For Public Release

Professor Henry Levinstein of Syracuse University and his impact on infrared detectors in the U.S.

Paul LoVecchio, Paul Norton, and Robert L. Strauss

In many ways, Professor Henry Levinstein was the patron saint of infrared detectors in the United States. His work on detectors was funded without interruption by the Air Force from 1947 to 1977. Even after that Henry did not retire from Syracuse University until 1986, and was kept on as a consultant by the Air Force who valued his advice on all aspects of detector technology. In his academic and research role, Henry educated a very large number of students who went on to become key members of the infrared detector community. That legacy is still important today. Research in his lab led to the production of PbTe detector cells used in many important experiments and demonstrations of the usefulness of infrared technology. The discovery by Henry and his student Seb Borrello in 1961 of mercury-doped germanium led to the first production of long-wavelength infrared imaging arrays in U.S. aircraft for the Air Force and Navy.

INTRODUCTION

Dr. Henry Levinstein, or "Doc" as students would call him, made a great impact on the infrared detector industry in the U.S. His talents and contributions as an experimental physicist, his love of teaching and the large number of graduate students he advised for over three decades have placed him at the forefront of notable scientists in the field of infrared detectors. What follows in this document is:

- Personal reflection on Henry's life from his stepson Robert Strauss
- Discussion of Henry's impact on infrared detectors both as an individual contributor and as an advisor to thirty seven graduate students doing research on infrared detectors
- Personal remembrances from a number of Henry's graduate and undergraduate students on what it was like to work in his laboratory
- Awards Henry received and presentations/publications by Henry and his graduate students

Those of us fortunate enough to have known Henry know that our lives are the better for it. Hopefully this document will provide some understanding to those who did not know him of why he was so special to those who did.

Henry at ages four and nine in Germany

DR. HENRY LEVINSTEIN— A PERSONAL HISTORY BY HIS STEPSON ROBERT STRAUSS

Professor Henry Levinstein was born Heinrich Levinstein on December 4, 1919 in the small Thuringian village of Themar where his father Moritz was the local Jewish *lehrer*, or religious instructor. His mother Nanette was a classic German housewife, excelling at cooking, baking, gardening, knitting and sewing. Henry was an only child.

As an adolescent, Heinz, as he was then known, was sent by his parents to *gymnasium* in neighboring Schmalkalden where he boarded in a room above the proprietor's dairy cows. In 1935, Heinz left Germany for New York City where he lived with his uncles. Three years after arriving in the United States, he graduated from Jamaica High School, number three in a class of 4,000 students.

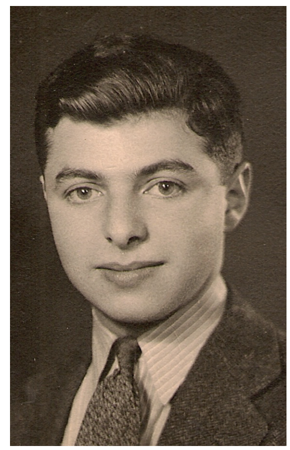
Moritz and Nanette visited Henry in 1938 when he was a freshman at the University of Michigan. They then made the fateful decision to return to Germany to care for Henry's maternal grandmother. On *Kristallnacht* (November 9, 1938) the Levinstein's home, which doubled as the Jewish community center in Themar, was ransacked and Moritz was sent to the concentration camp at Buchenwald. Released a month later, Moritz's body was found just outside Themar. His gravestone marks the last Jew to be buried in an area where human settlements date back more than 1,200 years.

After Pearl Harbor, Henry attempted to enlist in the U.S. military but was rejected on several counts including his still being a German national. A physical exam also revealed the extremely high blood pressure that would trouble him throughout his adult life.

Henry received his B.S., M.S., and Ph.D. all in physics from the University of Michigan. Rarely stepping outside the sciences, Henry took an English class at Michigan with W. H. Auden. Auden commented that Henry had no literary talent whatsoever but nevertheless gave him a superior grade for effort.

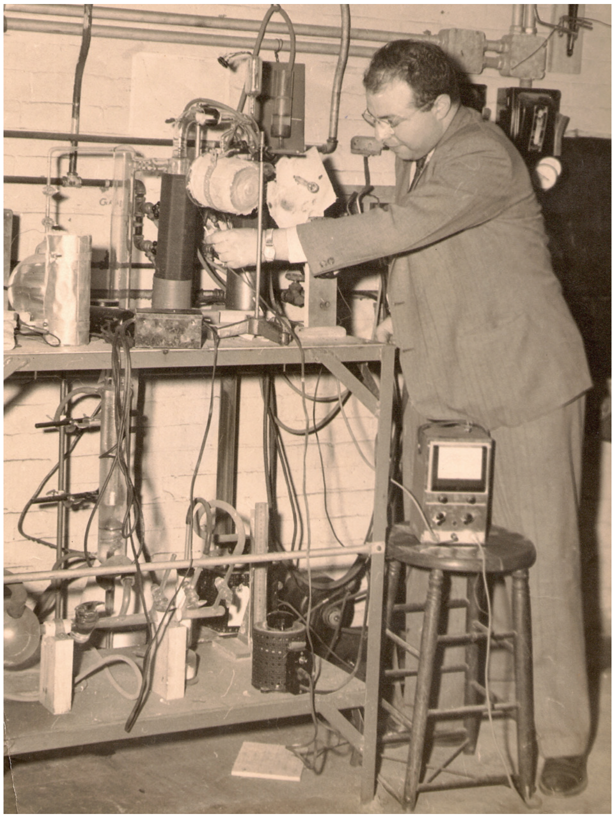
Henry spent summers in the late 1930s and early 1940s working as a busboy at Camp Equinunk in northeastern Pennsylvania and as a waiter at Borscht Belt resorts where compensation was limited to tips paid only at the end of the season. Following the death of her mother in Berlin, Henry's mother Nanette joined him in Ann Arbor in 1941.

In 1947 Henry and Nanette moved to Syracuse where he began his 39-year tenure on the physics department faculty as an assistant professor at an annual salary of



Henry in 1934 and 1937

Henry at the University of Michigan



Henry in his Syracuse University physics lab in 1950.

$3,500. Henry began his research on IR shortly after. By 1957, PbTe cells produced by Henry's lab were in use by more than 30 different academic, industrial and governmental institutions around the United States.

In 1962, Henry married Betty Strauss, the widow of a close friend with whom Henry regularly attended S.U. sporting events in the 1950s. Henry became an instant father, adopting Betty's three sons, then 13, 11 and 6, as his own. He famously proposed by asking Betty if she would "like to take care of four boys instead of three."

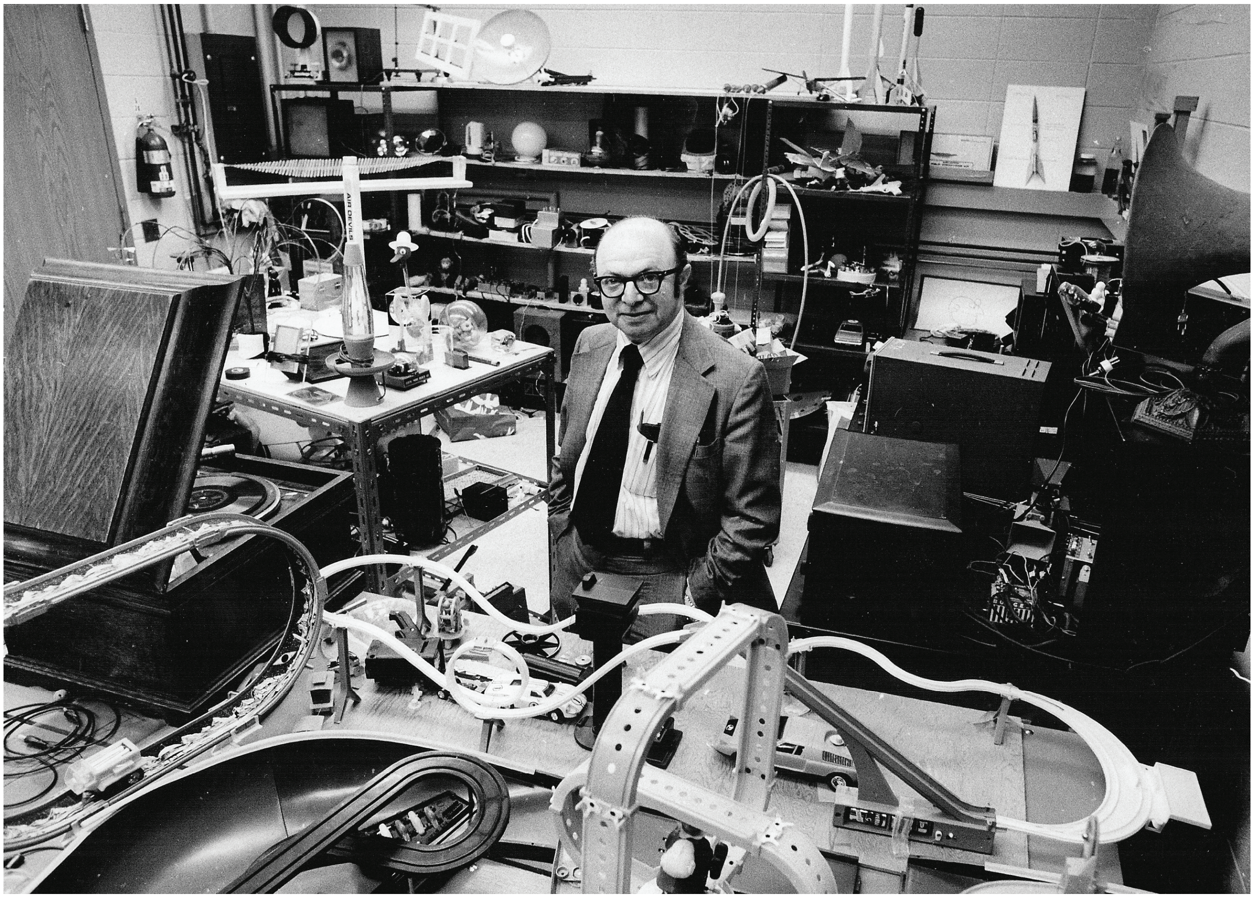
Henry and Betty from 1962.

Henry's scientific focus began to diversify in 1963 when he, in the guise of his youngest son Robert, won second place in the 1963 Charles Andrews Elementary School Science Fair with an exhibit on the early history of recorded sound. Henry (and Robert) returned to take the blue ribbon in the 1965 science fair with a demonstration of electric light that included a bulb powered by a grapefruit. Soon after, Henry began regular lectures on the physics of toys to high school students in the Syracuse area. These informal talks later developed into the very popular "Physics of Toys" class that he taught annually for many years at S.U. Henry also took his "Physics of Toys" lecture all over the United States and Canada presenting to audiences as varied as the American Association of Physics Teachers, commercial businesses selling infrared detectors such as Texas Instruments and Santa Barbara Research Corporation and York University in Canada.

In early 1984, Henry experienced a massive heart attack that reduced his coronary function by nearly 90%. Previously adverse to physical activity, Henry became a dedicated member of a walking for fitness program at the Syracuse Y. He was happy to show anyone his pacemaker, which fit in well with his love of technological gadgets such as miniature Minox cameras.

In the summer of 1986, while hospitalized for a change of medication, Henry's heart gave out. His memorial was held at Syracuse's Hendricks Chapel and attended by hundreds of his students, faculty

Henry with his family in 1984—top row left-to-right; Robert, Harvey, Pam Strauss (Richard's wife), Betty, Doc, —bottom row left-to-right; daughter-in-law Suanne Smith Strauss (Harvey's wife) and her daughter Lisa, granddaughter Jennifer, grandson Peter, Richard (Jen and Peter are Richard and Pam's). This photograph shows Henry after his heart attack which, together with the walking program at the Y, caused him to lose 30-40 pounds.



Henry in his Syracuse University toy lab. He taught "The Physics of Toys" to non-science majors at S.U., at many high schools, and around the U.S.

colleagues and friends. The award given for IR research in the United States carries his name and a fellowship at Syracuse University was funded in his honor.

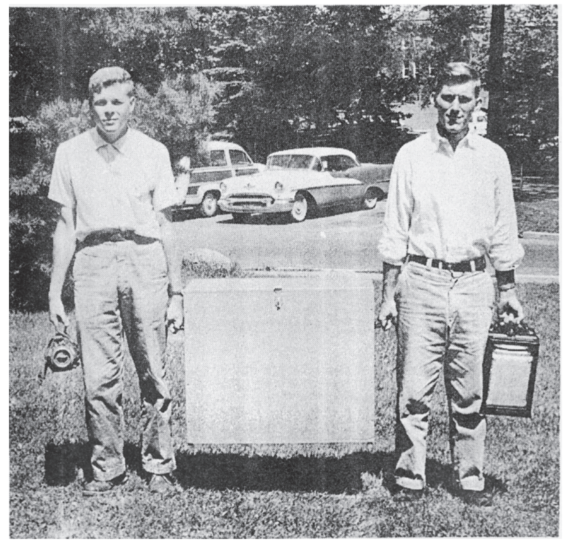
Professor Henry Levinstein enjoyed nothing more than going to work every day. He was a quietly dedicated scientist, who took pride in the accomplishments of his graduate students and yet also enjoyed the minor celebrity that his toy lectures brought him. He was a doting son who lunched with his mother every day until both his and her declining health made this impossible. He was a gracious spouse, and a gentle and loving stepfather and grandfather. He was the rare individual who never had a cross word for anyone and whose friendship and counsel were treasured by a very great many.

EARLY RESEARCH AT SYRACUSE UNIVERSITY

The basis of the infrared detector research at Syracuse University (SU) really began at the University of Michigan where Henry Levinstein received his Ph.D. in 1947. His thesis was titled "A Study of The Growth and Structure of Thin Metallic Films." For this thesis Henry investigated the growth and struc-

Henry at a Syracuse University Physics Department picnic in 1952.

Henry with early students in 1950: left-to-right — Ray Olson, Roy Paulson, Don Bode, Henry, and Channing Dichter.



A “portable” infrared radiometer used to measure infrared scene profiles at Syracuse University in the late 1950s. John Stannard, left, was an undergraduate at the time of this picture. He later received his Ph.D under Professor Henry Levinstein.

ture of thin metallic films as determined by the melting points of the metals being evaporated, the rate of evaporation, the nature of the substrate and the beam composition. Henry joined Syracuse University as an assistant professor in 1947 under the Physics Department head William F. Fredrickson. Henry soon found contractual support from Wright-Patterson Air Force Base for conducting and supervising research on infrared detectors. Dr. Neil F. Beardsley,[1] a well-known scientist in this field, was the contract monitor from 1947 to the late 1950’s followed by Thad Pickenpaugh until 1977. The total Air Force contract value over this thirty-year period amounted to $3M. The financial support and continuity of these contracts allowed Henry to establish a premier university research laboratory that would add 27 Ph.D.’s and 10 M.S.’s to the U.S. effort to improve infrared detectors for defense and civilian applications well into the twenty-first century.

Henry in his lab office under the Syracuse football stadium in 1962

In the early 1950’s, Henry’s experience and the knowledge he gained at the University of Michigan soon led to experiments at SU on thin film lead salt infrared detectors. The first Ph.D. thesis under Prof. Levinstein was awarded to Don Bode in 1953 for studying the effect of oxygen on PbTe films. Early experiments were performed in the basement of Steele Hall. A move in the early 1950’s into space beneath the football stands of Archibald Stadium allowed an expansion of laboratory research space and equipment. The final move of research laboratories came in 1968 with completion of the then new physics building where the laboratories were two floors underground. In a Syracuse winter, this meant that you never knew how much snow you would have to walk or drive through when you left your experiments, typically in the early morning hours.

