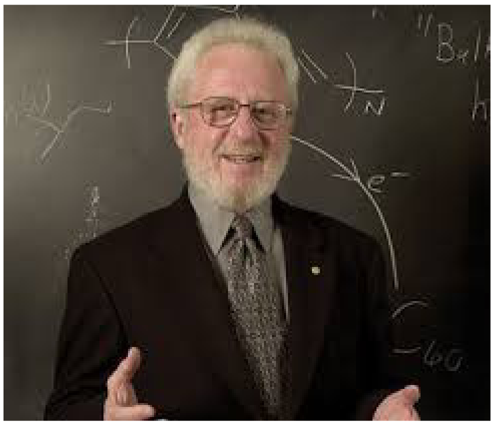
# Alan J Heeger



I was born on a bitter cold morning (20º F below zero) in Sioux City (Iowa) on January 22, 1936. I was told that when my father went out in the cold that morning to go to the hospital to visit his wife and newborn first son, his car would not start. Despite advice to the contrary, he walked to the hospital; his ears were frostbitten on the way.

The Heeger family came to Sioux City (Iowa) from Russia as Jewish immigrants in 1904 when my father was a small boy (age 4). My mother was born in Omaha (Nebraska); she was a first generation child of Jewish immigrants. My mother and father were married in the midst of the Great Depression.

My early years were spent in Akron (Iowa), a small midwestern town of 1000 people, approximately 35 miles from Sioux City. I went to elementary school in Akron. My brother, Gerald, was born in Akron. My father was the manager and, subsequently the owner, of a general store that served the local farming community. I have a strong memory of the day I was told that my father had a weak heart and that he had to go to the hospital. He died when I was nine years old on the same day that Franklin Roosevelt died; it was his 45th birthday.

After my father’s death, we moved to Omaha, so my mother could be closer to her family. She raised us as a single parent in a house that we shared with her sister and her sister’s children.

One of my earliest memories (long before my father died), is of my mother telling me of the importance of getting a university education. When she graduated from high school, she received a scholarship to go on to university but went to work instead; she was needed by her parents to help support the family. It was always clear to me that it was my responsibility to go to university; prior to my generation no one on either side of my family had an education that went beyond high school. I and my brother were the first in our family to receive the PhD degree.

My high school years were fun and frustrating, typical of the teen years. The most important accomplishment was meeting my wife, Ruth. I have loved her for nearly fifty years, and she remains my best friend.

I was always a good student, but I do not remember science being especially easy. On the contrary, I recall that in high school, physics was somewhat mysterious. I was impatient to get on with my education, to get on with more important things, and therefore completed high school one year early.

My undergraduate years at the University of Nebraska were a special time in my life; the combination of partying and intellectual awakening that is what the undergraduate years are supposed to be. I went to the University with the goal of becoming an engineer; I had no concept that one could pursue science as a career. After one semester, I was convinced that engineering was not for me, and I completed my undergraduate studies with a dual major in Physics and Mathematics. The highlight was a course (in my senior year) in Modern Physics taught by Theodore Jorgensen. Professor Jorgensen introduced me to quantum physics and twentieth century science. I was honored by the University of Nebraska in 1998 with a Doctor of Science (h.c.) and had the pleasure of giving a Physics colloquium at that time. Ted Jorgensen came to the lecture; he was 92 and working hard on revising his book on the Physics of Golf.

Again, I was impatient to get on with "real physics". I started the path toward my PhD in Physics at UC Berkeley while working part time for Lockheed Space and Missile Division in Palo Alto, CA. On Monday, Wednesday and Friday, I would wake up early and drive the Bayshore Freeway to Berkeley to attend classes. After sitting in class all morning, I had lunch and then got back on the freeway to return to work in Palo Alto. Naturally, after such a morning I fell asleep at the wheel almost every trip. Thus, it was not a terribly difficult decision; Ruth and I moved into student housing at Berkeley, and I started research on a full time basis.

When I started at Berkeley, my goal was to do a theoretical thesis under Charles Kittel. Thus, when the decision was made to go for my degree on a full-time basis, I went first to Kittel and asked if I could work for him. Kittel had just returned from a trip to Moscow where he met Landau, and he told me that Landau required that a prospective student had to pass a rigorous examination before he would agree to take the student into his research group. Kittel indicated that I should take the PhD qualifier and come back to him after I had done so. When I came back to discuss my future with him, Kittel told me that he would take me on. He said, however, that although I could do a thesis under his direction in solid state theory, he did not think I would be a first-rate theorist. He recommended instead that I consider working with someone who does experimental work in close interaction with theory. This was perhaps the best advice that anyone ever gave me – and I followed his advice. I joined the research group of Alan Portis.

