MANY DOORS

Reflections on my scientific career

J. A. Armstrong

July 2020

In its various stages, my scientific career lasted more than fifty years. This account is intended for a non-specialist audience, but by its nature it can not avoid a certain amount of technical detail and jargon. Furthermore, since its focus is decidedly personal, it should not be viewed as a history of IBM R&D, nor of the various scientific topics that are touched on.

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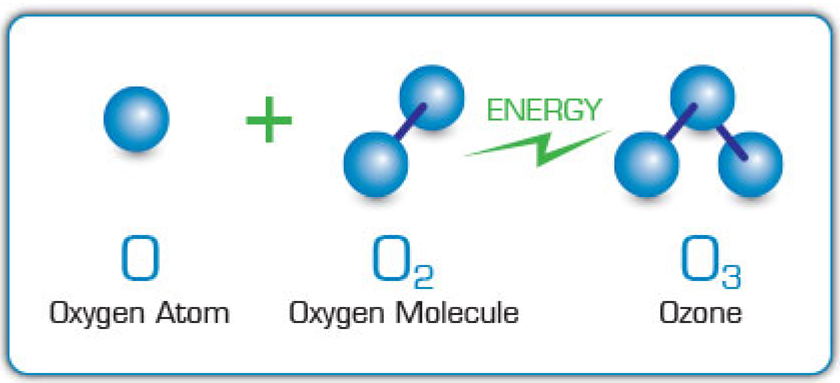
In the summer of 1947, when I was 13 and bored, my mother suggested that I find an interesting book to borrow from the local library. As a shot in the dark, I took out one on chemistry and was intrigued.

My parents gave me a chemistry set that Christmas, and I set up a 'lab' in the cellar of our house on Willett St. in Schenectady, NY. The lab was on a table next to the built-in stationary laundry tubs. Over the the next five years I would spend countless hours alone in the cellar, doing the experiments suggested in the book that came with the chemistry set, and then experimental projects of my own devising.

Liquid Ozone

The first of these was an experiment I conceived of for entering in the NY State Science Fair, held every year in Albany. Having read about exothermic and endothermic chemical reactions (respectively releasing energy, or requiring energy), and learning that ozone (a molecule with three oxygen atoms instead of the usual two) was produced in an endothermic reaction, I decided to produce, and then try to liquefy, ozone.



Endothermic reactions require the input of energy in order to occur, and I had just the thing: a salvaged neon-sign transformer that generated 12,000 volts and would produce fat sparks 12 inches long! It was while playing around with these sparks that I had learned to recognize the pungent, unmistakable odor of the ozone produced when the spark energized the oxygen in the air.

To liquefy ozone I decided I needed two things, a purpose-built glass apparatus with which I could expose a stream of air to a continuous electrical discharge, converting some of the oxygen to ozone, and a liquid nitrogen cooled coil which would liquefy the ozone.

Now, of course, I know that this was all naive fantasy; the experiment never had the remotest chance of working.

And it illustrates a lesson I have had to learn several times over. Being self-taught confers the great benefit that one learns how to learn, but also has the great disadvantage that one's learning is likely to be full of gaps.

However, I was blessed at the time by having mentors who never told me the idea of liquefying ozone was crazy. Instead they encouraged me and offered real assistance. For example, where was a junior high school kid going to get liquid nitogen? Or where was he going to get specially fabricated glassware?

I had met Joe Marcelli when he was chaperoning one of our junior high school Friday night dances. Joe lived nearby in Schenectady, but commuted to RPI in Troy, where he was a graduate student in chemistry. We talked about my experiment, and he volunteered to have a glassblower at RPI fabricate glassware to my design.

The General Engineering Lab of GE was then in downtown Schenectady, within a forty minute walk of home, and I had gotten to know an engineer/scientist there who offered to lend me a two-liter dewar (a scientific thermos bottle) of liquid nitrgen. When my experiment was ready, I could walk to the GE lab, get the dewar of liquid nitrogen, rush back up Crane Street hill to my lab and conduct the experiment.

Of course, no liquid ozone ever dripped out, but when the time for the science fair came around I took my apparatus and some handmade posters to Albany and won a prize of 10 silver dollars, which I promptly used to buy a slide-rule. My ambition to become a scientist was strengthened.

In retrospect I am struck by the generosity of those who offered help and support without trying to temper or ‘instruct’ my youthful enthusiasm.

The Colors of Mercury

In the 1940s and 50s, Dover Publications was selling inexpensive paperback editions of math and physics books.   I acquired Gerhardt Herzberg’s ***Atomic Structure and Atomic Spectra*** and read it avidly. Unlike sunlight, which Newton had shown was composed of a continuous spectrum of colors, the vapor of a single element, when strongly excited, emits light in separate, individual colors. I was fascinated with the colored illustrations showing the line spectra of hydrogen, sodium and mercury, and I wondered if I could build an apparatus to produce my own photographs of these atomic line spectra.

One type of instrument which records spectra is called a prism spectrograph. It consists of a narrow slit to admit light into a darkened space, a glass prism to spread the light into its constituent colors, and lenses that projected an image of the slit for each constituent color onto a photographic plate.

At this time Edmund Scientific Corp. was selling inexpensive optical parts salvaged from war-surplus military optics. I bought a prism and various lenses. A family friend (also GE engineer) gave me the photographic plate holder from an old portrait camera. Edmunds also sold ‘Geissler tubes”, glass tubes containing various gasses and containing electrodes for connecting the tubes to a source of high voltage, which would cause the gas to glow and emit its characteristic set of colors. I would use the 12,000 volt transformer to excite the tubes.   I recall having three tubes filled with hydrogen, mercury, and neon gas.

I built a wooden housing for the optics and the photographic plate holder, coated the inside with lamp black to suppress stray light, and made an entrance slit from two double edged razor blades placed very close together.

Since I would be making photographs of the atomic spectra, I also acquired the chemicals and paraphernalia to construct a photographic darkroom in what had previously been the coal bin.

Of all the spectra I was eventually able to photograph, the mercury spectrum was by far the most 'presentable', and I made the mercury spectrum the centerpiece of my project.

The emission spectrum of Mercury vapor. My photos were in black & white.

By this time I was in senior high and had a wonderfully supportive physics teacher, Margaret Karn, who encouraged me to enter my spectrograph project in the 1951-52 Westinghouse Science Talent Search (WSTS). I did so, was one of the 40 national applicants chosen to go to Washington D. C. to exhibit my project, was one of the top 10 winners, and was awarded a 4 year college scholarship. I recall that one of the judges was Prof. Harlow Shapley, director of the Harvard College Observatory.

Among those visiting the WSTS exhibition where we showed our projects was the chemist and spectroscopist Wallace Brode. He arranged that I be offered a summer job in the Radiation Physics section of NBS, the National Bureau of Standards (as NIST was then called).

