



A Career in Superconductivity

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November 19, 2019 (PA45). Recently I retired after about 60 years as a research scientist and as a scientific program officer at the (US) Air Force Office of Scientific Research (AFOSR), devoting my activities for the most part to the field of superconductivity. What follows is a personal history of my scientific career with emphasis on superconductivity, plus some observations I've made on progress in uncovering new superconducting materials and in finding new applications for them. Finally, I'll try to provide some

comments on the future of superconductivity and its applications.

In the Beginning

My early training as a graduate student at Cornell University, starting in September 1956, could be described as primarily in low temperature physics. I had the good fortune of being a research assistant for my entire time as a grad student there. At that time superconductivity was considered as an exotic phenomenon with considerable potential, but with little practical application thus far. My only direct connection to superconductivity during that period was use of a superconducting heat switch between a helium-3 refrigerator and a paramagnetic salt in achieving an operating temperature a little below 50 mK.

At that time there were few, if any, companies making reliable low temperature equipment, and useful superconducting solenoids had not yet been constructed with fields of greater than about 4 kilogauss (0.4 tesla). Then, in the late 1950s, theoretical and experimental discoveries were made which gave new life to the field of superconductivity. I refer to the creation of the BCS theory and Brian Josephson's subsequent contributions related to what became known as the DC and AC Josephson Effects., as well as the Russian theoretical contributions attributed to Ginsburg, Landau, Abrikosov and Gorkov, and to the discovery of the A15 class of superconductors, primarily at Bell Labs. In the following 20 to 30 years there were parallel developments in high magnetic field and Josephson junction technology, with commercial manufacturing of superconducting magnets for magnetic resonance imaging (MRI) and for high energy particle accelerators. Nb_3Sn became popular for the manufacture of high magnetic field solenoids. In the laboratory I established at the Illinois Institute of Technology in Chicago starting in late 1965, I was able to incorporate a 10-tesla solenoid with 5-cm bore over an active length of about 20 cm. This enabled us to make thermal conductivity and specific heat measurements as a function of magnetic field down to 0.3 K temperature in a home built He_3 refrigerator.

To be sure, attempts were being made to find other applications of superconductivity during that period. Although many were found, commercial success on a grand scale was still limited to companies that produced magnets for MRI systems and for large elementary particle accelerators, while a number of

rather small companies were able to deliver custom-built He₃ refrigeration systems and ultimately He₃-He₄ dilution refrigerators.

At the current time, it is possible to purchase “cryogen-free” cryogenic systems, which I consider something of a misnomer because these are in essence closed-cycle systems with the cryogens of choice totally enclosed. As long as there is no “leakage” of cryogens, nothing needs to be added. While these commercial refrigeration systems are rather expensive, I am envious of today’s low temperature scientists. They are able to take data utilizing commercial refrigeration and electronic equipment, and to analyze data via computer analysis. My first years as an experimental physicist involved taking data by reading the mercury level of a manometer to determine the temperature of a liquid-helium bath, and then carrying out hand calculations with paper and pencil. If I got lucky, I was able to borrow an electro-mechanical calculator to save some time. Eventually, strip-chart recorders, and oscilloscopes came into use. Finally, the burden of taking data “by hand” was replaced by modern computer analysis.

My Cornell PhD thesis research was done under David M. Lee, who had just finished his own PhD thesis research at Yale. He came initially to Cornell as an instructor while he finished writing his PhD thesis. Together, as his first and (for over a year) his only student, we moved into an empty room and constructed the first laboratory at Cornell that could achieve temperatures below 1 K. My thesis was based upon a search for a superfluid phase of helium three in the vicinity of 50 mK, as predicted about 1959 by several theorists who applied BCS theory to the He₃ nucleus. After earning my PhD in 1962 and continuing for a couple of months as a postdoc at Cornell, I became an assistant professor of physics at Michigan State U., where once again I built a He₃ refrigerator, but this time with glass walls in the cryogenic region so that it could be used to study the magnetic properties of antiferromagnetic insulators via magnetic resonance and specific heat. I moved to IIT in Chicago as an associate professor of physics in 1965, advancing to professor in 1973.

At IIT I initially carried out a variety of thermal conductivity studies down to 0.3 K on alkali halides and other materials as a function of radiation damage. This work was funded for 15 years by the Department of Energy (DoE). During that period I was encouraged by a DoE program officer to consider working with superconducting A15 alloys and to measure their critical current as a function of temperature, magnetic field and high-energy neutron irradiation. The connection to DoE interests was very strong because of plans to use superconducting coils to confine a radioactive plasma emitting high energy neutrons in a controlled fusion environment. The goal was to determine whether there would be degradation of critical current as a function of fast neutron irradiation. Such irradiation was to be done while the superconducting coils were immersed in liquid helium, and thermal annealing of some defects occurred above liquid nitrogen temperature. Neutron irradiation was done at Argonne National Laboratory (ANL) with the A15 samples immersed in liquid nitrogen. Without allowing each irradiated wire to warm above liquid nitrogen temperature, individual wires were mounted into a dewar and cooled further to 4 K, after which the critical current measurements were made at increasing temperatures.

In the summers of 1969 and 1971 I worked at Sandia National Laboratory (SNL) in Albuquerque, trying to construct a helium dilution refrigerator. Before leaving for Albuquerque in 1971, an IIT colleague who had once been employed at Los Alamos National Laboratory (LANL), suggested that while in Albuquerque, I should contact some scientists at LANL who might be interested in low temperature thermal conductivity measurements on carbon that had been done in my IIT lab. I made this contact and was invited to speak at LANL. The small group that I spoke to, informed me that they had moved on to other interests.

Nevertheless, in circulating my credentials, there was one senior scientist who seemed quite interested in talking to me. I had time to visit that scientist, Bill Overton (William C. Overton, Jr.) briefly and discovered an extraordinary scientist with whom I ended up collaborating, for the following 15 years. From 1972 through 1978 I spent one month each summer, primarily measuring (under various conditions) the velocity of propagation of the normal/superconducting interface in current-carrying wires for which a normal region was triggered at one end of a wire carrying a supercurrent. During the academic year at IIT, my lab measured thermal conductivity of these same wires with the goal of providing Bill Overton with data to test a theory he had developed for the velocity of the normal-superconducting interface.

The LANL group in which Bill Overton resided initially was funded by the LANL weapons division. Yet some of the first members of that particular group were primarily low temperature physical chemists. Some of the early members were hired in the late 1940s and along with some physical chemists at ANL, were the first, and for some years, the only US groups to measure the thermodynamic properties of He3. Just prior to the retirement of one of these cryogenic pioneers, I had the opportunity to ask about the reason for this fundamental science being associated with weapons. The response was that when work on a hydrogen bomb began, it was thought that to obtain sufficiently high density of deuterium and tritium, it would be necessary to maintain them in the liquid state, and that required low temperature, which could be obtained by thermal contact with a liquid helium bath. According to the history lesson I was receiving, in the initial concept, a hydrogen bomb would incorporate a helium liquefier. Although the final bomb design didn't require this component, these low-temperature scientists turned their attention to superconducting materials and how superconducting materials could be valuable to the DoE mission.

The LANL group working on superconductivity had 2 major projects when I joined it as a visiting staff member in the summer of 1972. One centered on building a DC transmission line. The other was to build a high field magnet for energy storage of up to 30 MJ. On completion of the latter project, the magnet was to be tested in the hydroelectric power system on the Columbia River in Washington State. Although, this energy storage application has not (to my knowledge) been universally adapted, it may be of value if the magnetic fields generated are sufficiently high to produce energy density storage greater than that attained using capacitive energy storage.

In 1979 I was planning to take a sabbatical leave encompassing 6 months at INSA de Lyon, France and 6 months at the Technion in Israel, when I received a large National Science Foundation (NSF) grant to establish a multimedia educational center at IIT. This involved matching funds from IIT and recruiting 2 faculty colleagues in chemistry and mathematics. Since I had conceived of this center on my own, I felt compelled to postpone my sabbatical and devote half of my time to the center's establishment for the next 3 years at least. On the other hand, my IIT lab was relatively dormant in preparation for my sabbatical. Then another employment opportunity presented itself. By chance I learned that the Chicago office of the Office of Naval Research (ONR) wanted to hire a local physics faculty member for a 1 or 2-year period. I looked into this, and was hired to work half-time for ONR in Chicago. Logistically this was very workable because the ONR office and the IIT campus were separated by about 10 kilometers.