I remember with clarity my first day in the laboratory. I was doing "original research"; at last I was involved with real physics. After only one day of carrying out magnetic measurements on an insulating antiferromagnet, $KMnF_3$, I wrote a theory of antiferroelectric antiferromagnets and presented it to Portis with great pride. He was patient with me then and again a few days later when I apologized and told him my theory was nonsense. Through my interactions with Portis (I recall spending many hours talking with him in his office), I learned how to think about physics; more important, I began to learn about good taste in the choice of problems.

After completing my degree, I went directly to join the Physics Department at the University of Pennsylvania where I remained for over twenty years. It was an exciting period for condensed matter physics at PENN. Eli Burstein had made major progress in building the solid state group; he convinced Robert Schrieffer to come to Penn, and he and Schrieffer attracted an outstanding group of young people. Beginning with my experimental studies of magnetic impurities in metals and the Kondo Effect, I learned many-body physics from Schrieffer.

Anthony Garito introduced me to tetracyanoquinodimethane (TCNQ); I brought him into my research group for post-doctoral research. We worked together from 1970 through 1975 on the metal-physics of TTF-TCNQ and on the discovery of the Peierls instability in quasi-one-dimensional p-stacked molecular crystals. Although the direct observation of the incommensurate Peierls distortion with wave number $q = 2k_F$ proved that we were on the right track, this was a time of controversy and stress.

In 1975, the first papers on the novel metallic polymer, poly(sulfur-nitride), $(SN)_x$ appeared in the literature. I was intrigued by this unusual quasi-1d metal and wanted to get into the game. I learned that Alan MacDiarmid, a professor in the Chemistry Department at PENN, had a background in sulfurnitride chemistry, and I made an appointment to see him with the goal of convincing him to collaborate with me and to synthesize $(SN)_x$. I recall that we met late in the afternoon of an autumn day. After quite a long discussion during which I made little progress toward my goal, I realized that while I was saying " $(SN)_x$", he was hearing " $(Sn)_x$". Needless to say, he was not impressed with my enthusiasm for $(Sn)_x$ being a metal; any chemist knew that tin was a metal!

Once MacDiarmid and I got past this initial language problem, a true collaboration began. We realized that it was a long reach across the Chemistry-Physics boundary, and we were determined to learn from one another. Although we collaborated during the week, we typically met on Saturday mornings with no agenda; just to try to learn from one another. At that time, I was fascinated with the metal-insulator transition as envisioned by Mott. I recall that I tried to convey my interest in this problem to MacDiarmid by asking him to consider a linear chain of hydrogen atoms as a model system. He balked right away; a linear chain of hydrogen atoms did not exist. After discussion, we focused in on the abstraction of a chain of p-bonded -CH- units as an example of a system that would have one unpaired electron per repeat unit. Shortly thereafter, MacDiarmid went to Japan for a visit. MacDiarmid is a very visual person. He loved the golden color of films and crystals of $(SN)_x$, and he showed samples and photos of this golden material during his lectures. After one such lecture, a Japanese scientist came up to him during the coffee break and told MacDiarmid that he, too, had some shiny films. Thus, MacDiarmid was introduced to Hideki Shirakawa and to polyacteylene.

When MacDiarmid returned from Japan, he told me with great excitement about $(CH)_x$. With the help of a small addition to an ONR grant from the Program Officer, Kenneth Wynne, we were able to bring Hideki Shirakawa to PENN as a Visiting Scientist. The initial discovery of the remarkable increase in electrical conductivity of $(CH)_x$ and the identification of that increase as resulting from a transition from insulator (semiconductor) to metal followed in a very short time.