MARCHANT BLUES

So I spent the summer of 1952, before entering Harvard College, in Washington working in the NBS group headed by theoretical physicist Ugo Fano. Fano had been a student of Enrico Fermi. My task for the summer was to calculate a certain graph describing the stopping of high energy particles by a concrete barrier. The problem had been set by one of the other theorists in the group, Conn Blanchard. It involved integral calculus.

I had taught myself differential calculus in high school, but I was entirely ignorant of integral calculus and not in any way prepared to understand, let alone question, the origin of my assigned task.

This was before the widespread availability of digital computers, so I had to do the calculation with the help of a Marchant electro-mechanical desktop calculator.

The Marchant electro-mechanical calculator, the type I used. An expert user could enter four or five numbers at a time.

It took me two weeks to calculate the graph; each point required hours of arithmetic calculations on the Marchant. When I showed the completed graph to Blanchard, he said immediately: “John, that’s not right. Do it again.”

Calculating it again took another two weeks and produced the same graph and the same response from Blanchard. After another two weeks of my punching away at the Marchant and getting the same graph, Blanchard decided to revisit the mathematical derivation he had used to set up the problem, and discovered he had made a mistake.

I was relieved to find that there had been nothing wrong in what I had done. Working with ‘real scientists’ was exciting, and I had been included in the group meetings and listened to many discussions of physics. And I had been included in the group’s social activities as well, some of them organized by Fano’s wife, who was also a physicist.

DEEP LEVELS

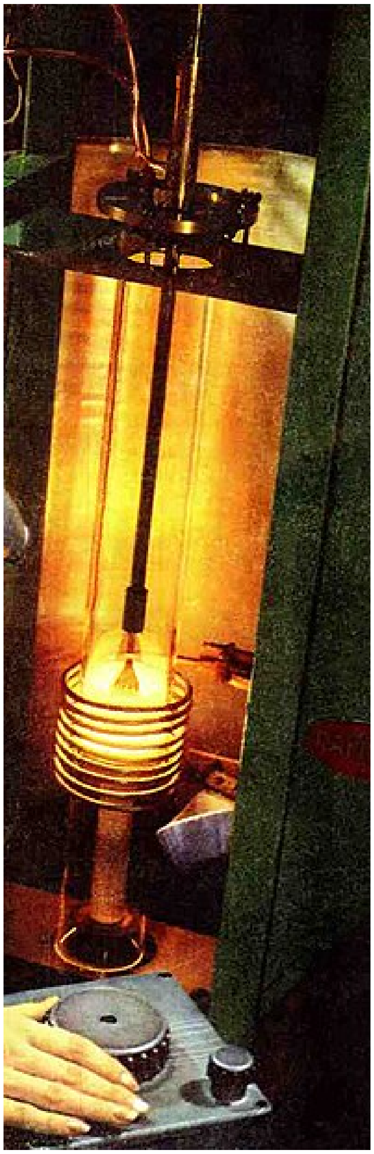
In the Fall of 1952, after my summer at NBS, I entered Harvard College, where I majored in physics and eventually graduated *summa cum laude* . Those four years at Harvard were immensely helpful to my subsequent career, but they did not provide any occasion for projects of the type I have been describing. Indeed, of the several lab courses I took, the only lesson I recall was learning how to account for the inevitable errors that accompany all experimental measurements.

However, during several summer vacations from college, I had the great good fortune to land summer jobs at the GE Research and Development Lab in Niskayuna, NY, a suburb of Schenectady. These summer jobs gave me first hand experience of basic, as well as applied science.

Growing up in Schenectady, then the site of GE's major operations, I was familiar with the name and accomplishments of Irving Langmuir. He was the first American to win the Nobel Prize in chemistry, and he won it for contributions to basic science made while researching how to prolong the life of incandescent light bulbs.

My summer jobs at the GE R&D Center were in the solid-state physics groups led by Dr. Roy Apker . The first summer I worked with Dr. Art Tweet, who was studying plastic deformation in germanium and silicon. My main memory of this summer is that I got my first patent, for a method of making ohmic electrical contacts to silicon.

The second summer, I worked in Dr. Winfield Tyler's group. It was studying the electrical properties of silicon and germanium single crystals when they had been grown with small amounts of other elements as impurities. My task was to grow single crystals of germanium while doping them with trace amounts of the elements selenium or tellurium.



The Ge crystal is the conical object in the bright white region of the picture. It is growing as it is rotated and slowly pulled from a pool of molten Ge.

The researchers would then measure a certain electrical property of the doped crystal to determine where, in the electrical conduction 'band gap' of germanium, were the electron energy levels associated with the Se or Te impurities. They determined that these energy levels were 'deep' within the band gap, that is, far from either the top or bottom of the gap, and therefore of no use in making bipolar transistors. This summer's work resulted in my first scientific publication.

GRAD SCHOOL & PhD

I had intended to go directly to graduate school after graduation from Harvard in 1956, but that plan was upended by the wonderful opportunity to spend a year abroad as a Sheldon Traveling Fellow.

When I asked my faculty advisor in the Physics Department about whether to accept the traveling fellowship or not, he said "Don't do it. It will ruin your career." But I did take the traveling fellowship, ignoring what I have ever since considered to be the worst advice I have ever received.

Returning from a year abroad, I entered graduate school in the Fall of 1957 on a graduate fellowship for study in the Division of Engineering and Applied Science (DEAS). October 1957 saw the Soviet Union's launch of Sputnik, the first artificial satellite. One of the minor consequences was that I, along with many other grad students in science, concluded (erroneously, as it turned out) that learning Russian would be an important career skill. I spent half my time that first year of grad school taking intensive Russian.

 But the most significant event during the 1957-58 academic year was that Elizabeth Saunders and I became engaged.

In the Fall of 1960 I finally passed my Ph. D oral qualifying exams and started work on a thesis under the supervision of Prof. George Benedek. It was to be an experimental study of the small changes in the nuclear magnetic resonance (NMR) properties of certain cobalt-containing molecules when subjected to high pressures and varying temperatures.

NMR is the phenomenon that underlies the now ubiquitous medical technology of Magnetic Resonance Imaging (MRI), but in 1959 NMR was being used mainly as an exploratory tool in physics, chemistry and materials science.

The tradition at Harvard was that anyone doing an experimental NMR thesis had to build the NMR apparatus himself. It consisted of a specially designed piece of electronics called a 'marginal oscillator' , whose job was to generate radio signals in which the material under study would be immersed, and then detect the slight radio-wave absorption which took place when nuclear resonance conditions were achieved.

Being given the task of building the NMR spectrometer was good news and bad news. 'Good news' because I would once again be doing a build-it-yourself science project, 'bad news' because I had absolutely no experience with practical electronics. This was long before the days of integrated circuits; electronic devices were built with vacuum tubes and discrete components such as resistors, capacitors, and coils.

My solution to this dilemma was to pay for an RCA-Institutes correspondence course in electronics while at the same time starting work on my Harvard Ph. D.