Upon joining ONR, I learned that the major activity for those in the Chicago office and 2 other ONR branch offices in Pasadena, CA and Boston, MA was to provide assistance to the program managers at ONR headquarters in Arlington, VA. From previous contacts in attempting to obtain an ONR research grant, I was familiar with Ed Edelsack, who funded a program primarily in superconducting electronics. When I did contact Ed, he informed me that what little assistance he did require, was provided by a program

manager in the ONR Pasadena office. While that was disappointing, I had to discover someone at ONR in Arlington who could find something useful for me to do. I mentioned that I had something of an amateur interest in geophysics and had taught a mini-course in geophysics in the recent past. As luck would have it, there was a program manager in Arlington who needed lots of help, particularly in organizing program reviews. He was a most likable person who had done field work in geophysics, but who had limited organizational skills. It was a perfect match. There was a need to uncover new reservoirs of crude oil around the world, as well as regions of mineral and precious metals beneath the earth's surface. The US Navy also used magnetometry in aircraft to search for submarines.

Another ONR program manager in the geophysics area, on learning of my background in superconductivity, mentioned that people doing various forms of magnetic mapping of the earth's surface were starting to utilize superconductivity-based magnetometry, and he said that it would be of some help if I could provide him and others with additional information on this technology which was somewhat foreign to those using conventional magnetometry. During this same period I was still in frequent contact with Bill Overton at LANL. The research done with him through the 1970s was coming to an end, and he had become involved with geophysicists. A group at LANL was investigating the potential for power generation using hot-dry rock geothermal energy. The concept is relatively simple. If one is over a sub-surface region of the earth that is found to be very hot, a hole is drilled into this hot region. The rocks near the newly-drilled hole become fractured. Another hole is drilled some distance away from the first hole. Then water is introduced into the first hole under high pressure. That water is forced through the cracks, becoming super-heated in the process, and then returns to the surface as high pressure steam after traversing to the other hole. It sounds rather simple, but how does one know where to drill the second hole? If it isn't in contact with the cracks from the first hole, no steam will be transmitted through it. Bill Overton suggested that after the first hole is made, that it be flooded by a ferrofluid. One can think of ferrofluid as a liquid magnet. Then on the earth's surface in the vicinity of the drilled hole, one can do a magnetic mapping, with the magnetic field primarily due to the ferrofluid filling the newly-made cracks. To achieve maximum sensitivity Bill used a superconducting (SQUID) magnetometer. It appeared to be sufficiently sensitive to provide the information required to know where to drill the second hole.

From our discussion at Los Alamos there developed a workshop titled SQUID Applications to Geophysics, held at LANL, June 2-4, 1980, supported by both LANL and ONR. Our original expectation was an attendance of about 30 people. In fact, there were 91 present (from the US and 4 foreign countries) and 26 presentations. One experienced geoscientist stated that in working in remote regions around the world, once one overcame the problem of transporting liquid helium, superconducting magnetometers were by far the most reliable and durable in comparison to all others. The proceedings were edited by Bill and me, and were published by the Society of Exploration Geophysicists in 1981.

While that workshop was of significant value to the geophysics community, personally it was the singular event that produced the trajectory that took me to a temporary job at AFOSR on September 1, 1984 and to a permanent job there from June 30, 1986 until my retirement on October 31, 2018. Word of the successful workshop at LANL reached Ed Edelsack at ONR. He then asked me to visit and report on the progress of scientists he had been funding on superconducting electronics at the University of Minnesota and at the University of Illinois (at Urbana-Champaign). A little later, he provided to me a small grant so I could publish a monthly Cryogenic Information Survey and a quarterly Superconductive Technology in Review. The latter of these contained in each issue a literature review of small-scale (electronic) applications provided by Marty Nisenoff at the Naval Research Lab (NRL) and a review of large-scale

applications provided by Moises Kuchnir at Fermilab. Prior to the universality of the Internet, these were labor-intensive activities.

Ed Edelsack had earlier exhibited interest in seeking applications of SQUID magnetometry, in particular medical applications. He became aware of the development of the RF SQUID by Arnold Silver and Jim Zimmerman at the Ford Research Lab in Michigan. After Jim Zimmerman moved to NIST in Boulder, CO, Ed contacted him and convinced him to join him on a visit to the Francis Bitter National Magnet Lab at MIT in late December 1969 and to bring a working SQUID magnetometer. Most research at that lab involved use of the highest steady state magnetic field that could be produced at that time. The goal of the Edelsack – Zimmerman visit was to use a magnetically-shielded room that had been constructed by David Cohen of the magnet lab staff. Taking turns, one of the 3 scientists would enter the shielded room. With the tail of the SQUID dewar near that person's heart, it was possible to record the magnetic signal produced by each heartbeat of the person within that room. A few years later, Ed provided funds to NRL to purchase a commercial SQUID magnetic gradiometer with the hope that he and Marty Nisenoff at NRL could connect with a local research hospital or with the National Institutes of Health (NIH) to investigate the potential usefulness of SQUID magnetometry for noninvasive medical diagnostics. The SQUID system arrived at NRL, but nothing much was done with it because of the press of more urgent matters and a lack of success in finding willing collaborators in the local medical community.

Fortunately, biomedical research utilizing the unique sensitivity of a SQUID magnetic gradiometer was progressing elsewhere with financial support from ONR. Most notably, the collaboration at New York University (NYU) of Lloyd Kaufman, a professor of cognitive psychology and Sam Williamson, a physics professor, created a new field based upon the magnetic response of the human brain's outer layer (cortex) to various stimuli. All of their early work was done in an unshielded magnetic environment on an upper floor of a building that had to contend with the electromagnetic noise of NY subway trains running beneath the building. One of their early experiments involved observing the magnetic responses in the brain to acoustic signals of various frequencies. Because of signal-to-noise issues, it was necessary to take data for 30 minutes at each location on a grid mapped upon the surface of a subject's head, move the magnetometer to a location about 1 cm from the previous location and take the average of another 30 minutes of data. Thus, it could take a few days to map the location of the cortex's response as a function of the acoustic frequency. This resultant location of the acoustic signal as a function of frequency, is known as a tonotopic map. It showed that the human cortex records acoustic signals in different locations for different frequencies. There were subsequent studies of where the cortex senses visual signals and physical activities. Research in Finland, Italy and Japan provided further neuromagnetic studies, often by scientists who had spent some time at NYU.

Having delayed my sabbatical to establish the Educational Technology Center at IIT over a 3-year period, I began to search for new opportunities to engage in research, preferably involving superconductivity. Ed Edelsack came to my rescue by arranging to fund me to work at NRL with Marty Nisenoff. Specifically, Ed had funded research to develop pulse-tube refrigerators with the intention of seeking more compact SQUID magnetometers for various applications. His intention was that I should determine whether this new type of refrigerator would be compatible with the operation of a SQUID magnetometer. I arrived at NRL about September 1, 1982. Marty showed me how to calibrate and operate the SQUID system which had been mounted in a conventional dewar system containing an outer jacket of liquid nitrogen and an inner jacket of liquid helium. This entire unit was seated in a wooden frame totally devoid of any magnetic object, e.g., there were no steel screws or nails. After a few weeks I was ready to do something with this

SQUID unit and its non-magnetic “harness.” Meanwhile there were no pulse-tube refrigerators being delivered. I decided to see if one could use the SQUID for nondestructive evaluation (NDE). Marty found in a junk pile at ONR a metal pipe that was about 1 meter long and about 5 cm in diameter.

Using a hacksaw to make one rather irregular hole and a drill press to make a round hole farther along this pipe, I then injected a 4.6 Hz current along the length of the pipe. The pipe was placed under the tail of the SQUID’s dewar, and the pipe was moved lengthwise under this tail. This was more convenient than trying to move the SQUID and everything that went with it. Using an oscilloscope to observe the 4.6 Hz voltage signal of the SQUID output electronics, I had made a poor person’s phase sensitive detector. For a magnetic field detector orthogonal to the flow of current in the round pipe and centered over it, there should be no signal in the section that had a perfectly uniform cross section. However, in the region of the pipe containing one of the holes, current was not uniform and had components in directions orthogonal to the axis of the pipe. This proved to be the case. The point here is that I had used the superior sensitivity of a SQUID magnetic gradiometer to produce a potential new application. Upon returning from NRL at the end of my sabbatical year, I contacted a major company in the business of finding cracks and corrosion in buried pipelines carrying either liquids or gasses. I received a good education on the state of the NDE art, and the company took an interest in this potential new NDE technique.