Henry Levinstein’s reflections on the infrared detector work conducted at Syracuse University in his laboratory are best seen in his own words to the forward of his 1976 Final Report to the Air Force:

FORWARD

Work on infrared detectors was started at Syracuse University in February 1948, under Air Force contract W33-038 AC 15160. It was continued under successive Air Force contracts until 1975. The research has represented a continuing effort in the development and analysis of new infrared detector materials, the construction of detectors from these materials as well as measurement and analysis of the parameters of these detectors. The two most important accomplishments of the work at Syracuse University have been the development of the first liquid nitrogen cooled detector in the 3-6 µm range to be used in the U. S. aircraft (PbTe), and the first detector in the 8-14 µm range which did not require liquid helium but could be operated with mechanical coolers (Ge:Hg). In addition, a great deal of progress has been made in developing test techniques, in preparing and understanding the behavior of impurities in InSb and in Ge. Most recently, p and n type impurities in silicon have been studied. Currently both HgCdTe and PbSnTe are being prepared in our laboratory and an effort is being made to prepare better and more reproducible material and to understand the behavior of both materials.

Forward to the 1976 final report for Doc's contract with the Air Force that began in 1948 and ran continuously for 28 years.

HENRY'S IMPACT ON INFRARED DETECTOR TECHNOLOGY IN THE UNITED STATES

Dr. Henry Levinstein and the graduate students who did thesis work under his supervision added a significant amount of knowledge to the science of infrared detectors from 1947 to 1975. The early years were spent understanding the details of how then, state-of-the-art, lead-salt infrared detectors worked. The later years focused on developing newer detectors with greater sensitivities and greater stabilities operating at higher temperatures. The following paragraphs briefly describe the state of infrared detector knowledge before and after Dr. Levinstein and his student's involvement in the field of infrared detectors.

Henry with InfraRed Information Symposium (IRIS) colleagues in 1960; Don Bode and George Pruitt

Infrared Detector Knowledge Before Dr. Levinstein's Involvement

Sulfide semiconductors were one of the earliest materials studied for their sensitivity to infrared light. A seminal paper that described many of these developments was "The Development of Lead Salt Detectors" by D.J. Lovell[1]. From 1916 to 1917 Theodore Case found that bismuth sulfide, lead-antimony sulfide, and thallium sulfide were the most sensitive to IR radiation after studying 200 substances[2]. Case focused on thallium sulfide melting the compound onto a quartz disc, attaching graphite electrodes to it and placing it into an evacuated glass tube. Using this approach he was able to demonstrate a communication system that worked over 18 miles. These thallium sulfide cells degraded however after exposure to short wavelengths. The next major advance in infrared detectors occurred in the 1930's when E. W. Kutzscher at the University of Berlin found that galena (PbS) crystal rectifiers had a short wavelength infrared response around the vicinity of the "cats whisker", or metal point-contact, better than anything else known at the time[3]. He was able to increase the sensitivity by placing a number

1. D.J. Lovell, *"The Development of Lead Salt Detectors"* American Journal of Physics, 37, No. 5, 467 (1969)
2. T.W. Case, *"Notes on the change of resistance of certain substrates in light,"* Phys. Rev. 9, 305 (1917)
3. E.W. Kutzscher, *"Review on Detectors of Infrared Radiation"*, Electro-Optic System Design, 5, 30 (1973)

of these rectifiers in electrical series. Kutzscher then undertook a study of PbS as an infrared detector material by comparing chemically precipitated vs. evaporated films. He found that films chemically deposited on glass slides and heated (presumably in air) were superior. He finally studied the influence of the chemical process used, layer thickness, role of impurities, and heat-treatment on sensitivity. Kutzscher's work and that of other German scientists lead to the development of the Kiel IV airborne infrared search and track system used by Germany in World War II.

In the early 1940's R. J. Cashman developed thallus sulfide detectors while at Northwestern University under contract from the Office of Scientific Research and Development. Conversations with Theodore Case lead him to study the influence of cell structure, oxidation process, quality of glass used in the detector cells, layer thickness and baking procedures. As a result of these careful studies he was able to develop stable infrared detectors and supply a manufacturing technique to General Electric and Radio Corporation of America[4]. Cashman then turned his attention to increasing the infrared sensitivity of PbS through the use of oxygen. He found that German PbS cells captured after the end of World War II had the same sensitivity as his once he made the geometry of his detectors the same as the German devices.

Through much of this history in the development of Pb-salt detectors, the importance of supplying oxygen to the film was recognized for its role in improving sensitivity. In 1948 R.G. Newburg working at Photoswitch, Inc. (later to become Electronic Corporation of America—ECA) used a chemical oxidant instead of oxygen gas during the chemical deposition of PbS films. By this technique he was able to make high-sensitivity detectors with a high yield and without requiring high temperature baking. It was thought that using the chemical oxidant during the PbS chemical deposition resulted in the chemical oxidant being incorporated throughout the thickness of the PbS film. Eliminating the high temperature bake was found to reduce the detector noise. Newburg hypothesized that this noise reduction resulted from the elimination of large PbS crystals formed during the high temperature bake. Eastman Kodak entered the business of making PbS detector cells in 1947 also using chemical deposition. Researchers there found that the rate of deposition influenced the crystalline form of PbS and its sensitivity. The resulting cells had high sensitivity, were stable and had no need for storage in vacuum. These detectors were then used for U.S. Navy "Dove" missile seekers. Unfortunately, in tests against ship targets, the only Dove missile to hit its target was one that the lens cap had been accidently left in place[5]. The problem was ultimately traced to the guidance system and not the detector itself.

In summary, before Dr. Levinstein's research, researchers in the U.S. and Germany knew that oxygen sensitized lead-salt detectors, but had not conducted detailed experiments that gave insight into exactly how this sensitization occurred.

Infrared Detector Knowledge Gained by Dr. Levinstein and His Students

Lead-Salt Detectors

Dr. Levinstein started research on infrared detectors at Syracuse University under a Wright Field Air Force contract in 1947. He and his graduate students began conducting research on MWIR lead-salt detectors PbSe and PbTe. In comparison to SWIR PbS detectors, the significance of the MWIR detectors was that they could detect true thermal radiation from ambient-temperature objects. PbS was only useful against hot targets or with infrared search lights.

4. R. J. Cashman, Contract NObsr 45068 Report (Northwestern University, 27 February, 1947)

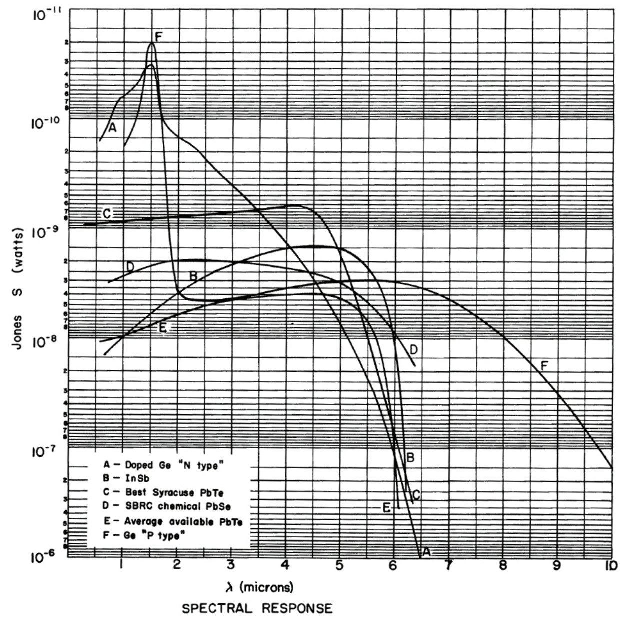
5. *Sidewinder: Creative Missile Development at China Lake*, Ron Westrum, Chapter 5, Naval Institute Press, 291 Wood Avenue, Annapolis, MD 21402, (1999). (https://books.google.com/books?id=xVEgAQAAQBAJ&pg=PT38&lpg=PT38&dq=Dove+missile+seeker&source=bl&ots=Ib1zgxOX9a&sig=b3XbwXm37aYyRjg3JGsAK8-Tgsw&hl=en&sa=X&ved=0ahUKEwjA99jvsovPAhXCPCYKHQhEDbQQ6AEIJzAG#v=onepage&q=Dove%20missile%20seeker&f=false

A prime focus in Doc's laboratory was developing an understanding the detailed mechanism of photoconductivity in MWIR Pb-salt detectors. In a 1954 Physical Review paper6 titled "*Effect of Oxygen on the Properties of PbTe films*", Donald Bode, Henry's first Ph.D student, and Levinstein described the results of carefully planned experiments in which evaporated PbTe films were exposed to oxygen in a controlled manner. Film sensitization variables investigated were film temperatures during deposition, film temperatures above room temperature during exposure to oxygen, and different levels of oxygen exposure. Detector properties measured after employing these sensitization variables were film resistance as a function of cryogenic measurement temperature, thermoelectric power, spectral response, and infrared sensitivity. Experiments were also conducted on the influence of intense white light on detector resistance while the detector was held at cryogenic temperatures. An analysis of these experimental results drove Bode and Levinstein to the conclusion that oxygen increases the sensitivity of the PbTe detectors to infrared radiation by being adsorbed on the surface of the film where electrons from the film are attracted to the oxygen atoms forming negative ions at the surface. As the oxygen concentration at the surface is increased, the density of electrons in the PbTe conduction band is decreased until the films become semiconductors. They concluded that this oxygen sensitization process is greatest for films evaporated at low temperatures where the films are more porous with more surface area available for adsorbed oxygen atoms. Further exposure to oxygen begins to create holes in the PbTe valence band thus explaining the film conversion from "n" type as grown to "p" type with higher levels of oxygen exposure. Long detector time constants observed were also attributed to deep surface traps produced by the adsorbed oxygen atoms.

As a result of these experiments scientists now had a much clearer understanding of how oxygen increased the infrared sensitivity of not only PbTe but also other lead-salt detectors.

6. Donald E. Bode and Henry Levinstein, *"Effect of oxygen on the electrical properties of lead telluride films"*, Phys. Rev. 96, 2, pp. 259-265, (1954).

In addition to the base of knowledge about detector materials and their optimization, Dr. Levinstein's lab provided detector samples to a wide variety of users in both government and scientific fields. By the mid-1050s, the best PbTe detectors coming from the Syracuse lab exceeded the performance of all other detectors in the MWIR (3-5 μm) band as shown in the following figure.



Comparative sensitivity of detectors in the mid-1950s as measured by Art Cussens at the Infrared Spectroscopy Division of the U.S. Naval Ordnance Laboratory. The vertical scale, Jones S, is NEP/[A ln(f_2/f_1)]$^{1/2}$ where NEP is noise equivalent power, A is the detector area, and f_1 and f_2 are lower and upper bandpass frequencies.

Subsequent Developments By Dr. Levinstein and his Graduate Students

Through the 1950's and 1960's work continued on lead-salt detectors by graduate students at Syracuse leading to an understanding of the role played by excess Pb in the as-grown PbTe films causing "n" type semiconductor behavior. Ultimately PbTe was replaced by studies of single-crystal detectors—InSb, Ge:Au, Ge:Hg, Ge: Cu, extrinsic Si, PbSnTe and HgCdTe. These detectors proved to be much more stable and sensitive leading to critical use in a number of Defense Department applications. Ge:Hg, first produced by Sebastian Borrello at Syracuse University in 1961[7], led to the first

7. S. R. Borrello and H. Levinstein, *"Preparation and Properties of Mercury□Doped Germanium"*, J. Appl. Phys. 33, 2947 (1962)

Henry with with Thad Pickenpaugh (upper left), Nate Sclar (left), Moshe Lanir (top), and Kevin Riley (right). Taken in 1981 at the Syracuse Physics Building

LWIR Forward-Looking IR (FLIR) system that was deployed on B52 aircraft during the late 1960s. This sensor covered the full LWIR band and was built as a 180-element scanning array by Hughes Aircraft.

HgCdTe is the detector of choice today for systems requiring the highest sensitivity and tailorable cutoff wavelength/operating temperatures.

The story of research under Dr. Levinstein at the Syracuse University Physics Department on lead-salt and single-crystal detectors above is given in the personal remembrances of his graduate students in the section that follows. Without question one of the greatest contributions Dr. Levinstein made to the U.S. infrared detector industry was providing scientists to this country's industrial and government laboratories who were trained to be independent investigators. A listing of Dr. Levinstein's graduates and the industrial or government laboratories where they went upon graduating is also listed in the following section.

Individual contributions—

The eulogy Donald Bode gave at Henry's memorial service succinctly captured many of Henry's accomplishments:

"Henry served as a consultant and advisor to many industrial and government organizations, such as: Aerojet General, General Electric, General Telephone, Honeywell, IBM, the Jet Propulsion Laboratory, the Night Vision Laboratory, RCA, the Santa Barbara Research Center, Texas Instruments and Westinghouse. Henry also served in many professional organizations in a variety of leadership roles. He worked as a key member in government/industry/university infrared coordination conferences before IRIS (the Infrared Information Symposium) was formed. He served on the Executive Committee of IRIS. He was the first chairman of the Infrared Detector Specialty Group of IRIS. He was the chairman of the Optical Society and chairman of the New York State Section of the American Physical Society.

"Henry also served as a Chairman and Editor of the Second International Photoconductivity Conference held at Cornell University in 1961. With the exception of 1984, when he was recuperating from a massive heart attack, Henry had been a guest lecturer at the Infrared Detection course at the University of California at Santa Barbara since 1971.

"In civic activities, Henry was President of the Syracuse Chapter of Phi Beta Kappa. In addition, he enjoyed interviewing prospective medical school students. He was noted for asking these students penetrating questions, such as, "What did you do last summer? Expecting that serious future physicians would say, 'I worked in a local hospital.' He also served on the Board of Directors of the Science Discovery Center of Syracuse and was active in local Science Fair activities."

Henry's impact on the field of infrared detectors was recognized by the Infrared Information Symposium (IRIS), now the Military Sensing Symposia (MSS), in 1985 when the Levinstein Award "For Lifetime Contributions to Infrared Detector Technology" was initiated.

Mentor and Thesis Advisor—

Henry was a thesis advisor to 37 students at Syracuse University from 1947 to 1978 where he trained/instructed/tutored them in the art of understanding and improving infrared detector performance. For a number of graduate students this training began during their undergraduate years. Henry would occasionally ask his undergraduate physics students if they would like to work in his laboratory to support graduate students doing their Ph.D. or M.S. research. This was a very instructive time for many undergraduate students, as it allowed the students to become familiar with crystal growth and/or semiconductor and infrared detector characterization. This training gave students a solid basis for their graduate research in Henry's laboratory. From 1947 to 1978 twenty-eight of his graduate students received Ph.D.s and ten received M.S.'s. These graduates entered positions in industry, academia, and government service after receiving their degrees. The table below lists those students who received Ph.D.s for their work in Henry's lab and is followed by a list of those who received M.S. degrees.