A 6AK5 vacuum tube, several of which were required in the 'marginal oscillator' that formed the heart of my NMR spectrometer.

In addition to an NMR spectrometer, the experiment involved a powerful electromagnet whose magnetic field strength had to be **very** carefully stabilized so that it varied by no more than one part in 10 million over ten hours.

Furthermore, the NMR measurements of the cobalt-containing molecules were to be done as a function of hydro-static pressure (up to 10,000 atmospheres), for which I used apparatus passed down by previous successors of the Harvard pioneer of high pressure physics, Prof. Percy Bridgman.

Once I had successfully built the spectrometer and mastered the magnet stabilization scheme, I was able to complete my theses project in a few months, and I received my PhD in January 1961. My thesis was entitled "The Temperature and Pressure Dependence of the Co59 Chemical Shifts in Octahedral Cobaltic Complexes".

It was clear to me at the time that the results of my thesis project were not of great scientific importance. However, the speed with which I had accomplished a fairly exacting experiment led Prof. Nicolaas (Nico) Bloembergen to offer me a post-doctoral position in his research group.

The connection with Bloembergen was to be of great importance to my scientific career.

NONLINEAR OPTICS: A.B.D.P.

In the Fall of 1961 Nico's scientific focus took an abrupt turn.

The ruby laser had been invented in May 1960, and it set off a flurry of experimentation in laboratories around the world. Then, in the summer of 1961, Prof. Peter Franken at the University of Michigan reported the observation of optical second harmonic generation. He and coworkers sent an intense pulse of red ruby laser light through a transparent quartz crystal and in the transmitted light observed not only the red laser light, but also a weak ultraviolet light pulse of *exactly half the laser wavelength*; the optical second harmonic, or 'first overtone' of the laser light.

This was 'new physics' . What were the conditions under which the second harmonic would be generated? What determined how much would be generated? Could a third or fourth harmonic (I. e. light with one third or one fourth the laser wavelength) be generated?

Within a few days a group had coalesced around Bloembergen to work on these and other questions, a group that came to be to be known as ABDP, from the authors' initials of the paper that we published later in 1962: Armstrong, Bloembergen, Ducuing, and Pershan. The ABDP paper[1] would eventually become one of the most cited physics papers of the last hundred years.

Jacques Ducuing was a French graduate student and Peter Pershan was an Assistant Professor in DEAS. Although the authors of ABDP were listed in alphabetical order, Bloembergen was the unquestioned scientific leader. His many contributions in nonlinear optics were a major factor leading to his 1981 Nobel Prize.

Every morning we would gather in Nico's office and report progress made on tasks we had agreed to undertake the previous day. What was the proper way to describe the nonlinear electric polarization caused by the intense laser light? How much could a light beam of one color (i.e. wavelength) be changed into beam of a different color?

This routine went on for six or eight weeks, and we eventually worked out the answers to these and many other questions. This collaboration was the most exciting and intense period of research in my scientific career.

It was also one of the most consequential. It was not uncommon in those days for scientists to assume that the first author of a paper was the scientific lead. I never said or behaved as if I had been the leader in the ABDP collaboration, but I know that the misconception did not hurt my career.

Along with this theoretical work, Ducuing and I set up a lab for doing experiments on second harmonic generation. These experiments eventually gave rise to my one and only scientific paper written in French [2] (by first-author Ducuing as part of the arrangement that allowed him to do graduate work in the U. S. with Nico, but get his PhD from the University of Paris).

HOW IS LASER LIGHT DIFFERENT?

During the time I was working on nonlinear optics at Harvard, I occasionally pondered the following question: How is laser light different from ordinary light? Obviously it is more pure in color, and more intense, but is there some more subtle way in which it is

1 J. A. Armstrong, N. Bloembergen, J. Ducuing and P. S. Pershan, Interactions Between Light waves in a Nonlinear Dielectric, Phys. Rev. **127**, 1918 (1962)

2 J. Ducuing and J. A. Armstrong. Influence de la largeur spectrale sur l'interaction d'ondes planes au sein d'un dielectrique non-lineare, *Quantum Electronics*, Proceedings of the Third International Congress, P. Grivet and N. Bloembergen eds., HColumbia Univ. Press (1964) p. 1643

different? This was the first research topic I pursued after joining IBM Research in April of 1963.

In thinking about what I would do when my two-year post-doc was over, I gave only cursory thought to seeking an academic position. My summers at the GE lab had made me fully aware of the opportunities in industrial research, and from the multiple job offers I received I chose IBM Research, which was in an aggressive hiring phase in 1963.

The word ‘laser’ is an acronym: Light Amplification by Stimulated Emission of Radiation. But despite the acronym, the term ‘laser’ as universally used denotes an oscillator, not an amplifier. Ordinary light, whether from the sun, or an incandescent lamp, a neon sign, or halogen lamp, is not being produced by a single oscillator but by the random, incoherent addition of countless independent spontaneous emissions of light from countless independent atoms.

One consequence is that the intensity of ordinary light fluctuates randomly, though very slightly, about an average value.

In a laser oscillator, the countless individual atomic emissions of light are coordinated to be all in step, to be coherent. What effect does that have, I wondered, on the fluctuations of the beam intensity?

It turns out that there is a significant difference, and that difference was elucidated in a pioneering set of experiments done with my colleague, Archibald (Archie) W. Smith.[3]

As a laser is powered up through ‘threshold’ , the statistics of its intensity fluctuations change dramatically, from gaussian random noise below threshold to poisson random noise when actually lasing.

I MEASURE A PICOSECOND WITH A RULER

3 J. A. Armstrong and A. W. Smith, Experimental Studies of Intensity Fluctuations in Lasers, in *Progress in Optics*, Vol.VI, pp. 213-257 (1967)

The mid 1960s saw the invention of many new lasers. An important example was the pulsed neodymium-glass laser. As originally developed by Elias Snitzer of American Optical, the Nd-glass laser emitted an intense burst of many, *randomly* spaced short pulses of light in the near infrared. Then it was discovered how to modify the laser so that it emitted a train of *regularly* spaced short pulses. But the duration of an individual pulse was too short to be measured by any existing combination of photo-detector and fast electronics. The fastest such combination could only measure pulses as short as a nanosecond...one *billionth* of a second.

It occurred to me that optical second-harmonic generation could provide a way to measure pulses **much shorter** than a nanosecond.

The speed of light is 300,000 km/sec, but a nanosecond is so short that a light pulse travels only 30 centimeters in a nanosecond...about a foot. In a picosecond (one thousandth of a nanosecond) a light pulse would travel only 0.03 centimeters. Could these easily measurable distances, along with the speed of light, be used to measure short pulse durations? I figured the answer was 'yes.'

With conventional optical beam-splitters, and mirrors I could split the laser pulse-train into two separate pulse-trains, introduce an ***adjustable time delay*** (in the form of a path-length difference...the 'ruler'), between the two pulse trains, and then recombine the two pulse-trains to go in the same direction.