One other idea I had in the realm of nondestructive evaluation, was to monitor the magnetic field near a steel rod as a function of stress. I mounted the SQUID sensor near a steel rod in a tensile-testing machine and was able to see large changes in the magnetic field near the rod being stressed. At about $\frac{3}{4}$ of the way to the elastic limit, there was a reversal of the field. This reversal was found consistently for several samples, and it struck me that this might provide a means to monitor stress in the steel superstructure of large buildings and bridges. In the lab with the sensor only centimeters from the steel rod, the signal was so huge that an NRL colleague repeated the experiment using a conventional flux-gate magnetometer and obtained the same results observed by the SQUID-based magnetometer. While this might indicate one doesn’t require the enhanced SQUID sensitivity, in evaluating the integrity of a building’s superstructure, the sensor being used can’t be placed so close to the beam being evaluated. This result became the basis for a patent that I was issued a few years later.

Mindful of Ed Edelsack’s interest in the potential for biomedical applications, I next took steps to make contact with NIH. Back at IIT I had a friend in the EE department who had a large NIH grant to develop an electrical interface between neurons and artificial limbs. I requested the names of people at NIH I might contact. These people were in an engineering section of NIH who worked with the biomedical staff to develop new SQUID-based biotechnologies. I contacted a leader of this engineering group and said I’d like to speak at a seminar on the capabilities of SQUID magnetometry and on some recent studies made with this technology. All I asked in return as “payment” were the names of people at NIH who they thought might be candidates for using SQUID technology. Following these leads, I had a response from 3 individuals. One person was a psychiatrist, and it was quickly determined there was no match. A second was from someone who inserted electrodes into the brains of monkeys. After observing the SQUID system and all that must accompany it, he decided that it was much less trouble continuing to insert the electrodes in the brains of monkeys. I finally had success when I met with Roger Porter, who headed the Epilepsy branch in the National Institute of Neurological Disease and Stroke (NINDS). He had been hoping there would be some noninvasive technique that could detect the presence of “interictal spikes” that are low amplitude, random electrical pulses emitted by the brain of an epileptic in the interval between major seizures. At that time the only technique available involved an EEG measurement in which 14 electrodes

are attached to a patient's scalp, with an electrical signal from each electrode recorded simultaneously on a strip chart recorder. Normally one sees low level noise on each of the 14 signals recorded as a function of time. For an epileptic person there may be an interictal spike buried in the noise at random intervals of several seconds. One of the staff doctors, Susumo Sato, was an expert at finding these spikes buried in the noise, and postdocs would come to NIH to observe his technique, based primarily upon human visual observation.

After numerous false starts, the SQUID magnetometer was employed in a room at NIH that had relatively low background noise, and data were obtained on 2 patients, with the results of this study subsequently published. Convinced that they were advancing the state-of-the-art, our NIH collaborators decided to purchase their own superconducting magnetometer system. It is noted that in order to collaborate with them, I was required to order a small truck and driver to transport the entire SQUID system and a 50-liter liquid helium storage dewar from NRL to NIH. Then after about a week, the equipment was returned to NRL. It also was obvious that for a patient undergoing an epileptic attack, there would be significant benefit if measurements could be made quickly. Thus, NRL ordered a 7-sensor magnetometer system, which at that time made it the largest SQUID-based magnetometer system in existence. Today there are over 130 systems worldwide with between 100 and 325 sensors, with many of these systems operating in magnetically-shielded rooms.

Quite unexpectedly, during my sabbatical year, I engaged in the 2 studies just described. Yet, the research project that occupied the largest part of my sabbatical year at NRL, and ultimately provided the greatest satisfaction intellectually, was suggested by an IIT colleague, Tom Erber. Tom is primarily a theorist who studies electrodynamics, although he has broad-ranging interests and works closely with experimentalists. Early in his IIT career, which now has spanned over 6 decades, he had a chance encounter with a civil engineer that ultimately led to a lifetime study of hysteresis in mechanical structures and in magnetic materials. In discussing with Tom the outstanding sensitivity of a SQUID magnetometer, he suggested the possibility of studying Barkhausen noise in ferromagnetic material. NRL had a rich history in studying magnetic materials, and I gained much insight by interacting with a number of the scientists there, among them I especially recall Conrad Williams and Stu Wolf, although there were others who helped educate me in this area.

The experiment that Tom Erber suggested involved acquiring a demagnetized piece of pure iron that would then be exposed to a steadily increasing ambient magnetic field while observing the change in the magnetic moment of the iron sample. The goal was to study the magnetization of the iron as a function of hysteresis cycles. The magnetic materials group at NRL was able to supply me with a suitable iron rod, but there were some barriers that were difficult to overcome. For instance, using a conventional degaussing coil in the earth's magnetic field, would not reduce the magnetization of the sample to zero because it would be subject to the earth's magnetic field, a few tenths of a gauss (or 10^{-4} of a tesla). Fortunately, Stu Wolf was aware of a unique facility at the nearby NASA Goddard Space Flight Center

in Greenbelt, Maryland. When one thinks of that facility, one sees modern buildings full of electronic equipment with a staff of astrophysicists and engineers, but a considerable portion of that NASA campus is comprised of virgin forest which only a small number of the Goddard employees are aware of. In the midst of that forest are 2 relatively small and unique buildings. One of these is a control center inhabited by 1 or 2 technicians at a panel that enables them to control the magnetic field environment in the other nearby building. This second building is totally nonmagnetic. There is not a single ferromagnetic nail or

screw in the entire building. . Within it is a 3-dimensional (Braunbeck) coil system. One can think of this as 3 mutually perpendicular Helmholtz coils, 2 meters in diameter. Each set of coils is fed a small amount of current as dictated by nearby sensitive conventional magnetometers, such that the net magnetic field within a central one-meter diameter spherical region, this coil structure is approximately zero to a rather high degree of precision.

After placing the iron sample and the SQUID system within these exceptional coils and degaussing, we had a starting point as close to zero magnetic moment as humanly possible. Then a slowly increasing current was applied to the coils that produced a magnetic field along the axis of the iron rod. The SQUID magnetometer measured the increasing magnetic field as a function of time. At first, the measured magnetic field increased linearly with time. At some point there would be a sudden jump in the magnetic field near the surface of the iron rod. This was interpreted as the observation of the threshold for Barkhausen jumps, meaning that there was sufficient field strength to cause a magnetic domain to reorient along the direction of the increasing magnetic field. Upon continued increase of the ambient field, additional Barkhausen jumps were observed. At some point we slowly reduced the current in the coils whose field was parallel to that of the rod. As this was done, no jumps were observed on the descent to zero external field. Upon increasing the field a second time, no Barkhausen jumps were observed while traversing the range of coil currents that had previously produced the Barkhausen jumps. However, those jumps reappeared once the previous highest field had been exceeded. After repeating this cycle several times and attaining a higher field value on each succeeding cycle, it was felt that we had observed the fundamentals of magnetic field training. However, at one point we reached a second threshold value of magnetic field for which it was impossible to eliminate Barkhausen jumps. This observed behavior and much more were recorded and analyzed, with additional information coming from similar measurements on iron whiskers. After detailed analysis by Tom Erber and me of all these observations during the following year at IIT, we published our findings as a 10-page article in Physical Review B.

In summary, I spent a sabbatical year (1982-1983) at NRL with the initial goal of using a SQUID magnetic gradiometer to measure the magnetic noise in a variety of pulse-tube refrigerators. Those refrigerators never were delivered. Instead, I initiated 3 distinctly different studies which had in common only the use of SQUID-based magnetometry. Initially I used this instrument as a tool for nondestructive evaluation while at NRL. This was followed by the study at NIH of neuromagnetic anomalies in the brains of people stricken with epilepsy. Lastly, at a unique NASA facility in Greenbelt, Maryland, I was able to study the magnetic behavior of ferromagnetic materials due to changes in the local magnetic field. With additional work done during the following year by myself and many others, there were a total of 6 refereed publications that appeared over the following 2 to 3 years, plus one patent on the use of a SQUID as a tool for nondestructive evaluation. I wish to emphasize that I could be considered only as a catalyst in these studies, with my largest contribution being my drive to not let go of anything until it was clear that nothing else could be done to increase our understanding of what had been observed.