Ph.D. Recipients/Year Ph.D. Received/Thesis Title/First Employment

Donald Bode	1953	Effect of Oxygen on PbTe Films/IBM
Searle Silverman	1953	Comparison of Thin Films with Bulk Properties/Bell Telephone Laboratories
Robert Broudy	1953	High Frequency Properties of Thin Films/Honeywell
Marvin Lasser	1954	Optical Properties of PbTe Films/Philco/U.S. Army
Jay Zemel	1956	Adsorption of Cs on Tungsten/U.S. Naval Ordnance Laboratory
Frederick Card	1957	Diffusion of Impurities into PbTe/Westinghouse
Leo Johnson	1959	Deep Impurities in Ge/Bell Telephone Laboratories
Dean Mitchell	1959	Electrical Properties of SnSe/Naval Research Lab/National Science Foundation
Alfred MacRae	1960	1/f Noise in Ge/Bell Telephone Laboratories
William Engeler	1961	Deep Levels in InSb/General Electric
Floyd Hughes	1962	Adsorption of Cs on Single Crystals of Tungsten/Philco
Werner Beyen	1962	Electrical Properties of PbTe Films/Texas Instruments
Claude Penchina	1964	Lifetime Measurements in Semiconductors by Optical Beating/University of Massachusetts
John Pehek	1964	Recombination Radiation in InSb/RCA Laboratories
Hans Stocker	1965	Boltzmann Equation Approach to Photoconductivity/Texas Instruments/Bell Telephone Laboratories
Peter Bratt	1965	Electrical Properties of Doped Ge Detectors/Santa Barbara Research Center
Joseph Wrobel	1967	Alloys of III-V Compounds/Texas Instruments
John Stannard	1967	Impurities in Ge/Naval Research Laboratory
Sherman Golub	1969	Breakdown Effects in InSb/Lockheed
Carl Stannard	1969	Oscillatory Effect in InSb/State University of New York at Binghamton
Paul Chia	1970	Preparation and Properties of PbSnTe/Santa Barbara Research Center
Frank Renda	1970	Tunneling in Semiconductor Films/Santa Barbara Research Center
Paul Norton	1970	Lifetimes and Recombination Processes in Ge/Bell Telephone Laboratories
Paul LoVecchio	1972	Impurity pairing in Ge/U.S. Army Night Vision Laboratory
Arthur Lockwood	1973	Electrical Properties of PbSnTe/Santa Barbara Research Center

Timothy Braggins	1976	Recombination Cross Sections of Impurities in Si/Westinghouse
Moshe Lanir	1977	Diffusion of CdIn into PbSnTe/Honeywell
Kevin Riley	1978	Electrical Properties of HgCdTe/Santa Barbara Research Center

M.S. Recipients/Year MS Received/Thesis Title/First Employment

Shirley Blowers	1951	Unknown/General Electric Utica
Howard Davis	1955	An Investigation of the Characteristics of the Conical Target X-Ray Tube/Philco
Joseph Marquisse	1960	Electron Focusing as a Means of Improving the Conical Target X-Ray Tube/Unknown
Norman Ford	1960	Domain wall velocities in thin iron-nickel films/IBM, Univ. Mass. Amherst
Sebastion Borrello	1962	Preparation and Properties of Mercury Doped Germanium / Texas Instruments
James Carmichael	1962	Unknown/General Electric
Alan Tanenbaum	1964	Electrical Characteristics of Thin Germanium Films Grown in Ultra-High Vacuum/Law practice in California
Anthony Hornung	1968	Diffusion of silver in borosilicate glass/IBM
Mikkillineni Rao	1969	Passed qualifying exam/Ph.D UCSD 1972/Bell Telephone Laboratories
Wayne Rudolf	1970	Lockheed

One of Doc's unique methods of teaching his graduate students how to conduct research on infrared detectors was to give them only general guidance on a possible research topic and to have them become familiar with the thesis work being conducted by the other graduate students working for him. After this he left it for them to decide a thesis topic. Many an effort resulted in failure, requiring a new start. As painful as this approach was, it prepared his graduates for what research in the "real world" would be like and how to persevere and succeed.

One experimental discipline Doc drilled into both undergraduate and graduate students was the need to make sure the experimental data made sense based on our knowledge of physics and electronics. He reinforced this discipline in his undergraduate electronics laboratory class by telling us before class that he intentionally gave some of us faulty components to use in our experiment. Shame on us if we spent the whole lab time taking detailed data with faulty components! He expected us first to change voltages and currents to see if we were getting the qualitative results we should expect. Once we were confident that the electronic set up was behaving as expected we could then spend our time taking the detailed measurements. Any of us who took this course learned this lesson for life.

PERSONAL REFLECTIONS FROM "DOC'S" GRADUATE STUDENTS

The following reflections from some of Doc Levinstein's students offer the best insight of what it was like to work under Doc's guidance and within his laboratory at SU.

Donald Bode (Ph.D. 1953)

> (The following is an excerpt of a eulogy of "Doc" given by Donald Bode in 1986. Donald, Henry's first Ph.D. graduate student, has since died but his eulogy and memories have not.)
>
> "I first met Professor Henry Levinstein in the fall of 1947 when I took a course in electronics that he taught in the Physics Department at Syracuse University. During the holiday recess at the end of 1947, I worked for Dr. Levinstein and built an ionization gauge power supply that he needed for his laboratory. In January 1948 Henry offered me a job to go to

work for him as a research assistant on an Air Force contract that he had just taken over from departing Professor Woody Johnson. I accepted this job upon completion of my Bachelor's Degree in February of 1948.

"This was the beginning of the close friendship and admiration that I have had for Henry ever since. His passing has been a deep personal loss to me."

Howard Davis (M.S. 1955)

Howard Davis was one of Doc's students who began his education later in life, due to serving in World War II. He volunteered for and served in the Army Air Corp (the precursor of the Air Force). He was a pilot instructor in the Pacific theatre during WWII and was the meteorologist who flew the weather reconnaissance mission over Hiroshima to report that the weather patterns were suitable for bombing. It was on the way back to Guam that he heard the first atomic bomb had just been dropped on Hiroshima. Howard's daughter Becky heard one of Howard's exploits while he was in the Army Air Corp that would not surprise any experimental scientist. Howard was working on the capabilities of the B26 airplane when at Wright Field. They loaded it up with as many 50 cal guns as they could stuff in it — dozens — and took it up and fired them all at once. Howard looked down and noted that they'd lost 12 knots of airspeed from the recoil.

After his service, Howard attended Syracuse University as an undergraduate and graduate physics major. He was one of the older graduate students in Doc's lab, teaching undergraduate physics while working full time and starting a family. As one of the senior graduate students in the lab, Howard was a mentor to many of the younger graduate students. He, along with Don Bode and Werner Beyen made the PbTe and PbSe infrared detectors. Howard used aquadag, a solution of water and fine carbon powder, to paint the simple electrode pattern on the flat front surface of the tube onto which either PbTe or PbSe were deposited. These early infrared detectors were then characterized for sensitivity. A more complete discussion of this activity can be found in Alfred Mac Rae's recollections further on in this document.

At this time in the early development of infrared detectors there was a great desire to understand the crystalline nature of the deposited PbTe and PbSe films for correlation with infrared sensitivity. This desire undoubtedly caused Howard to use and more thoroughly understand the X-Ray tubes used for this characterization. His daughter recalls that Howard was within four months of completing his Ph.D. thesis research on these X-Ray tubes when someone in Germany published a paper on almost the same subject. As is the custom with all Ph.D. research, this would require a new Ph.D. thesis topic and most likely two or more years of additional research. Since Howard was by now a father of three children, with another on the way, he decided to use his research as the basis of a Master's thesis entitled, "An Investigation of the Characteristics of the Conical Target X-Ray Tube."

Upon receiving his Master's degree Howard joined Philco to double his university salary to support his growing family. In 1963 he took a job as Vice President for Infrared with Networks Electronic, followed by a move in 1972 to Santa Barbara Research Center as a Senior Scientist for Infrared Research. He remained there until his first "retirement," some time in his 70s. While there, he helped design and build numerous advanced infrared detectors, and mentored many younger scientists in physics, optics, materials, and related topics. He never really retired, but continued mentoring younger scientists, and consulting on infrared physics until his death. He loved the fact that numerous Goleta optical physicists were Syracuse University alumni.

Jay Zemel (Ph.D. 1956)

While Henry was one of my Ph.D advisors and the dissertation work itself was based on his

PhD dissertation, it was not a IV-VI dissertation. However, and most important, I was involved with the early crew of Don Body, Searl Silverman, and Channing Dichter who were working the chalcogenides under Henry's direction. This exposure to the early film work at Syracuse provided me with the background that eventually lead to my joining the US Naval Ordnance program on IV-VI IR sensors in 1955. As you know, Henry enjoyed and welcomed having his graduates maintain contact with his program. In 1960, the emphasis on using single crystal material to determine the precise optical properties of semiconductors was becoming of increasing interest. When I took over the Surface and Film group at Naval Ordinance Laboratory (NOL), it was clear that there was a need to have a program addressing single crystal IV-VI films if there was to be any hope of identifying the specific mechanism responsible for the room temperature photoconductivity of these materials. That was what we did both at NOL and upon my moving to the University of Pennsylvania. I have no doubt that it would not have occurred had I not been involved with Henry and his students during my graduate work at Syracuse.

Henry provided a broad education for all the students, even those like myself who worked on molecular beams and surface ionization. As it turned out, it was the molecular beam ovens based on designs that Henry and I used for our dissertations that were central to the evolution of molecular beam epitaxy not only in IV-VI compounds but also the III-V compounds. I refer you to the well known review article of Arthur and Cho who refers to this as "early work" in molecular beam epitaxy.

But the importance of the work at Syracuse under Henry's mentorship was not only the well documented direct efforts on the IV-VI thin film materials but also the education it provided that spurred worthwhile research and development in fields removed from photoconductivity per se. The message of Henry Levinstein is to follow your creativity to generate new knowledge, whether in photoconductive devices or the physics of toys.

Alfred U. Mac Rae (Ph.D. 1960)

I joined the Levinstein Laboratory late in the summer of 1953. After apprenticing as the floor sweeper for room 13 in the basement of Steele Hall, I started working with Searl Silverman, who was doing research for his PhD thesis. Under the direction of Searl, I first made a Stockbarger furnace to grow single crystals of PbTe and PbSe. Following their growth, I cut the crystals into the appropriate bar shape and made Hall measurements to determine their carrier concentration and mobility. I well remember chasing the light beam from a ballistic galvanometer around the west wall of the basement room in the process of making these measurements. These crystals were ground up and became the basic ingredients for making the infrared detectors. Searl also used these crystals to make additional measurements that served as the basis of his thesis. This was a marvelous professional experience for me. Professor Levinstein and Searl never did hover over my shoulder. I was left alone and benefited from the experience of being an independent experimental physicist. It was a great learning experience.

I was a serious student and thought that this was the proper characteristic of a physicist. Searl changed that. He was lots of fun and was able to laugh at the least little mistake that seemed to be common in that room. I still laugh to myself when I think of Searl's venture into dentistry. His uncle was a prominent physician in Syracuse and it appeared to me at that time that Searl once had designs on becoming a physician. He attained that lofty position, albeit without the proper license, when an overweight construction laborer came into the room and asked the white lab-coated Searl if he were a doctor. Searl, of course, said, "Yes, what can I do for you?" Greatly relieved, the laborer said that he had a very sore tooth and asked Searl if he could remove it. That was all Searl needed. He sat the fellow down on a high lab stool, draped a white towel around the fellow's shoulder and looked into his mouth. The tooth was hanging loose, so Searl doused the offending tooth area with absolute ethyl alcohol, cleaned off a needle nosed pliers with alcohol, and gently removed the tooth. This extraction was followed up again with a liberal dose of ethyl alcohol. Of course, Searl refused payment for his services. I stood in the back of the lab watching this operation with amazement, but I am sure that I did not laugh. That is not the end of the story. The next day, the laborer came into the room and thanked and praised Searl, who then was in his glory. Needless to say, this episode was not shared with "Doc", our name for Professor Levinstein once you were accepted as a member of his lab.

After Searl climbed "Mrs Jennings wooden leg,"[2] to affix his properly engraved plaque onto the machine shop pole that supported the Physics Department safe upstairs in Mrs. Jennings office, I moved my activities over to the laboratory located under the north top stands of Archibald Stadium. That was where the real action of the Levinstein Lab occurred. Don Bode, Howard Davis and Werner Beyen, all graduate students, made and measured the characteristics of the infrared detectors that brought fame to the lab. The bodies of these pyrex detector dewars were fabricated by Charlie Greene in his glass shop. They were shaped like a small tubular cryostat, with a sapphire window and an interior flat surface that eventually received the evaporated film of either PbTe or PbSe. It usually was Howard Davis who used aquadag, a solution of water and fine carbon powder, to paint the simple electrode pattern on the flat front surface of the tube. Charlie Greene then completed the fabrication of the tube. Powdered PbTe or PbSe was placed in the tube, and it was then sealed onto one of the several vacuum stations in the lab and then pumped out by oil diffusion pumps to attain a pressure of $\sim 10^{-6}$ mm Hg. An oven was placed over the tube and cool air was blown into the cryostat section of the tube to cool the substrate where the vapor of the powdered material was deposited. Puffs of oxygen were let into the vacuum system to "dope" the film. Smoking was a required talent of these guys. An important procedure was to hold a lit cigarette up to the detector and to observe if the resistance of the film decreased, all done with an inexpensive ohmmeter. The puffs of oxygen were repeated until it was concluded that the detector exhibited the desired infrared response. After working on single crystals of PbTe and PbSe, I found this empirical approach to the fabrication of high technology detectors to be an example of dirty physics. To this day, I wonder why there was not an effort on the science of the sensitivities of these films. Our Lab sold these detectors to government agencies and private companies. A requirement of working in the Lab was to have government clearance to the Confidential level. Visitors were commonplace, especially users of "our" detectors. I well remember watching movies of the runway heat trails of the tracks of planes landing at airports. We were working on something that had practical applications – and that appealed to me.

At about the time when I moved over to the stadium lab, the General Electric Research Lab in Schenectady discovered that there were deep levels in gold doped germanium that had the possibility of infrared detector use. Henry gave me the assignment of setting up a Czochralski single crystal apparatus. I had considerable help from a long forgotten person from the GE Syracuse Electronics Park Laboratory as well as Charlie Johnston, the head of the Physics Department machine shop. With considerable pride, I was successful in growing a germanium single crystal on the first try. Doping the germanium melt with gold and a Group V donor, to compensate out a lower impurity level was a non-trivial challenge. Eventually, that was achieved. I assume now that it must have been on a trial and error level. I was introduced into modern semiconductor physics. What a thrill! I recognized the importance of being familiar with the physics literature and immediately joined the American Physical Society and subscribed to the Journal of the American Physical Society, otherwise known as the "green plague." I set up Hall measuring equipment and characterized all the crystals that I had grown. The next step was to set up a zone refining apparatus, again with the considerable help of Charlie Johnston. This process was required because much of the Ge material that we purchased contained unknown impurities that had to be removed. The next step was to use this equipment to grow doped Ge single crystals by using a single crystal seed at the starting end of the boat. I believe that it was Seb Borello who pointed out the possibility of this technique to me as a result of reading about it in some obscure literature. That became the crystal growing method of choice. It was much easier to control the doping of the crystals using that method than using the Czochralski process. The net result was that we were soon shipping gold doped germanium infrared detectors to our customers.

As a sidelight, the zone refining equipment was first set up using an old spark gap rf generator to produce the narrow molten zone. As you can well imagine, the rf not only heated the Ge to its melting point, it also emitted considerable rf radiation that interfered with the quality of the transmission of the nearby university radio station. With enough questions, the radio station managers narrowed down the source of the interference to the Physics Department and subsequently to me. We purchased a new rf generator immediately. For some reason, we felt that the perfection of the crystals had an affect on the infrared sensitivity of the detectors. I started to measure the etch pit density of the crystals and decided that an "after-burner" on the crystal growing apparatus would improve the perfection of the crystals. This after-burner consisted of nothing more than an external carbon tube that was also heated by the rf, which minimized the abrupt temperature gradient at the freezing interface. The results were impressive and I published crystal etch pit photos of the before and after consequences of using the after burner in one of our quarterly reports. As a result, I received several phone calls from people who were fascinated with the results. To this day, I don't understand why I did not publish those results in the open literature.