Then I would need a mechanism whereby there would be *a strong signal if two pulses overlapped in time at a detector, but no signal if they arrived separately:* such an apparatus is called a *pulse coincidence detector.*

Crystalline Gallium Arsenide is a material with strong capacity for optical second harmonic generation. And crucially, because of its crystal symmetry, no harmonic can be created by a pulse of light that is polarized along any one of the x, y, or z axes of the cubic crystal. An incident light pulse must have polarization components along *at least two* of the crystal axes in order for a pulse of second harmonic light to be generated.

With polarizers I could arrange the two pulse-trains so that one would have x-polarization, and the other y-polarization. Here x and y refer to two cubic axes of the GaAs crystal placed in the path of the two pulse-trains and acting as the coincidence detector. I would observe the second-harmonic signal emitted from the suitably oriented, polished single-crystal GaAs surface.

The experiment worked as I had hoped and was published [4] in 1967. **It represented a 1000-fold improvement over existing techniques**, and was widely adopted by other researchers. (The current state of the art produces and measures attosecond pulses. An attosecond is one billionth of one billionth of a second.)

This experiment was to have major consequences for my career at IBM, a fact that only became clear to me gradually. Within weeks of completing the experiment I was asked to describe it to a gathering of senior managers. I remember at the time being pleased for the high-level attention, but only later did I realize that this gathering probably contributed to the decision to enroll me in IBM's roster of 'high management potential' members of the research staff. Of course, nothing was said to me about this at the time.

IBM took succession planning very seriously. Doors would be opened to provide opportunities for growth and testing of 'high potentials.'

The first of these doors took the form of an offer of a one year assignment managing a small optical physics group in the IBM research lab near Zurich, Switzerland.

I realize now that I could have declined the opportunity, saying I wanted to devote myself to following the many research directions opened up by my work on picosecond pulses. But I accepted; IBM would move me and Elisabeth and our daughters, provide generous allowances for living expenses, private schooling for Sarah and Jennifer, and many other 'perks'.

A YEAR IN SWITZERLAND

4 J. A. Armstrong, Measurement of Picosecond Laser Pulse Widths, Appl. Phys. Letters **10**, 16 (1967)

A recent view of the IBM Research Lab in Rueschlikon.

My management career started modestly. I was leader of group consisting of Dr. Keith Blazey, an Englishman,and Dr. Eric Courtens, a Belgian, and me. Blazey had been in Zurich a long time and spoke fluent Swiss-German, with a heavy middle-class English accent. Courtens spoke French and good, accented English but was struggling to acquire the local German-Swiss dialect. He spoke French with his Swiss technician.

During my year-long assignment (Oct. 1967 – Oct. 1968) we added a technician, Herr Hoegg, and a research physicist, Dr. Dieter Pohl, from Munich. I used English when interviewing Pohl, but had to interview Hoegg in German. This worked pretty well since Hoegg spoke mainly Swiss-German, and so, like me, was not conversing in his mother tongue.

The language ‘scene’ of the Zurich lab was diverse. About half the personnel were Swiss-German, and spoke to each other in their cantonal dialects or in Schrift-Deutsch (as the Swiss call High German) . The scientific staff used German, French, or English as occasion warranted. Meetings of the lab managers were normally held in German, with occasional use of English.

Despite the fact that the IBM Research Division required a specially designed management training course for all new managers, no one at the Zurich lab had ever had such a course. That changed in early 1968...perhaps in part because of my presence.

The year in Zurich was a major cultural adventure for Elizabeth and me and Sarah and Jennifer, and it afforded me useful experience in management, but it was not a particularly fruitful time scientifically. I published only two papers based on work in Zurich.

On the other hand, the assignment must have been viewed as a successful first step, since management opportunities would continue to be created for me.

BACK TO AMERICA

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The Thomas J. Watson Research Lab in Yorktown Heights, NY

The next of these was as manager of the laser physics group back at IBM Research headquarters in Yorktown Heights, N. Y. My research in this period, done mainly with collaborators, involved nonlinear traveling-wave optical interactions, and the optical interactions of tunable lasers with alkali-metal vapors.

The laser group was part of the Physical Sciences department, headed by Dr. Philip Seiden. The department had a roster of about 200, one hundred of whom were Ph. Ds. In 1972 Seiden was granted a year long sabbatical to the Technion in Israel, and I was made acting manager of Physical Sciences until Seiden’s return.

I thoroughly enjoyed that year as acting manager. The department included research on many areas of physics and chemistry with which I was unfamiliar, and it has always been one of my chief satisfactions to learn about new areas of science. And of course, being in charge of a department of 200 scientists, technicians and support staff afforded many valuable management experiences.

As had been planned, Seiden returned from Israel after a year and I went back to being manager of the laser physics group. My research took a new direction: Atomic spectroscopy in the vacuum ultraviolet… but using neither vacuum technology nor ultraviolet light! [5]

The trick was to use two separate wavelength-tunable dye lasers. One laser excited an atomic vapor so that many atoms were promoted to the atom's first excited state; the second tunable laser further excited the already excited atoms into still higher energy states that were normally only reachable when excited from the atoms' lowest state by light in the 'vacuum ultraviolet'...so called because light in the deep ultraviolet is strongly absorbed in air, necessitating spectroscopic instruments to be operated in a vacuum.

DIRECTOR OF PHYSICAL SCIENCES

Then in 1975 another door was opened; Seiden stepped down to return to full-time research, and I was made Director of Physical Sciences, a post I held until early 1980.

The department was home to some remarkable scientists: IBM Fellow Peter Sorokin, inventor of the tunable dye laser; IBM Fellow Benoit Mandelbrot, for whom the fractal Mandelbrot Set is named, IBM Fellow and polymath Richard Garwin; IBM Fellow Dean Eastman, a pioneer of surface science studies, and many others.

The department was also home to first-rate material scientists, whose expertise was very helpful in solving production problems that cropped up in IBM's semiconductor manufacturing plants.

SOFT ERRORS & THE CORPORATE TECHNICAL COMMITTEE

During my years as Director of Physical Sciences, I became increasingly interested in the work being done in the many IBM product development labs, labs whose missions

5 J. A. Armstrong and J. J. Wynne, Autoionizing States of Sr Studied by the Generation of Tunable Vacuum U.V. Radiation, Phys. Rev. Lett. **33**, 1183 (1974)

and cultures were quite different from those of the three laboratories that made up the Research Division. Not many staff members in Physical Sciences had a good understanding of IBM's product development and manufacturing operations. Certainly I did not.

In 1978 an event occurred which caused me to knock on another open door... one that led me eventually to leave Research and spend time in an IBM product development laboratory.

The telephone switches of AT&T were special purpose computers, with very large semiconductor random-access-memories (RAM). AT&T engineers observed that, from time to time, a few memory cells would spontaneously flip their contents from zero to one, or vice versa. These flips constituted errors that could cause the switch to mis-route long distance phone calls.