Back at ONR headquarters, Ed Edelsack was pleased with the outcome of my sabbatical year activities, and perhaps with his help, Marty Nisenoff successfully nominated me for an American Society for Engineering Education (ASEE) summer faculty fellowship at NRL in 1984. During that summer of 1984 while I was extending some of the studies initiated during my sabbatical year, I received a phone call from Ed Edelsack saying that the long-serving program manager for superconductivity at the Air Force Office of Scientific Research (AFOSR), Max Swerdlow, had just resigned due to poor health – he had been diagnosed with metastatic prostate cancer. There were 2 jobs that had to be filled: a temporary one to take over

almost immediately, remaining until authority for a permanent position had been obtained, and then a permanent job after a successful search for Max's successor had been concluded. After providing a talk at AFOSR (then located at Bolling AFB) on my research in superconductivity, I was offered and accepted the temporary position for one year, after having received permission to take a leave of absence from the IIT dean I reported to and from my wife. Unlike my sabbatical just 2 years earlier, I would need to live alone in the Washington DC area. On the positive side, despite his poor health, Max Swerdlow was extremely helpful in educating me on the details of the job, and on the scientists whose research was being supported. At this point, I should mention that AFOSR's sole mission is to fund basic research for the US Air Force, research that could lead to superior Air Force technology at some future time.

I arrived at AFOSR on September 1, 1984, as did the man who would be my supervisor, Horst Wittmann. His title was Director of Electronic and Material Sciences. After he settled into his new job, and it was time to initiate a call for a new permanent program manager, Horst informed me that his primary goal was to hire someone with expertise in electronic circuit theory. When no suitable candidate answered that call, he was determined to advertise again, while keeping the superconductivity program intact for the immediate future. At about the same time, I was offered a new administrative position at IIT once my year at AFOSR concluded. Given no apparent future at AFOSR, it seemed most sensible to return to IIT on September 1, 1985. Responding to a plea from Horst Wittmann, I agreed to work part time for AFOSR after that date. A few weeks prior to my return to IIT, I received a visit from AFOSR's Technical Director, John Dimmock, with whom I had established an ongoing dialog on condensed matter physics based primarily upon discussions of contemporary articles in leading journals. As my supervisor's boss, John indicated that while he didn't wish to overrule Horst's hiring decision, he had discovered a way around it. John stated that he had found another unfilled position within AFOSR, and that he would like me to compose a job description for that position, as well as hoping that I would apply for said position. That I did. In January 1986 I received an urgent call from Horst Wittmann. Rumor had it that a government hiring freeze was scheduled to occur about 2 weeks later, and he hoped I would accept an offer ASAP. I did accept and reported for work at AFOSR on June 30, 1986. Horst Wittmann remained my supervisor for the next 10 years, at which time he was reassigned to a supervisory position in the Sensors Directorate of the Air Force Research Lab at Hanscom AFB. After over 34 years as an AFOSR program manager (or program officer), I retired on October 31, 2018. At a retirement ceremony 6 days earlier, I was happily surprised by the attendance of Horst Wittmann, who had come from his home in the Boston area to pay tribute. In those 34 years, I had an amazing opportunity to help advance the discovery of new, more useful superconducting materials and to assist in the development of new applications of these materials. Along the way, I was able to provide funding for other fundamental research on metamaterials and nanoelectronics, in addition to superconductivity.

Funding Basic Research in Superconductivity for over 3 Decades

In 1986 the record high temperature for superconductivity had been increased (to about 35 K) by almost 50% by 2 IBM scientists in Switzerland. At first there was little interest in the broader scientific community and in the general public for a lanthanum, barium, copper oxide ceramic material. Nevertheless, a significant number of scientists around the world successfully verified the discovery and proceeded to make an array of atomic substitutions, and in some cases, applied pressure to the resulting material structure. Via a number of sources in the superconductivity community of scholars, I became well aware

of what had been happening. One especially good source was Paul Chu at the University of Houston. He had arrived in Washington DC in early 1986 to be a “rotator” at the National Science Foundation (NSF) with initial plans to remain 1 or 2 years. Paul had been engaged in a search for new, more useful superconductors ever since he had been a graduate student under Berndt Matthias at the University of California at San Diego. I was fortunate to have discovered that Paul had come to DC when earlier that year I was contacted by Brent Mattes, a material scientist who was living in Michigan with no academic or industrial affiliation, and only a relatively small investment from a local industrialist to pursue a search for superconductivity at the interface between copper chloride and silicon.

Mattes believed he had discovered evidence of superconductivity after he had convinced Carl Foiles, a physics professor at Michigan State University (MSU), to measure the magnetic susceptibility of some of his sample films, using a SQUID-based magnetometer, which was not as ubiquitous an instrument as it is now. The results were embodied in a presentation by Mattes at an international conference on d and f-band superconductivity, held that year at Ames, Iowa. The measurements of susceptibility showed what appeared to be diamagnetic behavior below a temperature above 200 K, and the diamagnetic behavior seemed independent of thickness of either film. There was a limited amount of data backing up the interpretation of the MSU measurements. By coincidence, I had shared an office with Carl Foiles at MSU in 1964-1965, his first year there as a new postdoc. I had a high regard for his ability and his integrity. I called him and asked about his role in this research. He informed me that he stood behind the susceptibility measurements, and that was good enough for me. I invited Mattes to give a talk at AFOSR. Mattes mentioned that Paul Chu had just arrived at the NSF and asked if I could invite Paul to attend his presentation. I was pleased to do that because I was aware of Paul’s stature in the field of superconducting materials. After Mattes spoke at AFOSR, I felt that to consider supporting his research more seriously, it first would be necessary to measure resistance of his films as a function of temperature and applied magnetic field. I found an Air Force lab at Wright-Patterson Air Force Base that was able to help Mattes attempt to make such measurements. Unfortunately, such valid data never materialized. However, because of Mattes, I initiated contact with Paul Chu, and for the past 33 years he and I have interacted continuously in a quest for the discovery of more useful superconductors, although my contribution has been mainly to provide financial support and to arrange workshops, conferences and summer schools to aid Paul and many others who are leading the charge to make life on this planet a bit more sustainable through the discovery of superconducting materials that can operate effectively at somewhat higher temperatures.

Paul began commuting to Houston just about every weekend, and all those working in his lab were organized in the task of seeking other copper oxide materials that might be superconducting at even higher temperatures. In early November we met by chance at the Magnetism and Magnetic Materials Conference in Baltimore. He confided he had been applying pressure to a cuprate material, which I thought was that which Bednorz and Mueller had discovered to be superconducting, and that he had increased the superconducting transition temperature to 44.5 K. Furthermore, Paul said that he believed he could increase that transition temperature to the NBP of liquid nitrogen (77 K) by Thanksgiving.

Knowledge of the discovery made by Bednorz and Mueller began to spread quickly by the latter part of 1986, and there were few materials science, chemistry or physics labs anywhere in the world that were not attempting to discover some variation of that material that would exhibit a significantly higher superconducting transition temperature. Meanwhile Paul Chu had a former student, Maw-Kuen Wu, who was likewise in this hunt, and they decided to join forces, that is, to divide up the most promising

parameter space for the cuprate materials. About the end of November, M.K. Wu's lab at the University of Alabama at Huntsville, discovered superconductivity at a little over 90 K in yttrium barium copper oxide (YBCO or $\text{YBa}_2\text{Cu}_3\text{O}_7$). A paper was submitted to Phys Rev Letters (PRL) and scheduled for publication in the March 2, 1987 edition. The goal of Chu and Wu was to keep the composition of that material secret until the March 2 publication date in PRL. An abstract for a 10-minute talk at the 1987 March Meeting of the American Physical Society (APS) meeting, to be held 2 weeks after that publication date, in New York City was submitted just prior to the deadline for abstracts. It was rejected, but not for scientific reasons. It was one-half of a line too long! Nevertheless, this amazing increase in the known critical temperature of a superconductor was too important to ignore, especially since the APS March Meeting had become annually the world's largest assemblage of physical scientists.