I decided to study the surface properties of 1/f noise in germanium and that was the subject of my PhD thesis. In retrospect, I am puzzled why I didn't develop a thesis subject based on my material's interests and background. For instance, I doped the Ge crystals with a variety of elements in an attempt to extend the infrared sensitivity to longer wavelengths. I was successful with zinc and determined that I could make 10 micron detectors. Also, I investigated some interesting tertiary compounds and prepared thin films of many such materials and measured their infrared absorption characteristics to determine their energy gaps. It turns out that many of them had energy gaps of approximately 1 eV and thus were not very interesting for infrared detectors. Much to my pleasure, when I interviewed at the RCA David Sarnoff Labs in Princeton, NJ, I was told that they had a group looking at the energy gaps of tertiary compounds also. As a "wise kid" I told them the answers to their investigations, much to their amazement. They were impressed and offered me a job on the spot with the highest salary of all the places that I interviewed. Of course, I turned them down and accepted a job in the Research Area of Bell Telephone Laboratories. The primary point of this discourse is to point out that Henry encouraged us to pursue our own ideas, hopefully resulting in a promising area of study.

Now back to Henry Levinstein. He left the choice of a thesis subject to me. I assume that was true of his other PhD students of that time also. In retrospect, that was an important part of the education process. Henry trained us to become independent investigators. This struck home when I started work in the Research Area at Bell Telephone Laboratories. I was given freedom to select a project of my choice and I was off and running. However, at the time, I was somewhat envious of some of the other PhD students in the department who had professors who assigned them the subject of their thesis. Little did I appreciate that I benefited from this hands-off approach by Henry.

An extremely important advantage to being a student of Henry's was that he treated his graduate students as equals and as members of the world of physics. It was quite common for us to have visitors, many of them with world-wide solid state reputations. Henry included his students in the discussions with them. And we had an opportunity to discuss our work with these visitors, benefitting considerably from their feedback. We were invited out to dinner with these visitors – and as graduate students the opportunity to eat out at a good restaurant was a treat. We were the envy of other students in the department who were not given such privileges. Henry encouraged us to attend the March meeting of the American Physical Society and to present papers at these meetings. As a result of attending and giving talks at these meetings, I felt part of the world-wide community of physicists and was even on a first name basis of some of the heroes of physics. I well remember, as an early graduate student, Henry and Neil Beardsley tapped me to give our Lab's talk at an IRIS meeting in Chicago. Beardsley was the Wright-Field Program Manager of our infrared detector contract and visited us on a regular basis. Anyway, I assume that the rest of the members of the Lab were uneasy with me representing our Lab's efforts at this meeting of the cognoscenti of the infrared industry in the country. An important part of my talk was the zinc-doped Ge detectors. In those days, the Interstate Highway systems was not completed and we drove on the local roads to Chicago. Werner Beyen, Dean Mitchell, Howard Davis and I were together on this trip. A good deal of the trip was devoted to rehearsing my talk —seemingly continuously as far as I was concerned. They spared no criticism and asked the tough questions that they anticipated might be asked. Needless to say, the rehearsals in the long drive prepared me for that final talk and it went well—much to the delight of my inquisitors.

Henry provided a friendly environment in the Lab. We interacted socially. I well remember picnics at Howard and Betty Davis's house down in the Valley section on the south side of Syracuse. We purchased a group of season football tickets in the top stands on the 50 yard line in Archibald Stadium. This included a few extra seats that were reserved for special last minute girl friends or other visitors. Neil Beardsley managed to visit us during the football season and would refuse our invitation to go to a game – following the government rules about accepting gratuitous offers. However, usually we were able to get him to attend a game when we mentioned that one of our members was sick and thus not able to attend a game – "It would be shame to have an empty seat in our grouping." Beardsley treated us as his children. He had none of his own. He attended my going-away party at a restaurant on the north side and openly cried when people had nice things to say about me. Werner Beyen and I became close friends and spent many an evening at the Tecumseh Country Club after working in the lab. Howard Davis was an inexhaustible source of ideas on the design and setting up of equipment. As an interesting aside, I practiced my bagpipes on a regular basis, both in the laboratory after everyone left and in Archibald Stadium. The Daily Orange, the SU newspaper, wrote me up as the "Ghost of Archibald Stadium." To this day, I occasionally run into students of that era who remember the "ghost."

I would be remiss if I didn't tell a story about Henry and our twin daughters. During one of my many trips to Syracuse, I introduced my young twin daughters to Henry and asked him to give my daughters a tour of his toy lab. Henry was at his best as a showman with fascinating demonstrations of his toys that illustrated physical principles. It was no surprise to me that he was able to entertain my daughters. He put on a great show. He enjoyed interacting with children. Of course at their young age, the physical principles meant little to them, however I thought that they would long remember the toy show. As we drove away from the university, I asked them, "Did you enjoy that entertaining toy demonstration?" Their response was a giggle—a roaring giggle that only two young girls can generate. Obviously they had discussed this point earlier in my absence. When I repeated my question, they burst out laughing—the toy demonstration was not what fascinated them—it was Henry's ability to touch his nose with his tongue. To this day, they remember that interesting Professor at Syracuse University who could touch his tongue to his nose.

I owe a lot to Henry. He made physics fun, he introduced me to the community of physics, he trained me to be an independent physicist, he launched me on a career in physics and best of all, he was a friend.

Seb Borello (M.S.1962)

The best part of working in Henry Levinstein's Solid State Laboratory is the freedom Henry gave us graduate students to design our experiments. Henry, "Doc" to his students, would guide us on concepts and set the guidelines consistent with good science and the needs of those who were paying for it. Each student conducted a unique experiment, designed the apparatus, collected the data and performed the analysis. By being a low key advisor Henry engendered a high level of cooperation among all of his graduate students.

In my case I learned crystal growing techniques from Al MacRae as an undergraduate assistant. In fact as an undergraduate, I learned Hall and infrared optical measurements and cryogenic methods from several graduate students. I am still very grateful to Jay Zemel for getting me the job in Doc's laboratory when I entered Syracuse University as a freshman in 1953. Years later as a graduate student I designed and built a modified zone leveler and was able to dope germanium crystals with mercury. Henry watched my progress, was very supportive and let me find my way. When I wrote the paper for the *Journal of Applied Physics* Henry went to great pains to help me make it readable.

Claude Penchina (Ph.D. 1964)

I was an electrical engineering senior at Cooper Union in NYC in 1959. My favorite physics prof, A. Aaron Yalow (I never found out what the A. stood for) mentioned that he had seen Prof. Hendrickson recently, and suggested I consider going to Syracuse for graduate study. Yalow was a Syracuse native and, incidentally, married to Rosalyn Yalow, who did nuclear physics at some hospital because it was hard for women to get jobs at universities. As a result of her work, she received a Nobel Prize in 1977. That spring I went up to visit SU, and enjoyed meeting Fredrickson, Honig, and Levinstein and I enjoyed the beautiful sunny weather and green campus (Cooper Union is at the north end of the Bowery, with a small concrete triangular "park" for a campus.) I was offered an assistantship, so I soon accepted.

In September I drove up, and to my dismay, the weather seemed to change into the biblical 40-days/40-nights of rain, followed by 40/40 snow. I knew very little about graduate schools, and had been told that it usually took one year for an MS and three for a Ph.D.—maybe four if you had trouble with the thesis. I found out that in order to get a MS in one year, I would have to do a Master's Thesis, and would probably even have to spend the summer too. Luckily, Henry Levinstein had a research assistant position available, and thought my engineering background might be useful in his lab.

Doc—I don't remember ever calling him Henry, or even Prof. Levinstein—was 42 years OLD, if I recall correctly. He seemed to be a rather jolly, fatherly type, although he was not yet married and lived with his mother—note that my perspective on old men has changed drastically—my son is now 42. He introduced me to several students in the lab, I recall mainly Bill Engeler, John Pehek, and Carl Stannard, as well as undergrads Joe Wrobel and Kent Stannard. I think Peter Bratt, Al Macrae, and Dean Mitchell were either almost finished, or already finished but visiting frequently. Doc asked John Pehek to look after me and introduce me to the facilities to measure lifetimes in semiconductor IR detectors. There was also another 1st year grad student—I forgot his name—working in the lab, but he left after a few months to make more money at Perkin Elmer. Interviewers thought he was smarter than average to get admitted to grad school, and even smarter still to quit early. John Pehek conveyed to me the idea that the lab was a very friendly place, and that Doc was always available to help when needed, but otherwise seemed to choose good students and let them loose to do their work and teach each other without too much intervention on his part.

I enjoyed the time in the lab, and decided to continue for a PhD, so I never actually wrote up my Master's thesis, but incorporated some of that first-yr research into my Ph.D. thesis. I found all the students to be very dedicated. None of this leaving at 5 pm as they did in labs where I had previous summer jobs. Doc was not only available when we wanted him, but he also would occasionally "crack the whip" at Friday afternoon group meetings when he felt it was needed --- clean up the lab, OK to keep flexible hours but make sure I am there a few hours during the day to communicate with the machinists in the shop. He also objected to us fixing cars or engines in the stadium hallways, even though we were learning many practical skills. Eventually, I ended up spending much of my time on the theory of optical beating. Doc was very supportive of this, but also encouraged me to continue with experiments using this technique to measure lifetimes in IR detectors. He even arranged for me to go to Bell Labs to learn how to build a He-Xe laser to do the testing at an appropriate IR band.

Hans Stocker (Ph.D 1965)

At the end of 1958, I completed my studies at the Swiss Federal Institute of Technology, Zurich (ETH). The verbal exams by Wolfgang Pauli and Edward Stiefel were especially difficult. I completely froze up when Pauli asked me about Quantum Mechanics exchange relations. Only by his tolerance and my successfully solving one of his exercises using the hyper geometric functions, did I get a passing grade. This grade as well as good results in the written exams plus my research in Prof. Busch's Lab on the magnetic susceptibility of Si-Ge alloys and Sm-Oxide allowed me to pass the diploma exam.

While I was working on my highly sensitive instruments, Prof. Busch came by with Dr. Richard Petritz from NRL, Washington, D.C. who was on his way to becoming the research director of Texas Instruments (TI). I mentioned to him that I would like to go to the States. After playing the scheduled soccer game with other ETH students—I couldn't let the team down—I had a fondue dinner with Dick Petritz, his wife and daughter. At the end of the dinner, he offered me a job in the new Central Research Lab at TI. The formal offer came through by telephone three months later. It was another seven months before my immigration was approved by the US Consulate. Since this was 1959, shortly after Sputnik, I was granted exemption from military service in the US. I had already served 38 weeks in the Swiss Army, in the radar troops.

Finally, I was on the SS Westerdam, a freighter/passenger ship with about 60 passengers on the way from Rotterdam to New York. On the airplane from New York City to Dallas, Texas, they served something I had never heard of - Bourbon. I had to try two glasses. When I descended the stairway from the plane I nearly stumbled into Dr. Petritz.

My first project at TI was "GaAs Solar Cells." The group leader was Werner Beyen, who had received his Ph.D. from Henry Levinstein at Syracuse University. I did experimental and theoretical work on InSb diodes and published my first paper in the US on this subject.

I met Henry on two or three occasions when he visited TI and he seemed interested in my work. After one and a half years at TI, I felt I should get a Ph.D. I applied and got accepted by MIT, Stanford, and CalTech. However, in

January 1961, Werner Beyen told me that Bill Engeler had gone to General Electric in Schenectady, and that an assistantship was available in Doc's laboratory. A phone call to Doc was all that was needed to get me the job. I had met Henry twice while he was consulting at TI. Apparently, he did not hesitate to offer the assistantship to me. If I remember correctly, the salary was $250 per month.

Three weeks later, I took a swim in my apartment complex pool, packed my VW with the few goods I possessed and drove Northeast. By St. Louis, it started to snow. In Indianapolis, I was in the middle of a blizzard that made it impossible to get of the Interstate. In Syracuse they had 2-4 feet of snow, and Doc was very surprised to see me.

Soon, I had my desk in the lab behind Sherman Golub's. Doc suggested that I look at the spectral photoconductivity of InSb, the low energy gap semiconductor that I had studied at TI.

Doc pretty much left me alone. I found these oscillations in the spectrum which were a mystery at first. This topic, "Oscillatory Photoconductivity in Semiconductors," became my thesis project, both as an experimenter with Doc and a theorist with Prof. Kaplan. The title of my dissertation actually is: "Boltzmann Equation Approach to Photoconductivity." I postulated that the oscillations were due to the emission of LO-Phonons. Stocker, Levinstein and Kaplan published a paper in *Physical Review Letters* which demonstrated a 25 phonon!!! process. The impact of this process was to reduce the quantum efficiency of InSb detectors in the 5-10 μm wavelength range.

When I prepared for the Ph.D. exams, I was quite worried. In front of five of his students, Doc told me: "Don't worry. You'll do all right." Actually, he was right: I finished second (Marv Goldberg was first) of all students, better than the theorists.

On the basis of my work, the recommendations of Doc and Prof. Harvey Kaplan, and probably also my courses at ETH with Pauli, Scherrer, etc, I got accepted as a post doctoral student with Prof. John Bardeen, who had two Nobel prizes in physics. I was his only post doctoral student at the time. Bardeen was a very nice person, very quick to understand your work and really brief in his comments. After I talked about what I was doing, he gave a two sentence comment and I went back to my office to think about what he meant for two days. After two years, I realized I wasn't really cut out to be a theoretician. I was still on leave from TI (I hold the record for the longest leave of absence) and decided to return there. I also had an offer from Bell Labs, and in retrospect I wish I had followed Doc's suggestion that I should take Bell Lab's offer. The deciding factor for me was three weeks of vacation at TI vs one week at Bell Labs. I probably could have negotiated on that, but at the time I was too shy to try. However, since TI had a hiring freeze in 1967, I arranged for a temporary position at the IBM research lab near Zurich, my home town.

After 8 months there, I returned to TI, working again in Werner Beyen's group together with George Pruitt and Seb Borello. Doc visited again at least once. I was transferred to another group to work on GaAs devices, when the 1970 recession caused my project to be cancelled and I was given 3 months to look for another job in TI or leave. I tried for jobs in the US and Switzerland but finally I became an unemployed Ph.D. driving around the Eastern US in my Oldsmobile. I stayed with Claude and Mira Penchina in Amherst, MA for a few weeks and then passed through Syracuse, where Doc was still active. He offered me his recommendation but not a specific job. In Cleveland, I ran into Dieter Langer, who offered me a position as a contractor at the Wright Patterson Air Force Base Aerospace Research Labs. Four years later the Aerospace Research Labs were closed. I wrote a letter to Prof. Hans Queisser, who I had met in 1961, and got a visiting position at the Max Plank

Institute for Solid State Physics in Stuttgart, Germany. I met Henry in Zurich at a conference at ETH and we had dinner later. However I wasn't able to join him and Betty for a trip to the Jungfraujoch because of my teaching commitments. He seemed very fond of Switzerland. This was the last time I saw Henry.

After two years at Max Planck, I finally joined Bell Labs in 1977 until I retired 20 years later.

Peter Bratt (Ph.D 1965)

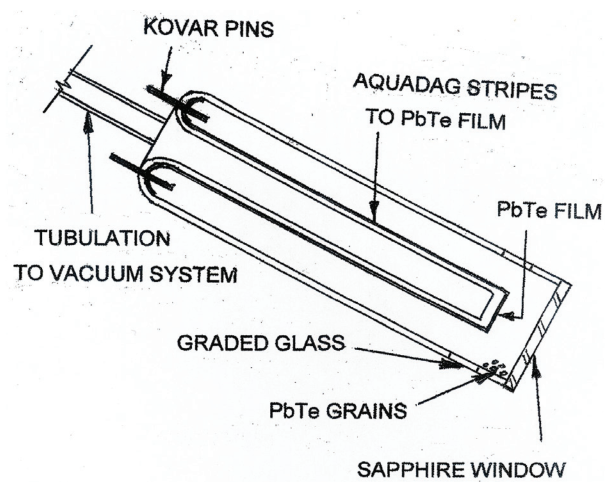
It was September 1954. I had completed four years of undergraduate work at Syracuse University doing reasonably well with an A-minus grade point average. I wanted to go on to do graduate work. Before even entering S.U. in 1950 I had mentioned to a good friend who worked at the General Electric Company in Syracuse that I wanted to be an electrical engineer. He advised me to major in physics instead of engineering because, with a physics degree, there would be a broader range of work opportunities, whereas, with an engineering degree I might be stuck in a job with only a narrow range of possibilities. This was one of the best pieces of advice I ever received because he was absolutely right. However it was now clear that just a B.S. in physics was not enough; I had to go further to broaden my knowledge base. My goal was at least an M.S. degree with a possibility of a Ph.D if I could pass the qualifying exams.