The malfunctions were not permanent; if the switch/computer was rebooted, the memory cells that had previously misbehaved went back to normal functioning...but then from time to time different memory cells would flip.

These errors were known as 'soft errors'.

Intel, the manufacturer of the RAM chips soon discovered what was causing these soft errors. The chips were soldered to the motherboards with lead-based solder, and some sources of lead are weakly radioactive. Energetic alpha particles coming from the radioactive lead were occasionally hitting a memory cell, depositing extraneous electrons in their wake, and flipping the memory cell from zero to one, or vice versa.

Was this happening with IBM's computers, too?

As it happened, the Physical Sciences Dept. was home to the only trained nuclear physicist in IBM. Working with IBM system engineers he soon determined that not only did IBM's computers exhibit soft errors due to radioactive lead solder, but IBM's and all manufacturer's large computer memories also suffered from soft errors **caused by cosmic rays** that passed through a memory cell and left a load of unwanted electrons in their wake. Clearly, Nature had not gotten the message that nuclear physics and computer engineering were separate subjects.

Various changes were made in the **design** of semiconductor memories that made them immune to soft errors. Design changes were the only option, since it is impracticable to shield computers against cosmic rays.

This connection between computer circuit design and supposedly unrelated areas of physics heightened my interest in learning how the Physical Sciences department could expand its contributions to IBM product development.

The obvious next step was to get an overview of IBM product development.

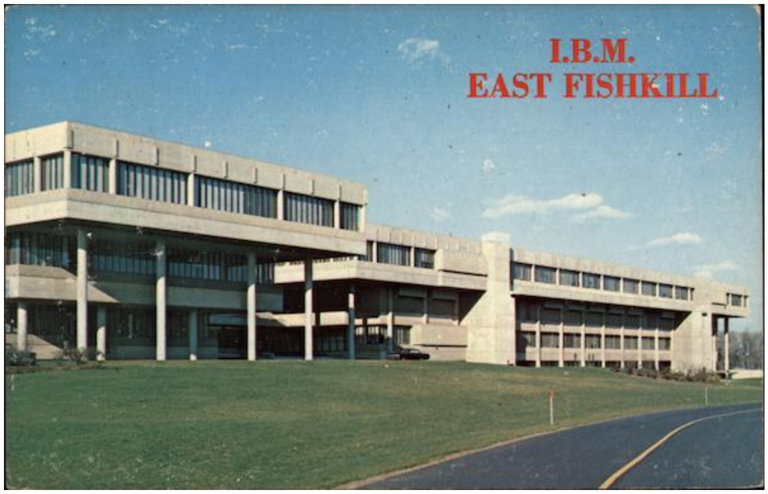
In 1980 I was offered a one year assignment as a member of the Corporate Technical Committee (CTC), based at IBM headquarters in Armonk, NY, and headed by the IBM Chief Scientist, Dr. Lewis Branscomb. The job of the CTC was to provide top management with an assessment of the technical fitness of the various product development labs in IBM. Many of these labs were in the US, but others were overseas.

During 1980 the CTC traveled to IBM labs in Boulder CO, San Jose CA, Lexington KY, Raleigh NC, Fujisawa Japan, and Boeblingen Germany, among other locations.

CTC members were expected to explore some special technical topic and then to present their findings and recommendations to senior management in a series of one-on-one meetings. At the time, IBM was a leading chip manufacturer, and I chose to concentrate on the optical lithography tools which are key to the production of semiconductor chips.

I have forgotten all of the one-on-one meetings except the one with Dr. Jack Bertram. Bertram was a mathematician who had started his career in Research, but moved rapidly up the IBM hierarchy until he was in charge of all of IBM's advanced' development operations. He was notoriously acerbic, and an inveterate cigar smoker.

The presentation I had prepared took up 14 transparencies to be shown on an overhead projector (this was long before PowerPoint). As I launched into the contents of the first transparency, he immediately interrupted and peppered me with expert technical questions and objections. We spent 50 minutes in lively discussion, and when the meeting was over, I realized that I had never gotten past the first of my fourteen transparencies!



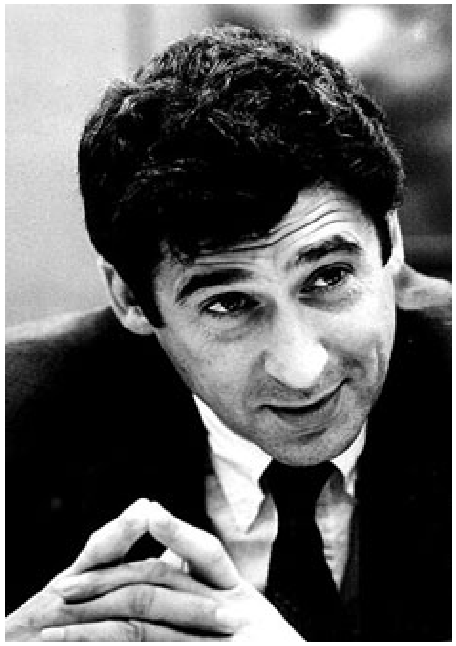
The East Fishkill Development Lab

The meeting with Bertram must have been a success, because when my year on the CTC was over I was offered a job running the Advanced Materials and Technology Department in IBM's East Fishkill development and manufacturing site. The department consisted of about 500 engineers working on new designs for bipolar transistors and on new polymer and ceramic materials for the mounting and 'packaging' (encapsulation) of the thousands of chips required to make an IBM mainframe in the 1980s.

My original plan had been to return to IBM Research at the end of 1980, so naturally I discussed the Fishkill offer with the director of Research, Dr. Ralph E. Gomory. He discouraged me from taking the job. But my sense of adventure and my desire to learn more about IBM's vast operations prevailed, and I took the job in Fishkill. I also sensed that at age 37 the time was ripe for a major change.

Before recounting my time in Fishkill, I should comment briefly on Ralph Gomory.

Gomory was an outstanding mathematician, an IBM Fellow, a remarkable leader, and my mentor for many years.



It was Gomory who built the IBM Research Division into a world-class institution. The quality of any research operation is determined primarily by the quality of the researchers that it hires. Gomory instructions were: “Hire people who are better than those who are here already.”

Gomory had a major impact on my scientific career. Fifteen minutes with him, after I had made some management mistake, was more valuable training than my three weeks at IBM’s residential Management School.

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Back to East Fishkill.

To put it mildly, I wasn’t in Research anymore. To be sure, there were Ph.Ds in Fishkill, but the culture was formed and dominated by the engineers and others who actually **delivered** on-time, high quality, state-of-the-art components to IBM Poughkeepsie, where they were assembled into mainframe computers.

In academia (and in the Physical Sciences department in IBM Research), original , fruitful scientific ideas are the most valued contributions. But in Fishkill it was different. As IBM Fellow Bob Henle, a key IBM circuit designer, once said to me: “IBM engineers don’t run out of ideas, they run out of time.”