To address this need, a special evening session was scheduled during the APS March Meeting, specifically with 10 minutes allotted for each featured speaker, and 5 minutes each for everyone else. There were a total of 51 presentations, and starting in the early evening, the session ended at 3:15 am. Because I had attended a small committee meeting on international affairs that ended just prior to the start of the special session on superconductivity, I was afforded an opportunity to enter the besieged room via a service entrance and claim a seat near the front of the room before the door had to be locked to keep the room from bursting with human bodies. While I don't claim to be an aficionado of rock concerts, I can't imagine an audience any more raucous than the hundreds of physicists packed into that room. I must confess that I failed to remain in that room to the bitter end, and the next day I noticed some attendees were wearing small badges stating that they had remained in that session until it had concluded. The next day the New York Times featured a first-page article, headlined as the "Woodstock of Physics".

It is impossible in a single page to convey the hysteria that existed among the science community, and by extension, the general population both in the US and in developed countries around the world. This was fueled by the statements of scientists and engineers at all levels who made wild projections of what to expect for the highest superconducting temperature to be discovered and what new applications would be found for these anticipated higher temperature superconductors. While my comments on the future of superconductivity were somewhat circumspect, I was not immune to making unfounded projections. Someone from the US Office of Technology Assessment (OTA) heard me chatting while on a hike with an AFOSR colleague near Washington. She introduced herself, and after a brief discussion, invited me to present a lunchtime seminar at OTA. This agency, which was disbanded in 1995, had served for over 20 years as a non-partisan advisory group of scientists and engineers employed by the US Congress. I made the requested presentation to the OTA on the recent advances in superconductivity. One questioner asked me how long it would be before we had superconductivity at room temperature. As the record temperature for superconductivity in YBCO had been exceeded by at least 3 other copper oxide compounds, with mercury, barium, calcium, copper oxide holding the record at about 134 K at ambient pressure. Furthermore, Paul Chu had applied 30 GPa to that record cuprate material and had been able to increase the critical temperature under that pressure to 164 K. Given the rate at which discoveries had been made and the enormous number of laboratories worldwide that were engaged in searching for even higher temperature superconductors. I foolishly stated that superconductivity at room temperature would probably become reality in 1 year. Some months later, OTA was commissioned to make a study of progress in developing new superconducting technology. The specific title of this report, which was dated April 1990, and which was produced after an 18-month study, is "High-Temperature Superconductivity in Perspective."

I mention the OTA report because the OTA Project Director for it, Greg Eyring, was invited to be an after-dinner speaker at Bolling AFB for a group of Navy and Air Force-funded scientists who had taken part in a joint program review earlier that day at NRL. Greg's talk was intended to provide a brief summary of the findings of the OTA report. He began by stating that he was introduced to this subject when he heard my lunch presentation at OTA. He then recalled that I had stated it would be 2 years before superconductivity would be found to exist at room temperature. I immediately arose and loudly proclaimed: "Wrong, I said one year." So much for my great predictive powers with regard to the discovery of room temperature superconductivity.

Joking aside, the discovery of superconductivity in copper-oxide ceramic materials was front-page news in newspapers everywhere. Part of my motivation in accepting the AFOSR job offer, came from the fact that AFOSR was then located within Bolling AFB, which in turn was adjacent to NRL, and I believed I would be able to continue my pursuit of SQUID-based applications in my spare time. While the first few months of my permanent residency at AFOSR did involve some research at NRL, the universal excitement over the discovery of ceramic materials that were found to superconduct at a temperature about 5 times higher than any known superconductor prior to 1986, had a profound effect on the path I would follow for the remainder of my career. The careers of many scientists and engineers were rerouted as well. The sad thing is that very few people were looking at this major discovery objectively. Substituting liquid nitrogen for liquid helium obviously would reduce the cost and complexity of operating superconducting magnets, but for the most useable ceramic superconductors, e.g., YBCO and BSCCO, it still wasn't enough of an improvement to operate at a temperature of about 60 K, well below the normal boiling point of liquid nitrogen. Furthermore, how does one take a ceramic powder and convert it into flexible tape capable of carrying a few hundred amps hundreds of meters in length and sufficiently flexible to be wound into a high- electromagnet?

There was no good response to this rather important question until the spring of 1987. A few months earlier I had been interviewed by a reporter for Fortune magazine, who after speaking with a number of people across a broad spectrum of the superconductivity community, was preparing to write an article that would state that most of the excitement associated with the newly discovered copper oxide "high temperature superconductors" (HTS) was highly overblown. Then on Friday, May 8, 1987, IBM issued a press release that a team of its scientists in Yorktown Heights, New York had produced a thin (crystal) film of YBCO that could conduct a supercurrent. I first learned of this press release late that afternoon from Ted Geballe, Director of the Center for Materials Research at Stanford University, an acknowledged leader in the quest for new superconductors and in his knowledge of this field over more than 3 decades, going back to his days at Bell Labs and to his establishment of the lab at Stanford that had been funded partially by AFOSR continuously for about 20 years. As the "eminence gris" of superconductivity, Ted had been contacted by a Washington Post reporter who read him the IBM press release and who then requested a quote for the article he was preparing for the Monday morning edition of the Post. Ted hadn't taken notes of the IBM achievement, but he was clearly excited. Unable to respond to my queries, he provided the reporter's phone number and suggested that I call to have him read the IBM statement. Because of the late hour, I made a call to the Washington Post the next day, and was fortunate to speak to Phil Hilts, who was working on the draft of an article scheduled to appear on the following Monday, May 11. He agreed to read the IBM press release to me if I would respond with a quote he could include in his article.

At that very same time, history was being made in the political arena. By far, the leading candidate for the nomination of the Democratic Party for President of the United States was Gary Hart, a senator from

Colorado, and according to the polls, Senator Hart had an excellent chance to be elected President in the general election in November of 1988. However, there had been rumors that Hart had been spending time with a young model. In fact, a number of media reporters bore witness to Gary Hart, a married man, spending that entire weekend at a hotel with a model who was about half his age. On the Monday following that infamous weekend, it would have been safe to say that this revelation should have been the "lead story" of the Washington Post and many other media outlets. While it did appear on page 1, this "hot" news item was relegated to the lower left side of the page. The lead story, the one whose headline scrolls across the top of page one in bold letters, read "Conductor Technology Advances", with a subheading, "Ceramic Material Offers Breakthrough in Handling Current". Although the article itself began by mentioning that this breakthrough was accomplished by IBM researchers, none of them is mentioned by name on page 1, although they are named toward the end of this news item on an interior page. The only people mentioned on the front page were Ted Geballe and I. What follows here is the entire paragraph relating to my comments:

"Up to now, many applications were still at the level of fantasies," said Dr. Herbert Weinstock, head of the Air Force's Office of Scientific Research. "Now they are not fantasies. We can go into the labs and start making them." He said the advance might "unleash" the industrial laboratories, few of which have been willing to commit to major new programs on superconducting products for fear that the full technology would not materialize."

Aside from the fact that the Post had an incorrect first name for me and elevated my position to that of Director of AFOSR, the quotation was correct. I note too that a motion picture titled "The Front Runner" on Gary Hart's actions in 1987 was issued earlier this year, although I know of no plans to film a story on recent progress in HTS.

In retrospect, I think the public's embrace of the progress in superconducting materials was fueled by a simple exhibit of levitation. This involved a YBCO disk about 4 cm in diameter and a few millimeters thick that was placed in a shallow dish, plus a ferromagnetic cube about 1 cm on a side. After introducing liquid nitrogen to the dish, the YBCO disk, after several seconds' reaches thermal equilibrium with the liquid nitrogen bath, and if the cube was resting on the disk, it rises and remains centered over the disk. Alternately, the cube can be lowered once the disk is at liquid nitrogen temperature. At some height over the YBCO it remains suspended on its own. I saw this demonstration performed for members of the US Congress. I attended a 2-day meeting in DC with over 1,000 attendees from both the scientific and business communities at which President Ronald Reagan was the keynote speaker, with the President bringing several of his cabinet ministers to this event. They too witnessed the levitation of a magnet over a superconducting disk. Not to be outdone, I visited a 2-star Air Force general and did a levitation for him. He was so excited that he led me to the office of a 3-star general nearby, and he ended by stating that he had \$2,000,000 dollars of excess funds that he would provide for the basic research program in superconductivity that I managed.