During the spring semester of that year Dr. William (Bill) Frederickson, who was chairman of the Physics Department, gave me a job as a Teaching Assistant, but I soon discovered that I really didn't like teaching. When I told him that, he suggested that I talk to Dr. Henry (Doc) Levinstein who might have a position for a Research Assistant. I don't recall the actual interview with Doc Levenstein but I do know that I started work in his lab in the basement of Steele Hall in the fall of the year.

His graduate students at the time were Howard Davis, Werner Beyen, Marv Lasser, Phil Chollet, Al MacRae, and others. The major interest at this time was the study of PbTe thin films. This material was used to make IR detectors which were sensitive out to 4.5 μm, an improvement over PbS which was sensitive only to 3 μm. Levenstein's group had discovered how to make very sensitive detector cells that were being employed by the U.S. Air Force in the early line scan IR reconnaissance systems. My first project was to learn how to make these cells under the tutelage of Howard Davis. Al MacRae, who preceded me in the group by one year, also wrote about making these cells.

The cells were made using an all glass vacuum system mounted on a work table which included the one-inch diameter main manifold connected to a 3-stage oil diffusion pump and a rotary mechanical fore-pump. An LN_2 trap prevented back streaming of oil to the manifold. Attached to the manifold was a glass bulb filled with O_2 gas and valves to meter out small amounts of O_2 to the cell during processing. Also on the work table were: a small oven to heat up the cell; a thermocouple gauge to measure oven temperatures; a hot nichrome wire source of IR radiation behind a chopper; and electronic systems to measure PbTe film resistance and sensitivity to the hot source.

The diagram below shows a cross-section view of the cell. It was a "dewar" type con-



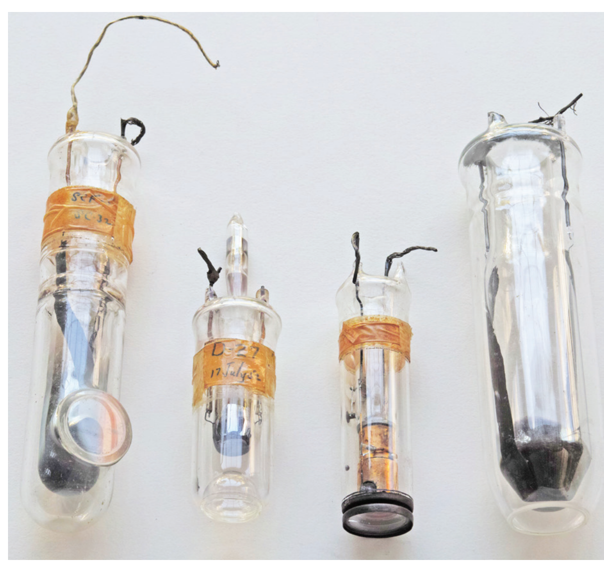
Cross section view of PbTe cell on the vacuum systerm during depostion of the thin film and oxygenation processing struction with an inner well to hold LN_2, kovar metal pins through the glass wall leading to conducting graphite (Aquadag) stripes going to the PbTe film, a sapphire window sealed to the outer glass cylinder and a tubulation connecting to the glass manifold. The glass work was all done by George Green who was the glass blower for the Physics Department. The cell required considerable skill to make because of the special glass used where the kovar pins went through and the graded glass seals required to match to the sapphire window expansion coefficient. The diagram shows the cell with a few grains of PbTe inserted at the window end ready to start processing which went as follows:

The first step was to heat up the cell to cause PbTe to evaporate from the grains and deposit onto the end of the inner dewar well—actually, this is a sublimation process because PbTe stays mostly in molecular form and does not break up into atoms of Pb and Te. The inner well was kept cool during the heating process by blowing air through a glass tube inserted in the well. After deposition of the film, its resistance at LN_2 temperature was measured.

The second step was to commence oxygenation of the PbTe film by letting a small amount of O_2 into the system and baking to diffuse oxygen into the film. The baking temperature for this was much lower than that used for the initial film deposition. After oxygenation, the film resistance at LN_2 temperature was again measured and typically found to be increased. Additionally, the photoconductivity signal was measured using the chopped IR radiation source and looking at the signal displayed on an oscilloscope. This oxygenation process was repeated step-wise many times until the PbTe film resistance had increased from a few ohms to megohms and the signal had maximized. This took about a half-day of work to complete. In those days we had no blackbody source nor calibrated test equipment. “Sensitivity” was relative to a previous signal measurement on the oscilloscope. I recall that Werner Beyen used his cigarette as the hot source behind the chopper wheel so he didn’t have to wait for the nichrome wire coil to heat up and stabilize. Later on the group built a state-of-the-art IR detector test set and became a sort of standards lab where various manufacturers sent their detectors for testing—the Navy also had a laboratory at China Lake under Lum Eisenman.

My first try at making a PbTe cell was successful and I think Howard was pleased. However, for the next run he got out a different batch of PbTe and said, “This time I will give you the good material.” I was a bit miffed that he hadn’t trusted me on the first try but Howard was just using the proper teaching method for a new recruit. First you have to show your ability in practice and then you go into the real game.

The PbTe cell fabrication technology was very important for Doc Levenstein’s Lab because these cells provided the highest sensitivity available at mid-IR wavelengths. They were used in the first IR line scan reconnaissance systems. My recollection was that these systems were made by the HRB-Singer Company. The resulting imagery was classified “confidential” so we didn’t get to look at the results until we got our security clearances. I believe that this initial work cemented the relationship with WPAFB and certainly helped to promote

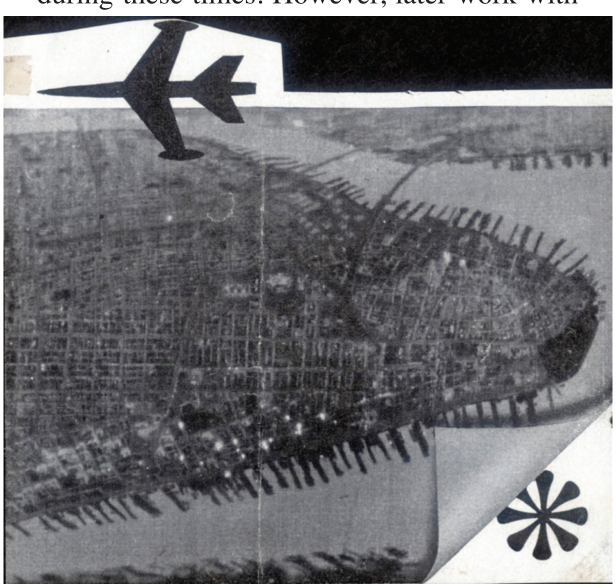


Old infrared dectector “cells” from Henry’s collection. The one second from left is dated 17 July ‘52.

the continued renewal of Doc's government contract for the many years that followed.

Sometime later I happened upon a photo in the *Aviation Week* magazine showing IR line scan imagery of Manhattan that is reproduced here. This magazine was noted for somehow getting access to classified information and publishing it without credits. Although I can't be sure, the timing of this publication led me to believe that the imaging system employed a Syracuse PbTe detector cell.

Besides making detectors, Levenstein's group also did pioneering work in remote sensing of IR radiation. Roy Paulson and Howard Davis built a small reflecting telescope to collect the IR radiation, focus it on a PbTe cell, electrically process the signal, and get an accurate measurement of the source emittance. This system could only look at one spot in a particular scene but, by moving the telescope around, could distinguish temperature differences between neighboring objects. In early work they found a thermal "wash out" effect at certain times of day (early morning and late evening) where all objects in the scene had the same emmittance (i.e. no contrast). This meant that IR viewing would be useless during these times. However, later work with a more sensitive InSb detector showed that the "wash out" effect was due to the short wave spectral response of the PbTe detector that limited the system to a temperature resolution of several degrees centigrade. Of course, modern IR imaging systems utilizing the 8 to 12 μm range view the whole scene with millions of pixels and temperature resolution on the order of millidegrees centigrade.



Line scan photograph of the tip of Manhattan Island taken with IR Thermal Reconnaissance System. The hot (white) spots are very likely to be power plants.

I believe that it was sometime in 1955 that we were able to move out of the very small lab space in the Steele Hall basement to new laboratory facilities in Archibald Stadium. Metal stands were added atop the old concrete bowl to increase seating capacity and there was space underneath these stands for Doc's laboratory. This new space was very welcomed except that during football games—if the people in the stands were stomping their feet, the laboratory ceiling would vibrate and the noise came through. However, most of the grad students as well as Doc had season tickets to the games so we were in the stands doing the stomping and the laboratory provided a welcome place to gather at half-time for hot coffee or cocoa on those cold autumn days.

Doc Levenstein was noted for a "hands off" policy with his graduate students. He was not personally directing their day-to-day work. There were no weekly conferences and we were free to conduct experiments pretty much as we saw fit. We had assistance in building equipment from the machine shop headed by Charlie Johnson and from George Green in the glass shop. Doc would sometimes wander into your work space and ask questions but would not criticize. Important results were written up for the quarterly reports required by the Air Force contract and also presented as papers at IRIS meetings or published in physics journals. I was also in the car with Doc and my fellow graduate students on that trip to Chicago where Al MacRae presented his first IRIS paper. My primary recollection is that Doc was exceedingly cautious and drove very slowly along the roads but the car swerved a little side to side as we went along. I don't know why; perhaps it was because of an overload of beefy passengers in that old Chevrolet, a soft tire, or just his way of driving (Stepson Robert notes that Henry was blind in one eye from a childhood accident and was overall a terrible driver!)

The contract monitor was Dr. Neil Beardsley who often came to visit the laboratory. He was a retired college professor then working for the government at Wright-Patterson Air Force Base and was a very friendly person who took great interest in what we were doing. He sometimes referred to us as "his boys." I remember one time when I had fabricated a detector using Ge:Au which gave superior performance. He wrote me a personal letter of congratulations which I still have in my files.

Because of this atmosphere of freedom and respect, these were non-stressful happy times even though the pay was so low that we were below the poverty level. By this time I was married and living in the married students housing area on East Colvin Street called Slocum Heights. As I recall the rent was only $35 per month so this certainly helped us get along. We had a son, Jeffery, who was about three years old at the time and, on days when my wife Cherie had the car she would drive down to the Lab with Jeff to bring me back home to dinner. I would let Jeff play with an oscilloscope because he liked to put his finger on the input terminal (no high voltage danger there) and watch the trace on the screen wiggle up and down because his body, acting as an antenna, was picking up stray electromagnetic radiation (mostly 60 cycles) in the lab. One day when she came to the lab Jeff had a new toy top that, once you got it spinning and tried to knock it over, would quickly pop up again and continue spinning. This was of course a good example of the gyroscope principle. When Doc saw this top he just had to have it for his toy room. I was happy to give it to him because it was a bit unusual as tops go and he probably wouldn't find one in an ordinary toy store, but little Jeffery was not. In the end Doc got it but Jeffery went home whining "that man stole my top."

The physics work in those days was mostly experimental but there was a small theoretical physics group at SU under Dr. Peter Bergman studying relativity. (Bergman was previously at Princeton working with Albert Einstein.) A friendly rivalry existed between these groups over whose work was more important. I remember one time when Doc challenged Bergman to solve the problem of how to wire up a light bulb in the stairway so that it could be turned on or off by either the downstairs or the upstairs switch. Of course every electrician knows the answer even without a college education. The story goes that Bergman was initially baffled by the problem but eventually worked it out. Doc got a big kick out of telling this story.

My final years in Doc's lab were spent studying the properties of doped-Ge single crystals pertaining to their use as long wave IR detectors. ("Doped" is laboratory slang—a more correct term would be "impurity activated.) Starting with Ge:Au, I then went on to study Ge:Cu, Ge:Cd, and Ge:Zn materials. Al MacRae added the impurity element and grew single crystal Ge boules. We completely overlooked Ge:Hg at that time. Later on Seb Borello discovered this material and provided one of the most important breakthroughs in IR detector technology to come out of Doc Levenstein's laboratory.

Sometime after graduation I was honored to be asked by Albert Beer to write a review article on "Impurity Activated Germanium Detectors" to be published as a chapter in one of the volumes in the *Semiconductor and Semimetals* series of books edited by Willardson and Beer. I don't know for sure but suspect that Doc might have hand a hand in steering Beer to me for this work. I accepted the task thinking—aha, book sales might mean royalties for the author. After a considerable amount of work the article was completed and published in Volume 12 of the series; and yes I did receive royalties; a reasonable amount the first year, somewhat less the second, a little bit the third and nothing after that. Apparently these books were purchased mainly by libraries for reference purposes. When I added up the time spent in writing and divided into the monetary income, I made about 25 cents per hour for my efforts. Even still I consider it a worthy endeavor and a useful contribution to the literature on IR detectors.

Much of the technology developed in Doc's lab is now obsolete but not gone from our memories. (Modern IR imaging employs mostly HgCdTe, InSb and Si materials.) The importance of it was in providing a starting point from which additional progress inevitably proceeded and led to a whole new field of technology which enriches and benefits today's world.

Looking back I can see that I was indeed fortunate to be able to become a part of this group. Not only did I receive the needed experience to go on to a successful career in industry but I entered into a fellowship of friends and associates that made up the driving force responsible for the early pioneering work on IR imaging in the world.

John Stannard (Ph.D 1967)

I was connected to the SU physics department from an early age.—from about age three. My father was teaching physics to non-comms prior to their going to the War. The hope was to lengthen their lives by teaching them how to use physics. What he was trying to do is now a very popular discipline. Belay that. It is becoming an increasingly critical discipline.

He would take me to his classes. Looking up all I saw was row upon row of freshly starched and pressed khaki pants legs. At will I can recall that image perfectly, even today. Guys from our neighborhood had not come back so to some degree, even at that age, I think I knew what I was looking at. Doc's having lost family in Europe made my time at SU all part of just one piece of cloth. There was no question in anyone's mind. What we grad students were about in Doc's lab was important.

Doc did not stand on ceremony and I have to say neither did any of us. His method for teaching the work ethic was straightforward. If you needed to learn something, you had to go out and learn it. This pretty well matched the environment I encountered later in life.

A single anecdote. Much later at Hughes I was sent to figure out why we were not able to assemble our first f2 program. By 2 AM I had a hypothesis, I got a young lady to laser cut some resistors and by 3 AM I knew I was on the right track. Since she had seemed down, I went back to her and excitedly told her "You did it!" Now crying she said, "I am so glad something finally worked."

No matter what we did or did not do, Henry Levinstein gave us kindness and generosity, seemingly without end. By nature I have neither of these two qualities. When she began crying I realized who it was I had been trying to imitate. I think it is only by knowing who it is we are trying to emulate that we learn who we would choose to be, if we but could. Doc yet remains with all of us.

Carl Stannard (Ph.D. 1969)

Dr. Levinstein (Doc, of course!) never gave me the feeling that he thought less of me when I decided I wanted to pursue the department's alternative track for preparing college physics teachers. He even suggested that as a thesis topic I could look into a new and unexplained effect that had been observed by Blunt, who had found periodic variations in the magnitude of the photo response of Cu-doped InSb photo-detectors as a function of the wavelength of the incident light (1958). When we looked at the long wavelength (8 to 34 μm) extrinsic photo-signals of InSb samples doped with Cu, Ag, and Au impurities, grown by Bill Engeler, we observed an exceptionally regular periodicity in the photo response as a function of the energy of the incident photons. This energy periodicity corresponded remarkably well with the energy of longitudinal optical phonons in InSb.