The soliton in polyacetylene was born with the observation of an electron spin resonance (esr) signal in the pure material where there should not have been one. Building on the earlier work by Michael Rice on phase-solitons, I realized that if one drew a domain wall between the two identical forms with opposite bond alternation, one would have an unpaired spin and postulated that the origin of the esr signal might be a bond-alternation domain wall. Curt Fincher, then a graduate student in my research group, had recently discovered the doping-enhanced infrared vibrational modes which became a signature of the doping. In a luncheon seminar before the solid state group at PENN, I argued that these doping-induced IR modes might arise from the enhanced electric field at IR frequencies that would result if a charged bond-alternation domain wall were to move back and forth driven by the external field of the incident IR radiation. Schrieffer listened closely and made some comments about "kinks" at the end of my talk. A few days later he showed me how the mid-gap state would arise from the formation of such a bond-alternation domain wall and how that mid-gap state would have a reversed spin/charge relation relative to that of fermions. Wu-Pei Su then worked this out in detail, and the SSH papers were written.

I was drawn to Santa Barbara by the promise of a singular opportunity to build a special Physics Department, by the promise of continuing my close collaboration with Bob Schrieffer, by the opportunity to work with Fred Wudl, and – frankly – by the lure of this beautiful place. Wudl, then a synthetic chemist at Bell Laboratories, and I were recruited to UC Santa Barbara together and enjoyed a close and productive collaboration over a period of 15 years.

Daniel Moses and I have worked together for twenty years, initially at PENN and then at UCSB. Dan dragged me into ultra-fast pulsed laser spectroscopy and into fast-transient photoconductivity as probes of the excited states of semiconducting polymers. Dan continues in his efforts to resolve the remaining fundamental scientific issues in the field of semiconducting polymers with creativity and with determination.

In 1986, in the process of building the Macromolecular division of our newly formed Materials Department, we convinced Paul Smith to leave DuPont Central Research and come to UCSB. Whereas I and Alan MacDiarmid and most of the early players in the conducting polymer field were amateurs in the field of polymer science, Paul was a professional. He quickly hammered into my head the importance of making conducting polymers processible, and he had the annoying habit of asking me embarrassing questions such as "What is the intrinsic electrical conductivity of a conducting polymer?". Anything I know about the processing and mechanical properties of polymers, I learned from Paul.

In 1990, Paul Smith and I decided that conducting polymers as materials had developed to a level of maturity that commercial products were possible. With this as a goal, we founded UNLAX Corporation. Fortunately, on a trip to China in 1986, I met Yong Cao and immediately realized that he was a remarkable scientist. I was able to bring him to Santa Barbara in 1987. Initially, he worked with Paul and with me at UCSB. When we founded UNIAX, Yong Cao was the first employee. His creativity, determination and scientific strength were critical to our scientific progress and to the success of UNIAX. During the 1990's, UNIAX played a leading role in developing the science and technology of conducting polymers with many important contributions.

The twenty-five years since the discovery of conducting polymers have taken me on a great ride; always on the frontier and always with the challenge of exciting discoveries. In 1990, the discovery of polymer LEDs by Richard Friend and colleagues at Cambridge gave the field a boost with the promise of important technology and with the excitement of an entirely new set of phenomena to study. In 1992, while doing post-doctoral research in my group at UCSB, Serdar Saricifici discovered ultrafast photo-induced electron transfer from semiconducting polymers to acceptors such as $C_{60}$. This discovery resulted in the development of polymer photodetectors and photovoltaic cells that offer promise for use in a variety of applications. In 1996, the discovery of amplified spontaneous emission and lasing (simultaneously by our group, by Richard Friend's group at Cambridge and by Valy Vardeny's group at Utah) opened yet another potentially important direction. And it goes on.

None of this could have been accomplished without the hard work, dedication and creativity of the students and post-docs with whom I have had the pleasure of working over the past forty years. I thank them all.

I have enjoyed the life of a scientist while sharing both the exciting days and the disappointments with Ruth. She has filled my life with love and surrounded me with beauty. She has also gallantly put up with my eccentricities for more than forty years. We have succeeded in starting an academic dynasty; our two sons, Peter and David are both academics. Peter is a professor and medical doctor who is doing research on immunology at Case Western Reserve University. David is a professor and neuroscientist at Stanford University where he studies human vision. I have had the great pleasure of collaborating and publishing articles (as co-author) with both of my sons. Now I am looking forward to the emergence of my four grandchildren, Brett, Jordan, Julia and Alice, as the next generation of the Heeger family. Of all the congratulations that I have received as a result of the Nobel Prize, I took greatest pleasure from their pride in their grandfather.