There was no shortage of applicants for the increased funding available for R & D in superconductivity at almost every government funding agency. Projects involving superconductivity were initiated by existing aerospace and electronics companies and at scores of new companies that were established specifically either to find new superconducting materials, to make the newly-discovered superconductors more "user friendly" and/or to produce products that would achieve almost instant demand. Venture capitalists and government agencies did not hold back in the desire to build a new domestic and military world based

upon the unique properties of so-called HTS materials. Numerous workshops were held, sometimes by ad hoc organizations designed specifically to make a profit by charging hundreds of dollars for each attendee who would listen to government scientists and administrators who received no compensation for their contributions. New publications sprung up, e.g., “Super Currents” and “Superconductor Week”.

I was kept particularly busy by being in charge of monthly meetings of representatives from every Air Force lab in which each attendee was required to report on what he or she was doing in developing new superconducting technologies. The name of this meeting activity was “Superconductivity Technical Action Group” or STAG. It seemed somewhat strange to have to tell my wife that I had to attend a STAG meeting every month. In the vernacular, a stag meeting referred to a gathering of men only, generally to discuss whatever it is they wish to discuss, but quite often the female of the species was the most popular topic.

More usefully, I often served as a resource person to high-level committees within the federal government, the Department of Defense, and the US Air Force. One particularly interesting meeting involved an IBM senior engineer who had been in charge of a Josephson Junction production line designed to culminate in the replacement of semiconductor electronics in some of IBM’s mainframe computers. This project was initiated in the late 1970s and was terminated in 1983. There was no public announcement as to the reason for the termination of this effort, but most people seemed to believe that the project was unable to meet its goals. However, the engineer in charge of that project, assured those present that every technical goal had been met.

Why then was the project cancelled? The answer was supplied by a member of the executive panel, Joseph Goode. He arose and stated that as an IBM executive, he had cancelled this project because when it was initiated, an estimate was made that a Josephson computer would be an order of magnitude faster and more energy efficient than the existing semiconductor-based system. However, the semiconductor world was not stationary. The difference in performance was only a factor of 2 or 3 by 1983, and improvements were still being made. The lesson learned in this instance is that it is often necessary to know where the competing technology will be when the new superconductivity-based technology will have been brought to market.

This was just one valuable lesson I learned from the many reviews and meetings I attended in connection with the hysteria surrounding HTS. Over time, the euphoria within the government, the general population, and the media subsided, although technical progress was made in laboratories around the world. In the US and in Europe, China and Japan (among other countries) this progress resulted in a superconducting cable technology. Earlier in this discourse I mentioned that the challenge would be to take a ceramic powder and somehow turn it into a cable carrying hundreds of amps over hundreds of meters, and ultimately, hundreds of kilometers. Thanks to intensive efforts, mainly, but not exclusively in Japan and the US, many of the technical goals were achieved. At first there was the powder-in-tube (PIT) process which was used for $\text{BiSr}_2\text{Ca}_2\text{Cu}_3\text{O}_{10}$ (BSCCO) and later in the 1990s, the 2 dominant processes for YBCO were designated as IBAD, for Ion Beam Assisted Deposition, which was done under a major DoE project at LANL, although the original concept was developed by Bob Hammond at Stanford (under AFOSR funding), and RABiTS (Rolling Assisted Biaxially-Textured Substrates), which was developed and used at ORNL. The US DoE invested \$30 M to \$40 M each year in the last decade of the 20th century and during the first decade of the 21st century to develop this cable technology. A first US demonstration of electric energy transmission occurred in Georgia, where a 30 m section of an HTS cable operated flawlessly for years. I served on the DoE review panel each year, and considering when this all started, progress had

been impressive. I attended one non-government meeting where we discussed for a vast network of underground superconducting power cables by 2050.

I began to consider the possibility of a real HTS-based power distribution system, and thought about the problem of repairing broken power lines after some natural disaster. It seems that no matter how severe the damage, with the help of nearby power companies, “linemen” descend upon the devastated area, and power is usually restored for most people within 1 or 2 days. A buried superconducting power line would be more vulnerable to natural disasters, yet despite a relatively safer environment, an earthquake or a deliberate attempt of sabotage could shut down electric power distribution over a large area and not be repairable for several days or weeks. I asked one DoE project leader how one could deal with such an event. He suggested the use of 2 parallel lines, with each carrying a little less than $\frac{1}{2}$ of the critical current. That would work if only one of the power lines was disabled, but it then doubles the cost without eliminating vulnerability. The suggestion was then made that there could be a conventional power line as additional backup. For whatever reason, this major DoE effort has been scaled-down considerably over the past decade.

At this time, development of improved HTS power cables, magnets and other potential applications continues, and I make no claim to know the current state of these development efforts in the US, although it is clear that interest in superconductivity has returned to normal. By this I mean that there are almost no special government funds to promote superconductor-based technology. Both the federal government agencies that fund R & D, and major industrial labs provide support for this field much as they did prior to 1987, at least in those areas that relate to power applications of superconductivity. Most, but not all, of the companies that were formed in the first couple of years of the HTS era are a distant memory. A couple of them have been quite successful. One of these, Superpower, now is wholly owned by Furukawa Electric, a Japanese company, which lately has been promoting a 275 kV HTS cable system. The other company, originally called American Superconductor, has done well, but mainly because it acquired another company that in addition to products involving superconductivity, sold voltage regulation equipment which was well suited for power generated via wind energy. It ultimately changed its name to AMSC and advertised itself as an energy solutions company. AMSC is now a global company engaged in power control and distribution. After checking recent press releases, I did see one from October 21, 2018 in which AMSC announced an agreement with Com Ed, a company that supplies electric power to 4 million customers in northern Illinois. It mentions that deployment of “AMSC’s high temperature superconductor technology is expected to make the electric grid more reliable for Com Ed customers.” Thus, AMSC is still in the business of producing HTS-related products, but HTS no longer accounts for the majority of its income. However, there is a relatively new company, Commonwealth Fusion Systems (CFS), with plans to produce compact fusion energy generating systems which rely on HTS electromagnets. CFS announced in June of this year that has raised 115 million dollars, and I’ve been informed that it anticipates placing orders for a rather substantial amount of HTS tape.

There has been some success in the area of electronic applications of superconductivity. While there has been recent progress, much of it involves the employment of traditional superconducting materials. I refer first to Josephson Junction (JJ) arrays for quantum computing. While a commercial quantum computer has yet to hit the market, a leading contender in this arena relies on JJs at rather low temperature. Another niche area where superconductivity plays a role is the use of transition edge bolometers for single photon detection, an action that is critical for secure communications. The major reason that HTS hasn’t done much in promoting superconducting electronics is because there has been little development of a chip-

based technology. While HTS JJs can be fabricated in single units, it had been exceedingly difficult to produce a chip for communication applications that required more than a few JJs to be within acceptable margins of performance that leaves exposed those parts of the film that one wishes to be non-superconducting. Then bombard the resulting film with sufficient radiation to destroy superconductivity in the unmasked regions of the film. The initial goal of the project funded by AFOSR was to show that one could produce a JJ in this manner. The first grad student who worked on this project succeeded in producing a JJ, although it wasn't of sufficiently high quality to consider building circuits with desired functionality. Fortunately, the next PhD candidate to work on this project, Shane Cybart, was able to build upon what had already been accomplished, and by the time he had earned his PhD, the JJs produced looked quite respectable.

This concept might have come to fruition sooner, if not for the fact that, despite Dynes's attachment to physically working in his lab every day, he had been taking on increasingly more demanding administrative positions at UCSD, culminating in his being named Chancellor of the UCSD campus. Even that position couldn't keep him from spending some time just about every day that he was on campus, and when I did make an annual visit, I would come on a Saturday, when he typically spent all day in the lab. However, Bob's administrative and leadership skills at UCSD were recognized beyond that campus, and after about 5 or 6 years he was promoted to President of the entire UC system. The office of the UC President is located in Oakland, not far from Berkeley, but over 400 miles from UCSD. Although Bob eventually was able to move his lab equipment to the Berkeley campus, there was a period of more than a year before that lab was functioning at full capacity. The situation could have been worse, had Shane Cybart not stayed on as a postdoc and moved to Berkeley with Bob Dynes.