I soon discovered that one of my main management challenges was to mitigate the intense feelings of rivalry that existed between engineers in my Advanced Materials and Technology Department and engineers in the departments of the Fishkill laboratory that worked more directly with manufacturing.

My other main management challenge was to mitigate the mistrust and rivalry that existed between engineers in Fishkill and the scientists and engineers in IBM Research.

To deal with first of these challenges, I cultivated a constructive working relationship with Harvey Ezrol, the leader of the team whose members were most competitive with my AMTD team. Teams take their cue from their leaders. If the leaders ‘make nice’ in public, but run each other down in private, that will poison the inter-team relationships. Conversely, if the leaders are mutually supportive in private as well as in public, that will help counter the inter-team rivalry and suspicion. .

Harvey Ezrol, Erich Nielsen, and me: East Fishkill 1981

To improve relations with Research required a different strategy. No matter how rich a large company is, there is never enough money at the lowest levels of the organization. Bench-level scientists always have more ideas than funds for pursuing them. This was as true in Fishkill as in Yorktown Research.

Working with former colleagues in Research, we formulated the concept of Joint Programs. There would be **new money** provided for advanced technology projects. But only for new projects which were **jointly conceived and executed** by technical workers in the two locations. Progress in these programs would be monitored by senior management *in both divisions*. Thus was born ASTL, the Advanced Silicon Technology Laboratory. ASTL was successful, and eventually resulted in the creation of an additional joint program: APTL, the Advanced Packaging Technology Laboratory.

The Joint Programs did, in fact, significantly improve the cooperation between Research and East Fishkill.

Meanwhile, I became so enthusiastic a member of the East Fishkill team that, when in 1983 Gomory offered me a new job back in Research, my first reaction was “I can’t leave my post.”

RESEARCH VP, LOGIC & MEMORY

However, I did return as Research Division Vice President, Logic and Memory. I once quipped to Bloembergen that I was the first person since Aristotle to have that title.

As VP, Logic & Memory I had a much different set of responsibilities than when I had been Director of Physical Sciences position. The new areas included: (1)The Research Division’s long-standing program in silicon technology, which now it had its own ‘pilot line’ complete with a class 100 clean room and tools that could fabricate advanced integrated circuit chips; (2) A program in the polymer technology used to encapsulate chips before mounting; (3) a small program in the gallium arsenide semiconductor devices which were then viewed as a possible successor to silicon devices; (4) a large program in Josephson-Junction-based superconducting computer technology.

The joint programs ASTL and APTL were also part of my responsibility.

The Josephson program was to be a computer-technology-great-leap-forward. When originally conceived, it promised a 1000-fold advance over the silicon technology of the early 1970’s. It exploited superconducting quantum tunneling switches But by 1983 it was clear that achieving superiority over silicon was not going to be possible.

This was for two main reasons. First, silicon technology had improved exponentially in the intervening years. Second, making a computer from lead-based superconducting switches (i.e. Josephson junctions) required inventing a whole new class of electronic circuits and inventing and perfecting a whole new set of materials and manufacturing technologies. This took years, and required many trade offs that compromised the hoped-for advantages of Josephson junctions as computer elements.

By 1985 IBM had spent well over $100 million on this moon shot. And although the team had built a working 30,000 logic-circuit signal processor, it had proved impossible to construct a working Josephson-based memory. The signal processor only ‘worked’ because it had an attached conventional semiconductor memory.

IBM Fellow Joe Logue was manager of the Josephson project at this time, and in mid 1985 he came to me and said that he and his key lieutenants had concluded that it was time to terminate the project.

Logue and I immediately shared the proposal to drop the Josephson project with Director of Research Ralph Gomory, who said “Let me talk to (IBM CEO John) Opel first.” Research had spent a huge amount of IBM’s money on the project, and would have little to show for it.

It was evidence of the high regard in which Research and Gomory were held by IBM senior leadership that, in fact, we did abandon the Josephson computer project without noticeable corporate recriminations.

There had been about 125 staff working on the Josephson computer project. They had proven to be resourceful, creative, and dedicated. We reassigned half of them to augment our silicon technology program, a quarter to add to our packaging work, and the remainder to increase our effort on GaAs technology.

1986: A FIRST NOBEL PRIZE & an NSFNET PROPOSAL

In 1986 Gomory moved up to become IBM Sr. VP and Chief Scientist, and I replaced him as IBM Director of Research. At that time the IBM Research Division had a staff of about 2600, 1200 of whom were Ph.Ds, in three laboratories located in Yorktown

Heights NY, Almaden CA, and Zurich Switzerland. Its annual expenditures of $650 million were about 10 percent of IBM's R&D budget. The type of curiosity driven research that I had done constituted less than two percent of the Research Division's budget, which was overwhelmingly targeted to the needs of IBM.

My memory of that first year as Director of Research is marked by two events.

In October, Gerd Binnig and Heinrich Rohrer of our Zurich lab won the Nobel Prize in Physics for their invention of the Scanning Tunneling Microscope (STM).

The STM enabled researchers to 'see' a crystal surface in unprecedented atom-by-atom detail. It was the first Nobel Prize won for work in IBM Research and it understandably focused great attention on IBM Research in general and on the Zurich lab in particular. As Director of Research I came in for a certain amount of reflected celebrity even though I had had no role in the STM project.

The second event which marked my first year as Director of Research was signing off, for IBM Research, on a proposal to the National Science Foundation to build NSFNET, a new digital network which would link together 7 academic supercomputer centers at the then unheard of speed of 1.5 mbps (megabits per second). The proposal was made jointly with telecommunications company MCI and with MERIT., a Michigan consortium of university networks. IBM's contribution was to be a joint effort between Research and Academic Computing Systems, based in Milford, CT. The technical work would be done by Research Division experts in TCP/IP networking and by systems engineers in Academic Computing Systems.

1987: A SECOND NOBEL PRIZE & NSFNET

Late 1987 was an eventful time for IBM Research. Not only did our consortium win the NSFNET competition, but in October IBM Zurich scientists K. Alex Mueller and Georg Bednorz won the Nobel Prize in physics for their discovery of high temperature superconductivity.

Their publication in 1986 had electrified the world of physics.

For decades it had been believed that superconductivity (ie zero electrical resistance) could only be achieved by cooling certain materials to within about 10 degrees Celsius of absolute zero. Bednorz and Mueller showed that certain copper-oxide ceramic materials exhibited superconductivity when cooled ‘only’ to 77 degrees above absolute zero.

Some wags in IBM senior management commented that the performance plan for the Director of Research obviously contained the expectation of a Nobel Prize per year.

Alex Mueller invited me and Elizabeth to be his guests at the Nobel ceremonies in Stockholm that December. The King of Sweden awarded the prizes during a convocation in the Stockholm Opera House, followed by a gala dinner for hundreds held in the City Hall. Inge Mueller was seated next to King Carl Gustav XVI, and Alex next to Queen Sophia. Elizabeth and I made do with a baron and baroness.

