Remembrances

Morton B. Panish

It is a wonderful privilege to stand before you today to present this commemorative lecture. On considering what I should say to you I decided that it is important to reflect first on the factors that have influenced my life and set me on the path that led to this wonderful occasion. Those factors were operating long before I was born.

As a result of the destruction of the small Jewish towns of Eastern Europe during the Nazi's war against the Jews, it is impossible for me to trace my roots back beyond my great grandfather on my mother's side, and my grandfather on my father's side. However, I don't need to know who my ancestors were, to know what I inherited from them. In the Jewish towns of Eastern Europe the most respected people were not necessarily those who were materially successful. In those places, over a period of hundreds of years, genuine respect and the support of the community was reserved mostly for the scholars who spent all their time studying and interpreting the ancient religious texts. Theirs was true scholarship, and the community felt it to be so important that it was considered to be an obligation and a privilege to support them by providing them with food and shelter.

During the latter part of the 18th century Jews in parts of Western Europe began to be allowed civil rights comparable with the rest of the population. The French Revolution and then the victories of Napoleon accelerated this "emancipation" of the Jews, as did the upheavals of the mid 19th and beginning of the 20th century, all over Europe. The achievement of liberalized civil rights made it easier for Jews to assimilate, and made it inviting for some of those oppressed in Eastern Europe to move west and give up many of their old ways. This western movement reached its peak in the great migrations, especially to the United States, around the end of the 19th century. My grandparents were among those immigrants.

Although I am rendering a complex story in an oversimplified manner, it is fair to say that in the West a significant fraction of the Jews forsook many of the ways of their forbears. The approach to the religion was, for many of the people, vastly modified. Even when the religious impulse remained strong, the tradition tied to the study of the old texts faded for many of those of us who became westernized. The word was, and is, "assimilation." We were called, and we accepted the term, "assimilated Jews," and we thrived in the United States, England, France, and—for a while—in Germany. Oddly enough, the tradition of respect for scholarship remained undiluted. That tradition itself became assimilated and was sublimated to the communal respect for scholarship in any of the intellectual disciplines, but especially medicine, science and law, the disciplines that were also a ticket out of poverty. Those who studied benefited from the respect and support of their family and their community. The present vastly disproportionate number of assimilated Jews in medicine, science and law, and in intellectual pursuits of any kind, is due not to our genes, but to the respect for scholarship, and the emotional, financial and communal support that such respect engenders.

My grandparents all arrived in the United States in the 1890s. They were young, all teenagers, when they arrived. I only know the details about my maternal grandfather. He was born in East Galicia, now Ukraine. He was orphaned at about the age of fifteen and immigrated to the United States at the age of sixteen. He, as so many others, emigrated partly because of persecution in the land of his birth, and partly because of the opportunities that he, and the other poor of Eastern Europe, envisioned in the West. He had three younger siblings, and his first task upon arriving was to earn enough money to bring over his two sisters and a brother. He, as most of the Jews of the east, was literate, certainly in Yiddish, the language of East European Jews that was derived from Middle High German. He rapidly learned English, and in the following few years was able to bring over his siblings, who, in the meantime, had been staying with relatives. I tell this story because it is typical. My background in this sense is not very different from that of millions of the descendants of the immigrant surge into the United States near the turn of the century.

My parents, were both born in New York City. Because of the limited resources of their families, their formal education did not progress beyond high school. In my father's case he was not even able to complete high school because of the need to help support his family. His father was unable to work much at his trade, tailoring, because of a debilitating tremor. While working, my father attended business school at night, and eventually was able to work as an industrial credit manager and thus entered the middle class. Then he was able to extend his education, but without the benefit of further formal schooling. He was largely self-educated, read the classics, and would often recite Shakespeare sonnets from memory. He also loved classical music and had an intense interest in literature and science.

My parents were married in 1927, and I was born in April of 1929. My brother arrived six years later. We lived in a very nice rented apartment in a two family house in Brooklyn, a borough of New York City. Somehow my father was able to maintain our standard of living even during the depression years. The owners of the house we lived in were East European Jewish immigrants like my grandparents. They lived in the upstairs apartment with their two sons who were very talented musicians: The oldest was a professional concert pianist who tried, and failed, to make a pianist of me. The younger son was at the time the first violinist in Toscanini's NBC Symphony Orchestra. Perhaps it is important that my childhood was filled with music as the boys practiced upstairs. Also important to me was the fact that the violinist's hobby was photography. He showed me how to take and develop photographs, and by the time I was twelve I was an enthusiastic photographer. I had acquired an avocation that has brought me great pleasure ever since.