The mechanisms involved were subsequently explained from basic principles by Hans Stocker. His work showed that the periodic oscillations in the photo-signals arose from a transfer of energy into lattice vibrations via a resonance-like mechanism produced as the changing photon energies caused the electrons excited by them to couple with the lattice in a regular fashion as a function of the photon energy transferred to the free electrons. This whole process gave me an exceptional opportunity to act as a participant in the process of discovering and helping work toward a better understanding of a unique, new and unexpected phenomenon. I still remain very grateful to Doc for allowing me to participate in such an effort.

As I approached completion of the degree and began a job search, Dr. "Freddy" (as Mrs. Jennings called Dr. Fredrickson) called me into the office and suggested that I seriously consider applying to the small college in the Southern Tier, whose founding, Syracuse, under Chancellor Tolley, had assisted after the War, as they also had with Utica College. In an era before ubiquitous community colleges, it made sense for Syracuse to do so. If nothing else, such outliers could act as feeders for transfer/post-graduate students.

The new college, named Harpur College, after Robert Harpur, a member of the NYS legislature early in the nation's history, was led by a geologist from Syracuse University, Glenn G. Bartle, who was responsible for setting its mission to become the premier public liberal arts college in New York, "The Public Swarthmore." Dr. Freddy knew that I was aiming toward a teaching career, so he pointed out that Harpur College was pursuing both research and teaching equally, with teaching accomplishments to be given weight equal to research in personnel cases. He stressed that it would be a rare opportunity to join a small, high quality school, which was about to be transformed into one of four elite "University Centers" of the State University of New York. Dr. Freddy was very persuasive and it turned out well for me.

It always made me feel good when the students at Harpur would call me "Doc," since it would recall Doc for me. I knew, of course, that I was not his equal, but maybe I had at least gotten part way. I must admit that this "Doc" copied that "Doc" shamelessly—but I never could do the tongue-to-nose trick!

I often used Doc's version of the SU motto: "Science crowns all knowledge" and when an experiment or demo didn't go as expected, I'd observe: "Such is life!!"—another of his favorite sayings. When there was a point where I wanted the class to give more thought, I would stand back, looking at the board and say: "Oy," or "Oy vey" in obvious distress. Pretty soon they saw that to be the sign that more thought was required. When a student would ask at the end of the term if I was Jewish, I would just talk about Dr. Levinstein, telling them how I copied him, and how much impact he had had on me.

After being asked to act as Chair of the Undergrad Curriculum Committee, we undertook a revamping of our entire physics laboratory curriculum. With only a master's program and a small faculty, it was hard to cover large numbers of traditional two-hour lab sessions in General Physics. We changed those labs into an augmented, but shorter, one-hour lab/demonstration/computer-simulation format, replacing our previous labs with simpler versions. The new format was based on demonstrations of topics the students could relate to, including ordinary, everyday things drawn from cars, simple biological systems, household devices and even toys, which seemed especially appropriate for a school emphasizing the liberal arts. We used the "extra" hour that was left over for an additional discussion section.

But the new format left a gap in the preparation for majors. So the sophomore curriculum was revamped to strengthen the lab experience, starting with requiring individual library work, no longer using detailed lab instructions, but modeled on Cornell's Advanced Lab. And, having completed the General Physics course, an entire range of physics topics and techniques became accessible in each experiment to provide a more fully integrated lab experience. Individual write-ups, intended as an introduction to preparation of "mini-papers," were required. The term ended with students presenting one of their labs as a "seminar" talk, modeled on departmental seminars. Students were encouraged to consult with each other, as we used to do over the coffee table in Doc's lab.

Similarly, the same pattern was followed for a range of experiments in the new Junior Lab, covering topics from intermediate level courses, replacing the embedded labs of several courses, and leaving more time for class work in those courses. Likewise, a similar pattern was followed for the Senior Lab and the Graduate Lab. The new structure has worked well for the students, giving a staged ladder to work through the processes of developing their experiences and abilities with all components and at all levels of experimental physics.

In writing this essay, I see more clearly how the experiences in Doc's lab and courses better prepared me, as co-leader in the revision of the lab curriculum, to follow the patterns Doc used in his lab: start new undergrads on the bottom rung—although not quite sweeping floors—then work them progressively upward, but without micromanagement. In the process, we gained familiarity and experience with our tasks, library work, building of apparatus, making measurements, understanding the underlying principles, writing reports, and talking about our set-ups, work and results, with Doc always wandering through the lab, quietly watching and making suggestions only when necessary.

I realize that this is a common process in general use, but it seemed to me that Doc was especially effective with its use in a manner that developed competence, independence and confidence. This is the pattern we have tried to duplicate in our labs. It continues to provide good preparation to our graduates. Many of whom have gone on to significant achievements.

Somewhere in the late 60's or early 70's, I served a three-year term as Department Chair and learned that I would not have wanted Dr. Freddy's or Doc's job! My task was further complicated by the severe financial constraints the University was facing at the time. For instance, one summer, our liquid nitrogen generator needed a complete overhaul. The technical specialist who babied it and kept it going couldn't find anyone to act as his assistant. So I took off my jacket (we had abandoned ties by then), went down to the basement to hold things and hand him tools. The machine worked well when we were done and the whole experience was consistent with Doc's approach—"Do what you have to!"

An unexpected benefit of the curriculum change arose when it became clear that our administration assigned departmental resources based on the student enrollment being served. To increase our enrollment, we needed courses at a level below that of Introductory Physics that would appeal to liberal arts students.

I was asked to develop and initiate several such courses: Physics And The Automobile, Medical Physics, and, during the Carter years, Solar and Solar-Related Energy. The auto course was first. When the Harpur Curriculum Committee complained that it was a technical "repair course," we had to explain that, yes, the students who wanted to work on their cars would benefit from knowing physical principles, but that ours would be a genuine physics course which just happened to be based on automotive applications. My preference was to use car parts that had failed, so that I could show how they were intended to work, as well as how and why they had failed. The students donated old parts as well, volunteering parts that had been removed from their own cars.

One of them gave me a complete manual transmission from a Buick, which his dorm advisor had told him to get out of the dorm store room or else! I was given a huge (about 14" long) marine engine intake valve, which must have come from a very large boat! At least, it could be clearly seen from the back of the lab room! I patched together a lot of demos with scrap materials and instructional apparatus on hand. To make a "working" Bernoulli demonstration for a carburetor, much as Doc would have done, I made a rickety wooden frame to hold the carburetor itself, filled the carb-bowls with water, simulated the intake engine air by blowing the air from an air-track supply-hose downward from above, and showed the class that my hand and the table below it were wet.

Similarly, I patched demos together for the medical course. Pre-medical students in the course were also very helpful with suggestions.

The energy course was popular during the late 70s energy crisis, but interest dropped off with Reagan's election and the return of the "petro-economy."

The development and offering of these courses was done before I had heard of Doc's "Toy Talks," but I see real similarities in our approaches to non-traditional students. Our courses, of course, drew very heavily on ideas and approaches I had learned from Doc. At least once, we invited Doc down to present his seminar on toys, which was a big hit.

In the 70's, together with Bruce Marsh of SUNY-Albany, I served as the Co-Project Director of one of four Development Centers for the Tech Physics Project, an NSF supported project with overall direction by Arnie Strassenberg of the AAPT. The national project developed about 30 modules of instruction designed for Technical Students in 2-year colleges. The intent was to use particular devices or systems that those students would encounter in their careers or lives to motivate the study of the physical principles. Unfortunately, the public release of the program coincided with a national recession which prevented investments in new programs.

By that time, due to the same recession, Binghamton University—as Harpur College had become—was suffering a dearth of liberal arts students, since jobs in those areas were scarce. Our president proposed a collaboration with Broome Community College, the local technical college, for what he proposed to call a "Joint Degree Program," in which liberal arts students, worried about post-college employment, could get both a two-year technical degree and a four-year liberal arts degree, in the same four-year period. They then could use the more "salable" technical degree for a first job, while they either kept looking for a job using liberal arts skills, or just waited for better times. The president asked me to be the director of the program and provided assistance in preparing a proposal to FIPSE (the national Fund for the Improvement of Post-Secondary Education). The program was funded and served a number of students, mostly as hoped. But then the job crisis eased and program enrollment dwindled.

In an effort to bring more science to young children—again, shades of Doc!—I would often visit local elementary schools with instructional physics demos, often taking liquid nitrogen, which was, of course, always a big hit, with no one staying in their seats. Over the years, it was clear that the teachers were grateful for the exposure to the physics in their classes, and wanted more visits than I was able to cover.

Seeing the teachers' needs, a local high school teacher, together with a member of our School of Education, and I formed a network of about 100 local schools across the Southern Tier to help K-8 teachers who might want personal enrichment in physics which could be integrated into their classes. At the start, we gave occasional Saturday workshops on ideas and topics useful in their classes. After a few years, we submitted a proposal to the NYS Education Department requesting a grant to allow us to set up a series of summer workshops for teachers to be recruited from the network, together with a few intensive Saturday programs for the summer participants during the year, in order to help them integrate the summer experiences into their classes. After three years of summer workshops, we were funded by NSF to initiate a project to try to extend our local network model on a statewide level. That project ran for another three years, but statewide collaboration proved too difficult for local districts to maintain. After my retirement, the visits to local schools have been assumed by our superb lab supervisor, with the help of large numbers of our more advanced undergraduates, who find the experience valuable, educational and fun.

After retiring with emeritus status, as Bartle Professor, in late 1999, I taught the Sophomore Lab for one term per year afterward for three years.

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Rao Mikkilineni (MS 1969)

I came to the US as a graduate student and I am deeply indebted to the Doc as he was known to his students for giving me the assistantship to work in his semiconductor laboratory. When I met Professor Henry Levinstein, the first thing that struck me was his Santa Claus like demeanor and the second thing was the close relationship and the deep bond that existed between him and his students. The atmosphere in the laboratory where the students worked at all odd hours was like a family living together and carrying out daily chores. I felt right at home when I saw how the senior graduate students took me under their wings to guide me and help me. I am especially grateful to Paul Norton who took special effort to guide me through various trials and tribulations I underwent as a fresh graduate student in a foreign country for the first time. I had only known about Henry from his papers he had published that inspired me to pursue my interest in semiconductor physics at Syracuse university. When I wrote to him of my interest in working in his laboratory, I had no idea of the world-class laboratory he had and the very bright students who worked day and night to advance the ideas he encouraged them to pursue. More importantly, his warm welcome and the familial environment in his lab made me feel at home and get right into my studies. The university itself provided a plethora of courses from very talented professors both in solid state physics and high energy physics. Thanks to Henry's confidence in me and the help from my colleagues and professors at Syracuse University, I successfully pursued a career in Physics that led me to a career in Telecommunications, IT and Cloud Computing. I am deeply indebted to the Doc for my career and I always fondly remember my days in his lab.

Frank Renda (Ph.D. 1970)

The fact that I went to SU—and even graduate school—was "by chance and not design." At Brooklyn College I did OK and ended up majoring in physics. I worked on and off, although a full time student, and took my eye off the ball when I decided to continue at Brooklyn College for a ninth semester (Sept. '61 to Jan. '62) so as not to have a very heavy a schedule in my eighth semester. I say "eye off the ball" because I forgot that you only can get a draft exemption for 8 semesters—not 9. So, very shortly afterward I received a letter from the Draft Board telling me to take a physical. I became 1A and was told not to make any commitments since in an estimated 2 months I'd be drafted.

About that time I met the Chairman of the Physics Dept. at Brooklyn College and he asked me "To what graduate school are you planning to go?" I told him that I wasn't planning to go to grad school for two reasons: I had no money and, besides that, I would be drafted shortly. He said that he'd give me the names of 5 grad schools to which he felt I should apply. So, I followed his advice and applied to the 5 schools—4 of which told me to reapply in April as they didn't offer assistantships in midyear. SU wrote back to me and offered an assistantship that started in February ‘62. Needless to say, I accepted the appointment and, luckily I registered at SU before being drafted.

So in my first semester at SU, I was a teaching assistant (TA). Around Easter-time of ‘62 I went to see Prof. Frederickson, the Chair of the Department, about the possibility of a TA job for the summer. Since I was self-supporting and had no commitments back in B’klyn, I preferred to do something in physics for the summer rather than a usual job—e.g. truck driver—back in Brooklyn where I could temporarily make a fairly good salary but would not advance my career. I was told that all the summer TA jobs were decided in January of that year but to “go see Prof. Henry Levinstein. He may be able to hire you for the summer.” So I was hired and was initially under the tutelage of Seb Borello, who was shortly to leave for a job at TI. I knew almost nothing about Solid State Physics and was fascinated by Seb’s knowledge and the ease with which he explained things. So I learned how to make dip-tube Hall measurements. Also, around June ‘62, there was an IRIS Detector Specialty Group Meeting at SU. Doc suggested that I go to the meeting and sit with him at the dinner. I did so and the person sitting next to me was wearing a name card that said Peter Debye. I remember thinking that the guy sitting next to me was probably too young to be “the Debye of the Debye length.” But being too shy to ask the question directly, I just waited until I could ask someone safe and found out the person who had been sitting next to me was the son of “the Peter Debye”.

So with that as a start, I undertook my sojourn at SU, and indeed, it was a sojourn as I didn’t finish untill July ‘69. I remember being in the lab on a Sunday morning in early June of 1969 collating copies of my thesis on some tables that I had temporarily put together in an aisle near one of the doors when Prof. Ginsburg opened the door to the lab. He was escorting Dr. Lee DuBridge, Science Advisor to then President Nixon. Dr. DuBridge was going to address the graduating class later that day and was being taken on a tour of Doc’s lab. Prof. Ginsburg introduced me to Lee DuBridge, who asked a polite question about my thesis. Luckily I—who was not known for concise answers—was able to give him a concise answer using the graphs about my research which were right on the table in front of us.

I’d like to thank my colleagues in Doc’s Lab for their ideas and suggestions, especially Julius Cohen, an earlier student of Doc’s who turned me on to tunneling devices. Doc certainly was a “hands off” advisor and only once in a while did he make a suggestion or comment about one’s research.

After leaving SU in ‘69, I went on to a career at Santa Barbara Research Center where I was joined by lots of colleagues who were SU grad school alumni.

Paul Norton (Ph.D 1970)

I came to S.U. in the fall of 1965 having majored in physics as an undergraduate at Carleton College—with a modest C^+ average in my major—all A’s and C’s. The first year I was a teaching assistant under the baton of Paul Gelling. As the summer of 1966 approached I was asking around my fellow graduate students about where one might find a summer position—one of them suggested that I talk to Henry because he had funding.

When I met with Doc he asked me one question—”What is your aim in life?” I answered—”To get my degree as fast as possible.” That was apparently good enough for him to take a chance. I was hired for the summer. Henry took me off to the lab under the S.U. stadium and offered my services to Joe Wrobel, the most senior graduate student at the time.

Joe was a character, he used to prepare purified III-V materials while smoking a cigar, forged his S.U. parking sticker every year so that he could park next to the lab, and conscientiously bribed the S.U. guard staff with holiday gifts. Joe taught me how to purify antimony and indium and grow InSb crystals. One of those crystals was used by Sherm Golub to get his Ph.D. Joe also taught me how to use the spectrometer and how to make Hall measurements. A subset of that skill was interpolating six-figure logarithm tables to get the seventh digit—not in much use today.

Joe worked late every night along with John Stannard. We frequently went out for donuts to Abe's shop on Erie Boulevard for coffee and a snack, and also to Drumlins and Weber's for dinner or lunch. The lab action was mostly seven days a week, but John took off many weekends to rock climb in the 'Gunks, and Joe and I spent a few days hiking mountaintops in the Adirondaks. John also had vertically-challenged climbing routes set up around lab using the overhead plumbing lines and door frames for hand-holds—interesting characters.