(Remarkably, in 2020 there is still no widely accepted theory that accounts for high temperature superconductivity. And despite much time and investment, there are still no widely useful practical applications.)

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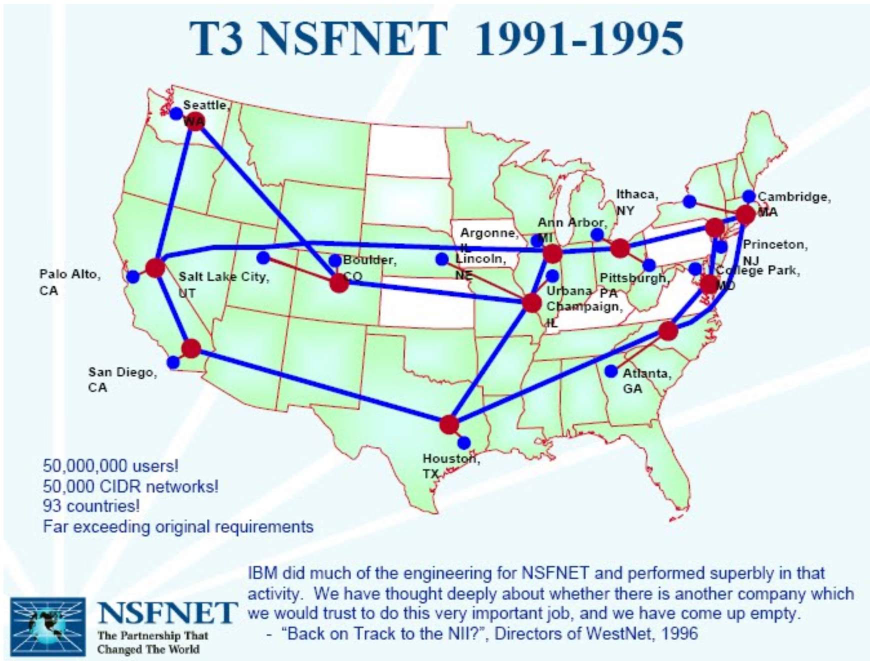
In Nov. 1987 our NSFNET consortium won the competition and began the hugely ambitious and significant project which led directly to the Internet we have today.

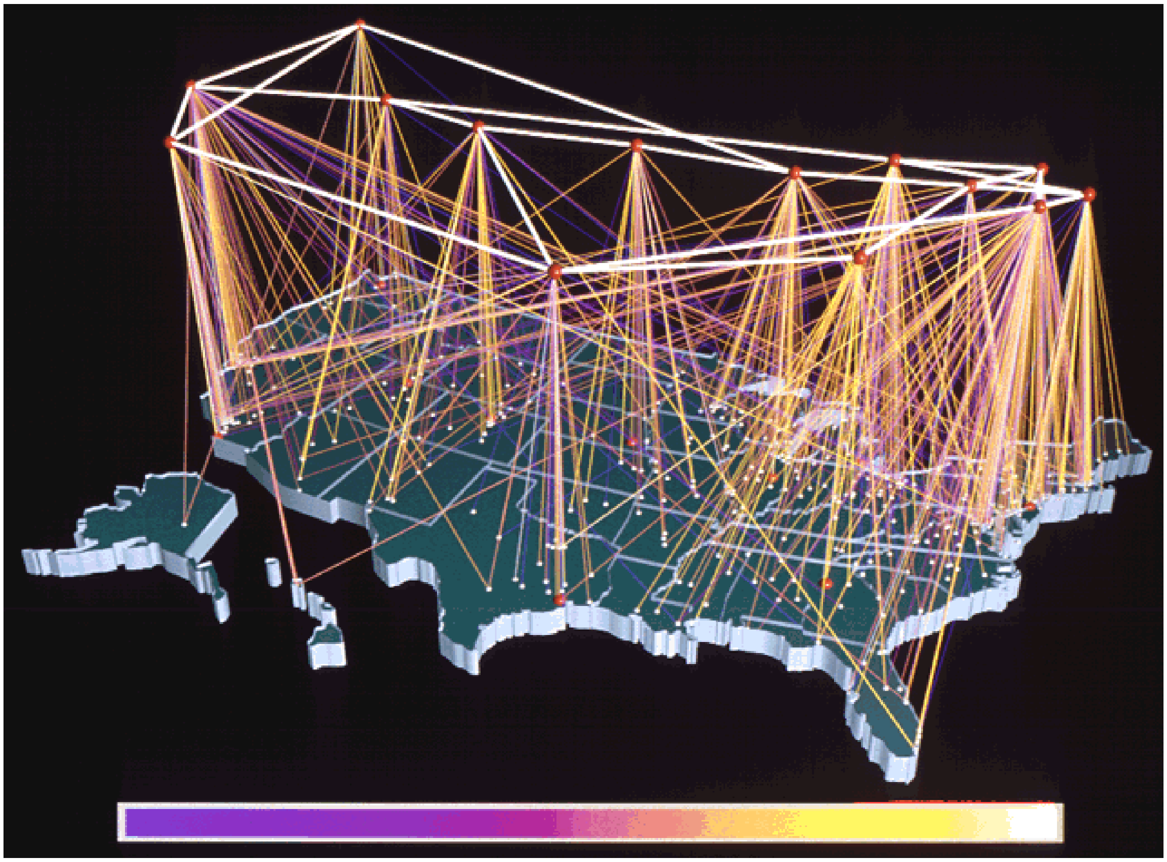
By late 1989 we had achieved the goal of connecting seven academic supercomputer centers on a 1.5 mbps backbone.

Success with the T1 backbone led to a contract extension to upgrade to T3: 45 mbps. This too was a success and connected additional sites.



The successful upgrade to T3 speeds stimulated many additional networks to connect to NSFNET, and traffic on NSFNET was growing at about 20% per month.

In 2002 a not-for-profit corporation, Advanced Network Services, was formed to manage the continuing explosive growth in connections. It was headed by Alan Weis, who had lead the NSFNET team from IBM. I served for several years on the ANS board.



The network when the not-for-profit ANS was formed.

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The American Physical Society is the professional society for American physicists. In March of 1989 I was awarded its George Pake Prize, which recognizes "outstanding work by physicists combining original research with leadership in the management of research in industry." The citation reads in part: "*For his brilliant creativity [and] excellent judgment...he has made seminal contributions in nonlinear optics and was a pioneer in the technology for measuring ultra-fast phenomena...*"

APS Pake Prize reception. Elizabeth, Me, UCB Prof. Ron Shen, my colleague Dr. Jim Wynne, Prof. Nicolaas (Nico) Bloembergen

In late 1989 Gomory retired and I moved up to IBM headquarters in Armonk, NY as IBM VP, Science and Technology, with the Research Division, the Corporate Technical Committee, and University Relations reporting to me. I also managed the process for choosing IBM Fellows. This highly regarded distinction was achieved by roughly 0.1 % of the approximately 60,000 R&D staff members in the Corporation.