My mother was a successful self-educated interior designer—first as a hobby, and later professionally. She had exquisite taste and I think that I owe my interest in things artistic, and thus my ability in photography, to her.

Starting at age five, my brother and I attended New York City public grade and high schools. In those years the public schools of New York were the equal of, or better than schools almost anywhere else in the United States. Sadly, that is no longer true. My parents, true to their cultural heritage, tried to impress me with the importance of achievement in education, the more so because they had been unable to pursue formally such a course themselves. Support and encouragement were provided, and in spite of it all, I was, for the first eleven years of my schooling, an indifferent and unenthusiastic student. This doesn't mean that I wasn't getting the message that my parents were sending; I just wasn't mature enough to act upon it.

I also believe that I was receiving another equally or even more important message that arose at least partly out of our cultural heritage. Perhaps as we backed away from religious orthodoxy, or religion entirely, this was one of the replacements. But perhaps it is a universal message. For me it was in the air, arriving from poorly definable sources. It was a moral imperative not stated clearly by my parents or friends or teachers, although I have certainly heard it clearly stated later on. Within your own capability, do what you can to contribute something, even a very little thing that is positive for humanity, and keep doing it. It's not a difficult assignment. There are an infinite number of ways. Be a nourishing parent. Paint a nice picture. Make a beautiful photograph. Write a symphony. Donate something to charity. Volunteer to help some worthy cause. If you are capable, push science or technology a little further.

When I was about 12 years old I read Paul de Kruif's book *Microbe Hunters*, a clearly written account of the lives and work of some of the earliest microbiologists and the impact their work had on the understanding of the causes of many illnesses. It was perhaps the perfect book for an impressionable and idealistic youngster. It seemed to me that the life of a scientist must be filled with constant excitement and adventure. The impression made was so strong that I can still remember the feeling some 60 years later.

This part of my story obviously took place during World War II. However, compared to the difficulties, hardships, and tragedies suffered by so many others, my immediate family got off very easily. I was too young to enlist or be drafted into military service. My father was too old. We had to endure only the minor inconveniences of shortages, and of course the tensions of living in a country at war, no matter how remote. At some point we became aware that the Jews of Europe and the Soviet Union were being murdered. While that added to the deep sadness that the knowledge of all the death and destruction engendered, we were not touched directly.

When I was in my final year in high school I began to get serious about my studies and had made the transition from being a "C" to being an "A" student. All during the period when I wasn't performing well in school, I was, nevertheless, developing an avid interest in science while maintaining the fascination with photography. At the age of sixteen or seventeen I took a course with the late J. G. Lootens, a well-known photographer and a wonderful teacher, who went a long way towards inspiring my lifelong interest in photography.

During that same year the teacher in my chemistry class became ill and was replaced by a substitute, a chemistry graduate student from Columbia University. He described his Ph. D. thesis work. He was synthesizing several entirely new organic compounds. I don't remember for what purpose, but the idea that a person could do such a thing captured my imagination. I determined then, that someday I would be a chemist. Unfortunately I don't remember that teacher's name. It would be nice to be able to tell him about the effect that his, I am sure, brief excursion into high school teaching, had on at least one student's life.

My father, who frequently took me to one or the other of New York's famous museums, fostered my interest in science. Our favorite museum was the American Museum of Natural History. We also sometimes visited the Brooklyn Academy of Science where there were classes for young people. My father was trying to transmit to me his own fascination with science. I have suspected for years that in becoming a scientist myself I was fulfilling my father's dream of what he would have liked to do with his life. He died at the age of 64 in 1965. I think that in many ways I have lived out my father's dream and am deeply saddened that he didn't live long enough to see that I did well at it.

During my fourth year in high school I took the competitive examination for entry into Brooklyn College, the Brooklyn branch of the City University of New York that was free to those who could qualify. I needed to do well in the exam because my high school grades alone would not have qualified me. Fortunately I did, and was admitted for the winter semester of 1947. Brooklyn College was at the time a place of academic excellence. While it was primarily a liberal arts college, there were excellent beginning courses in chemistry, physics and mathematics. I partook of them as well as the remainder of the curriculum in English, History, Literature, and a foreign language, German. I did very well in all except German.