John Stannard taught me a lot about making electrical—oscilloscope mostly—measurements as Joe began thesis writing. I also learned how to measure D*. Doc had several small contracts, for example with the Army, to make D* and spectral response measurements on commercial and/or developmental detector samples. At that time his lab served as check on contractor veracity—a worthwhile function and a source of flexible revenue (the cell fund) for lab equipment and other expenses outside the main Air Force contract scope.

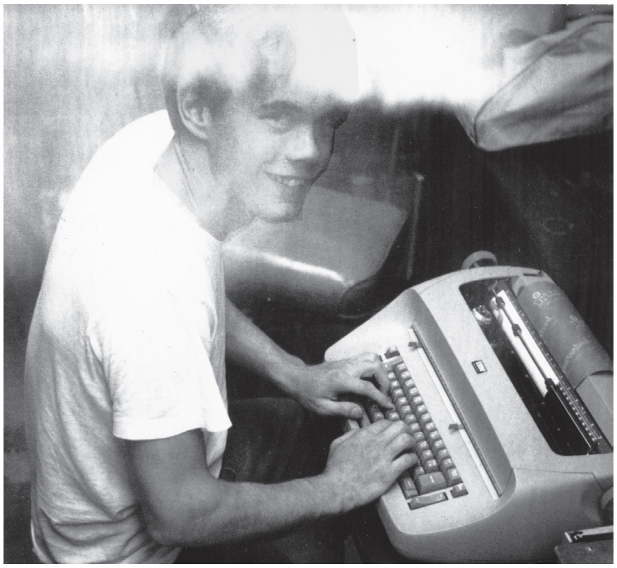
In the fall of 1966, Doc kept me on as a research assistant. I was really enjoying the laboratory work and was learning a great deal from it as well. My first independent assignment from Doc was to build a CO_2 laser. General Electric—Henry called them Generous Electric—had one at their campus north of Onondaga Lake and we went out to see it. Basically a quartz tube with gas ports at the ends and neon sign electrodes powered by a neon sign transformer. Somehow I thought that such a thing should be built in Corning NY, so I contracted with a glass lab there. Later I came to realize that George Green could have built it—wouldn't want to say blown-it—just as

Paul Norton in the new Physics Building sub-basement lab with the CO_2 laser around 1968

well. That fall or winter I got it lasing for the first time and wrote Doc a note that was laser-burned into a sheet of thermofax paper—"how about a raise?" Doc had a good sense of humor, but I didn't get a raise.

The laser, running off a neon sign transformer, operated at 120 Hz. There were some higher-frequency side-bands in the range of a few dozen MHz, but their behavior was erratic. Doc envisioned using the laser for time-constant measurements. Sometime later I read that a scientist at Monsanto in St. Louis, MO, Robert Weil, had used a GaAs crystal as an electro-optical modulator. He agreed to give me one in exchange for a gold-doped germanium crystal. I used the student machine shop to build a holder for the crystal having two N-type connectors in a transmission line configuration with a 5-pound, 50-ohm load resistor. The laser was converted to DC operation using the power supply for a surplus x-ray generator together with a huge HV capacitor and bleed resistor that Paul Gelling supplied from his secret stash.

With modulator and CO_2 laser running DC we could measure detector response times down to less than a nanosecond. Doc signed me up to give an IRIS paper in 1968 on this development at a meeting in Dallas. When Doc, Thad Pickenpaugh, and I showed up at the Ramada, it turned out that Thad didn't have a valid reservation so Doc suggested he share my room.


Paul Norton typing a quarterly report—two copies with carbon paper. Photo by Joe Wrobel.

No problem until Thad pulled out a Baby Ben wind-up alarm clock—it seemed that Thad didn't trust motel wake-up calls. All night I listened to that thing ticking and I was semi-comatose giving my first professional talk.

It was around 1967 or 1968 that we moved from under the stadium to the sub-basement of the new physics building. We had master keys to the stadium, and as luck would have it, the first night in the new building the cleaning crew mistakenly left their master key in our lab where it quickly experienced a very low vapor pressure. That key was better than even Doc's—I had access to forbidden places like the machine shop! Many an experiment was saved by midnight access to a critical 2-64 stainless screw. The master key also helped us route an FM cable from our antenna on the roof to the sub-basement so we could listen to music.

Doc's lab was a truly great resource for launching experiments and trying them out, sometimes the same day. Between George Green blowing glass or quartz and skilled machinists like George Rabe, there wasn't much that couldn't be tried in short order. I felt very lucky to stumble upon not only an interesting field of research, but a superbly well-equipted place in which to exercise that opportunity.

Some items of ready-access were probably ill-advised. We had lots of asbestos in the lab—rolls of asbestos paper for insulating quartz tubes while they were being sealed and actual asbestos table tops—many of which were lovingly sanded smooth! Another item was 180-proof grain alcohol—tax free for the lab. We seldom thought of water for cleaning and instead mostly used grain alcohol. One time I nearly passed out from cleaning the stadium lab windows with carbon tetrachloride—not the brightest move. I was lucky to avoid any addiction to drinking the grain alcohol, but there were rumors of earlier students who may done so.

Anyone could apparently order most anything from a pad of order forms in the lab. Every time a new issue of *Research* magazine came out there was activity. Some of my lab cohorts took the trouble to etch their initials on these precious items—such as tweezer—but that was frequently asking for trouble—one large high-vacuum machine somehow had other peoples etched onto it late at night! Henry wasn't fond of these dust-ups though.

The new building also had a large service elevator that provided on-campus parking at night and allowed me to bring my motorcycle down to the lab for repairs and maintenance—until Department Chairman Nate Ginsburg pointed out some pesky insurance clauses to me.

As I got into my thesis work on copper-doped germanium, John Stannard, who had gone to work at NRL sent me a sophisticated curve fitting program in Fortran that proved invaluable in my work. It also served another important function—I was able to convince Nate Ginsberg to let me use Fortran to fulfill my required second foreign language requirement.

Every three months the Air Force wanted a quarterly report, and once a year we had an annual report to write. This was left chiefly to anyone who had an experimental result that could be documented. Sometimes the annual report would largely consist of a students thesis. The good news for students was being able to have their thesis published gratis. A few of us still have a hoard of these old reports.

In 1968 there was a summer Photoconductivity Conference at Stanford—Doc said it would be the last one in that series but didn't explain why. Although I didn't manage to get a paper accepted for presentation, Doc let me attend. This prompted my first sabbatical as a graduate student. I packed my tent, sleeping bag, and camping stove and drove my motorcycle out to California. Wanting to dress appropriately for the conference, I asked Doc to bring my suit when he flew out. He looked a little stunned, but he did it. I did make it to the conference, dressed up for the meeting, and then spent another several weeks in British Columbia, Minnesota, and Chicago. There were some perks for working seven days a week for 16 hours a day!

Around 1970 we received a PbSnTe laser from Joe Wrobel who had gone to work at Texas Instruments. Paul Chia, Tim Braggins, and I decided to try monitoring air pollution with the laser, sending a beam from the roof of the physics building to the roof of a girls dorm over on Ostrom Avenue—I think it was Booth. The beam was columnated with a large spherical mirror, bounced off a large flat mirror on the dorm roof, and back to another spherical focussing mirror. The laser was pulsed for high output and the temperature was ramped to vary the wavelength. This led to our first publication. Henry was our coauthor. No girls in the dorm showed any interest in our work however.

My thesis led to three papers in *Physical Review B* journal that were published with Doc. He told me that John Blakemore called him up to congratulate him. Doc had connections to everyone important in the field and this helped over the years in navigating the general physics and the infrared detector field—if I said I was Doc's student it commanded some respect.

After getting my degree in 1970, Doc agreed to keep me on as a postdoc. What a life—I was making more that twice as much as a research assistant! And I loved doing research in the lab. A second sabbatical trip was fit into the postdoc years—a tour of northern Scandinavia on a semi-road worthy dirt bike. My postdoc work involved extrinsic silicon electrical transport and recombination.

One of the things I miss the most about Doc was being able to sit with him at IRIS meetings and listen to his asides on many of the speakers. He had a very sharp sense of humor—generally kind—but one that did not suffer fools, at least in confidential remarks. As a consultant to many companies, Henry was sometimes presented with nonsense ideas. He left a number of proponents in shambles from the stories I heard from him and from others—without pointing out the 'failure to comprehend' explicitly.

Another endearing memory is stopping into his office almost daily—the dewar was usually pumping down in the late morning and one had to go to the second floor of the new physics building to check for mail too. These visits usually revolved around his latest toy, a semi-silent conversation with Thad—Thad had an almost inaudible voice and would be on the phone for extended periods without saying anything. Doc thought that Thad was calling sometimes just to document that he was keeping up on things, but much of the phone call was silent and Doc would be saying "Thad, are you there?" Sometimes the topic would actually be my research—my thesis was on copper-doped germanium crystals and I recall being puzzled when one of them came out poly-crystalline. Doc asked how much copper I had put in—too much it turns out—so I learned something about the maximum solubility of that dopant.

One year I recall there was an IRIS meeting in San Diego at NOSC. Doc, Thad, and I drove back to Los Angeles after the meeting and Henry directed me to a store in LA that sold music boxes. Doc liked collecting these

On my second sabbatical trip in Trondheim, Norway

mechanical music machines and had several in his house on East Genesee Street. There was one particularly spectacular large music box at this store and Doc asked about it even though it was maybe a little out of his range. He was told that it came from a "house of ill-repute in Skaneateles, NY" not far from Syracuse. Doc liked to tell that story.

The University of California at Santa Barbara held an infrared technology course every summer and Doc was the detector professor. This led to annual reunions with his west-coast students.

In the early 1980s, after Doc retired and his contract had ended—I was now at the Santa Barbara Research Center (SBRC)—I got a call from Thad. Thad asked whether I might be able to hire Doc as a consultant on a classified contract so that he could maintain his security clearance and be able to attend IRIS meetings and continue advising Thad on the technology. This was done—I hired Doc as an expert on 1/f noise, knowing that the subject wouldn't ever go away. Doc came to visit us and we had another west-coast reunion. The arrangement subsequently ran into a buzz saw, however. Doc went to an IRIS meeting and wrote a trip report to me for the consulting contract—actually it was intended for Thad. Anyway, he wrote it on his home typewriter, marked it "SECRET" and sent to SBRC. I got the call from security. Turns out that E. Genesee St. wasn't a valid location to generate a secret report. As I heard the story, the FBI went in and confiscated the typewriter ribbon. Pity is that security destroyed the document, so I only had one chance to read it and I'm afraid that Thad didn't get to see it at all.

Thinking back—Doc used to eat an apple, including the core, seeds, and stem—the whole thing. I now appreciate what I thought was a little odd as a reflection of the hard times that he and his family had growing up in Germany under a legacy of persecution. That legacy I think also led to him replacing the windows of his mother's house in Syracuse with lexan. I couldn't understand that at the time, but his family experience with *Kristallnacht* makes it understandable.

After his heart attack Doc came out to Santa Barbara one more time to teach detectors at the UCSB course. Don Smith held a reunion for Doc and his local alumni. That was the last time I saw Doc, but there still are a lot of great memories that I think of often and that bring a smile and sometimes regret that Doc can't share in some of today's technology advances that he helped to initiate.

John Keem (BS Physics, 1970)

I was in the class of '70 at Syracuse. I was a physics major from the first day there in September of 1966, and Doc was my adviser. I saw Doc a few times a week for all four years at SU. I stopped by his office near the physics main office most days to say Hi and check in.

He rarely looked for me, but he was always around when I went looking for him. He came to my wedding and he advised me to go to Purdue for graduate school.

I took his electronics class and learned what 'any child knew' (one of his favorite sayings in the lab part of the class) about circuits and measurements, as well as how to balance a bridge with two hands! He was a great teacher. His style was humorous and enthusiastic, he always looked tickled about everything he taught. This accounted for most of my direct contact with Doc.

As with all the rest, Doc had a light touch with me, but even with his light touch, his impact on my professional life was profound. Perhaps his greatest influence on me came indirectly through how he trained and dealt with his graduate students.

For various reasons, Doc offered me a job as a lab assistant my freshman year, and I snapped at the chance. I began working immediately for the princely sum of $10.00 per week ($1.00/hr.). As a lab assistant, I started under the stadium with John Stannard and Paul Norton, but the second subbasement of the new physics building was my real home. For the rest of my undergraduate career, I was in the second subbasement almost every day. There, I mostly worked for Paul Norton and Paul Chia.

I learned to grow crystals from Norton (Au-doped Ge), I did dip tube measurements, photoconductivity, photo hall and almost learned how to do spectral measurements.

Working in the subbasement with Norton and Chia (directly and indirectly under Doc) was a formative experience for me as an experimental physicist. I learned material preparation, data analysis on big pads of multi-column paper (the original spreadsheet!), hardware construction including working with quartz at the glass bench in the lab and with metals in the machine shop, instrument assembly (we wired up our own sample probes etc). I wrote an undergraduate thesis on photo response of gold doped germanium. As I recall I determined the level energy and possibly measured the hall mobility as well. During Norton's trip out west, I stayed in the lab and did measurements for him. He checked in on me by land line phone, no internet and no cell phones at that time.

Norton, Chia and Stannard (John) were also a big part or my social life at SU. John invited me to go climbing with him in the 'Gunks where he lead me up my first climb, Shockley's Ceiling!! I think I was a bit of a smart ass, so I was absolutely shocked by that first climb with its overhanging move on the last pitch. I did go with him one more time, but I never really got over the first shock. Norton and I and two coeds went hiking in the high peaks of the Adirondacks one fall weekend. Norton, Chia and I often ate dinner at the Drumlins or Webers and worked late into the night. In my senior year I rented an apartment in the same house as Norton. Doc, his lab and his students were really a kind of a family for me, in an important way I am what I am because of Doc, he was my teacher in the deepest sense. I am very grateful to have had the chance to be his student.

Joyce A. Roberts (B.S. 1971)

I came to Syracuse University as an undergraduate majoring in Physics in 1967. In my second year I was looking for a way to get some real laboratory experience and I was fortunate enough to land a position in Dr. Levinstein's laboratory. I worked there for three years until I left Syracuse. I mainly worked with Paul Norton and Paul Chia, and a little with Art Lockwood. It was a great experience for me to be in a laboratory setting with a great mentor like Dr. Levinstein. I learned a lot about infrared semiconductors, even got to see the graduate students play with semiconductor lasers. Mostly I was taught how to make Hall measurements and D* measurements on graduate student samples. Despite the mistakes I made along the way, I was always treated with kindness. I was even lucky enough to get my name on a published paper on a computer program during this period—my first. While I didn't stay at Syracuse past my BS degree and I did not study solid state physics in graduate school, I will always remember the opportunity given my Dr. Levinstein to have a true laboratory experience prior to graduate school.

Paul LoVecchio (Ph.D 1972)

I first met Doc when I was a sophomore undergraduate taking one of his courses. I was a commuter at the time and also had a paper route of about 110 customers to earn some extra money. Winters in Syracuse, New York were brutal with an average of 120 inches of snow. Therefore, toward the end of my sophomore year when Doc offered me a job as a research assistant in his lab, I jumped at it. Imagine doing interesting research in a laboratory heated in the winter and cooled in the summer! Thus was the fortuitous chance that led me into the field of infrared detectors where I would remain for my entire professional career.

As a research assistant I was mentored by, and assisted, Joe Wrobel in zone refining and doping germanium crystals to be used for making infrared detectors. At this time the lab was under the grandstand of Archibald Stadium. Joe taught me how to prepare all of the parts needed for the zone refining and doping. I thought it strange that while emphasizing cleanliness with Kim Wipes spread over any surfaces the parts would touch after etching, Joe would be happily smoking a cigar while he worked. His response to my obvious question was that the cigar smoke contained trace amounts of arsenic which, acting as a donor in germanium, would compensate the residual shallow acceptor impurities and allow the deeper intentionally-added acceptors to be electrically active. To his credit the germanium crystals generally made good infrared detectors. I was never sure how much of a role the cigar smoke played.