During the period of inactivity, which I believe coincided with the expiration of Bob's AFOSR grant, I had been asked to give an invited talk at the Spring Meeting of the Materials Research Society. Thinking this might provide an opportunity to speak to Bob directly, I sent him a note saying that I would be in San Francisco for 2 days and inquiring whether we might be able to meet. He replied that he was free for lunch one day and that he would come to the hotel near the San Francisco Convention Center at which I was residing. I assumed that in his exalted position, he would come via a limo or taxi, but in fact, he came via the BART, the local high speed rail line. We spent over 2 hours discussing how we could revive the project to develop an HTS JJ technology. While it wasn't always possible to provide funding for a basic research grant, I hit upon the idea of submitting topics that were for Small Business Innovation Research (SBIR) contracts, and most years at least one of those was at Berkeley's topics was chosen for funding. The important point was to have a topic which required HTS JJs, and most years that required chips that only Shane Cybart, regardless of where he was located. Initially that was at Berkeley; then back at UCSD and finally now at UC Riverside, where Shane is now a member of the faculty. He provided most of the topics that ultimately were chosen for support over a variety of subjects. Although the SBIR contracts go only to small businesses, a company can use some fraction of a contract to purchase supplies and consultants. Such SBIR contracts are vital to a few relatively small companies selling products based upon superconducting electronics. Furthermore, as a result of Shane's postdoc experience at Berkeley, he now can pattern a chip with 3 angstrom resolution, and without requiring a mask.

Prior to the HTS era, superconducting electronics, outside of magnetometry, was not going anywhere. It is my firm belief that the future is much brighter. Partly, this is because Moore's Law is "expiring." In plain English this means that it is no longer possible to shrink the dimensions of semiconductor structures, while the increased resistance of these smaller structures generate relatively high amounts of waste heat.

Conversely, superconductivity has no waste heat as long as the resistance is zero, although there is some energy dissipated in the process of providing cooling.

Over the years that I was employed at AFOSR, I had the good fortune to engage in a modest amount of research, both in the US and abroad. It usually involved applications of SQUID-based magnetometry. I was able to conduct most of my AFOSR duties via the Internet from almost anywhere for periods of from 5 weeks to 6 months, plus I was able to work in Fred Wellstood's lab at the University of Maryland to pursue the use of an HTS SQUID in proximity to thin wires to attempt to discover defects in those wires prior to final reduction in diameter. This was the continuation of studies begun at the University of Houston in November 1997, where I was a Welch Visiting Professor for 6 months. An international company that, among other things, supplied wires for bonding in the electronics industry, wanted to improve the integrity of those wires and to reduce defects in wiring. I had just arrived in Houston when I received a call from a representative of the company that supplies these fine wires. The company was trying to find a better technique to identify voids and defects at an early stage of the process in which a rod goes through a succession of dyes before becoming remarkably thin wire.

While at Houston I received a message from a Russian engineer who had come to the US specifically to try to sell a SQUID-based magnetometry system to detect heart disease by monitoring the magnetic signal emanating from a human heart. Typically, this involved a 3 x 3 array of SQUID-based magnetometers operating at liquid helium temperature. As I had been planning to determine other applications for SQUID magnetometry, I started a dialog with this engineer, Sasha Bakharev, and with Michael Gurvitch, a physics professor at Stony Brook University, who has served as a facilitator for would-be Russian engineers and technology companies. I invited them to visit me and Paul Chu, the founding Director of the Texas Center for Superconductivity. With the 2 visitors at Houston, I arranged a visit to the vast medical complex in Houston. In particular, 2 renowned cardiac surgeons had established 2 of the premier centers for cardiology. In speaking with one cardiologist who was anxious to consider a new approach to noninvasive detection of serious cardiac problems, he stated that "there isn't a single noninvasive technology for detecting heart disease worth a damn.". The "gold standard" for detection of clogged arteries is known as an angiogram. Crudely speaking, this is a procedure that begins by inserting a catheter (tube), usually in the groin, and extending it through an artery until the heart is reached. A contrast dye is introduced, and then an X-ray "movie" of the heart's operation is made. This is invasive surgery, with a small number of cases leading to infections and very occasionally to death. Having X-rays impinge on a patient's body for several minutes, for perhaps 10 to 20 minutes involves other risks. Despite these risks, angiography provides a "roadmap" for a cardiologist to know how best to treat someone with chest pains and/or a non-fatal heart attack, and in so doing, save and/or improve the patient's quality of life.

The 2 visitors to Houston further mentioned a study in which patients who came to a hospital emergency room with symptoms of a heart attack, were given typical noninvasive tests, e.g., blood analysis and ECG, plus the SQUID-based magnetic field analysis. A subset of those patients whose condition was considered rather serious, were considered to yield the correct diagnosis of heart disease. The correlation between the results of the angiogram and those from the magnetic field measurements, was above the 90% level, whereas the correlation from bloodwork and ECG were as much as an order of magnitude lower. Furthermore, a heart with no apparent disease, produces for part of the heart's cycle, a somewhat stationary magnetic dipole pattern for the magnetic field in a plane above the heart. Conversely, when there is altered blood flow through one or more arteries, one sees a distorted dipole pattern which changes with time over the same part of the heart cycle. Additionally, software had been developed to

process the data taken automatically by the 3-by-3 array of SQUID sensors for 90 seconds. This array covered only about ¼ of the area above the heart. A larger number of SQUID sensors could alleviate this problem, but to limit the number of SQUID sensors required, the platform upon which the patient lies, can be locked into 4 different positions. Over a period of about 10 minutes, the magnetic field is measured at 36 positions. The resulting data are tabulated and presented as a dynamic plot of the magnetic field in a horizontal plane above the heart. Another major advantage of this type of cardiac evaluation, is that it isn't necessary for the patient to disrobe. Only metallic objects must be removed. This is a completely noninvasive process. The ambient magnetic field that is measured by the SQUID array is produced by electric currents in the heart. The SQUID-based system puts out no electrical signal of any kind. It merely measures the ambient magnetic field above the heart. That cyclic time-dependent electromagnetic signal is produced by internal electric current in and about the heart.

I became rather enthusiastic about the potential of this new application of superconducting magnetic field sensing, although more clinical studies would be required before it could become as ubiquitous as MRI has become. I encouraged Paul Chu to consider buying one of these units and to then collaborate with some of the local cardiology specialists. A purchase of one system was made by the University of Houston, and for a brief period in the spring of 1998, I considered the possibility of joining a new company to market SQUID arrays to evaluate cardiac health. Paul Chu did, in fact, purchase a system and convinced a local oil tycoon to make an offer to provide start-up funds to relocate the company to Houston.

Upon returning from my 6-month hiatus in Houston, I did contact people at NIH about this new diagnostic tool. I then recalled a presentation made by Carl Rosner. Carl had headed a successful company, Intermagnetics General Corporation (IGC) that primarily manufactured superconducting magnets for MRI systems. At that time he had just stepped down as CEO of IGC and had stated he was spending some of his time helping secure funding for small companies involved in the manufacture of new technology. Recalling Carl's new role, I contacted him and asked if he would consider helping Sasha Bakharev and colleagues to become a viable business. Several days later I was being driven by Carl from IGC's plant in Schenectady NY to Springfield MA, where Bakharev had established his nascent company. At the end of that day, I had to take a flight back to Washington directly from the airport nearest Springfield. However, Carl was so impressed by the potential for detection of heart disease via SQUID magnetometry, that he decided to stay overnight in Springfield and to do what was necessary to form a viable company. Thus, Cardio Mag Imaging (CMI) was born in late 1999. Carl moved this new entity to Schenectady and hired about 6 scientists and engineers to work on improving and marketing its one rather unique product. I served as an unofficial advisor to Carl and CMI for the following years. Two of the units were sold ultimately to medical centers in China. I don't know how many additional systems were sold in the US and elsewhere, but I know of only one other unit that was sold in the US, although there may have been 1 or 2 others. In any case, this wasn't sufficient to maintain a viable company. Stock was sold and Carl Rosner invested millions of dollars of his own funds to keep CMI afloat. Unfortunately, despite his best efforts and a little part-time assistance from me, CMI exists in name only, while Carl is currently attempting to sell the remaining assets.

