move quickly.

Nowadays, perhaps only the government can act as a monopoly and perform or support mission-oriented long range research as, for example, through the Defense Advanced Research Projects Agency (DARPA), with its support of exploratory projects in both academia and industry. For most industries, including the fragmented remnants of the old Bell System, the competitive but often wastefully duplicative marketplace – though it may sharpen up product designs to more closely meet immediate customer needs or interests – forces companies to focus on the short term and incremental products to such an extent that very little room is left for the longer term research and development periods that truly revolutionary and complex engineering advances usually require. For this, most companies now have to rely mainly on the universities and hope that they will be smart enough to pick up quickly on any promising results.

But there were also other forces working against the Bell monopoly. Before the discovery of the transistor, telecommunications – focused on conveying voice messages reliably and without distortion to their intended destination across complex networks – was a rather specialized, even arcane branch of engineering. The transistor was to sow the seeds of change.

Though it led to a flowering of solid state science and technology and would eventually revolutionize telecommunications, it also, once it had evolved into integrated circuits and digital technologies, lent itself to revolutionizing almost every other area of electronic engineering, information technology, and product development. It became everyone's technology, the genie was out of the bottle. And though the food chain still operates, nowadays each step along the chain is populated with numerous competing companies specializing in that step so that a company in the next step has a multiplicity of sources of technology from which to choose. In particular, it has made it relatively easy for new competitors to develop their own versions of telecommunications devices, systems, and services. Perhaps it can be said that with the discovery of the transistor, forces were set in motion that would eventually and inevitably lead to Bell Labs losing its unique status.

I regard myself as being very lucky to have been in Bill's Research Area during what many regard as its glory days. But besides being lucky in the general sense, I am also acutely aware of, and humbled by recalling, the very many occasions on which a chance remark, a bit of advice, and genuinely friendly interest from schoolteachers, professors, colleagues, and, especially, the various bosses I had, in Canada, at Bell Labs, and later at Bellcore, influenced my career. Once, when receiving some praise from one of these bosses for some research results I had achieved, I self-deprecatingly murmured that I felt I had simply been lucky. To which he replied, "Well, Bell Labs likes lucky people."