During my time as an undergraduate research assistant I also remember assisting Frank Renda characterizing the lifetime of the then new HgCdTe photoconductors which we had received from, as I recall, Sam Stein of Fort Monmouth. We used a carbon arc as the light source and a rapidly spinning mirror driven by a loud air-turbine to flash the light across the detector. An oscilloscope was used to measure the rise and fall time of the detector response. We also used a conventional blackbody set up to measure detector sensitivity and a prism spectrometer for spectral response measurments. These results along with the detectors were sent back to Sam Stein and in turn Doc received a small sum of money for each detector characterized. Each year this little pot of money, called the cell fund, was used to fund a lab picnic. These years as an undergraduate research assistant in Doc's lab were very enjoyable. They provided a good learning experience into the experimental techniques associated with growing infrared detector materials and characterizing the infrared detectors. I also learned the great value of being around such talented and supporting people as Carl Stannard Jr., John Stannard, Claude Penchina, John Pehek, Peter Bratt and Sherman Golub. Others to be mentioned later were important support resources during my later graduate research in Doc's lab.

Upon graduation with my B.S. I had a choice between Brown University and the University of Illinois for graduate school. Doc recommended Brown University as being more suitable for me. However, I had received a teaching assistantship from the University of Illinois and therefore decided to leave for Illinois in the fall of 1964. It didn't take long for me to realize that Doc was right and I headed back to my home in Syracuse toward the end of the first semester. Doc offered to help get me a research assistantship in his lab while I pursued a Ph.D. at SU. I was and will always be extremely grateful for his support of me at this time of my life and career.

Embarking on a successful thesis in Doc's lab was not easy. Doc gave graduate students the general direction to do something new in the field of infrared detectors. That was it! He left it to them to browse through the literature and talk to his other graduate students doing their theses before settling on a thesis topic. I had two false starts, each taking a year or more before I hit upon a successful topic involving the pairing of dopants in germanium. My aim was to find an alternative to Hg-doped germanium for infrared detectors sensitive in the 8-12 μm region. Hg-doped germanium detectors operated at 28 Kelvin requiring a large energy-intensive mechanical cooler or liquid argon. I was hoping to find a dopant in germanium that would still give sensitivity in the 8-12 micrometer spectral region but operate at a higher temperature. I found that lithium diffused into manganese–doped germanium resulted in a "pseudo-dopant" with the desired activation energy of 0.1 eV and gave a spectral response in the 8-12 μm region. The amount of manganese that I could incorporate by zone leveling, however, was insufficient for a decent quantum efficiency so these detectors were never developed further in the industry. Nevertheless, the research was original enough that I did receive my Ph.D. in 1972 and had the pleasure of climbing "Mrs. Jenning's wooden leg" to attach a plaque with my name, date and thesis topic on it.

In the course of doing my thesis research many people, in addition to Doc, supported me. Kevin Riley, then an undergraduate, was a great help preparing germanium samples for characterization. His dry sense of humor kept me sane during stressful times. Other graduate students researchers supported me with advice and encouragement. Paul Norton, in particular, helped me with encouragement at a critical time. Support and congenial discussions at all hours of the day and night were had with Frank Renda, Art Lockwood, Paul Chia, John Stannard, Sherm Golub, Wayne Rudolf and Alan Tanenbaum. Due to this supportive, family-like environment in Doc's lab through the years, we looked forward to the earlier IRIS and now MSS Conferences to catch-up with each other on work and family status. We all felt very fortunate to be a part of such a wonderful environment for which we had Doc to be thankful for.

I was honored in 2010 when The Levinstein Award was given to me by MSS. For me it was a recognition not only of my own achievements, but the support I received from Henry and our larger SU infrared "family."

Moshe Lanir (Ph.D. 1976)

I was the last Ph.D. candidate Doc accepted as a student. I was introduced to him by Kevin Riley who was my neighbor at the graduate married student housing at Syracuse University. Upon approaching Doc, he suggested that I talk to members of his group and get a better understanding how the lab operated and the type of research being conducted there. At the time, the group consisted of Paul Norton (post doc), Art Lockwood (who was writing his thesis), Tim Braggins and Kevin. Doc "suggested" that if I was interested in pursuing my research under him, I might want to consider taking over Art's work on lead-in-telluride. And so I did.

I remember Doc as always being very friendly, approachable, and caring for his students. He liked very much to see the members of his group getting along with each other and not bothering him with lab "politics." I recall an incident when an equipment priority issue was brought up to him for resolution; instead of addressing the conflict, Doc started to tell us about his latest improvement in his physics of toy course, giving us the opportunity to work out the issue amongst ourselves. I believe that was an important part of the education we all got working for him.

His care for his students extended beyond the graduate program at Syracuse University. He always wanted to make sure that prior to graduation, his advisees had jobs lined up for them. Being on a student visa, I encountered difficulties in obtaining a job offer before getting my degree. Doc was very concerned with my situation during that period, and made several phone calls on my behalf until a job offer came through. Like with his other graduates, he continued to inquire about my professional career over the years, and was visibly happy to hear that things went very well.

Kevin Riley (Ph.D. 1978)

Upon graduation from high school in 1967, I elected to attend Syracuse University as a dual major in Physics and Engineering. As a seventeen year old freshman I was assigned an undergraduate advisor, Professor Henry Levinstein. It was in the fall of 1967 that I stood outside his 2nd floor office adjoining the office area of the Physics Dept itself. My first meeting with him began a course that has defined the rest of my life; I am indeed ever indebted to "Doc" in so many ways. Doc's first input to me was that for a "smart" kid, I was sure "dumb" to be pursuing a dual BS and especially one in Engineering suggesting I focus on Physics and to consider pursuit of advanced degrees. In my ensuing freshman year I had periodic meetings with Doc and as my freshman year ended Doc suggested that I work as an undergraduate in support of his graduate students. I began working in the basement IR labs in 1968. As I entered Doc's labs I met Paul Norton, Paul Chia, Al Tannebaum, Wayne Rudolf, Frank Renda and Paul Lovecchio. I had the opportunity to support Paul Lovecchio. In those years Tim Braggins and John Keem also supported other graduate students in Doc's labs.

In my initial years I assisted Paul Lovecchio learning to purify Ge and grow doped single crystals, make electrical and optical measurements such as Hall effect measurements, IR responsivity and crunch analysis numbers on a mechanical 10 digit calculator or fortran punch cards. I can still remember Doc coming down to the labs to talk with each of his students. I continued to work as an undergraduate throught the 1968-1971 timeframe. During this time I came to appreciate the 'living history' and impact of Doc, learning of his pioneering work in IR and the stories of preceeding generations of graduate students including Don Bode, Seb Borello, Al McRae and many others. I grew to appreciate the specialness of the program including the sponsorship of ARFL under Thad Picanpaugh. I also met a very young Bill Rogatto of AFRL when he spent a few weeks in Doc's lab learning the art and science of IR detectors.

Upon graduation with my BS degree in Physics I elected to stay and join Doc's program as graduate student. In these years the program had shifted from extensive research primarily in extrinsic detectors to a focus on intrinsic. Paul Chia was the first to study PbSnTe followed by Art Lockwood and Moshe Lanir. I took on HgCdTe—growing single crystals via solid state recrystalization. During these years David Bishop was an undergraduate who worked in Doc's lab. David would later attend Cornell and go on to a successful career at Bell Labs.

I attended my first IRIS (now MSS) in 1972 and grew to appreciate the 'front row' of Doc, Don Bode (SBRC), Nate Scalar (Rockwell) and George Pruitt (TI) as well as the hospitality suite of Art Cousins where all the IR war stories were told and re-told. I received my PhD from Syracuse and Doc in 1978.

In these years we also saw Doc's love of toys become a passion which provided the foundation of his course and lectures on the Physics of Toys. Doc was always coming to the lab with his latest toy to expound on its uniqueness and his "toy" lab began to expand and become a passion. Doc at various times would also delight in showing his ability to touch his nose with the tip of his tongue or while in conversation utter "sheesh" at something that surprised him. In these years there were numerous summertime picnics or lunchtime whiffle ball games on the Quad as well as occasional wheeled desk chair races around the basement. All of his students worked hard and played hard. Personal friendships developed and deepened and I wish to acknowledge the lifetime friendship that developed amongst all of the students I collaborated with but especially Paul Lovecchio, Art Lockwood and Moshe Lanir.

Doc's legacy is multidimensional. First and foremost he led a program of students whom he cared about personally and deeply. His guidance was candid, personal and balanced with pragmatic realities. Secondly his IR program at SU literally created and provided a foundation to what was to become a multibillion dollar industry. His students significantly contributed to the subsequent scientific and technological developments. SBRC, TI, Honeywell, and Rockwell all benefited from contributons of Doc's students as they entered industry. His program provided the scientific as well as the technological foundation to space-based Pb salt detectors, tactical and strategic extrinsic Ge and Si detectors and the subsequent development of second generation intrinsics such as PbSnTe and HgCdTe. His legacy is in first principals, his legacy is in the students he recruited and developed, his legacy is in the personal advice he gave to government sponsors and his legacy is in the technological foundation. His legacy is also in the subsequent networking of his students throughout their professional careers. I owe much to his impact on my life and I am sure the government appreciates the impact of his legacy in shaping the defense of our country. Doc made a difference to all of us.

I began my career (although I was unaware of this future) as I entered Doc's office in the fall of 1967. Don Bode was Doc's first graduate student and I was his last PhD student. In 2009 I was honored to receive The MSS Henry Levinstein award. My time with Doc was a decade but his impact upon me has been throughout the lifetime of my professional career.

Don Smith (an admirer)

I first met Doc in 1957 when I was at ECA. We were both at an IR get together at Cornell U. He came over to me and said he was astonished that anyone from ECA attended the meeting since we were so secretive about all our work on PbS. He advised me to give up PbS and switch to InSb and InAs if I wanted to participate in the future of IR. We became close friends and I kidded him every time we landed a big PbS program.

He was delighted when I left ECA for SBRC (1963) and told me that finally I would be working where they worked on the detectors of the future. I told him we would fund them with the profits from PbS. I was very honored that he always referred anyone that called him about PbS to me. He was truly one of a kind. As an aside, I see you copied George Pruett. I haven't seen him in ages. I always remember him at the Iris meeting where I gave a paper on PbS and I had invented some ridiculous name for "trapping mode" and referred to it about 10 times in the talk. (I don't even remember what it was). When I finished the talk, George raised his hand and said, "Don, have you ever thought of calling it trapping mode?" Much laughter from the audience, especially Bode.

Indeed, my thought was to give trapping mode a more fancy name. The last laugh was on me! Incidentally, Bode was session chairman and he introduced me as a chemist trying to keep up with the physicists. For my opening remarks I said, "I'm tired of you physicists making slight of us chemists, here is a picture of my lab" and threw it up as the first slide. The picture had a cauldron, a witch with a broom, bat wings and a few other such things. Doc came up to me after the session and asked to borrow it—I gave it to him for keeps. He told me years later that he always used it in his classes.

As Project Director of this and preceeding contracts beginning in 1948, I would like to thank my students both past and present for their efforts. Without them, this work would not have been possible. I would also like to express my appreciation to the administrators at Wright Patterson AFB who, over the years, have approved contracts and permitted them to continue uninterrupted until 1975. Above all I would like to thank Mr. Thad Pickenpaugh who, as project engineer, has helped me when he could, has encouraged me when I needed encouragement and has seen to it that our work has gone on smoothly and successfully.

Henry Levinstein

Project Director

SUMMARY

Reflecting upon all the years of research and development in his laboratory at Syracuse University, Henry Levinstein was very appreciative of the students who had carried out the research and the Air Force contract administrators who supported the work. His words at the end of the forward in the 1976 Final Report illustrate this:

As can be seen from the history above, Prof. Henry Levinstein was an unusual combination of individual scientific contributor, enthusiastic teacher who loved physics, and mentor who gently guided his graduate students into learning what true research was like, where the solutions to problems and indeed the problems themselves are not given to you. Finally, "Doc" was a warm caring father and husband as well as a continuing personal friend to his extended graduate student "family." All of us who knew him are much the better for his friendship and mentoring in our lives.

REUNIONS

In July 2007 there was a reunion of Doc's students at Paul Chia's house in Santa Barbara. On the following page is a photos of the group.

Another gathering in Syracuse was held in 2010 to honor Arnie Honig. A few suspects from Doc's lab attended that event, such as Jay Zemel and Paul Norton. A report is here:
http://www.phy.syr.edu/PhysicsMatters/Volume%205/Volume%205.pdf

REFERENCES:

1. Maxwell KRASNO, IN MEMORIAM DR. NIEL F. BEARDSLEY, Proc. IRIS, 6, (1961). [Declassified January 6, 2010: Code 5596.3, Research Reports Library, Naval Research Laboratory at the request of Teresa Newton-Terres on behalf of the MARIE Project and the MARIE Commemoration Event www.MARIEevent.com, The Proceedings from the Infrared Imaging Symposium (Proc. IRIS) is a publication of the Office of Naval Research, Boston, MA.]

2007 reunion of Doc's students in Santa Barbara at Paul Chia's house.

2. Mrs. Jennings was the physics department secretary. The physics department safe was near her desk and required a strong wooden pole under the floor to support it. Thus came about the term "Mrs Jennings wooden leg." This pole was used in a tradition carried out after each of the experimental physics students in the department received their Ph.D. degree. Charlie Johnston, the head of the Physics Department machine shop, initiated this ceremony and presided over the proceedings. The student would put on telephone line-man's gear, climb the pole and attach a brass plaque with his name, thesis topic and date. This event was Charlie's way to honor the experimentalists who benefited from his group's design and fabrication of their equipment. However, he did not ignore the theoreticians. Their names were memorialized on this pole with a plaque located with a number that was a multiple of 13. I (A. U. MacRae) well remember when Professor Peter Bergmann, well known world-wide as the "grandfather of relativity" following the death of Albert Einstein, climbed the pole and affixed plaque number 13 with the names of the numerous theoreticians who had received their PhD up to that date.

APPENDIX:

a) Levinstein Award recipients:

1981	Sebastion Borrello	TI
1982	Edward Hutchenson	NVESD
1983	Henry Levinstein	Syracuse U
1984	Wayne Grant	NVESD
1985	Thad Pickenpaugh Don Bode Lum Eisenman George Pruitt	WPAFB SBRC SPWAR TI
1986	Forney Hoke	BMD
1987	Joseph Longo	RSC
1989	William Frederick	WPAFB
1990	William Rogatto	SBRC
1991	Charles Freeman	NVESD
1992	Gordon Griffith Richard Reynolds	WPAFB HRL/DARPA

1993	William Tennant	RSC
1994	Raymond Balcerak	DARPA
1995	Joseph Killiany	NRL
1996	John Pollard	NVESD
1997	Freeman Shepherd	RADC
1998	Mark Gurnee	LMIRIS
1999	Paul Kruse	Honeywell
2001	A. Fenner Milton	NVESD
2002	William J. Parish	Indigo
2003	Arthur Lockwood Richard Schoolar	Raytheon Aerospace
2004	Marion Reine	BAE
2005	James Robinson	DRS
2006	James Waterman	NRL
2008	Mike Kinch	DRS
2009	Kevin Riley	Teledyne
2010	Paul LoVecchio	BAE
2011	Lyn Brown Roger De- Wames	WPAFB NVESD
2012	Mel Kruer	NRL

b) Early Journal Publications by Henry Levinstein

Phys. Rev. 74, 500 (1948)
A Dynamic Method For The Determination Of The Velocity Distribution Of Thermal Atoms —with Irving L. Kofsky

Journ. Appl. Phys. 20, 306-315, (1949)
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