I stayed at Brooklyn College for two years and did well. Then I transferred to Denver University, not because of any academic reason, but because I had friends there and wanted the experience of living away from my parents home in an environment that was different from that of New York City. Although I was, and still consider myself to be, a New Yorker at heart, I thoroughly enjoyed living in Denver. The university was less challenging than Brooklyn College, but the courses were generally good. My experience with the substitute teacher in high school had caused me to aim at a specialization in organic chemistry, which I enjoyed. But I found myself drawn to the more mathematical, and I thought more challenging discipline of physical chemistry. The experience at Denver University was enhanced by the fact that now I was in classes that were dominated by returning veterans. They were older, more mature, and very well motivated. In spite of my perception that the courses were less demanding than those in Brooklyn College, the presence of the veterans offered competition that would otherwise have been lacking, and upon which I thrived.

Two other things happened at Denver University. My physical chemistry professor, Alan Vander Weyden urged me to specialize in physical chemistry—and I decided to do so, and, what is much more important, I met my future wife, Evelyn Chaim, in an organic chemistry class. She had recently arrived in Denver, having spent the war years in Shanghai as a refugee from Nazi Germany. We became engaged shortly before I left for graduate school during the summer of 1950.

Graduation at Denver University took place in June 1950. My undergraduate experience had been very positive. The cultural imperatives instilled by my parents and the assumptions of my upbringing were still operating, so that the next step, graduate school, was obvious. The only problem was that of finances. I was now on my own, and needed financial help to continue. The selection of a graduate school was dictated by the fellowship or scholarship that would be offered, and I ended up the following fall with a teaching assistantship at Michigan State University.

I chose to major in physical chemistry and minor in organic chemistry. The advent of the Korean War caused modification of the plans to start work on a Ph. D. program. It was expected that I would be drafted to serve in the military, and I therefore elected to study for a master's degree, which I could complete rather quickly, so that even if drafted, I would have something to show for my efforts at graduate school. As it turned out, I was rejected from the draft for medical reasons, but completed the master's program anyway. The course work was interesting, especially the courses devoted to quantum theory and chemical thermodynamics. I also found that I was more interested in physics than organic chemistry, and strengthened that part of my program. The master's thesis was not very challenging, a series of measurements of the electric dipole behavior of some organic compounds. Nevertheless, it was exciting to see my name on my first publications, and I did feel that I had started to do my little bit to add to mankind's store of knowledge.

I married Evelyn after the first year in graduate school. That was the best decision I have ever made. We are different in many ways, and those differences have enhanced our lives. Evelyn accepted a teaching position in a grade school in nearby

Lansing, Michigan.

My master's thesis work was done under the direction of Prof. Max Rogers, a Canadian who had been a student of Linus Pauling. I continued with him while studying for the Ph.D. degree, continuing the course work that emphasized my interest in quantum chemistry, chemical thermodynamics, and physics. Very soon the major thrust of all my effort was the thesis work on the chemistry and properties of interhalogen compounds. This topic was partly the result of the fact that by this time I was being supported by a fellowship paid for by the Atomic Energy Commission, and interhalogen compounds were used for the processing of reactor fuels. They are extremely reactive, and indeed, dangerous materials. That made the work more exciting, but after I finished my experimental work, another student was seriously injured by an explosion in our laboratory. By that time I had a son, Steven, and I wanted to work with less exciting materials. As before, the publications that resulted from this work were very satisfying in that I again felt that I had made a contribution, however small.

While in the final year of the Ph.D. program I began to consider where I would work. I wanted a full time research program and was not particularly interested in teaching. The ideal place seemed to be Bell Labs. I interviewed there, but was not accepted. Their decision was correct. I know now that I needed to be a more mature scientist before I could be successful in the Bell Labs environment. Instead, in 1954 I went to work for Oak Ridge National Laboratory in Tennessee and spent three years studying the very interesting chemistry and chemical thermodynamics of molten salts. My son Paul was born in Oak Ridge but didn't get to see much of it as we left the following year for North Andover, Massachusetts. A few months after arriving in Massachusetts my daughter Deborah (Debbie) was born. So I am the father of children born in three different states.