In analyzing why this unique diagnostic tool failed to become part of the arsenal used by cardiologists and emergency room personnel, it seems that CMI failed to convince such individuals that superconducting magnetometry would provide a quick, reliable and perfectly safe means to assess the health of the human heart. There also is the issue of transferring liquid helium to the system 2 to 3 times per week. If there had been time and resources to build a closed cryogenic system that didn't require periodic addition of

liquid helium, it would have made this system more user friendly, although because of noise created by a compressor, it would be necessary to shut off the compressor while magnetic measurements are being made. Another possible improvement could be made if the SQUID sensors were made using high temperature superconductors (HTS). At this time that can't be done, although because of recent advances in HTS nanofabrication (supported by AFOSR), in the near future this could change. Nevertheless, unless one gets the attention of a group of respected medical professionals, it is unlikely that superconducting magnetometry in any form will achieve the kind of universal acceptance one sees for MRI. While I don't anticipate seeing this acceptance in my lifetime, I do believe that eventually this particular application of superconducting magnetometry will come to pass. Meanwhile, there is an existing application of it that has received some current interest. In the last month of pregnancy, it becomes quite difficult to detect a fetal heartbeat with a stethoscope. Fortunately, the magnetic signal of a healthy heart is unaffected by the thickened sac enclosing the fetus.

In addition to my major duties at AFOSR, I have had a penchant for organizing workshops and summer schools to illustrate my involvement with superconductivity. Also, I have engaged in research involving this phenomenon, although in later years that was mostly in applications of SQUID magnetometry. I wish to end this summary of my career in superconductivity over about the past 60 years by making some observations and a suggestion.

Finding more useful superconductors remains a primary goal. Usually, but not always, that goal translates to finding materials with higher superconducting transition temperature. Unfortunately, there is no known method to achieve that goal. I'm reminded of an invited talk given by Bob Dynes of UCSD at an APS March Meeting a few years ago in which he made the statement that every discovery of a new class of superconductor has come as a surprise. The phenomenon of superconductivity itself, had not been predicted prior to its discovery in 1911, nor had there been any successful prediction of new classes of superconductors since that time, with a couple of minor exceptions. Marvin Cohen in the 1960s predicted a class of superconducting semiconductors. All of them were found to have a T_c of less than 1K. A few years later, Neil Ashcroft of Cornell successfully predicted that Li under high pressure would have a T_c of less than 1 mK.

Periodically I would receive phone calls or messages from theorists and others who would claim to have found the magic formula for creating superconductivity at room temperature and above. One respected theorist asked me to choose some arbitrary temperature above room temperature, and he would tell me what ingredients to use. While skeptical of all such claims, I sometimes passed on such ideas for evaluation by acknowledged experts. So far, there has not been any such idea worth pursuing. This doesn't mean that all theorists should abandon hope of making a breakthrough. Early in the HTS era I asked one of the leaders in the field what has changed, such that theorists have so much hope of finding the key to fabricating higher temperature superconductors. He responded that the theorists have much greater computing power at their disposal. Despite this enhanced computational power, there has been no breakthrough. For example, in 2007 at the University of Illinois, a symposium was held celebrating 50 years of the BCS theory. At one point, a panel of 10 leading theorists was assembled, and the moderator got them all to agree that none of them had found a theory to explain cuprate superconductivity.

Final Thoughts

I have presented in the above narrative, a history of my involvement in superconductivity research including both my own lab experience in academia and at various research centers in the US, as well as

the funding of basic research as a program officer at AFOSR. I mentioned briefly my activities in directing a number of 2-week summer schools on some aspect of superconductivity. In each of these schools, there were between 80 to 100 attendees, primarily graduate students and postdocs from NATO countries, including senior scientists from a variety of countries. These 6 NATO Advanced Study Institutes (ASIs) ran from 1988 to 1999 and in 2005 there was a similar type of 2-week program supported by AFOSR and European funds secured by Horst Rogalla of Twente University in The Netherlands. Attendees of the NATO schools generally received copies of the books published containing all of the presentations. These summer schools exclusively on aspects of superconductivity were quite valuable in turning young scientists on to the unique properties of superconductors and to associated applications. I'm not certain if the opportunity is still available to obtain NATO funding for the type of ASIs that I organized, usually with one other scientist, in which there were few restrictions. Since the last ASI I organized in 1999, NATO criteria for ASIs has changed. However, I maintain that activities of this type involving superconductivity, are beneficial to the growth of the field. I recommend some continued activities of this type. Finally, if there is no requirement to produce a book based upon a summer school, it should be sufficient to send each participant a CD containing all of the slides presented at the school.

The phenomenon of superconductivity is clearly of great interest to physical scientists, but more importantly, applications of superconductivity may have a profound effect on the goal to generate and distribute electric power more efficiently. In the area of electronics, there are numerous opportunities for superconductivity. Although quantum computing is still in a development phase, qubits based on Josephson junction circuitry at very low temperature looks most promising. With Moore's Law coming to an end in the semiconductor industry and with so much heat generated in semiconductors, superconducting components comprised of YBCO, would go a long way toward generating denser circuitry with minimal power dissipation. The suggestion that I make here is to convene a panel of leading practitioners of superconducting applications. The goal of this group would be to produce a document that for various functions compares performance, cost and other complexity for superconductivity and competing technologies. Such a document would be periodically updated.

The cost of producing this suggested study and report could be done via a contract or grant from some funding agency in the US or in some other country. However, this might be a project done under the auspices of the IEEE Council on Superconductivity. It would be relatively inexpensive, especially if done by the Council. A good starting point would be to use the book, "100 Years of Superconductivity," and then to update it to include any progress in the intervening years.

Finally, I wish to comment on the future of superconductivity. Given the onset of global warming and the finite supply of fossil fuels, as well as the problems associated with the pollution caused by burning those fuels, life as we now know it, will be a distant dream to the 10 billion people who are projected to be alive at the end of the 21st century. Surely, it will be a world that will depend on new technology that operates more efficiently than current technology. Obviously, there will be greater reliance on renewable energy such as solar and wind, but whatever form of energy is involved, its distribution, storage and utilization would benefit significantly from superconductivity-based technology.

I now end this history of my life in superconductivity with a brief review and a forecast. When I began my graduate studies in the mid-1950s, superconductivity was considered a most interesting phenomenon with little practical use because of the low transition temperatures and the low critical magnetic fields involved. There was no accepted theory to properly explain this unexpected electromagnetic behavior,

nor to successfully predict where one should try to discover more useful superconductors with much higher critical currents, magnetic fields and Tcs. Just a few years later, useful theories originating in Russia, the UK and in the US, plus the discovery of compounds that had higher critical temperatures, led to practical high field magnets, that in turn, gave rise to MRI and more energetic particle accelerators. In 1973 a record high Tc of 23.2 K was reached (in a film of Nb₃Ge), but when I first arrived at AFOSR in 1984, that was still the record high Tc.

Then in April 1986, J. Georg Bednorz and K. Alex Mueller working at an IBM lab near Zurich, found for a ceramic material, namely BaLaCuO, with a Tc in the range of 30 K to 35 K, a different class of superconductors that could be successfully mined for even higher Tc values. Less than a year later, Paul Chu at the University of Houston and M. K. Wu at the University of Alabama at Huntsville and their colleagues discovered a phase of YBaCuO (YBCO) that had a Tc of over 90 K. That meant one could exhibit superconductivity using only liquid nitrogen for the first time. Labs almost everywhere worked on making substitutions for the yttrium and the barium, and Tc did go higher in some cases, with the record high Tc of 134 K and a Tc of 164 K at high pressure. The problem was that Y was substituted by Hg, which is toxic, among other problems. Other discoveries were made: MgB₂ was discovered to superconduct at about 39 K in 2001; for several years now, ferromagnetic superconductivity was found; and for the last couple of years there are reports of Tc of 269 K for H₃S under extraordinary high pressure. This last feat does not in itself allow anything practical to be done, but it opens a door to help lead the way to room temperature superconductivity. The future of civilization as we know it, would look much more promising if a practical room temperature superconductor can be produced. It is a goal that should receive the attention of physical scientists around the world.