By the late 1980s IBM's decades-long dominance of the computer industry was waning. The corporation was late to realize that the Internet and personal computers were drastically reshaping and enlarging the market for information technology. In part this blindness was brought on by the huge success of its traditional businesses.

For example, in 1988, well before the explosive growth of the Internet, IBM already had a $2 billion dollar, digital, proprietary hardware and software networking and communications business, and it was slow to realize how it was being overtaken and out-flanked.

Warnings of the danger were met with hostility and dismissal.

I retired in 1993 after 30 years. My IBM career had been richly rewarding, it had coincided with major progress in science, with the long arc of IBM's corporate success, and with developments in information technology which led directly to the world of today. But I was not sorry to be leaving.

A PARALLEL CAREER

Independent of these developments, my position as a senior R&D executive and my status as a Harvard graduate led to my election to the Harvard Board of Overseers. This was in addition to many other opportunities which were offshoot consequences of my IBM career. In this section I will deal with a few of these offshoot activities that occurred both during and after my time at IBM.

THE NATIONAL ACADEMY OF ENGINEERING: A DOOR NOT ENTERED

In the 1970s, when I was Director of Physical Sciences, I was asked the join the Commission on Physical Sciences of the National Research Council. The NRC is the operating arm of the National Academies of Science, Engineering and Medicine. Its task is to conduct studies which government agencies requested be done by the Academies. Although the NRC has a paid staff, I, like the other Commission members, was a volunteer, reimbursed only for travel expenses while on NRC business.

My involvement in NRC affairs increased markedly when, in 1987, I was elected a member of the National Academy of Engineering (NAE). The citation accompanying my election to the NAE reads: *"For contributions in nonlinear optics and* *quantum physics and for technical leadership in advanced very-large-scale integration technology. "*

NRC study teams are composed of subject experts, but Academy members usually re chosen to chair them, whether or not they are experts. Over a ten year period I chaired studies for the National Weather Service, NASA, the Ocean Studies directorate of the National Science Foundation, and a number of others.

Chairing these NRC studies was rewarding for at least three reasons. First, I got to learn about new fields from scientists at the forefront; second my fellow study-group members were often very enjoyable colleagues, as well as experts; and third, on rare occasions the government agency that had asked for the study actually took some of the study's recommendations.

The President of the NAE, Dr. Robert M. White was due to retire in 1995. To my surprise and consternation, the NAE Council's nominating committee proposed to nominate me to run unopposed for NAE President.

The nominating committee was chaired by Dr. Paul Gray, former MIT president, and included Erich Bloch, former IBM VP and former NSF Director, Dartmouth Prof. Elsa Garmire, (later Dean of Engineering), and others. When informed of the committee's intention, I told Paul Gray that I could not imagine living and working in Washington. Bob White, who was probably the main proponent of my nomination, was completely puzzled by my refusal run for NAE president.

Although I had enjoyed my stints on NSF and NAE business in Washington, I had also seen first hand that Washington was a place of constant political one-upmanship, institutional jockeying, and sparring with Congress. The last thing I wanted was to plunge into that milieu. Some thrive in that atmosphere because they have the requisite character traits to succeed. I had learned by this time that I do not have those traits. Indeed, the analogous aspects of my career as an IBM executive had been the things I had enjoyed least and was least good at.

My refusal to run resulted in a contested election for NAE President, the election of the disastrous Hal Liebowitz, the subsequent struggle to change the NAE by-laws to allow for removal of the president, and the ultimate removal of Liebowitz, who was succeeded by University of Virginia Prof. William Wolf as interim NAE President.

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In the late 1970s, when I was Director of Physical Sciences, I was invited to join the National Science Foundation's Physics Advisory committee. Then as now NSF was concerned primarily with the support of university physics.

I recall two episodes of the committee's work.

The first concerned the perpetual dilemma faced by funding agencies: There is never enough money. So to support new programs, older programs have to be cancelled. But which ones? The specific case involved recommending which of the eight or ten university cyclotrons would be shut down so as to free up money for new initiatives in nuclear physics research. Led by Prof. D.Allan Bromley of Yale, the committee did its unpalatable job, and NSF ended support for several university cyclotrons.

The second episode was from the early 1980s. This was at the beginning of the decades-long gestation of the Laser Interferometric Gravitational Wave Observatory, LIGO. Our NSF committee heard one of the first presentations by the proponents of LIGO. Many on the committee were sceptical that it would ever work, and I was among them. But despite the less than wholehearted committee reaction, NSF let the project move forward. As is turned out, this was to be one of NSF's triumphs in supporting ground-breaking science. Gravitational waves, predicted by Einstein's general theory of relativity in 1916, were finally detected by LIGO one hundred years later, in 2016.

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NSF also supports NRAO, the National Radio Astronomy Observatory. It is a consortium of universities, none of which can by themselves afford the capital costs and running expenses of leading-edge radio telescopes.

The management of NRAO is provided by a Federally Funded Research and Development Corporation known as AUI (Associated Universities, Inc.) that originally was established to manage the Brookhaven National Laboratory, and later added NRAO to its responsibilities.

I had joined the board of directors of AUI while still at IBM and continued to serve after my retirement until the time when I joined the National Science Board.

Getting to know the radio astronomy community was great fun. First, of all the different types of telescopes, radio telescopes provide the highest resolution images and so often reveal startling new phenomena. Second, the radio telescopes are located in interesting places, and as AUI board members we (and our spouses) were duty-bound to visit those places. Third, the AUI board members, men and women of remarkable achievement, were very enjoyable to be with.

The Very Large Array, Plains of San Augustin, near Soccoro NM

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Other chapters in my parallel career included appointments as Karl Compton Lecture at MIT, visiting professor at the University of Virginia at Charlottesville VA, five years as Chair of the Board of the American Institute of Physics, and membership on the Board of Trustees of the University of Massachusetts system.

But the highlight of my parallel career was the opportunity to travel to the South Pole as a member of the National Science Board, the presidentially appointed group which oversees NSF.

NSF supports university research in both the Arctic and the Antarctic. In December 1999 NSB member Dr. Robert Suzuki, president of Cal Poly, Barnard professor Stephanie Pfirman, chair of the NSF advisory committee for Polar Programs, and I spent a week visiting the research programs at McMurdo Station and at the Amundsen-Scott South Pole Station.

The South Pole Station is at 9200 ft elevation, 8000 ft of which is ice. And the ice moves an inch per day! Although the South Pole does not move, its location on the ice does. Once a year, on January 1, an American flag is planted at the then location of the Pole. In the following picture, taken in December 1999, the flag can be seen in the distance.

Earlier in the week we had helicoptered from McMurdo Station out to research groups working in the ‘Dry Valleys’, and on the way back we had stopped in Bull Pass to see the amazing ventifacts sculpted by sand and ice crystals blown by the 200km/hr katabatic winds that occasionally ravage the pass.

I'd go back tomorrow.