In Massachusetts I was a researcher for seven years at AVCO Corporation's Research and Advanced Development Division. AVCO had a contract with the Air Force for the production of the reentry vehicles for hydrogen bombs. I definitely didn't want to work on that, but the government permitted 5% of the major contract funds to be used for basic research. I wanted that, and was hired to do basic research in their physics laboratory. My projects were primarily studies of the high temperature chemical thermodynamic properties of refractory compounds. Before long I was managing a small department in that area as well as doing my own research. This effort resulted in about a dozen publications that I was satisfied with. However, in my sixth year at AVCO the government ended the 5% provision. I had either to work on the chemical problems associated with the reentry of a bomb through the atmosphere or go somewhere else. I chose to go somewhere else.

I sent résumés to several research laboratories along with a letter stating that I didn't want to work on military contracts. In spite of the restriction, after interviews I received several job offers, including one from Bell Labs. Apparently I was now a mature enough scientist for them. Work at Bell Labs in their research area started in June 1964. I believe that the name of the department then was Device Physical Chemistry, which didn't mean much. The department head was Carl Thurmond, an outstanding chemical thermodynamicist, and the department was in the Solid State Electronics Research Laboratory that was under the direction of John Galt, a physicist with a dynamic management style. The Device Physical Chemistry Department consisted of about a dozen people who worked mostly on so called III-V compound semiconductors and some devices made from them. III-V compounds are compounds formed between elements in the group III and group V columns of the periodic table of the elements, such as gallium arsenide (GaAs). I had been hired because of my knowledge of high temperature chemical thermodynamics and my experience in conducting experiments in that area. However, the physics of semiconductors was new to me. I was generously granted some months to study semiconductor physics, and there were plenty of experts around to help me. At that time Bell Labs provided a stimulating intellectual environment, a scientific community that enhanced the abilities of its members.

While learning about the physics of these interesting compounds I was also planning a series of experiments that would provide knowledge about the composition of the liquid in equilibrium with the solid semiconductor with and without added impurity elements (dopants), at elevated temperatures. There had been a problem in understanding how to incorporate the impurity elements needed to control the electrical properties of the III-V compound semiconductors such as GaAs, and this work elucidated the conditions under which control over impurity incorporation into the semiconductor could be obtained. It took a few months to get my lab properly equipped, but then the experimental work proceeded rapidly. I was pleased with all of this and satisfied that my publications were useful to others. However, having learned enough physics to understand that these compounds had interesting potential for semiconductor devices, I was anxious to get involved in studies associated with devices. I was especially interested in devices involving light emission, as I had already used my new knowledge of the phase relationships to grow gallium arsenide crystals that had high photoluminescence efficiency.

Sometime in 1966 John Galt invited Izuo Hayashi and me into his office. Hayashi was a physicist from a different part of the research area. Neither of us knew much about injection lasers, and Galt gave us a quick description of the device and its problems that derived from the very high current density needed to initiate lasing. High current density meant that the devices could be operated only at very low temperatures or for fractions of a second at room temperature. The goal was to improve the device so that it could operate continuously at room temperature and thus have potential for a practical large-scale optical communications system. This was an opportunity to make a truly important contribution. We were both very interested and decided to work at solving that problem. There followed a period of self-education and preliminary experimentation.

Various approaches were considered, including the use of layers of III-V compounds with different group III or group V elements than were in the substrate crystal. However, it was not until late in 1967, when we heard a paper by Hans Rupprecht concerning the growth of aluminum gallium arsenide (AlGaAs) on GaAs, that we realized that this combination of materials were so perfectly matched in size of the crystal lattice that a perfect interface could probably be grown. We immediately began studies on using this fact to help make a semiconductor structure that would lase with reduced current. This talk is not the place for a technical discussion, so let it suffice for me to say that we began with a very simple structure that consisted of one AlGaAs layer grown onto a GaAs substrate crystal while simultaneously diffusing zinc to form a p-n junction in the GaAs near the layer. We later designated this as single heterostructure in anticipation of more complex structures to follow. Properly fabricated and with electrical contacts, this becomes a single heterostructure laser.

The use of the single heterostructure permitted a reduction of the threshold current density of the laser by a factor of about five. Not enough to permit continuous operation at room temperature, but enough to let us know that we were on the right track. Our own and other workers' analyses of our results showed that if loss of light and electrons out of the GaAs side of the device could be reduced the threshold current would come down even further. This realization led to the idea of the sandwich structure of GaAs between layers of AlGaAs that we called the double heterostructure laser. Part of my effort was to start work on the understanding of the compositions of the liquid metal solutions from which the layers of the laser structure could be grown. I also devised a rather crude apparatus for the growth of successive layers of AlGaAs and GaAs onto a GaAs substrate crystal. After considerable experimentation my assistant, Stanly Sumski, and I achieved the desired structure. The yield of useful lasers was small, but after examining many lasers that failed quickly, Hayashi found one that lasted long enough to make the spectral measurement that he wanted to prove that the device was actually lasing while running continuously at room temperature. For approximately the next year we continued our studies of the double heterostructure lasers, improving the crystal growth and studying laser parameters. Izuo Hayashi returned to Japan in 1971.

In 1969, during the time we had been working on the heterostructure lasers, my Department Head, Carl Thurmond, left to direct a laboratory in the Bell Labs' development area. I was asked to take over the direction of our department. At that time I thought that I now had the best job a scientist could hope for. I headed a department of a group of scientists who were all outstanding in an organization that permitted maximum freedom and gave maximum support. The mandate that I received from John Galt was that he expected world-class technology or world-class science. The operative words were "world-class." A bit daunting, but inspiring too. Management duties were very limited, and I could continue to pursue my own experimental program.

The mode of collaboration that had been established with Izuo Hayashi and me had proved to be very productive. John Galt's idea of combining our skills in one very closely collaborative operating research group proved to be very successful. Indeed, it became the model for other device research groups and it became very common to find materials scientists, electrical engineers, and physicists functioning with equal status in such collaborations. I maintained such an approach to studies of devices and the physics of small semiconductor structures for the rest of my career.

With the departure of Hayashi I had to decide upon the future course my research might follow. I was sure of two things. First, that the heterostructure laser was just the beginning for a potentially very productive area of research: the exploitation of the extra degrees of freedom that heterostructures provided for physics and device research. Second, that I had to find a colleague to work with in the same mode as my collaboration with Hayashi. In fact, during the next 20 or so years my closest collaborators were, in succession: Craig Casey, an electrical engineer, Henryk Temkin, an applied physicist, and Richard Nottenburg, an electrical engineer.

The research programs undertaken with these partners covered studies of quantum well structures, a variety of laser structures, optical detectors, and very high-speed transistors. All were heterostructures. Also exploited were new III-V materials systems, mostly based on lattice matching to indium phosphide rather than gallium arsenide. In other collaborations, and on my own, I studied the details of the interface between two different lattice matching III-V compounds, the incorporation of dopant elements into these new materials, and new epitaxial growth methods such as gas source molecular beam epitaxy, based partly on the molecular beam epitaxy technique that had been pioneered by Albert Cho in my department.

I retired from Bell Labs in 1992 with the feeling that I had had the good fortune to have a productive career that permitted me to do work that I enjoyed while making a useful contribution to society. My interest in science remained high, but my career had led me into a narrow specialty. Since my scientific interests were broader than that specialty I took advantage of invitations from the National Research Council to serve on several committees that advised NASA on various programs. The National Research Council committees permit scientists to contribute by providing advice, mostly to the government and governmental agencies, when it is requested. The first of these was the Microgravity Committee that at that time provided advice on materials and related studies in the microgravity environment of the space shuttles. Then I joined the Committee on the Future of Space Research—Research Prioritization, followed by the Space Studies Board, which provides advice on NASA space research in general by managing a number of committees that report to the Board. Membership on these committees permitted an insider's view of the exciting science done by NASA. I came away from it all wondering whether the Space Station could ever provide important cost-effective research and an enthusiast for non-manned space missions.

It is currently my privilege to serve on the Committee on Human Rights of the National Academy of Sciences, the National Academy of Engineering, and the Institute of Medicine. This committee attempts to use the prestige of the Academies to convince governments to end persecution of scientists, engineers, and medical personnel who have suffered as the result of exercising rights guaranteed by the UN Declaration of Human Rights. Sometimes we succeed, but even when we fail it is important to have tried. Efforts like this are important because more than 50 years after the end of World War II, and a decade after the end of the cold war, nations and peoples have failed to curb racial, ethnic and religious violence, and progress in the establishment of civil societies has been slow. In fact, the end of the cold war released suppressed hatreds rather than reducing them. Individuals and small committees can't do much, but it is important that people make some effort to improve the situation.

I started this talk with a description of my heritage, the source of the assumption that scholarly work was something to be respected and supported. The question then becomes: Did Evelyn and I provide the same for our children? I think I can say yes. They are well educated and motivated to contribute to society in their own way. I'm proud of them.